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Cultural Proximity and Loan Outcomes†

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We present evidence that cultural proximity (shared codes, beliefs, ethnicity) between lenders and borrowers increases the quantity of credit and reduces default. We identify in-group lending using dyadic data on religion and caste for officers and borrowers from an Indian bank, and a rotation policy that induces exogenous matching between them. Having an in-group officer increases credit access and loan size dispersion, reduces collateral requirements, and induces better repayment even after the in-group officer leaves. We consider a range of explanations and suggest that the findings are most easily explained by cultural proximity serving to mitigate information frictions in lending. (*JEL* D82, D83, G21, G28, O16, Z12, Z13)

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 of cultural proximity on the quality and efficiency of transactions where parties are asymmetrically informed is ambiguous, however. There are two prominent explanations, both of which predict a higher level of transactions between culturally proximate parties, but with divergent predictions on the economic value of these transactions. 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 resource misallocation. Alternatively, if cultural proximity reduces asymmetric information by, for example, reducing the cost of communication or contract enforcement, in-group transactions may create more economic value for both parties. Given these opposing forces, the effect of

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cultural proximity on outcomes in markets with asymmetrically informed participants is an empirical question, and the focus of our analysis in this paper.

There are a number of challenges in empirically examining the various consequences 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 been conducted at a high level of aggregation (as in Guiso, Sapienza, and Zingales 2009, for example); or they 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 improvement in outcomes from 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 expected profitability of transactions, 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 distortions introduced by information frictions in many 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, a setting that is well-suited to studying the consequences of, and rationales for, preferential in-group treatment in individual interactions with private information. The setting makes it possible to better confront 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 almost 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, using detailed records on loan characteristics and their ex post performance we can identify the degree to which cultural proximity may reduce the rationing of credit, the main distortion that arises in the face of severe information asymmetries (Stiglitz and Weiss 1981). In addition to the econometric advantages of our data, understanding the link between culture and information frictions is 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 as belonging to the same group when both are members of the same minority religion (Christian, Muslim, Sikh, Parsi, or Buddhist) or, conditional on belonging to the majority religion (Hindu), when both belong to the same official caste (General Class, Scheduled Caste, Scheduled Tribe, or Other Backward

[1] For evidence and discussions, see Banerjee, Cole, and Duflo (2004); Banerjee and Duflo (2014); and Field et al. (2008).

Classes). In our preferred (and most conservative) specifications, we find that on average, the total amount of new loans to borrowers in a group increases by 6.5 percent when the officer assigned to the branch belongs to the same group. Having an in-group officer also increases the number of new loan recipients by 5.7 percent and the probability that a member of the group receives any credit by 2.5 percent. The inclusion of branch-quarter, district-group-quarter, and group-branch fixed effects in our analysis 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 our findings result from systematic errors in the classification of individuals in the bank's records, or by targeted lending policies based on this classification.

We then go on to examine the effect of in-group lending on the quality of credit provision and the cost of credit to borrowers. Loans made to in-group borrowers have better repayment performance ex post. The economic magnitude of this effect is large: in-group borrowers are 0.6 percentage points less likely to be late in loan payments, a 7 percent decline relative to the average default probability in the sample (8.6 percent). The decline in default probability persists even after the in-group officer is replaced by an out-group one, implying that the effect on loan quality is not driven by officers rolling over loans to bad borrowers (*evergreening*). Since all loans have the same interest rate, we evaluate the effect of proximity on the cost of borrowing through its impact on the collateral required per rupee of credit. We find that cultural proximity lowers the cost of borrowing measured this way. Under mild assumptions, our credit quality and quantity results also imply that cultural proximity improves the profitability of lending.[2]

Standard models of in-group favoritism predict resource misallocation, as transacting agents trade off efficiency against higher payoffs from the utility gain of favoring their in-group counterparts. In our context this would imply loan officers bearing the cost of higher default rates in exchange for lending more to their own group. In contrast, standard models of credit markets with asymmetric information predict that, if cultural ties reduce information asymmetries (either ex ante because of better communication or ex post because of better enforcement), they should lead to less credit rationing, e.g., more and cheaper credit to lower risk borrowers. The prima facie evidence from our main results is consistent with the latter type of models.

We emphasize that our data do not allow us to rule out all favoritism-based explanations. In particular, the distinction we describe in the previous paragraph focuses on types of favoritism that generate lower performance for the loan officer, which captures the cost he bears for favoring his own type. We offer some additional results that help to further evaluate our rationing interpretation, in particular repeating our

[2] Specifically, we require the further assumption that government banks in India lend too little relative to their marginal cost of capital. This view finds empirical support in Banerjee, Cole, and Duflo (2004) and Banerjee and Duflo (2014).

estimation using only branches in districts where the bank we study is the only formal lender. We find that cultural proximity leads to an expansion of credit even in areas where the only alternative source of funds are expensive moneylenders. In these areas, borrowers cannot simply substitute across similarly priced funding sources. This reinforces the interpretation that cultural proximity may reduce credit rationing.[3]

In a final set of analyses, we look at the specific predictions of existing models of information frictions in lending, and examine the extent to which these predictions are borne out in the data. Cornell and Welch (1996) suggest that cultural proximity may reduce information asymmetries in a credit transaction by improving the precision of the signal that the officer obtains of a borrower's creditworthiness. A direct prediction of their model is that cultural proximity should increase the variance of loan sizes, as the officer's distribution of prior beliefs of borrower quality widens with the more precise signal. Consistent with this prediction, we find that in-group loans have a substantially larger size dispersion than out-group ones. Rajan (1992) argues that the repeated interaction between a lender and a borrower provides the lender with "soft" private information about a borrower's credit quality. We can examine the role of repeated interaction by looking at heterogeneity of the in-group effect across first-time and existing borrowers, and over time as the loan officer and the borrower interact. We find that officers expand lending to in-group borrowers that have a prior relationship with the bank as well as to new borrowers, and that the credit expansion occurs immediately on the officer's arrival at a branch and persists throughout his tenure. These results suggest that the benefits of cultural proximity are distinct from and additive to those that potentially derive from a borrower's observable track record with the lending institution or those that come from repeated interaction between officer and borrower.

Our main conclusion, that cultural proximity improves the quantity, quality, and cost of lending, has a number of economic and policy implications that are independent of the specific mechanism through which it operates. The first is that the net positive effect of cultural proximity on credit outcomes can be mistaken for discrimination in the data, since minority borrowers are far less likely to be "matched" with an in-group lender than borrowers that belong to a large group. A naïve regression of loan access on borrower group identity that ignores the group identity of the lender would indicate discrimination against minorities rather than preferential in-group treatment among all groups. This calls for caution in the interpretation of, and policy prescriptions that can be derived from, empirical studies that identify differential treatment based solely on the identity of one of the parties of the transaction. This point is raised theoretically in Cornell and Welch (1996).[4]

[3] Our estimates of the effect are also significant and of comparable magnitude in areas where there are many alternative sources of formal funding. In these areas, however, we do not have means of pinning down whether or not cultural proximity alleviates credit rationing.

[4] 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 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, Levine, and Zimmerman 2003), trade credit (Fafchamps 2000, Fisman 2003), and online person-to-person lending (Pope and Sydnor 2011).

A second, related implication is that a naïve data analysis may also lead to the formation of statistical discrimination against minority groups. Even if all groups in the population have the same ex ante average propensity to default, minority group borrowers will have a worse credit repayment history because they are more likely to be matched to an out-group officer. Since all consumer credit scoring models ignore the identity of the lender, minorities will face higher borrowing costs in the marketplace purely due 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. If statistical discrimination is a consequence of in-group preferential treatment, a policy that increases the likelihood of a group match between lenders and borrowers would unambiguously improve the efficiency of credit allocation in consumer credit markets.[5]

Prior work that examines the role of group identity using dyadic data finds mixed results. Ayres and Siegelman (1995) finds evidence of race and gender discrimination in an audit study of price bargaining in the US new car market, but finds no evidence of in-group preferential treatment. In contrast, Price and Wolfers (2010) and Parsons et al. (2011) find evidence consistent with race-based discrimination among NBA referees and MLB umpires, and Schoar, Iyer, and Kumar (2008) find that sellers bargain for lower prices when the seller belongs to the same community in an audit study in India. The key contribution of our study is to provide evidence of the effect of cultural proximity in a context where the negative effects of animus-based discrimination may be countered by the positive effects of better information exchange and enforcement.

Our paper also relates to the literature on the economic consequences of social ties between transacting parties.[6] Our results indicate that cultural proximity increases the likelihood of two individuals who have likely never met interacting in the market and forming a tie. This suggests that social ties are endogenously formed as a consequence of common cultural endowments. The effects of cultural endowments and social ties are typically confounded in existing work that associates endogenous past social interactions with future market transactions.[7] 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 that we document in our analyses can persist in the long run, and potentially perpetuate inequality.

Finally, our study relates to the literature on the role of soft information in economic transactions. Much of this research has focused on credit relationships, and indeed many of the outcomes that we examine—the extent and price of credit, as

[5] 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.

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

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

well as loan quality—are also the focus of this literature (see, for example, Keys et al. 2010; Petersen and Rajan 1994). We note, however, that owing to officer rotation, we are better able to identify the effects of soft information relative to papers that have used time-invariant proxies for soft information between lenders and borrowers.

In the next section, we provide a brief discussion of models of lending under conditions of asymmetric information, and discuss the implications for the analysis that follows. Then, Section II, we turn to providing an overview of the data and a description of the Indian bank that we study—its organization, the incentives of its officers, and so forth. In Section III we present the baseline empirical specification for the analysis. Section IV presents our results on lending quantity and quality; Section V presents an additional set of results on loan dispersion, and heterogeneity by borrower, officer, and branch characteristics, which we use to explore which prominent models of in-group lending are consistent with the data. In Section VI we conclude with some policy implications and directions for future work.

I. Theoretical Background and Motivation

In the discussion that follows, we will lay out the comparative statics provided by prominent models of information asymmetries in credit markets, describing how we expect loan quantity, repayment rates, and loan dispersion to be affected by reduced informational frictions. These frictions may be either ex ante, relating to the lender's ability to assess project quality, or ex post, affecting the lender's ability to ensure that funds are used as agreed and that the loan is repaid. We will take whether bank officer and borrower are of the same group—henceforth referred to as *SameGroup*—as a measure of cultural proximity, and consider the empirical predictions of a model where cultural ties reduce information asymmetries. It is beyond the scope of our analysis to decisively pin down the mechanism through which cultural proximity affects lending, but we can explore the extent to which our data are consistent with these existing models.

In the canonical models of credit markets under asymmetric information, the effect of better information on credit access is ambiguous. For example, asymmetric information may increase or decrease the level of credit depending on whether borrowers have private information on the level or the variance of project returns (De Meza and Webb 1987, Stiglitz and Weiss 1981). At the extreme, a lender that faces severe information asymmetry will pool all borrowers, which may lead to a larger amount of credit than in the high-information *SameGroup* case, or to no credit at all. Given this theoretical ambiguity, our empirical exercise can be viewed as an assessment of which type of effect dominates in an important real-world setting.

There is greater agreement across these well-known models in their predictions on the effect of information frictions on default. In both of the cases above, a better-informed lender can screen out low-quality (from the bank's perspective) projects ex ante thus reducing default. Improved information similarly reduces the cost of borrowing for those that receive credit. The emphasis in these models is on screening, but reduced informational frictions can also improve credit access and outcomes by allowing for improved ex post enforcement as documented, for example, by Fafchamps (2000) in ethnic trading networks in sub-Saharan Africa. Thus, a primary pair of predictions shared by these (and other) models of lending in the face of information frictions is that *default rates and the cost of funds are lower for SameGroup borrowers.* Since interest rates are fixed in our setting, we focus on collateral as a measure of borrowing cost (higher risk borrowers will post more collateral holding the interest rate constant), and examine whether collateral to loan ratios are lower for *SameGroup* loans.

Cornell and Welch's (1996) screening model shows that lower information frictions within a group will be reflected in larger loan size dispersion for the group. The reason for this is that better-informed lenders receive more precise signals of creditworthiness, which implies that the variance of the distribution of priors of *SameGroup* borrower quality across borrowers will be greater. Thus, a prediction that may help us to identify the presence of better in-group screening is that *loans to SameGroup borrowers have higher dispersion.*[8] Note that this does not rule out enforcement-based explanations, which make no strong predictions about the ex ante loan size distribution, but will allow us to assess whether our data are consistent with models where better screening plays a role.

We conclude our background discussion with an overview of how models of reduced informational frictions through shared culture contrast with models of preference-based discrimination in the spirit of Becker (1957). Favoritism in this class of models is manifested in the cost that an individual incurs as a result of his in-group preferences. For example, a business owner sacrifices profits or, in our context, a loan officer reduces his performance (and hence career progression) in order to indulge his discriminatory preferences. Such models of *SameGroup* favoritism predict a higher level of *SameGroup* interactions and on more favorable terms—predictions potentially shared by models of information asymmetries—but predict *lower-quality* transactions.[9] In our context, preference based discrimination implies a *higher* default rate for *SameGroup* borrowers—the cost to the loan officer of favoritism. This also highlights some limitations of our analysis—our data are less suited to picking up on the effects of *SameGroup* favoritism that do not entail a cost to the loan officer.

II. Data

The main variables in the analysis are obtained from the individual loan portfolio and personnel records of a large state-owned bank in India, which operates over 2,000 geographically dispersed branches (see Appendix Figure A1). The sample starts in the second quarter of 1999 and ends in the first quarter of 2005. This section describes in detail the structure and construction of the dataset and relevant background information on the organization of the bank itself.

[8] Rajan, Seru, and Vig (2015) use a similar test to show that an increase in "distance" brought about by securitization leads to lower information production on loans.

[9] Parsons et al. (2011), for example, find evidence of racial bias in baseball umpires' strike calls, but only in stadiums where their calls are not monitored, and thus there is no possible sanction to their biased behavior. This represents evidence that agents trade off the benefits of acting according to biased preferences against the potential costs of doing so.

A. *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). Loan contract characteristics and repayment status are reported on a quarterly basis (1.23 million borrowers per quarter on average). Since we are interested in comparing the lending decisions of in-group and out-group officers around officer rotations, we focus our analysis on the flow of new debt from a branch b, to a group g, in quarter q. The bank issued just over 1.8 million new loans during the sample period. The mean (median) new loan amount is 59,681 (22,506) rupees. We focus on flows, given that the incoming officer played no part in the decision to issue a loan prior to their arrival at a branch.[10]

We use the bank's quarterly personnel records to identify the head officer of each branch in each quarter (4,270 distinct officers in total). Loan officers are classified into six grades, with increasing seniority and ability to approve larger loan amounts. The highest-ranking 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 at the head officer's discretion, and based on information collected at the branch level. Officers have control over loan and collateral amounts, but 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 using a range of criteria.[11] 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." Officers are held accountable for loan defaults after moving branches. Typically, officers are responsible 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 are motivated through promotion to higher grades or better postings. As a result, branch heads face strong incentives to issue profitable loans and perform well along other qualitative dimensions that serve as inputs into their evaluations. Since 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.

[10] The analysis in a previous version of the paper focused on the stock of debt: amount of debt outstanding for the borrowers in a group at any point in time. The results are qualitatively similar, but noisier. We thank the editor and an anonymous referee for suggesting this change.

[11] Information on evaluation and compensation of managers within the bank come primarily from interviews with bank staff; we do not have access to individual evaluations.

B. *Religion*, *Official Caste*, *and Religious Caste*

The bank records 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.[12] Although the SC, ST, and OBC categories include a 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 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 Bühler 1886), and a person's surname typically reflects the Jati they belong to. We exploit this link with surnames to classify each individual into their Varna (see Banerjee et al. 2009 for a further discussion of the link between surnames and castes in India). In the Appendix we provide a description of the matching procedure and some specific examples.

Using surnames to classify borrowers and officers by religious caste results in several sources of additional noise and imprecision. First, many surnames can be classified into two or more Varnas.[13] We create three special categories for individuals where this ambiguity arises (Kshatriya-Brahmin, Kshatriya-Brahmin-Vaishya, and Kshatriyas-Vaishyas). We note, however, that within a region there is usually only a single Varna associated with each surname. So once we condition on region—as we do throughout our analysis—there is a clearer link between names and communities. Second, it is unclear how to categorize individuals into the Shudra Varna according to their community affiliations, which precludes using surnames for individuals outside of the General Classes. Finally, in less than 1 percent of cases, the surname-based classification conflicted with the bank classifications assigned to loan officers and borrowers. For example, many bank-classified Muslims had

[12] The categories of Scheduled Caste (SC) and Scheduled Tribes (ST) which 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 years later the category of OBC was extended to include a significant segment of the non-Hindu population, notably Muslims, Christians, and Sikhs.

[13] For example Saxena is grouped under both Brahmins and Kshatriyas. Similarly Desai is grouped under both Brahmins and Vaishyas.

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

	Borrowers (percent)	Head officers (percent)
Panel A. 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 B. By official caste		
General	66.66	74.31
SC	10.67	15.68
ST	6.02	5.12
OBC	16.64	4.89
Panel C. 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

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

Source: Authors' calculations

"Hindu" surnames, and vice-versa. Still, exploring the effect of proximity along the Varna dimension is interesting in 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.

C. *Descriptive Statistics: Group Composition*

The religion, official caste, and Varna compositions of the borrower and officer populations are shown in Table 1. By religion, Hindus represent the majority of borrowers (89.4 percent) and officers (93.8 percent). The largest group of minority borrowers is Muslim (6.33 percent), and the largest officer minority is Christian (2.1 percent). Hindus are over-represented and Muslims under-represented in the borrower and officer populations relative to the total population (80.5 percent Hindu and 13.4 percent Muslim according to the 2001 census). Most borrowers and officers are classified as General Class (66.7 percent and 74.3 percent, respectively). The largest borrower minority is the OBC category (16.6 percent), while the largest officer minority is ST (15.7 percent). SCs are under-represented in the borrower sample and STs under-represented in the officer sample, relative to the population (16.2 percent SC and 8.2 percent ST in the 2001 census).[14]

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

Table 2—Summary Statistics

	Mean	SD	p1	p50	p99
Panel A. Branch-quarter statistics, N = 46,753					
Total new credit (millions of rupees)	2.36	3.74	0.00	1.50	14.22
Number of borrowers	39.6	48.9	0.0	31.0	200.0
Number of different borrower religions	1.85	0.91	0.00	2.00	4.00
Number of different borrower castes	2.35	1.08	0.00	2.00	4.00
Number of different borrower groups	3.18	1.50	0.00	3.00	6.00
Number of loan officers (including head officer)	3.53	4.20	0.00	2.00	16.00
Number of clerks	6.40	7.12	0.00	4.00	31.00
Panel B. Group-branch-quarter statistics, N = 339,366					
Sum new credit (1,000s of rupees)	245.8	1227.9	0.0	0.0	4,183.0
Fraction of branch new credit	0.116	0.259	0.000	0.000	1.000
Number of new credit recipients	4.11	15.13	0.00	0.00	55.00
Fraction of branch number of new credit recipients	0.116	0.249	0.000	0.000	1.000
Standard deviation new credit (1,000s of rupees)	29.9	124.1	0.0	0.0	337.6
IQR new credit (1,000s of rupees)	22.6	96.3	0.0	0.0	312.7
Dummy = 1 if new credit > 0	0.338	0.473	0.000	0.000	1.000
Total collateral/outstanding debt	10.50	633.61	0.33	2.37	14.92
Maturity (years)	2.83	2.95	0.00	2.43	15.00
Interest rate (percent)	10.98	2.22	3.62	11.46	15.50
Fraction borrowers over 60 days late after 1 year	0.086	0.233	0.000	0.000	1.000
Fraction new debt over 60 days late after 1 year	0.036	0.159	0.000	0.000	1.000
SameGroup	0.110	0.312	0.000	0.000	1.000

Notes: Panel A: branch-quarter panel statistics. Panel B: branch-quarter-group panel statistics (group: borrower's religion and caste conditional on Hindu religion). There are nine groups: five minority religions and, conditional on Hindu religion, four government sanctioned castes.

Source: Authors' calculations

We are able to match surnames to Varnas for a subsample of the population. A total of 502,723 borrowers (18.3 percent Brahmin, 60.5 percent Kshatriya, 6.6 percent Vaishya, 1.7 percent mixed categories, and 5.72 percent in other categories) and 1,689 officers (23.0 percent Brahmin, 43.4 percent Kshatriya, 11.7 percent Vaishya, 15.5 percent mixed categories, and 6.4 percent in other categories) have Varna assignments. These represent approximately 17 percent of borrowers and 40 percent of officers in our sample. All identifiable Varnas in our sample belong to the General Class according to official caste definitions. The average size of new loans issued to surname-matched borrowers is 4 percent larger than the average size of loans issued to unmatched borrowers classified as Hindu General Caste in the bank's records. The difference is statistically significant, indicating that the surname-matched sample is comprised of borrowers with access to marginally higher loan amounts relative to the general population of Hindu General Caste borrowers.

D. *Descriptive Statistics: Branches and Groups*

In the average (median) branch quarter, the total flow of new loans is 2.36 (1.50) million rupees, issued to 39.6 (31) borrowers (Table 2, panel A). The borrower composition is generally heterogeneous: the median branch issues new loans to borrowers of two different religions and two different official castes. The median branch is small, with two loan officers including the head officer, and the modal branch has 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. In our main specification we use the full set of religion and caste information to group borrowers into nine categories: five minority religions, and four official castes conditional on the religion being Hindu. In other specifications we consider group definitions based on Varna classifications. The panel employing our main group classifications has 339,366 branch-group-quarter observations (descriptive statistics shown in Table 2, panel B). In the average group-branch-quarter cell the sum of new loans is 245,800 rupees. Not all groups receive new loans in all periods from all branches: only 33.8 percent of the cells have positive debt flow (as a consequence the median group-branch-quarter debt flow is zero).

In order to have a dependent variable that captures both changes in the amount of credit to a group (group-intensive margin) and the probability of a group receiving credit (group-extensive margin), our preferred specifications use the flow of credit to a group in a branch divided by the total flow of new credit allocated by the branch in the same quarter (in what follows we will use the terms *new credit* and *new loans* interchangeably to refer to the flow of new credit issued). The group-branch-quarter average (median) *fraction of branch new credit* is 0.116 (0.0). Similarly, we define *fraction of new loan recipients* as the ratio of the number of new loan recipients in a group-branch-quarter divided by the number of new loan recipients in the same branch quarter (the average is also 0.116).

We use two measures of the cross-sectional dispersion of new loan sizes among the individuals in a group, the standard deviation (SD) and the interquartile range (IQR). The average branch-group-quarter SD and IQR of loans are 29.9 and 22.6 thousand rupees, respectively. The bank records the sum of all the collateral pledged by a borrower at any given time (not the amount of collateral that secures a particular loan). Thus, we report and analyze collateralization based on the stock of credit outstanding to the set of borrowers receiving new loans in each quarter. The median branch-group-quarter ratio of total collateral to total outstanding debt for these borrowers is 2.37. The average maturity is 2.8 years and the average interest rate is 11 percent. New loan recipients must be current in repayments in order to receive a loan. We measure default as the probability of a new loan recipient being over 60 days late (in any loan) 1 year after the loan is issued. The branch-group-quarter average fraction of borrowers that default according to this definition is 8.6 percent.

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 to group g if $g =$ Muslim, and zero for all other groups in that branch quarter. Since this dummy is equal to 1 for one and only one group at any given branch quarter, its sample average is $1/9 = 0.11$ by construction.

E. *Officer Rotation*

The bank follows an explicit policy of geographical officer rotation, with the stated objective of reducing opportunities for corruption, nepotism, and other

Table 3—Empirical and Theoretical Officer Group Transition Rates

	Empirical transition rates (theoretical transition rates)									
To group:	Muslim	Christian	Sikh	Parsi	Buddhist	Others	Hindu/SC	Hindu/ST	Hindu/ OBC	Hindu/ general
From group:										
Muslim	0.0009	0.0006	0.0000	0.0000	0.0000	0.0000	0.0018	0.0009	0.0012	0.0130
	(0.0004)	(0.0004)	(0.0003)	(0.0000)	(0.0000)	(0.0001)	(0.0027)	(0.0008)	(0.0009)	(0.0133)
Christian	0.0003	0.0045	0.0000	0.0000	0.0000	0.0000	0.0033	0.0006	0.0027	0.0166
	(0.0004)	(0.0006)	(0.0004)	(0.0000)	(0.0000)	(0.0001)	(0.0034)	(0.0010)	(0.0011)	(0.0166)
Sikh	0.0003	0.0000	0.0024	0.0000	0.0000	0.0000	0.0024	0.0003	0.0003	0.0109
	(0.0003)	(0.0004)	(0.0003)	(0.0000)	(0.0000)	(0.0001)	(0.0025)	(0.0007)	(0.0008)	(0.0122)
Parsi	0.0000	0.0003	0.0003	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0003
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0004)
Buddhist	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0003	0.0000	0.0000	0.0018
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0003)	(0.0001)	(0.0001)	(0.0015)
Others	0.0003	0.0000	0.0000	0.0000	0.0003	0.0003	0.0000	0.0000	0.0003	0.0009
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0004)	(0.0001)	(0.0001)	(0.0021)
Hindu/SC	0.0030	0.0033	0.0024	0.0000	0.0006	0.0006	0.0344	0.0063	0.0066	0.0781
	(0.0027)	(0.0034)	(0.0025)	(0.0001)	(0.0003)	(0.0004)	(0.0204)	(0.0060)	(0.0068)	(0.1002)
Hindu/ST	0.0009	0.0009	0.0003	0.0000	0.0000	0.0000	0.0054	0.0069	0.0027	0.0178
	(0.0008)	(0.0010)	(0.0007)	(0.0000)	(0.0001)	(0.0001)	(0.0060)	(0.0018)	(0.0020)	(0.0297)
Hindu/OBC	0.0009	0.0015	0.0000	0.0000	0.0000	0.0000	0.0060	0.0027	0.0048	0.0305
	(0.0009)	(0.0011)	(0.0008)	0.0000	(0.0001)	(0.0001)	(0.0068)	(0.0020)	(0.0023)	(0.0334)
Hindu/general	0.0130	0.0145	0.0139	0.0006	0.0012	0.0018	0.0856	0.0232	0.0299	0.5314
	(0.0133)	(0.0166)	(0.0122)	(0.0004)	(0.0015)	(0.0021)	(0.1002)	(0.0297)	(0.0334)	(0.4929)

Notes: In this table we report branch officer empirical transition rates by officer group, based on 3,316 officer transitions. In parentheses are the theoretical transition rates that would result if the officers had been randomly assigned to branches.

Source: Authors' calculations

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 1 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 Table 3 we report the empirical distribution of branch transition rates by group, along with the theoretical distribution of transition rates that would result from random matching across branches nationally. We observe a total of 3,316 officer transitions. To examine the extent to which the empirical distribution differs from that dictated by random assignment, we perform a permutation test as follows: we fill each of 3,316 positions in an array in proportion to the number of officer-quarter observations of that group observed in our data. So, for example, given that 70.2 percent of officer-quarter observations are General Caste Hindus, 2,327 ($0.702 \times 3{,}316$) of the initial positions are assigned to General Caste Hindu. We then randomly permute the ordering of the initial assignment, and use the observed transitions to generate a transition matrix. We repeat this process 1,000 times, and test whether the empirical transition rates fall within the 90, 95, or 99 percent confidence interval of the simulated distribution. A comparison of the empirical and simulated distributions indicates that, overall, there are significantly more within-group transitions than would be expected from random rotation.

The relatively high proportion of within-group transitions is expected since some states in India have reservation policies that require a minimum representation of SC, ST, and OBC officials in any government position. The key assumption of our identification approach, which we discuss in detail in the next subsection, is that the rotation policy induces variation in the matching between officer and borrower group identity that is plausibly uncorrelated with the demand for credit. We will return to the issues raised by possible nonrandom assignment, and the validity of our empirical approach, in Section IV where we present "event study" patterns around officer transitions to show that assignment to an in-group officer is not preceded by abnormal increases or decreases in credit to a group. In addition, our main empirical specification uses saturated regressions that incorporate branch-quarter (which subsume officer fixed-effects), group-quarter, and district-group-quarter dummies that address various selection-based alternative explanations.

III. 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:

$$y_{gbq} = \beta SameGroup_{bgq} + \alpha_{gb} + \tau_{bq} + \sum_{d} \gamma_{gq}^{d} + \epsilon_{bgq}. \tag{1}$$

The dependent variable in most specifications is a lending outcome of a group in a branch quarter, relative to the overall outcome in the same branch quarter. For example, when we analyze the impact on the flow of new credit, the dependent variable is the ratio of new credit to all borrowers that belong to group g in branch b and quarter q to the new credit issued to all groups in branch b in quarter q. *SameGroup* is an indicator variable denoting whether the branch head in branch b belongs to group g in quarter q.

For most of our results, we present specifications that include group-branch (α_{gb}), branch-quarter (τ_{bq}), and district-group-quarter (γ_{gq}^{d}) dummies, where d indexes the district in which the branch is located. This full set of fixed effects helps to rule out a range of identification concerns. The group-branch dummies capture time-invariant attributes of each group within each branch, and ensure that the estimation of β does indeed come from the time series variation induced by officer rotation. The branch-quarter fixed effects 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 only one branch head at a time in each branch, the branch-quarter fixed effects also account for changes in an officer's behavior over time, for example, due to experience or learning. Finally, the district-group-quarter dummies capture shocks to and trends in the demand for credit of specific groups in narrowly defined geographical areas (conditional on having a branch, the median district has three bank branches in our sample). This helps us to rule out the possibility that the estimated β is the result of, for example, reverse causality driven by the endogenous allocation of officers into areas where their own group is thriving. In the estimation we allow the error term ϵ_{bgq} to be clustered at the branch level. This accounts for serial correlation in lending and for the mechanical correlation of *SameGroup* across groups in the same branch.[15]

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 fraction of new credit as the dependent variable and, 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* will be the weighted average of (i) the difference between the fraction of new credit obtained by Hindu borrowers in the branch when the officer is a Hindu (in-group) officer relative to when the officer is a Muslim, and (ii) the difference between the fraction of new credit obtained by Muslim borrowers with a Muslim officer relative to a Hindu one. While our main results show the average effect across all groups, we will also allow the effect of *SameGroup* to vary across religions and castes.

IV. Results: Loan Quantity and Quality

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 Christian, the Christian borrowers are in-group before the officer change, and borrowers from all other religions 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 Christian officer is replaced by a Muslim one. Then, Christian borrowers transition from in-group to out-group, Muslim borrowers transition from out-group to in-group, and other religions remain as out-group throughout. Alternatively, if the replacement officer is also Christian, then Christian 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 changes. The horizontal axis of the plots in Figure 1 measures time in quarters since the officer change in a branch. Event time $t = 0$ represents the first quarter when 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

[15] 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.

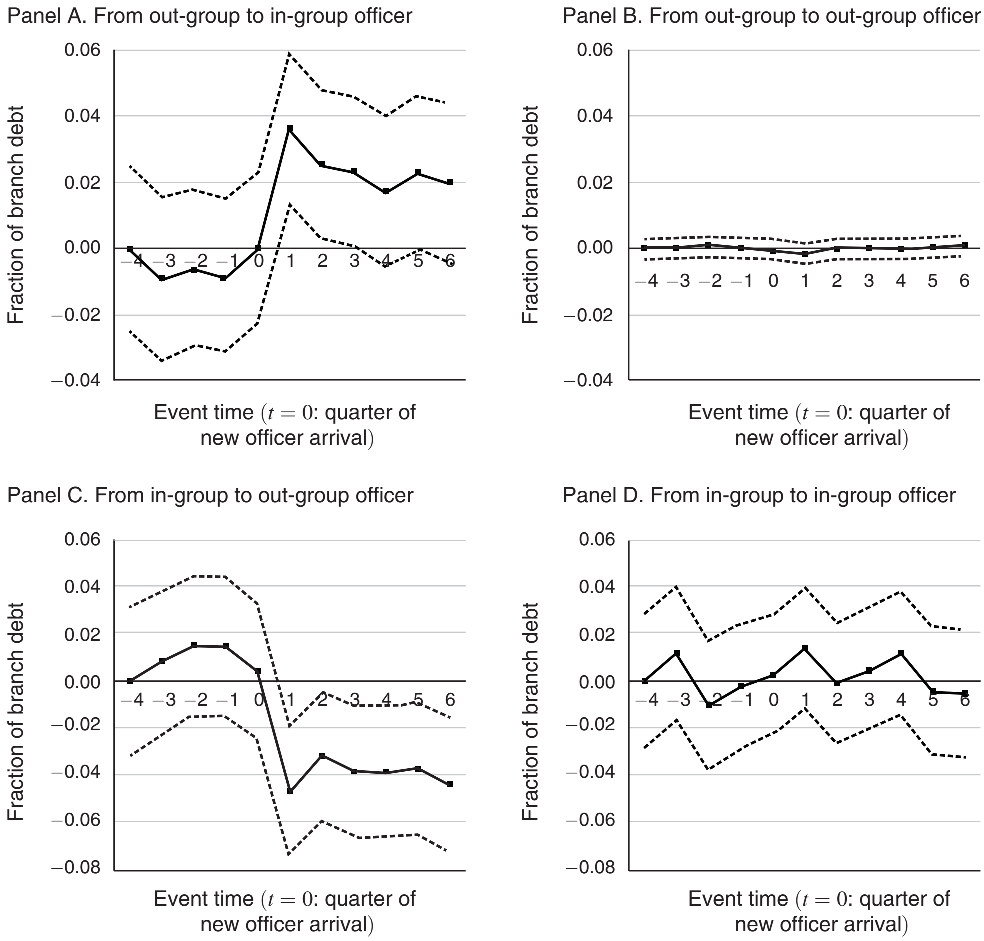

Figure 1. New Credit around Officer Transitions, Partitioned by Group Identity of Officer and Borrowers before and after the Transition

Notes: The horizontal axis measures time, in quarters, since the group experienced an officer change (0 represents the first quarter of the new officer). The vertical axis measures the average fraction of credit to a group, with group-branch means removed, and normalized to zero at $t = -4$. Group is defined based on a classification of borrowers and officers into five minority religions and four government sanctioned castes (conditional on the religion being Hindu). Borrowers are partitioned into four subsamples depending on whether they experience a transition from an out-group to an in-group officer (panel A), an out-group to an out-group officer (panel B), and so on. The dashed lines indicate the 95 percent confidence interval of the mean by quarter.

Source: Authors' calculations

before the observed entry time.[16] The vertical axis measures the average fraction of new debt in a branch allocated to a group of borrowers and the 95 percent confidence interval (with group-branch means removed, and normalized to zero at time $t = -4$ to facilitate visual inspection of the time series). Since officers remain at an assignment for a minimum of two years, we use an eight quarter window around their arrival. This ensures that we do not double-count observations in the figure.

[16] This measurement error in the time of arrival of the new officer will tend to bias toward zero our estimates of the in-group effect in specification (1).

There are, however, fewer observations at points further from $t = 0$ owing to truncation of officer spells at the beginning and end of our sample period.

To construct the plots we partition the borrower groups into four subsamples depending on the type of in-group/out-group status change they experience due to officer rotation. Panel A is constructed for the subsample of borrowers for which an in-group officer replacing an out-group one leads to increased cultural proximity for these borrowers. Panel C contains borrowers who have experienced the opposite transition, from in-group to out-group officer. For these borrowers, the officer change thus led to a decrease in cultural proximity. Panel B (D) is for borrowers who have experienced no change in their out-group (in-group) status, and thus no change in their cultural proximity to the officer.

We highlight three patterns in these plots, as they are strongly suggestive of a causal relationship of cultural proximity on the flow of credit. First, officer changes that involve an increase (decrease) in cultural proximity between the officer and set of borrowers are immediately followed by a statistically significant 3 to 4 percent increase (decrease) in the fraction of new credit allocated to those borrowers. Second, officer changes that do not affect officer-borrower cultural proximity are not followed by changes in the flow of credit (implying that our estimates are not confounded by a "new officer" effect).[17] Third, there is no pre-officer-change trend in the fraction of credit allocated to a group in any of the four plots. This observation represents strong evidence that the group identity of the new officer is unrelated to either the group identity of the outgoing officer or the evolution of credit market conditions leading up to the officer change. Taken together, the patterns observed in Figure 1 validate the identification assumptions behind the difference-in-difference estimator of the in-group effect in specification (1).

A. *In-Group Effect on Credit Quantity*

In Table 4, we present the effect of having an in-group branch head on new credit, estimated using the specification (1) above. We emphasize that these analyses use a combination of branch-quarter, district-group-quarter, and group-branch fixed effects.[18] Outcomes are measured at the level of group g in branch b in quarter q, starting in column 1 with total new debt obtained by group g as a fraction of total new debt in branch b, and in column 2 with the number of new credit recipients in group g as a fraction of the total number of new credit recipients in branch b. In both cases, we find a positive and significant effect of *SameGroup* on credit access. The estimated coefficients indicate that the fraction of credit obtained by group g borrowers increases by nearly 6.5 percentage points, and the fraction of new borrowers from group g relative to total new borrowers increases by 5.65 percentage points following the transition to an in-group branch manager.

[17] These plots do not mean that a "new officer" effect does not exist, merely that it is second order relative to the effect of cultural proximity and statistically indistinguishable from zero in the data. The patterns also indicate that an increase in the fraction of new lending to in-group borrowers does not mechanically decrease the fraction of lending to all other groups, as would occur, for example, if branches were subject to a binding capital constraint. The decline in lending occurs exclusively for those borrowers that lose in-group status with the officer change.

[18] There are 37,709 branch-quarter dummies, 19,155 group-branch dummies, and 43,723 district-group-quarter dummies.

Table 4—Effect of Cultural Proximity on New Credit to a Group

Dependent variable	Group credit/branch credit (1)	Number of borrowers/ number of branch borrowers (2)	Dummy = 1 if credit > 0 (3)	ln(credit) (4)	ln(number of borrowers) (5)	ln(average new loan size) (6)
SameGroup	0.0647	0.0565	0.0250	0.1391	0.0691	0.0691
	(0.006)	(0.006)	(0.005)	(0.027)	(0.018)	(0.018)
Branch-group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Branch-quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes
Group-district-quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	331,053	331,053	350,711	105,610	105,701	105,610
R^2	0.852	0.869	0.745	0.806	0.856	0.687

Notes: In this table, we report the estimated effect of cultural proximity on the new debt to a group as a fraction of branch debt (column 1), on the number of new loan recipients from a group as a fraction of the number of new loan recipients in a branch (column 2), on the probability that a group receives credit (column 3), on the (log) total new debt received by a group (column 4), on the (log) number of borrowers that received new loans in a group (column 5), and on the (log) average new loan size (column 6). 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. Standard errors are clustered at the branch level.

Source: Authors' calculations

In the remaining columns of Table 4, we explore the group-extensive and group-intensive margins to examine how cultural proximity affects the probability that a group will receive credit, and conditional on receiving credit, how it affects the amount given. We report on the coefficients on *SameGroup* using our baseline specification with four dependent variables: a dummy equal to one if the group receives any new credit (column 3), the average (log) new credit (column 4), the (log) number of new credit recipients (column 5), and (log) average new loan size (column 6). Due to the log transformation the last three variables are defined only for group-branch-quarters where there is a positive debt flow. The point estimates indicate that all these measures of lending increase when an in-group officer is present in a branch, and the effect is significant at the 1 percent level.[19]

We present a pair of additional tables in the Appendix to probe the robustness of our results to different specifications and definitions of cultural proximity. First, in Appendix Table A1 we present analyses similar to those of Table 4, columns 1 and 2, with various combinations of fixed effects that are less stringent than those in our saturated specification (1). Neither the magnitudes nor the significance of the estimated parameters change markedly. We take the stability of these estimates as strong evidence that the relationship between our variable of interest, *SameGroup*, and credit market outcomes, is unlikely to result from problems such as omitted variable bias.

Appendix Table A2 repeats the estimation (again using saturated specifications) using the group definitions based on the traditional religious caste system (Varna), obtained through surname matching. The dependent variable is now scaled by the

[19] The estimates on total debt, number of borrowers, and loan size (columns 4, 5, and 6) are still significant after accounting for the fact that these are estimated conditional on the group receiving credit using Lee (2009) bounds (not reported).

total lending in the branch to all matched borrowers (and not all branch loans), so the estimate magnitudes of this specification are not directly comparable to those above. The estimated effect of cultural proximity on lending is again positive and significant for the fraction of credit, the number of borrowers, and the probability of receiving credit (the effect on loan size and other intensive margin measures is not statistically significant). 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) for the bank to use 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.

B. *In-Group Effect on Loan Quality and Cost*

As highlighted in Section I, prominent models of credit markets under asymmetric information predict that, if cultural proximity reduces information frictions, then the expansion of in-group credit access that we document in the preceding section should be accompanied by improved repayment. By contrast, if favoritism is the dominant source of within-group preferences, standard models predict that 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 first examine the impact of cultural proximity on future loan performance by estimating specification (1) using the fraction of borrowers who are more than 60 days past due in a year.[20] As before, the unit of analysis is the branch-group-quarter level, and our outcomes of interest are calculated over all the borrowers in branch b, group g, that received new loans in quarter q. The outcome of interest is the fraction of borrowers that received new loans in quarter q who are past 60 days overdue in quarter $q + 4$ ($FractionInDefault_{bgq+4}$). Since the bank keeps record of late repayments as a borrower outcome (and not a loan outcome), our default measure reflects payments overdue on *any* loan, not just the loan received in quarter q.

The estimated coefficient on *SameGroup* for loan performance is presented in Table 5, column 1. The point estimate of the effect of cultural proximity on the fraction of loans more than 60 days overdue 12 months forward is negative and significant at the 5 percent level. The coefficient of -0.006 (-0.6 percentage points) implies an 7 percent reduction in the default probability for in-group loans relative to the mean of 8.6 percent.

A taste-based model of higher in-group lending that would also lead to higher repayment rates is one where cultural proximity induces loan officers to extend additional loans to insolvent in-group borrowers to make payments on past loans. This "evergreening" 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. In column 2, we test whether

[20] The results are almost identical when we use 30 and 90 days past due. We also employed specifications that used the log of one plus the number of days late as the outcome variable. These regressions generated results that are qualitatively very similar to those we report in the text, but do not have any clear economic interpretation.

Table 5—Effect of Cultural Proximity on Loan Repayment and Cost

	Loan quality		Loan characteristics		
Dependent variable	Fraction borrowers in default in $t+4$		ln(collateral/debt)	Interest rate	Maturity
	(1)	(2)	(3)	(4)	(5)
SameGroup	−0.0060		−0.0398	0.0162	0.0614
	(0.003)		(0.012)	(0.039)	(0.045)
$SameGroup \times SameGroup_{t+4}$		−0.0082			
		(0.005)			
$SameGroup \times (1 - SameGroup_{t+4})$		−0.0137			
		(0.005)			
Branch-group fixed effects	Yes	Yes	Yes	Yes	Yes
Branch-quarter dummies	Yes	Yes	Yes	Yes	Yes
Group-district-quarter dummies	Yes	Yes	Yes	Yes	Yes
Observations	98,229	81,482	205,811	239,106	239,106
R^2	0.814	0.515	0.782	0.927	0.650

Notes: In this table we report the estimated effect of cultural proximity on the probability of default, the (log) average collateral to debt ratio, the interest rate (in percentage points) and the maturity (in months) of debt 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.

Source: Authors' calculations

the positive effect of cultural proximity on performance disappears or is attenuated when the in-group officer is replaced with an out-group one by using a pair of interactions to distinguish between the following two cases (i) $SameGroup = 1$ 12 months after the loan is issued by an in-group officer, versus (ii) there is no longer an in-group officer 12 months after an in-group loan is made. Specifically, we consider the separate effects of $SameGroup_{bgq}$ on default four quarters ahead ($FractionInDefault_{bgq+4}$) when the officer still belongs to the same group four quarters ahead ($SameGroup_{bgq} \times SameGroup_{bgq+4}$) and when the officer does not belong to the same group four quarters ahead ($SameGroup_{bgq} \times (1 - SameGroup_{bgq+4})$). Intuitively, the latter of these terms captures the extent to which default declines *even when the SameGroup officer is replaced by an out-group one*. We find that both terms are negative and significant, implying that the improvement in performance observed at quarter q that is caused by having an in-group officer originate a loan in quarter $q - 4$ is present even if the loan officer at time q is no longer an in-group one. This result suggests that evergreening is unlikely to account for the higher quality of *SameGroup* loans. It also indicates that the increase in performance is unlikely driven by direct supervision, monitoring, or other actions that require the loan officer's presence.

The view that in-group lending reduces information frictions further predicts that the lower default rates we observe 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, and examine whether collateral to loan ratios are lower for in-group loans (holding the interest rate constant, higher risk borrowers will be required to post more collateral to borrow the same amount). To do so, we employ our baseline specification (1), using as a measure of collateral intensity the logarithm of the group's average ratio of total collateral to outstanding loan amount (note that, in contrast to ratio-based outcomes, our collateral measure is defined only for groups with positive credit outstanding).[21] In Table 5, column 3, we show that the estimated in-group effect on collateral to loan ratios is -3.98 percentage points, significant at the 1 percent level. This indicates that in-group borrowers post on average 4 fewer rupees of collateral per every 100 rupees of credit outstanding, relative to out-group ones. This is consistent with cultural proximity reducing the cost of borrowing, and rules out preference based explanations in which the borrower becomes safer (e.g., because they do not want to default on someone from their own group) but the officer is unaware of it.

Finally, in Table 5, columns 4 and 5, we repeat our baseline specification using two additional loan characteristics—loan maturity in years and interest rate—as the dependent variable. In both cases, the coefficient on *SameGroup* is statistically insignificant. The coefficients also indicate that if there is an effect on these contract dimensions, it is of low economic significance. For term length, the coefficient implies that average maturity increases by 0.06 years (21 days), and for interest rates the coefficient implies that the annualized interest rate increases by 0.016 percentage points, both negligible magnitudes relative to the average loan. This is an indication that, as we note in Section II, loan officers exercise very little discretion over interest rates or maturity. It further indicates that officers do not reclassify loans—e.g., investment versus consumption—to achieve the same end.

C. *Heterogeneity by Caste and Religion*

In our main results, we presented the average effect of cultural proximity over all castes and religions. It is natural to ask how our findings may differ by group. In Table 6 we allow for the impact of shared culture to vary by religion and caste by interacting *SameGroup* with dummies for each of the five major minority religions and, conditional on the religion being Hindu, the four government sanctioned castes. The effects are on average larger, sometimes by an order of magnitude, for minority religions than for castes within Hindus. Since Hindu castes represent larger groups, one interpretation of these results is that there is a strong negative correlation between group size and the effect of cultural proximity. This across-group heterogeneity potentially indicates that the benefits of cultural proximity in access to credit may be limited by group size.

The effect of cultural proximity also exhibits substantial heterogeneity across minority religions: the effects range from 0.66 (significant at the 1 percent level) for *SameGroup* $\times$ *Muslim* to 0.03 (not significant) for *SameGroup* $\times$ *Parsi*, and the difference of the effect across groups is always statistically significant. A plausible explanation for the very small effect for Parsis is that they are, as summarized by a 2012 article in the popular press, widely known for "diligence and trustworthiness." The same article goes on to observe that Parsis tend not to be "clannish," which

[21] Recall that the bank does not record the collateral pledged to secure a specific loan, but the total amount of collateral pledged by the borrower. For that reason we cannot measure the collateral associated with new loans only.

Table 6—Heterogeneity by Religion and Caste

Dependent variable	Group credit/branch credit (1)	Number of borrowers/number of branch borrowers (2)
SameGroup × *Muslim*	0.6370	0.6354
	(0.038)	(0.039)
SameGroup × *Christian*	0.3901	0.3905
	(0.027)	(0.027)
SameGroup × *Sikh*	0.4039	0.4137
	(0.043)	(0.044)
SameGroup × *Parsi*	0.0303	0.0077
	(0.028)	(0.015)
SameGroup × *Buddhist*	0.3542	0.3397
	(0.114)	(0.105)
SameGroup × *General caste*	0.0310	0.0185
	(0.007)	(0.007)
SameGroup × *SC*	0.0115	0.0057
	(0.004)	(0.004)
SameGroup × *ST*	0.0112	0.0041
	(0.010)	(0.009)
SameGroup × *OBC*	0.0323	0.0267
	(0.012)	(0.011)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Group-district-quarter dummies	Yes	Yes
Observations	331,053	331,053
R^2	0.854	0.872

Notes: In this table we report the estimated effect of cultural proximity on the new debt and number of recipients (as a fraction of branch debt and number of recipients) by religion, and, conditional on the religion being Hindu, by caste. Standard errors are clustered at the branch level.

Source: Authors' calculations

might suggest that any role of favoritism in increasing loan quantities is also muted amongst Parsis.[22]

Across castes the heterogeneity is much smaller: the effects range from 0.033 for *SameGroup* × *General* to 0.012 for *SameGroup* × *SC*, and the differences across groups are not always significant. This is a natural result of government-mandated castes serving as less potent sources of identity and differentiation relative to religion. (Given that the effect of caste is much smaller, combined with the fact that Hindus are a significant majority in India, our estimated effect of *SameGroup* in Table 4 may be seen as an overestimate of the average impact of cultural proximity for the overall population. In unreported results, we find that the coefficient on *SameGroup* falls by about half in estimating the effect of cultural proximity on lending quantity (significant at the 1 percent level) when we weight observations by one plus the number of borrowers.)

[22] Arti Sharma, "Straight, Honest Parsimoney," *Outlook India*, October 1, 2012, http://www.outlookindia.com/magazine/story/straight-honest-parsimoney/282353.

Overall, these results highlight the nuanced relationship between the effect of cultural proximity and group size. While there is a rough concordance between group size and strength of identity on the impact of in-group lending, the relationship is more complex: for example, the impact of *SameGroup* on borrowing is greatest for Muslims, the largest religious minority. Among Hindu borrowers, the biggest effect is also among the largest group (General). In the future, we may shed further light on these patterns by delving further into general attitudes toward particular groups, and into the extent of assimilation of individual religions or castes.

V. Further Results: Heterogeneity and Loan Dispersion

The preceding section documented robust patterns along the two main dimensions of credit market outcomes: quantity and quality of lending. Among prominent models of credit markets under information asymmetries, our findings are more easily reconciled with those where cultural proximity reduces information frictions rather than those that emphasize favoritism. In our final section, we present findings on how credit provision differs across borrower, officer, and branch characteristics, as well as results on loan size dispersion, to explore a broader set of potential explanations for our results.

A. *Heterogeneity by Branch Density and Size*

We have assumed, up to this point, that in-group favoritism is driven by lender rather than borrower preferences. If borrowers prefer same-group loan officers, they may choose to reward them with higher-quality lending opportunities. While our data cannot fully rule out this possibility—as we note in our introduction, they are best-suited to detecting favoritism which comes at a cost to the lender—we may probe the plausibility of this argument by examining how the *SameGroup* effect varies by branch attributes.

In particular, we examine whether the impact of *SameGroup* is affected by the presence of other borrowing options as proxied by branch density in a district, given by 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 in this measure across districts, with 0.18, 0.54, and 1.88 as the first, fiftieth, and ninety-ninth percentiles, respectively. The districts with the highest branch densities typically correspond to urban areas and the lowest densities to rural ones.

In Table 7, panel A, we estimate specification (1) using only the 89 branches where the bank is the only one in its district (since there is only one branch per district in this sample we have to amend the specification to include state-group-quarter dummies instead of district-group-quarter dummies). These are rural areas where there are essentially no other formal financing opportunities available. We observe that, if anything, the *SameGroup* effect is stronger in isolated areas, with the coefficient in column 1 taking a value of 0.082 (significant at the 1 percent level) compared to 0.065 for the full sample. We further observe that these isolated branches

Table 7—Heterogeneity by Branch Density in District and Branch Size

Dependent variable	Group credit/branch credit (1)	Number of borrowers/number of branch borrowers (2)
Panel A. Sole branch/bank in district subsample (88 branches)		
SameGroup	0.0818	0.1224
	(0.024)	(0.030)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Group-state-quarter dummies	Yes	Yes
Observations	13,358	13,358
R^2	0.895	0.920
Panel B. Effect heterogeneity by branch density in the district		
SameGroup	0.0758	0.0687
	(0.013)	(0.013)
SameGroup × branches/1,000 district inhabitants = quartile 2	−0.0476	−0.0416
	(0.020)	(0.019)
SameGroup × branches/1,000 district inhabitants = quartile 3	−0.0176	−0.0213
	(0.017)	(0.016)
SameGroup × branches/1,000 district inhabitants = quartile 4	0.0003	−0.0013
	(0.016)	(0.016)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Group-district-quarter dummies	Yes	Yes
Observations	329,325	329,325
R^2	0.853	0.870
Panel C. Effect heterogeneity by number of officers in the branch		
SameGroup	0.0462	0.0363
	(0.010)	(0.010)
SameGroup × number of officers in branch = 2	0.0208	0.0213
	(0.014)	(0.015)
SameGroup × number of officers in branch > 2	0.0306	0.0340
	(0.013)	(0.013)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Group-district-quarter dummies	Yes	Yes
Observations	322,654	322,654
R^2	0.853	0.870

Notes: We report the heterogeneity of the estimated effect of cultural proximity on lending outcomes using specification (1) across districts of different branch density and branch size. 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. Panel A is estimated using only the subsample of branches located in districts where there is no other branch (of the bank or any other) in the same district. Standard errors are clustered at the branch level.

Source: Authors' calculations

tend to be small, with a median of just a single officer (i.e., the branch head) across all branch-quarter observations compared to a median of two for the full sample, so the absence of other branches more plausibly indicates an uncompetitive market rather than an entry deterrence strategy by the bank.

In Table 7, panel B, we present results for the full sample that allow the effect of *SameGroup* to vary across branch density quartiles; the effect of cultural proximity is *strongest* in branches with few nearby banking options. (We also find a strong positive correlation between branch density and branch size, again suggesting that the lack of nearby branches indicates lack of competition rather than entry deterrence.)

These findings are difficult to reconcile with demand-side explanations for the in-group effect where, for example, there are readily observable low-risk borrowers that generate relatively high profits for any bank attracting them as customers. If these sought-after borrowers choose to take their business to bank branches based on cultural affinity then the arrival of a same-group officer would trigger an increase in lending and also an improvement in loan performance with same-group. However, this type of "business-stealing" effect should be attenuated in regions where the bank holds significant monopoly power, counter to our finding of a strong *SameGroup* effect in districts with low bank density. This does not fully rule out the possibility that our results are driven by borrower preferences—for example, cultural ties may be so strong that they lure low-risk borrowers across district boundaries—but given that districts with a single bank branch are geographically isolated, our full set of findings is more easily reconciled with an interpretation based on same-group officers serving to reduce information frictions. (Additionally, the fact that the effect of cultural proximity remains strong even in small markets with few competitors suggests that the expansion of credit for in-group borrowers is unlikely to crowd out credit by other banking institutions.)[23]

B. *Heterogeneity by New versus Pre-Existing Borrowers*

We now turn to evaluating how the effect of cultural proximity varies across borrowers who have an existing credit record with the bank versus those who do not. This comparison is useful in understanding how cultural proximity interacts with hard sources of information used in the credit assessment process. Since there is no centralized credit registry that collects borrowers' credit histories during our sample period, the only source of hard information available to lenders is a customer's own borrowing and repayment record at the bank. We can therefore use heterogeneity in the effect of cultural proximity across first-time and preexisting borrowers to evaluate whether the information advantage from cultural proximity is a substitute or a complement for the hard information contained in credit histories.

We partition the borrower sample into two groups: (i) borrowers that have established a credit relationship with the bank prior to the arrival of the current officer, and (ii) borrowers that receive credit from the bank for the first time with the current officer. We scale the dependent variable by the total new loans of the branch to each group of borrowers and estimate the group-branch level regressions on these

[23] In Appendix Table A3 we show that the fraction of lower-level loan officers in a branch from a particular group increases lending to borrowers from their group, but the effect is about a tenth of that which we estimate for the impact of the group ties of branch heads. There are several factors that can account for the modest in-group effect of lower-level officers. First, it is consistent with branch heads playing a dominant role in loan decisions. This is particularly the case for higher loan amounts, which require approval from a higher-ranked officer. As we observe in Appendix Table A4, the in-group effect is increasing in loan size percentile (consistent with the increased loan dispersion associated with in-group lending that we document in subsection VD), which already comprise a disproportionately high fraction of total loan amounts. A further potential explanation for the muted in-group effect of lower-level officers is that, according to bank officials, these data are updated less frequently than branch head information, resulting in classical measurement error.

Table 8—Existing and First-Time Borrowers

Dependent variable	Group debt/branch debt (1)	Number of borrowers/number of branch borrowers (2)
Panel A. Subsample of borrowers who obtained credit from bank prior to officer's arrival		
SameGroup	0.0512	0.0446
	(0.007)	(0.006)
Branch-group and quarter dummies	Yes	Yes
Observations	266,273	266,539
R^2	0.737	0.770
Panel B. Subsample of borrowers obtaining credit for the first time		
SameGroup	0.0510	0.0473
	(0.006)	(0.006)
Branch-group and quarter dummies	Yes	Yes
Observations	360,910	360,910
R^2	0.809	0.809

Notes: In this table we report the estimated effect of cultural proximity on lending patterns (specification (1)) separately for existing borrowers (panel A) and first-time borrowers (panel B). Existing borrowers are those that have 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.

Source: Authors' calculations

subsamples, which provide the effect of cultural proximity on the flow of credit to preexisting versus new borrowers.

Table 8, panel A, shows the estimates of a branch-group level specification that includes in every branch-group-quarter *bgq* those borrowers who had positive credit at any time before the officer in charge of branch *b* in quarter *q* arrived. The estimates indicate that the arrival of an in-group branch head increases the amount of new loans (column 1) and the number of new loan recipients (column 2) amongst those borrowers with a pre-existing relationship with the bank. Table 8, panel B, shows a very similar impact of *SameGroup* for the subsample of borrowers obtaining credit from the bank for the first time.

The similarity of the point estimates suggests that the informational advantage conferred by cultural proximity is not a substitute for the hard information held by the lending institution in the form of a history of past borrowing and repayment behavior. In this were the case, we would expect to see a smaller effect of cultural proximity on loan outcomes for existing borrowers. By contrast, the evidence suggests that cultural proximity and loan history have additive effects on loan outcomes. This in turn suggests that the documented effect of cultural proximity is unlikely to be mitigated by the introduction of a credit bureau or other changes in the information environment that rely on past borrower behavior to evaluate creditworthiness.

C. *Heterogeneity by Officer Tenure at Branch*

We conclude our analysis of heterogeneity by examining the dynamics of the effect of cultural proximity over an officer's tenure in the branch. These dynamics

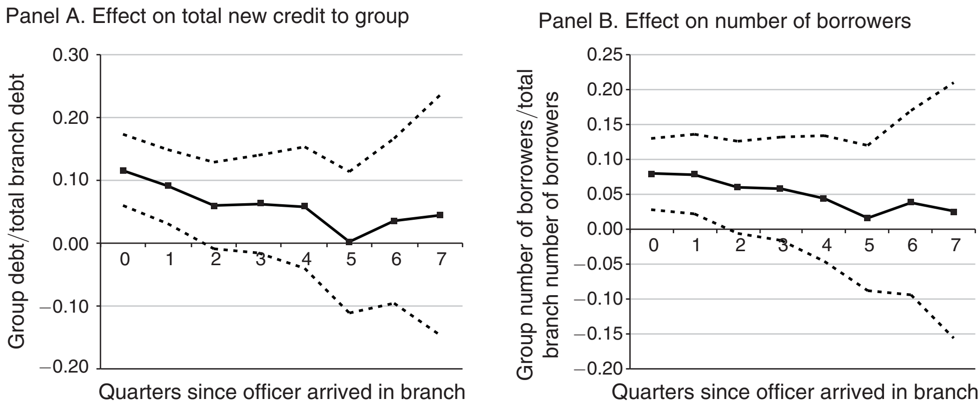

Figure 2. Effect Heterogeneity by Officer Tenure in the Branch

Notes: The horizontal axis measures time, in quarters, since the officer arrived in the branch (0 represents the first quarter with the new officer). The vertical axis plots the point estimates and 95 percent 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).

Source: Authors' calculations

may inform our understanding of whether the effect of cultural proximity requires time to develop (this would be the case, for example, if the officer collects information through a network of acquaintances that takes time to build), or if its importance diminishes over time as a result of the loan officer's experience and interaction with out-group borrowers.

Figure 2 plots the coefficients on the interaction between *SameGroup* and a set of indicator variables for the officer's quarter of arrival at the branch ($t = 0$), his second quarter at the branch ($t = 1$), and so on until $t = 7$. The graphs for the effect of cultural proximity on both the fraction of new credit (panel A) and the fraction of new credit amongst first-time borrowers (panel B) in a group show an immediate impact of the new in-group officer: In the first quarter of the officer's recorded arrival ($t = 0$), both measures increase by about 10 percentage points. This immediate increase in the flow of credit is followed by slow decay, and although the point estimates are positive throughout, the estimates are no longer statistically different from zero after $t = 1$. We also observe that, while the point estimates decline with officer tenure, the confidence intervals increase such that we also cannot reject an *increase* in the *SameGroup* effect over the officer's tenure.

These dynamics suggest that cultural proximity confers an immediate advantage to the officer, and that the officer uses it to extend new credit to a "backlog" of in-group borrowers (both borrowers that already received credit from the bank and new ones). The integral below this plot represents the change in the stock of lending to in-group borrowers, and suggests that cultural proximity generates a permanent increase in access to credit while the in-group officer is in place. The effect of cultural proximity on the flow of credit never becomes negative, as would occur if the in-group advantage disappeared as the loan officer became more acquainted with out-group borrowers. Overall, the observed patterns suggest that loan officers are

Table 9—Effect of Cultural Proximity on Loan Dispersion

Dependent variable	ln(loan size standard deviation) (1)	ln(loan size interquartile range) (2)
SameGroup	0.1182	0.1110
	(0.027)	(0.027)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Group-district-quarter dummies	Yes	Yes
Observations	72,329	71,842
R^2	0.726	0.703

Notes: In this table we report the estimated effect of cultural proximity on (log) measures of the size dispersion of new loans to a group (interquartile range and standard deviation of the distribution of loan amounts to a group). 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.

Source: Authors' calculations

"endowed" with an information advantage in evaluating in-group borrowers, and that the advantage of lending to in-group borrowers is not attenuated with the officer's interaction with other groups.

D. *In-Group Effect on Loan Size Dispersion*

Cornell and Welch (1996) provide the novel prediction that improved ex ante screening should increase the dispersion in lending: officers receive higher precision signals of creditworthiness from in-group borrowers, increasing the variance of the distribution of priors across in-group borrowers. In our setting, a higher variance of priors will imply a higher variance of loan sizes to in-group borrowers.

To assess the effect of cultural proximity on loan dispersion, we estimate the baseline specification (1) using two measures of within-group loan dispersion: the standard deviation and the interquartile range of the new loans issued in branch b, group g, quarter q. The estimated in-group effects, presented in Table 9, are positive and significant for both measures. The point estimates indicate that cultural proximity increases the standard deviation (interquartile range) of loans outstanding by 11.8 percent (11.1 percent). In Appendix Table A4 we show that an in-group officer has a positive and significant effect on all percentiles of the loan size distribution in a group (10 percent, 25 percent, 50 percent, 75 percent, and 90 percent), though the coefficient on *SameGroup* increases monotonically with percentile. The estimated effect on the ninetieth percentile of the distribution of new loans (0.033) in Appendix Table A4 is smaller than the average effect in Table 4 (0.08). Thus, cultural proximity shifts up the entire distribution of loan sizes, but the effect on loan amount is large for a small fraction of in-group borrowers.

These results indicate that cultural proximity has heterogeneous effects on access to credit across borrowers. Combined with the observed effect on loan performance, the dispersion results suggest that cultural proximity improves the sorting and allocation of credit across in-group borrowers. These findings are consistent with better in-group ex ante screening as in Cornell and Welch (1996) or with heterogeneous monitoring effectiveness across in-group borrowers.

VI. Discussion and Conclusion

In this paper, we have measured the extent of differential treatment in the loan market for those with a shared cultural background. Our empirical context is well-suited to 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. Furthermore, exogenous officer rotation allows us to distinguish in-group preferences from changes in officer branch assignments. Finally, since we focus on credit markets, by analyzing loan outcomes we may explore whether the data are more easily reconciled with standard models of reduced information asymmetries versus standard models of favoritism. Overall, our findings are best explained by cultural proximity serving to reduce information frictions in the credit markets we study.

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 percent of the officers belong to the General Class category. This implies that the probability of a backward caste borrower (SC, ST, or OBC) facing unfavorable loan conditions is nearly 75 percent, purely for reasons of cultural affiliation.

Moreover, our findings suggest one possible mechanism through which statistical discrimination against minorities can arise. Minorities will not often be "matched" with a loan officer of their own group and will 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 preference-based rationales for political reservations (Chattopadhyay and Duflo 2004). Our analysis suggests that reservations may improve contracting outcomes because they reduce information asymmetries between loan officers and borrowers.

While it is impossible to fully evaluate the overall welfare implications of our findings, we may assess the conditions under which borrower welfare and the bank's profitability improve, which are useful metrics in evaluating the impact of in-group lending. As concerns borrower welfare, increased access to credit and a lower cost of borrowing unambiguously increase welfare. As we show in Appendix Table A4, an in-group officer has a positive and significant effect on all percentiles of the loan size distribution in a group. Thus, under the assumption of equal utility weighting across good and bad borrowers, cultural proximity leads also to an increase in borrower welfare.

From the lender's perspective, a welfare analysis requires first taking a stance on whether the bank's objective is to maximize loan profitability. If we take the normative view that the goal of a government bank in India should be to improve borrower welfare, then the discussion in the previous paragraph already implies that, under plausible assumptions, cultural proximity furthers this objective. If one augments the bank's objectives to include profit maximization, the welfare analysis is complicated by the difficulty of measuring loan profitability in our context. Specifically, it requires that we make an assumption on whether the marginal cost of capital is lower than the cost of funding the marginal loan. Existing work evaluating lending practices of government banks in India suggests that banks lend too little relative to their cost of capital (Banerjee, Cole, and Duflo 2004; Banerjee and Duflo 2014). Under this assumption, the combined effect of an increase in lending and a reduction in default probability would necessarily increase bank profits, assuming that the loss-conditional-on-default does not substantially increase. We showed in Table 2 that the median loan in the sample is over-collateralized by a factor of 2—the total collateral posted by the borrower is more than double the total amount of credit outstanding. This means that the relatively small decline in the collateral requirement observed with in-group officers is unlikely to affect the recovery rate on defaulted loans. We also obtained data on recovery rates for a small number (16,924) of loans in default; we find no difference in recovery rates for *SameGroup* loans (whether defined at time of write-off or initiation). It is thus likely that the expansion in lending caused by cultural proximity improves loan profitability.

Our findings also provide valuable input for policy discussions on the group-based assignment of loan officers or other bureaucrats. It is important to observe that there are many considerations involved in such decisions. As such, our findings should not be taken as a blanket endorsement of a policy of maximizing cultural proximity through officer rotation as it may, for example, reduce officers' incentives to learn about the cultural traits of out-group borrowers. That is, while the "local" effect of in-group matching could be positive, it may be outweighed by longer-term consequences. Further, a policy of maximizing cultural proximity could also impact the level of corruption within the bank, or even affect the average quality of loan officers selected to be branch managers. A more detailed analysis of such trade-offs may serve as fertile ground for future analysis.

There are a number of additional areas for research that are needed to draw out the full policy consequences of our findings. First, it would be useful to assess whether policies directly aimed at reducing cultural differences across groups—for example, by teaching a common language—lead to improvements in cross-group contracting. Second, while our findings highlight improvements in transaction-level efficiency from cultural proximity, to make an overall assessment of the efficiency consequences for the bank as a whole, it would be necessary to understand how the increase in *overall* lending that comes from matching borrowers and lenders affects allocation within the bank more generally. As noted in Banerjee, Cole, and Duflo (2004), bank officer incentives may lead to under-lending in Indian state banks and as such a credit expansion of the sort we document in our paper may represent efficiency gains more broadly. Providing rigorous evidence on this question of bank-level efficiency would require more detailed information on the bank's fuller set of funding and lending opportunities; we leave this for future work.

Appendix

A1. *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 Brahmin, Kshatriya, and Vaishya groups followed four steps:

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

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

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

(iv) 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 for the surname found it listed in one of the matrimonial sites of the Maheshwari Samaj community (Maheshwari 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 (Singh and Bhanu 2004); following up with 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;[24] a search on Kamat.com showed that this community belongs to the Brahmin Varna.

[24] "Deshastha Brahmin," *Wikipedia*, http://en.wikipedia.org/wiki/List_of_Deshastha_Brahmin_surnames.

A2. *Auxiliary Figures and Tables*

Table A1—Robustness of the Saturated Regression

Dependent variable	Group credit/branch credit (1)	Number of borrowers/ number of branch borrowers (2)
Panel A. Baseline branch-group fixed effects		
SameGroup	0.0814	0.0765
	(0.006)	(0.006)
Branch-group fixed effects	Yes	Yes
Quarter dummies	Yes	Yes
Observations	364,056	364,056
R^2	0.823	0.841
Panel B. Branch-quarter dummies		
SameGroup	0.0719	0.0569
	(0.005)	(0.005)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Observations	364,056	364,056
R^2	0.152	0.169
Panel C. Branch-quarter and group-quarter dummies		
SameGroup	0.0721	0.0575
	(0.005)	(0.005)
Branch-group fixed effects	Yes	Yes
Branch-quarter dummies	Yes	Yes
Group-quarter dummies	Yes	Yes
Observations	364,056	364,056
R^2	0.159	0.191
Panel D. State-group-quarter dummies		
SameGroup	0.0884	0.0741
	(0.006)	(0.006)
Branch-group fixed effects	Yes	Yes
State-group-quarter dummies	Yes	Yes
Observations	364,056	364,056
R^2	0.077	0.085
Panel E. District-group-quarter dummies		
SameGroup	0.0889	0.0748
	(0.006)	(0.006)
Branch-group fixed effects	Yes	Yes
District-group-quarter dummies	Yes	Yes
Observations	364,056	364,056
R^2	0.242	0.251

Notes: We report the estimated effect of cultural proximity on loan outcomes using specification (1) with alternative sets of dummies as controls. 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. Standard errors are clustered at the branch level.

Source: Authors' calculations

Table A2—Robustness: Alternate Group Definition Based on Surnames (*Varnas*)

Dependent variable	Group credit/branch credit (1)	Number of borrowers/number of branch borrowers (2)	Dummy = 1 if credit > 0 (3)	ln(credit) (4)	ln(number of borrowers) (5)	ln(average loan size) (6)
SameVarna	0.0149	0.0164	0.0344	−0.0667	−0.0364	−0.0303
	(0.009)	(0.007)	(0.009)	(0.064)	(0.029)	(0.051)
Branch-group fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Branch-quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes
Group-district-quarter dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,578	82,578	92,214	21,196	21,196	21,196
R^2	0.731	0.797	0.735	0.796	0.880	0.742

Notes: 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.

Source: Authors' calculations

Table A3—Effect of Cultural Proximity on the Flow of Credit: Other Officers

Dependent variable	Group credit/branch credit		Number of borrowers/number of branch borrowers	
	(1)	(2)	(3)	(4)
Fraction of same group officers	0.0087	0.0076	0.0090	0.0080
(Excluding head officer)	(0.002)	(0.001)	(0.002)	(0.001)
SameGroup		0.0957		0.0873
(Head officer)		(0.008)		(0.008)
Branch-group fixed effects	Yes	Yes	Yes	Yes
Branch-quarter dummies	Yes	Yes	Yes	Yes
Group-district-quarter dummies	Yes	Yes	Yes	Yes
Observations	213,896	213,896	213,896	213,896
R^2	0.859	0.860	0.876	0.877

Notes: We report the estimated effect of cultural proximity on the new debt to a group as a fraction of branch debt (columns 1 and 2), and on the number of new loan recipients from a group as a fraction of the number of new loan recipients in a branch (columns 3 and 4). The estimates are obtained using specification (1), but using as the right-hand-side variable the fraction of loan officers in the branch, excluding the branch head, that belongs to the same group as the borrower (group defined as before). This variable is only defined for branches with more than one loan officer. Standard errors are clustered at the branch level.

Source: Authors' calculations

Table A4—Effect of Cultural Proximity on the Distribution of New Loan Sizes

Dependent variable	Percentile: loan amount/branch credit				
	10th (1)	25th (2)	50th (3)	75th (4)	90th (5)
SameGroup	0.0175	0.0180	0.0212	0.0266	0.0331
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Branch-group fixed effects	Yes	Yes	Yes	Yes	Yes
Branch-quarter dummies	Yes	Yes	Yes	Yes	Yes
Group-district-quarter dummies	Yes	Yes	Yes	Yes	Yes
Observations	229,865	229,865	229,865	229,865	229,865
R^2	0.580	0.586	0.625	0.646	0.664

Notes: We report the estimated effect of cultural proximity on the ratio of the p-th percentile of the flow of new debt to a group in a branch quarter divided by total flow of branch debt. 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. Standard errors are clustered at the branch level.

Source: Authors' calculations

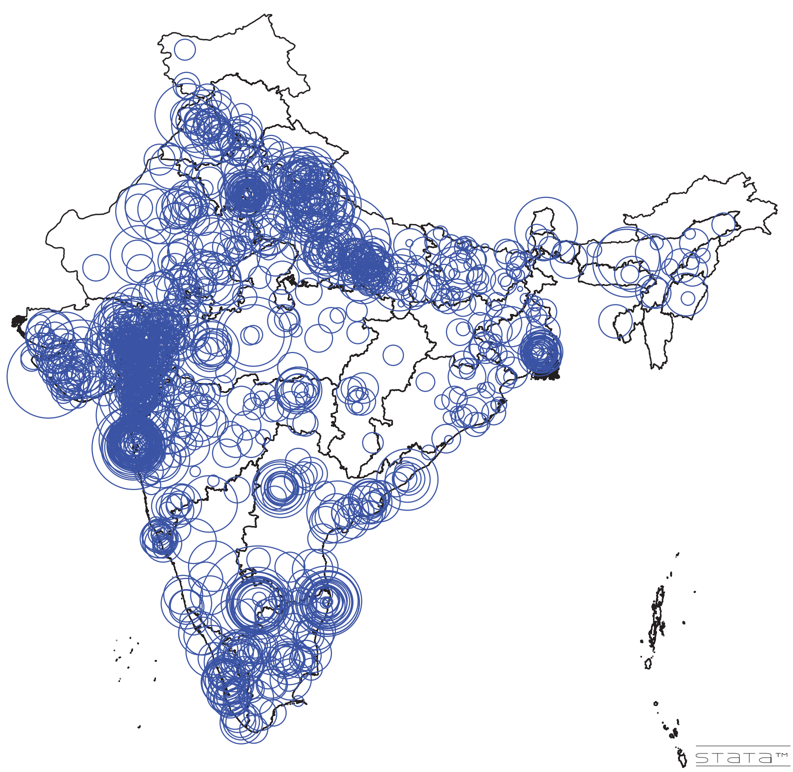

Figure A1. Geographical Distribution of Branches, Weighted by Total Lending

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

Source: Authors' calculations

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