Experience of Communal Conflicts and Inter-group Lending

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Abstract

We provide microeconomic evidence on the link between ethnic frictions and market efficiency, using dyadic data on managers and borrowers from a large Indian bank. Our analysis builds on the idea that exposure to religion-based communal violence may intensify branch managers' same-group preferences, and thus result in lending decisions that are more sensitive to a borrower's religion. We find that, in our sample of Hindu loan officers, those with substantial riot exposure prior to joining the bank lend relatively less to Muslim borrowers. Riot-exposed officers' loans to Muslims are also less likely to default, suggesting that the lower lending rate for Muslims is driven by taste-based discrimination. This bias is persistent across a bank officer's tenure, suggesting that the economic costs of ethnic conflict are long-lasting, potentially spanning across generations.

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I Introduction

At a macro level, inter-group frictions are associated with poor economic outcomes. Easterly and Levine (1997) in particular estimate that ethnic divisions, created by arbitrarily drawn colonial borders, can account for a third of Africa's economic underperformance. The empirical literature on the microeconomic foundations that underly this macro relationship between ethnic divisions and various social and economic outcomes is less well-developed. What determines the depths of a country's ethnic fissures? Beyond outright ethnic violence, how might these fissures impact economic progress? And to what extent are ethnic tensions malleable across — or indeed even within — a single generation?

In this paper, we provide microeconomic evidence on the link between ethnic frictions and market efficiency, in a setting that allows us to examine the extent to which these frictions can worsen in the course of a single generation. Specifically, we analyze the lending decisions of approximately 1800 loan officers at a large India bank, during 1999 – 2006. Using a database of Hindu-Muslim riots in India during the years 1950-1995 (compiled by Varshney (2006)), combined with data on each loan officer's year and city of birth, we may infer whether a loan officer's hometown experienced ethnic riots before he joined the bank. Bank records also require both loan officer and borrower to list their religion, allowing us to determine whether a given pair share the same religion. Finally, because of frequent officer rotation, we may infer the role of past experience of religious conflict on current lending decisions based on the turnover of "riot-exposed" Hindu branch managers (we focus on Hindu managers because of the extreme paucity of other religions among bank employees at this rank). Finally, since we observe whether each loan goes into default, we have a credible measure of the efficiency consequences of preferential in-group lending that allows us to distinguish between statistical discrimination, prejudice, and information frictions as the underlying mechanism.

In our main results, which use local riot deaths of greater than or equal to 10 as the definition of riot exposure, we find that the presence of a riot-exposed branch manager is associated with 4 percentage points higher lending to Hindu borrowers relative to all other borrowers. Qualitatively, this pattern is similar if we use less stringent cutoffs of 1 or 5 deaths to define riot exposure, a more stringent cutoff of exposure to at least one riot with 10 or more deaths, or use $log(1 + Deaths)$ as a continuous measure of riot intensity.[1]

The decline in lending to Muslims by riot-experienced managers could be due to taste-based discrimination (in-group favoritism) or statistical discrimination if riot-exposed managers are less capable of assessing the creditworthiness of out-group loan applicants. The former explanation would imply a lower quality of loans made to same-group borrowers, while the latter explanation implies that in-group favoritism should diminish for borrowers whose creditworthiness is already known (Altonji *et al.*, 2001). We find that the presence of a riot-exposed branch manager is associated with a 2.5 percentage point increase in defaults by Hindu relative to Muslim borrowers, consistent with in-group favoritism as the dominant explanation for the branch-level shift in loan composition across religions. We also find that riot-experienced managers lend less to first time as well as to repeat Muslim borrowers who have an established relationship with the branch, which suggests little impact from riot exposure on statistical discrimination.[2]

We may, to a large extent, rule out alternative explanations for the patterns we observe by exploiting the granularity of our data. District × Quarter fixed effects enable us to control for local demand shocks, and Branch fixed effects further allow us to control for idiosyncratic (though time-invariant) differences in credit demand or supply for a

[1] We also use exposure to the 1969 Gujarat riots – by far the biggest instance of post-partition Hindu-Muslim violence during 1950-1995 – as our "treatment" variable, and similarly find a reduction in lending by loan officers exposed to this event.

[2] There are two primary explanations for the effect of riot exposure on in-group favoritism — an increase in in-group affinity, or intensified out-group animus. While our data do not allow us to adjudicate decisively between these two cases, we may provide some suggestive evidence by comparing the effect of a riot-exposed branch manager's arrival on Muslims borrowers versus other non-Hindus. Given that India only experienced Hindu-Muslim riots during the period we consider, an animosity-based explanation predicts that Hindu managers will reduce loan disbursements to Muslims borrowers specifically, whereas increased in-group affinity would imply a relative decline in lending to all non-Hindu borrowers (relative to Hindu borrowers). We observe that lending to non-Hindus is invariant to a branch manager's riot exposure, suggesting that out-group animus (rather than stronger in-group identification) is responsible for our main results.

particular group in a given branch. Finally, Home District × Quarter fixed effects ensure that we can distinguish the riot-exposure effect from a broader in-group bias among branch managers from areas that have experienced relatively frequent riots.[3]

Our main results provide evidence that differences in out-group animus based solely on childhood exposure to religious conflict leads to significant inefficiencies in loan allocation. Furthermore, our source of identification — local riots during loan officers' childhood years — indicates that in-group favoritism can intensify even within a single generation.

We next explore several dimensions of heterogeneity in our data, with the objective of further probing the robustness of our results, as well as evaluating the causal pathways underlying the lower rate of Muslim lending by riot-exposed officers. We begin by examining the impact of riot exposure on lending decisions as a function of when the loan officer was first exposed to Hindu-Muslim violence, grouping loan officers based on whether exposure first occurred before the age of 10, between 10 and 18, or older than 18. Consistent with research in developmental psychology (Raabe and Beelmann, 2011), which finds that prejudice develops relatively early in childhood, we find that exposure prior to age 10 is the most important determinant of later lending decisions.

We also explore whether the effect of riot exposure depends on characteristics of a branch manager's posting, in particular whether the branch has a local monopoly, and whether it is in an urban or rural area. We find that the effect of riot exposure is invariant to the branch's circumstances in both cases. The similar impact of riot-exposure in monopoly versus competitive branches mitigates the concern that we are overestimating the overall impact of riot exposure on Muslims due to switching by borrowers facing discrimination. The similar effect of riot exposure for urban versus rural areas indicates that our results are stable across very different social and economic circumstances.

In our final analysis we turn to a contemporaneous shock to loan officers' preferences

[3]Given that we perform most of our analysis using shares of lending to each religion, we effectively account for Branch × Quarter shifts in overall lending, and also (time-invariant) religion-specific differences across branches.

resulting from the 2002 Gujarat riots that resulted in over 2000 fatalities. We find that, following these riots, lending to Muslims declines by 8 percentage points with the arrival of a branch manager who was stationed in Gujarat at the time of the riots. For bank officers stationed outside of Gujarat during the riots, we find that subsequent lending to Muslims is correlated with state-level media coverage, as captured by newspaper circulation and television viewership (although these results are not statistically significant across all specifications). The first set of findings provides some validation for Hindu-Muslim riots as a credible source of variation in Hindu-Muslim animosity, and extends our results to show that such shocks – if sufficiently severe – can impact preferences even if they occur during adulthood, a finding which echoes those of Hjort (2014) and Shayo and Zussman (2017). The findings on the role of newspaper and television penetration on subsequent lending emphasize the role of the media in aggravating intergroup frictions, consistent with the findings of Yanagizawa-Drott (2014) and DellaVigna *et al.* (2014).

Our research contributes most directly to the emerging microeconomic literature on the causes of in-group preferences and the consequences for economic transactions. The current paper builds on the data and insights of Fisman *et al.* (2017), which shows that loan quantity and quality is *improved* by a religion/caste match between branch manager and borrower. While the previous study emphasizes the two potentially counteracting effects of cultural proximity — increased favoritism versus reduced information frictions — our current work focuses on the *changes* in favoritism that may be induced by events that intensify inter-group frictions.

Our paper joins a small set of papers that document the microeconomic consequences of inter-group frictions on economic transactions. Most notably, Hjort (2014) studies the consequences of ethnic divisions for team production in flower packaging firms and, like us, uses ethnic riots to identify the impact of inter-group frictions. Beyond the distinct settings — India versus Kenya; credit markets versus team productivity — because of India's religious diversity we are able to draw a sharper distinction between increased in-group amity versus intensified out-group animus. Furthermore, in contrast to Hjort

(2014) as well as, to our knowledge, all prior research on the topic, we document the lifelong consequences of racially divisive personal experiences in childhood, rather than shorter-term increases in in-group favoritism as a result of current events. In this sense, our work is also distinct from Shayo and Zussman (2011), who document an in-group bias by Israeli judges as a result of nearby terrorist attacks in the preceding year (Shayo and Zussman (2017) shows that the effects persist even after violence subsides a few years later). Such work – ours included – aims in turn to link qualitative accounts and the theoretical literature on ethnic conflict and economic development (e.g., (Horowitz (1985) and Esteban and Ray (2008)) to empirical evidence, while also providing a foundation for the more macro-level research on ethnic divisions and economic outcomes (e.g., Guiso *et al.* (2009), Easterly and Levine (1997), and Alesina and Ferrara (2005).

Finally, our work contributes to the literature on the long-lasting impacts of personal experience on individual decision making. Prior work has largely focused on how early life experiences impact financial decisions (see, for example, Malmendier and Nagel (2011) on exposure to the Depression and savings, and Bernile *et al.* (2016) on CEOS' exposure to early life disaster and corporate risk-taking). We similarly document long-lasting effects from early life experiences in the distinct domain of in-group preferences.

The rest of the paper proceeds as follows. In the next section, we provide an overview of the dyadic data on bank managers and borrowers as well as the data on communal conflicts. Section III lays out our baseline empirical specification and presents our results. Section IV concludes.

II Data

We use two primary data sources — individual loan portfolio and personnel records of a large public sector bank, and data on Hindu-Muslim violence from Varshney (2006). The bank loan data provide information at the branch-borrower dyad level, which may in turn be matched to data on the branch manager at the time the loan is issued. Critically, both manager and borrower data include information on religion. Our bank data begin with the second quarter of 1999 and end with the first quarter of 2006, while the Hindu-Muslim riot data includes all riots involving the two religions for the years 1950-1995.

II.A Bank Loan Data

Our bank dataset includes loan-level data (including interest rate, collateral, and repayment status) for every borrower, in each quarter that the borrower has a loan outstanding. To ensure a match between the loan officer's riot exposure and lending practices in a branch, we focus on branches in which the branch manager interacts more directly with borrowers (in the bank's classification, levels 1 – 3 branches). This omits larger branches for which interaction between the branch head and individual borrowers is limited, and for which the loan portfolio is more heavily skewed toward corporate loans (see Skrastins and Vig (2015)).

Since our focus is on the group-level match between a branch manager and borrowers, we aggregate the lending data for all borrowers in the same religious group in a given branch at the quarterly level, which is the frequency of reporting of loan information (i.e., we aggregate to the branch-group-quarter level). We include Hindus and Muslims as distinct religious groups, and combine all other religions (Christians, Sihks, Parsis, Buddhists, and others) into a single "Other" category. Because we analyze how bank manager *turnover* induces *changes* in lending practices, our main outcome variables focus on lending flows, in particular new debt issued, number of new loans, and the repayment rates of these new loans. In a small number of branches (1.4 percent of the total sample) Hindu borrowers account for all loans outstanding throughout our entire sample period. We omit these branches from our analysis since they are generally in locations in which there is no non-Hindu borrower demand to identify officer lending supply effects; in practice the inclusion/exclusion of these branches makes little difference to our point estimates.

We use the bank's quarterly personnel records to identify the head of each branch.

For every branch there is a single manager who is responsible for the approval and disbursement of loans.[4] Though branch heads have control over loan and collateral amount, they have no discretion over interest rates, which are set based on the type of loan.

In addition to information on the loan officer's religion, the personnel records also provide data on the hometown, year of birth and the year the officer joined the bank for approximately 75% of the loan officers in the sample. We use this information to link each officer with a measure of riot exposure, which we calculate based on the number of Hindu-Muslim riots that took place while the manager resided in his hometown, which we assume to be the period from the manager's birthdate until the year he joins the bank. Since the bank forbids any loan officer from working in his hometown, loan officers necessarily leave their birthplace at that point in time.

We emphasize that after joining the bank, loan officers experience frequent rotation among branches, which will help us to identify the effect of riot exposure on managers' lending decisions.

Finally, we observe that, despite having fixed salaries (i.e., pay that is invariant to performance), officers in Indian state banks such as the one we study do have incentive to perform well. Rewards come via promotion to higher grades (with higher compensation) or better postings: loan officers 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 a similar vein, poor performers might be moved to less desirable places, which have a weak infrastructure and poor schools. Hence there exist incentives to issue profitable loans and perform well along other qualitative dimensions that serve as inputs into their evaluations (though these incentives in state-owned banks may be relatively weak compared to private banks).[5] Thus, to the extent that we observe favoritism in lending that worsens an officer's repayment rates, we may say that he faces

[4] If the branch is small, a lower-ranking officer is in charge of the branch, and requires approval of a more senior officer to make a lending decision. However, even in these cases the decision to send the application to the senior officer at a central branch rests with the local branch manager.

[5] Beyond pay-for-performance, officers at state-owned banks have greater job security, as they can be fired only under exceptional circumstances.

a cost to obtain the utility benefits from prejudice.

II.B Conflict Data

Our conflict data come from Varshney (2006). These data have been used extensively by researchers studying the causes or consequences of conflicts in India (Mitra and Ray (2014), Sarsons (2015), Jha (2014) among others). The dataset is based on news reports from *The Times of India*, one of India's leading newspapers, which is used to collect reports of instances of communal violence in India during 1950-1995. For each report of Hindu-Muslim riots, the dataset provides information on the number of deaths, injuries, and arrests, as well as the timing of the riot and city/town/village where it occurred.[6] As Varshney emphasizes, the city (rather than a higher level of aggregation such as state) is the"the most logical and significant level of analysis," because of the substantial within-state variation in the extent of riots (Varshney (2006). Our measure of riot exposure is thus also constructed at the city-level: for each loan officer, riot exposure is based on the number of riot deaths in his city of birth, during the period spanning his birthdate to the date he joined the bank. Larger cities have more riot deaths, conditional on a riot occurring (though this correlation is surprisingly modest — the officer-level correlation between $log(population)$ and riot exposure is 0.22), so we will control for (the log of) hometown population in some of our specifications below.

Because our riot data are for the years 1950-1995, we limit our sample to loan officers who were born on or after 1950, and who joined the bank no later than 1995. Throughout, our main definition of "riot-exposed" (*Riot*) is an indicator variable denoting whether a loan officer was exposed to 10 or more riot deaths while resident in his hometown. While this is an arbitrary cutoff, we wish to avoid describing an officer as riot-exposed if the events that took place during his youth were modest in scale. We also present our main results with $log(1 + RiotDeaths)$ as a riot exposure measure; additionally, we present

[6]The dataset does not, however, indicate the religion of the casualties and arrests. Finally, the data also provide a possible cause of each riot, but in most cases this is the subjective assessment of the authors.

results in which we relax the cutoff to 1 or 5 riot deaths, and also results in which we strengthen the riot exposure criterion to include only loan officers exposed to a single riot with ten or more deaths.

In Table 3, we present summary statistics for the main variables we employ in our analysis, with all observations presented at the branch-group-quarter level that we employ in our analysis. We observe that only 14.4 percent of observations have loan officers with $RiotDeaths > 0$. 11.9 and 9.6 percent of loan officers have $RiotDeaths \geq 5$ and $RiotDeaths \geq 10$. In Figure 1 we show the distribution of riot death exposure at the level of the individual loan officer, for the 256 officers with non-zero riot deaths. We censor the distribution at 50 deaths (around the 87th percentile) for ease of exposition, since a small fraction of officers are exposed to very high riot deaths (e.g., 9.8 percent of the officers in Figure 1 are exposed to more than 100 deaths, and 7.9 percent exposed to more than 400 deaths). The patterns indicate that a sizeable number of officers are exposed to a very small number of riot deaths: 21 officers (7.9 percent) were exposed to just a single riot death, while 9 officers (3.4 percent) were exposed to two deaths.

II.C Additional City- and State-Level Data

While most potential controls are absorbed by our various fixed effects, we utilize several state- and city-level attributes as controls and in exploring the heterogeneous effects of riot exposure. We obtain city and town population data from the 2011 national census, conducted by the Census Organization of India. We also employ two measures of media exposure in examining how the 2002 Gujarat riots affected loan officers stationed across India. Our first measure is based on survey responses from the National Family Health Survey (1998-99). We define *TV Share* as the fraction of respondents who report watching television at least once a week, which is provided for each state disaggregated by community size (rural, semiurban, urban, and metropolitan). As an alternative measure of media penetration, we use newspaper circulation per capita at the state level, from the Registrar of Newspapers for India maintained by the Ministry of Information and

Broadcasting, available via India's open government data platform.

III Results

Our empirical strategy hinges on the variation in exposure to communal conflicts by a manager early in life coupled with the policy of exogenous rotation of managers across bank branches. The baseline empirical specification identifies the effect of riot-exposure through the time series variation in loan outcomes for a particular religion in a particular branch following the rotation of managers with different exposures to communal conflict. More specifically, our baseline empirical specification takes the following form:

$$ReligShare_{bq} \quad = \quad \beta RiotExperience_{m(bq)} + Controls_{bq} + \alpha_b + \gamma_{d(b),q} + \upsilon_{h(bq),q} + \varepsilon_{bq} \quad (1)$$

$ReligShare_{bq}$ is the fraction of new lending obtained by a religion (Muslim, Hindu, or Others) at branch b in quarter q; $RiotExperience_{m(bq)}$ is an indicator variable denoting whether branch manager m stationed at branch b in quarter q was riot-exposed; α_b is a set of branch fixed effects; $\gamma_{d(b),q}$ is a set of district $\times$ quarter fixed effects; and $\upsilon_{h(bq),q}$ is a set of home district $\times$ quarter fixed effects for each home district of our set of managers. The branch fixed effects capture time-invariant characteristics of each branch, which ensures that the estimation of β comes from time series variation induced from rotation of branch managers. Since we run the above regression separately for each religion (Hindu, Muslim, and Other), district $\times$ quarter fixed effects control for any shocks and trends in the demand for credit of a particular religion in a district.

We express our primary dependent variable in loan shares because it lends itself to a straightforward interpretation of the overall effect of riot exposure on lending, capturing substitution between religions as well as expansion or contraction for particular religions (holding lending to other religions constant). We will also present results using $log(1 + AmountBorrowed)$ as the outcome variable, for each religion (Hindu, Muslim, and Other). This will allow us to explore whether the shift in the composition of lending is from expanded borrowing by the favored group, contracted borrowing from the disfavored group, or both.

III.A Impact of Riot Experience on Loan Quantity

We begin by showing the results of specification 1 in Table 4. Recall that our main definition of riot exposure uses $Deaths \geq 10$ as the threshold, but we will also present results that use cutoffs of 1 and 5 deaths, as well as a riot intensity measure based on the natural logarithm of (one plus) the number of deaths in hometown riots. In the first three columns, we present the results for Muslim, Hindu, and other borrowers respectively. The negative coefficient on Muslim lending, combined with the positive coefficient on Hindu lending of near-identical magnitude, imply that the presence of a riot-experienced loan officer is associated with an offsetting reallocation of lending from Muslim to Hindu borrowers. It is near-mechanical that we then observe only a small effect on other borrowers in column (3). The magnitude of this reallocation is very large when compared with the base rate of new lending to Muslims, which is 6.2 percent for our sample of bank-quarter observations in which a non-riot officer is the branch head. In the second set of columns, we present results for the number of new loan contracts (rather than new loan amounts); the patterns are qualitatively very similar.

We next show an "event study" to illustrate how the average effect of riot exposure varies around branch manager transitions. If riot exposure has a causal affect on lending across religions, we expect a discrete increase in the fraction of lending to Hindus (Muslims) that coincides with the presence of a riot-exposed manager.

To examine the timing of the change in lending around the arrival of a riot-exposed branch manager, we estimate the following specification separately for Muslim and Hindu shares of total lending:

$$ReligShare_{bq} = \sum_{i=-3}^{2+} \beta_i RiotExperience^{i}_{m(b)} + Controls_{bq} + \alpha_b + \gamma_{d(b),q} + \upsilon_{h(bq),q} + \varepsilon_{bq} \quad (2)$$

where $ReligShare$ is a religion's share (either Hindu or Muslim) of total borrowing at branch b in quarter q, and $RiotExperience^{i}_{m(b)}$ is an indicator variable denoting time i relative to the arrival of a riot-exposed manager at branch b. Thus, $RiotExperience^{-3}_{m(b)}$ is equal to one if, in three periods, a riot-experienced manager arrives at the branch. We define this variable for $i = -3, -2, -1, 0, 1$; finally, we define $RiotExperience^{2+}_{m(b)}$ to be one for all quarters for which a riot-experienced manager has been present for at least two periods, and no transition will occur for at least two quarters (to avoid overlap with the other variables).

In the top panel of Figure (2), we plot the coefficient estimates from specification 2, for both Muslim and Hindu borrowers. Consistent with riot exposure having a causal effect on lending patterns, we find that the increase in Hindu borrowers' share of lending increases discretely with the riot-exposed manager's arrival; we observe an offsetting decline in the Muslim share (the residual is lending to other religions, which is a relatively small fraction of overall lending). We observe similar patterns in the bottom panel, in which the dependent variable is the share of the number of loans (rather than total value of loans) disbursed.

We next show the results of specifications that use $log(1 + AmountBorrowed)$ as the outcome variable, rather than share of borrowing. As noted above, this allows us to examine whether the shift in borrowing composition under riot-exposed branch managers takes place through expansion of lending to Hindus, reduced lending to Muslims, or both. The results, shown in Table 5, indicate that the shift in lending composition comes primarily from a reduction in Muslim borrowing rather than an increase in Hindu borrowing. For Muslim borrowing, the coefficient on riot exposure is negative, whether measured by borrowing amount or number of borrowers, and is at least several times larger than the comparable coefficient for Hindu borrowing, which is also of inconsistent sign across the specifications based on loan amounts (column (2)) versus number of loan contracts (column (5)).

We present a series of appendix tables which show the robustness of our main results to alternative definitions of riot exposure, as well as additional controls. In Appendix Tables A.1 – A.3, we show the patterns for alternative definitions of $RiotExperience$, based on cutoffs of 5 and 1 deaths (Tables A.1 and A.2), as well as a continuous measure using $log(1 + Deaths)$ (Table A.3). For both the cutoff of 5 deaths as well as the continuous measure, we observe results that are similar to those we present in Table 4 (albeit marginally weaker for a cutoff of 5). For a cutoff of 1 death, we find our results are considerably attenuated – while the Muslim coefficient is still significant at the 5 percent level in Column (1), it is half the size of the comparable coefficient in our main results, and the Hindu coefficient, while positive, no longer approaches significance. We interpret this as resulting from the noise added by assigned $RiotExperience = 1$ for cities with relatively little religion-related rioting that may have been insufficient to have a lasting influence on local Hindu-Muslim relations or perceptions. In Appendix Table A.4 we use the most significant riot even during our sample period, the Gujarat riots of 1969, to define riot exposure (i.e., a loan officer is defined as riot exposed only if his hometown was affected by the 1969 Gujarat riots, and the officer was present in his hometown when the riots occurred). We again find a negative and significant relationship between a loan officer's riot exposure and Muslims' share of borrowing. Finally, we show that our main results are unaffected by controlling for hometown population — while we have no ex ante expectation that Hindu or Muslim borrowing shares would be affected by city size, we investigate the robustness of our results to its inclusion, given the correlation between city size and riot deaths. We show results controlling for $log(CityPopulation)$ in Appendix Table A.5; none of the coefficients of interest are affected, and in no case does the coefficient on city population approach significance.[7]

Finally, we examine whether riot exposure results in discrimination among Hindus based on caste affiliation. These results may be seen as a placebo test of the effects of riot exposure, since we have no ex ante expectation that Hindu-Muslim conflict would spill over to affect credit access based on caste. (Increased in-group preferences could in theory lead to increased caste-based discrimination. However, given the lack of any effect of riot exposure on "other religion" borrowers, this would be surprising.) We divide borrowers into four groups based on whether they are identified as General Caste, Scheduled Caste, or Scheduled Tribe, and look at borrowing at the branch × caste group × quarter level. We define an indicator variable, $SameCasteBorrowers_m(bq), c$, to be equal to one if the branch manager at branch b in quarter q is of caste c, and zero otherwise.[8]

In Table 6, we show our results on caste-based differences in lending. Consistent with Fisman *et al.* (2017), we find a positive and significant effect of shared caste on lending. However, the coefficient on the interaction with riot exposure is close to zero (though imprecisely estimated), emphasizing that the link from Hindu-Muslim violence to lending is specific to religious differences rather than other social divisions.

III.B Impact of Riot Experience on Loan Quality

As highlighted in section I, if the decline in Muslim lending associated with riot-experienced managers is the result of animus-based discrimination, we would expect better repayment rates for loans issued by riot-exposed officers to Muslim borrowers. We emphasize that, if this is the case, there is a real cost to the loan officer from doing so: since promotion and posting assignments – and the resultant increases in pay grade and perquisites – depend on loan performance, a loan officer sacrifices benefits in order to derive the utility gained by acting on his prejudices.

[7]We have similarly examined whether our results are robust to dropping managers from very small communities, or controlling more flexibly for city size by using population decile dummies. We find that the estimated coefficient on riot exposure is largely unchanged in these alternative specifications.

[8]See Fisman *et al.* (2017) for details on the loan officer and borrower caste data.

We explore the effects of riot exposure on repayment in Table 7. In our first pair of regressions we include branch $\times$ religion (α_{br}), home district $\times$ quarter ($\upsilon_{h(bq),q}$), and district $\times$ religion $\times$ quarter ($\gamma_{d(b),qr}$) fixed effects, as well as the same branch-quarter controls we employ in Table 4:

$$\begin{aligned} Default_{bqr} \quad = \quad & \beta_1 RiotExperience_{m(bq)} + \beta_2 RiotExperience_{m(bq)} \times NonMuslim_{bqr} \\ & + \alpha_{br} + \upsilon_{h(bq),q} + \gamma_{d(b),qr} + \varepsilon_{gbq} \end{aligned} \tag{3}$$

$Default_{bqr}$ is the fraction of loans issued to borrowers of religion r in branch-quarter bq that are more than 90 days past due within a year of issuance, and $NonMuslim$ denotes both Hindu and "other" religious groups. We present the results of this regression in column (1). The direct effect of $RiotExperience$, which captures the effect of riot experience on defaults by Muslim borrowers, is -0.035 (significant at the 10 percent level), indicating that loans issued to Muslim borrowers by riot-exposed loan officers have a default rate that is 3.5 percentage points lower than those issued by non-riot loan officers. As a benchmark, the default rate among non-riot loan officers to Muslim borrowers in the sample of branches with non-zero Muslim default is 6.3 percent, indicating that riot exposure leads to a 50 percent decline in Muslim default. The coefficient on the interaction term, β_2, is 0.025 (significant at the 5 percent level), indicating that the lower default rate for riot-experienced loan officers manifests itself primarily for lending to Muslims, consistent with Muslim borrowers (and only Muslim borrowers) facing a higher credit standard from riot-experienced officers. In column (2) we disaggregate non-Muslim borrowers into Hindu versus others. Since a relatively small fraction of loans go to borrowers in the "other" category (4.5 percent of total lending)the coefficient on $OtherBorrowers$ is noisily estimated, though near-identical in magnitude to the coefficient on $HinduBorrowing$, further reinforcing the view that the relative decline in default rate for Muslim borrowers is a result of higher standards for Muslims rather than a slackening of standards for 'in-group' Hindu borrowers.

We present more stringent variants on specification 4 in columns (3) and (4), which also include Branch × Quarter fixed effects. In these saturated specifications, we can no longer identify the direct effect of $RiotExperience$, which varies only at the branch-quarter level, though we can still identify the *differential* effect of riot exposure on different groups via $RiotExperience$ interactions. While the coefficient on $RiotExperience \times NonMuslimBorrowers$ in column (3) is marginally smaller than its counterpart in column (1) (0.18 versus 0.25), and significant only at the 10 percent level, the two sets of results are broadly consistent with a tightening of lending standards to Muslims under riot-experienced branch managers. The results in column (4), which disaggregate non-Muslim borrowing into Hindu versus Other borrowing, indicate a larger impact on Hindu borrowing, though the coefficients are imprecisely estimated.

III.C Learning about borrowers and loan officer experience

There are several channels through which lender experience could attenuate the effects of riot exposure if they resulted from excessively negative prior beliefs about Muslim creditworthiness. First, a given loan officer may learn over the course of his tenure at the bank: given the relatively high repayment rates for Muslim borrowers on loans issued by riot-experienced managers, one would expect that the effect of riot experience would dissipate with experience if the bias against Muslim borrowers were based on statistical discrimination. Second, additional information on a Muslim borrower – in particular whether he or she has repaid loans in the past – should mitigate a riot-exposed manager's negative priors on the borrower's creditworthiness.

To explore the first of these possibilities, we augment equation 4 to included the interaction of riot exposure and an indicator variable denoting whether a loan officer's years with the bank is above the sample median of 24 years. We present these results in Table 8. If loan officers learn that their beliefs of Muslims' creditworthiness are excessively negative, we expect the interaction term to be positive in column (1) (and negative in column (2)). We find instead that the point estimate is negative, though it does not approach significance. (If we instead measure experience via the logarithm of years with the bank, we generate qualitatively identical results.) In Appendix Table A.6, we consider a separate margin of exposure and learning – the time that a loan officer has spent in a particular branch. We do so by defining an indicator variable denoting observations for which the loan officer has been at a branch for 4 or more quarters (the sample median). We find that the interaction of (branch-specific) experience and riot experience is very close to zero, again suggesting that our main findings do not result from statistical discrimination against Muslims.

We next turn to the potential effect of providing the loan officer with hard information on borrowers' creditworthiness by splitting our sample into lending to new versus repeat borrowers. In Table 9 and 10, we show the estimation of equation 4 for the two types of borrowers separately. The coefficients are quite similar for both groups.

Overall, our results in this section suggest that the negative effect of riot exposure on Muslim lending is not driven by different beliefs in Muslim borrowers' creditworthiness, since the effect does not dissipate with lender experience, nor with more precise information on borrower quality. Rather, the results we document in the earlier part of the paper appear to be driven by animosity toward Muslim borrowers by Hindu loan officers.

III.D Geographic heterogeneity in the impact of riot exposure

In this section we examine the heterogeneity of the effect of riot exposure as a function of a branch's location along a pair of dimensions: urban versus rural, and branches for which no other bank branch is located within a 10 kilometer radius (what we refer to below as "monopoly" branches) versus those where prospective borrowers can choose among two or more banking options ("competitive" branches). While these branch characteristics are correlated ($\rho = 0.737$), they also reflect distinct concerns.

In looking at the urban versus rural split, we aim to explore whether the patterns we describe in our main results hold broadly across very different socioeconomic settings. To compare behavior in rural versus urban branches, we augment specification 4 with the interaction of $RiotExperience$ and a branch-level indicator variable, $Urban$, which captures whether the branch is in an urban (semiurban, urban, or metropolitan) area. We report these results in Table 11, where we observe that the coefficient on the interaction term is consistently small in magnitude and never approaches statistical significance, indicating a similar impact of riot experience on loan officer behavior in rural and urban branches.

We next turn to a comparison of riot experience on lending in monopoly versus competitive branches. We do so to address a pair of concerns that result from borrower switching across branches. Switching across branches within the bank could lead to double-counting as a result of, for example, a Muslim borrower switching from a branch where there has been a transition to a riot-experienced manager to a nearby branch there has been no such transition. Borrowers switching to other banks as a result of a riot-experienced manager's arrival, while it would not bias our regression estimates, would lead to an over-estimation of the broader economic consequences that result from in-group favoritism by riot-experienced managers. If these were substantial concerns for our analysis, we would expect to see a more muted impact in monopoly branches. In Table 12 we augment specification 4 with the interaction of $RiotExperience$ and $Monopoly$, an indicator variable which denotes monopoly branches. Again we find that the direct effect of $RiotExperience$ is significant (at least at the 5 percent level) in predicting lending to Hindus and Muslims; the interaction term is small in magnitude and never approaches statistical significance, providing suggestive evidence that branch switching is unlikely to be a major concern for our analysis.

III.E Heterogeneity by age of exposure

To this point, we have not taken a position on how in-group favoritism might vary with age of exposure to Hindu-Muslim frictions. Extant evidence from developmental psychology suggests that out-group prejudice develops by the age of 10 and that, more important from our perspective, environmental influence on prejudice is strongest prior to age 10 (see Raabe and Beelmann (2011) for a meta-analysis).[9]

We group loan officers based on their age of first exposure to riot fatalities: those first exposed before the age of 10; those first exposed during adolescence (11-18); and those first exposed during adulthood (but not yet employed by the bank). In Table 13, we interact the riot exposure dummy variable with indicator variables for first exposure before age 10 and first exposure at 11-18; the direct effect of riot exposure thus reflects the effect of first experiencing riots during adulthood. Across all specifications, we observe a near-zero effect of riot exposure first experienced during adulthood (though the standard errors are such that we cannot rule out potentially sizeable effects). We find a much bigger impact of riot exposure among loan officers first experienced during early childhood. For example, in the first two columns, the effect size for officers who experienced riots during early childhood is nearly twice that of loan officers who first experienced riots during adolescence.

Given our priors based on the child development literature, we view the findings in this section as providing a further validation of our interpretation of our main results as reflecting a causal link from riot exposure to in-group favoritism. We also see this finding as making a contribution to this literature, as we know of no prior work which links age of exposure to inter-group frictions and later life prejudice, particularly based on real stakes outcomes.

III.F The impact of bank managers' exposure to the 2002 Gujarat riots

Our analysis thus far has focused on the effect of riot exposure in bank officers' early years on lending decisions that take place potentially decades later. In this section, we examine the effect of exposure that is concurrent with tenure at the bank. This distinct

[9]While researchers have found that survey-based measures of prejudice decline during adolescence, there is no such decline in measures of implicit bias, leading researchers to conclude that survey responses of older children may suffer from social desirability bias.

analysis serves several purposes. First, it provides a clearer parallel to earlier work, such as Hjort (2014) and Shayo and Zussman (2017), which looks at relatively short-run responses to ethnic strife, and allows us to provide a quantitative comparison between the impact of recent versus early life riot exposure. Second, as we elaborate below, our analysis below based on the 2002 Gujarat riots allows for a sharper identification of the effects of riot exposure, and thus provides some validation for our broader set of empirical estimates.

Since we have data for the years 1999-2006, the riot occurs in the middle of our sample and we can study how managers' decisions change as a result of exposure to this riot.

The Gujarat riots were triggered by the burning of a train carrying Hindu pilgrims near the city of Godhra on February 27, 2002. The cause of the fire, which resulted in 58 deaths, remains the source of controversy. But it was blamed on the local Muslim community, and in the days that followed anti-Muslim riots broke out across the state. Reports put the death toll at around 2,000, making it one of the worst episodes of communal violence since Indian independence in 1947 (see Field *et al.* (2008) and Mitra and Ray (2014)). It is also important to note that the riots were contained within the state of Gujarat, and did not spread to other parts of the country.

Our empirical strategy is as follows. We consider the 28 branch managers stationed in Gujarat when the riots took place. We look at the bank branches where these Gujarat-exposed managers are subsequently rotated, and examine whether lending patterns shift upon their arrival. (We do not include branches in Gujarat, since the riots were a sizeable shock to the expected creditworthiness of Muslims in the state, given the loss of property and life. Since the timing of rotation is staggered across branches, all branches in this restricted sample experience turnover from a manager who was not exposed to the Gujarat riots to a Gujarat-exposed manager, but at different points in time, allowing us to identify a "Gujarat exposure" effect.

In Table 14 we report the results from the following specification:

$$ReligShare_{bq} \quad = \quad \beta Gujarat\,Riot\,Experience_{m(bq)} + Controls_{bq} + \alpha_b + \gamma_{d(b),q} + \varepsilon_{bq} \quad (4)$$

where $ReligShare_{bq}$ is the fraction of new lending obtained by a religion (Muslim, Hindu, or Others) at branch b in quarter q; $Gujarat\,Riot\,Experience_{m(bq)}$ denotes whether branch manager m stationed at branch b in quarter q was present in Gujarat during the 2002 riots; $\gamma_{d(b),q}$ is a set of quarter and branch fixed effects in Panel A and a set of state $\times$ quarter and branch fixed effects in Panel B.

The results in Panel A, indicate that when a Gujarat-experienced manager joins a branch, the Hindus' share of lending increases by 9.6 percentage points, while the Muslim share declines by 8.1 percentage points; there is no significant change in the share of lending to other religions. We obtain qualitatively similar results when we use the fraction of loan contracts as the outcome variable, or add quarter-state fixed effects (Panel B). The results suggest an impact from contemporaneous exposure to religious frictions that is of roughly the same scale as the effects we report in our main analysis (though the violence and upheaval associated with the 2002 riots were of a different scale from those taking place during 1950-95).

In our final set of results we explore whether, given the scale of the 2002 riots, managers elsewhere in India were also affected. In doing so, we also explore the joint hypothesis that the channel of influence is via the media. To do so, we look at lending by loan officers who were *not* present in Gujarat during the riots, in branches located outside of the state of Gujarat, to minimize any direct influence of riot exposure on in-group bias.

We use two measures of media exposure: TV viewership and newspaper circulation per capita, both at the state-level. Since we may disaggregate TV viewership by community type (rural, semiurban, urban, metropolitan) in our analysis based on TV exposure, we may include (as in Table 14) branch fixed effects, district $\times$ time fixed effects, and home district $\times$ time fixed effects as controls. Since newspaper circulation is at the state level, we cannot include district × time fixed effects in our analysis of the role of newspaper penetration. Finally, we define *Post* as quarters that occur after the 2002 riots took place.

The results, which we present in Table 15 and Table 16 for television viewership and newspaper circulation respectively, suggest that loan officers in areas with greater media exposure respond with a greater increase in in-group bias following the 2002 riots. In particular, the coefficient on the interaction of TV viewership and *Post* is negative for Muslim lending, and positive (and of comparable magnitude) for Hindu lending. While these results are more fragile than our main findings — the coefficients are not consistently significant across specifications — they provide suggestive evidence that media exposure may exacerbate bias as a result of inter-group frictions. The results in Table 16 are even more fragile, but directionally consistent with an increased in-group bias as a result of media exposure to the 2002 riots.

IV Conclusion

In this paper, we provide evidence which indicates that personal exposure to ethnic frictions can have long-lasted consequences for inter-group animosity. Our findings can help to better make sense both how ethnic frictions can be self-reinforcing: as each subsequent generation is exposed to ethnic friction, he or she may adopt stronger in-group preferences that, in turn, perpetuates existing cleavages within a society. Our results further indicate that these ethnic frictions have allocative consequences (in our case via credit), which adds to efforts to provide some micro-foundation for the macro association between ethnic divisions and economic growth. Since we study lending decisions in a state bank, where loan officers have relatively weak pay incentives, it is natural to ask the extent to which the discrimination we observe is lower in private banks where officers face higher-powered performance incentives.

Our findings also emphasize the relative rapidity with which group-based animus can shift as a result of salient events. On the one hand, this can lead to rapid aggravation of inter-group frictions (perhaps highlighting the value of efforts to mitigate such cleavages from occurring in the first place). Yet our findings have a more hopeful message when combined with those of Blouin and Mukand (2018), which studies reconciliation as a result of government messaging in Rwanda. Their work finds that government efforts at healing inter-group animosity led to an improvement inside of a generation, even in the wake of ethnic cleansing of tragic proportions. Thus, inter-group frictions appear malleable in both directions – they can worsen as a result of clashes, or improve via deliberate efforts.

As more work emerges on individual responses to shocks to community relations – both positive and negative – we can hope to gain a fuller sense of the consequences of ethnic frictions, and the potential of such frictions to worsen or lessen over time.

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Figure 1: Distribution of Riot Death Exposure

This figure provides a kernel density plot for the number of deaths in Hindu-Muslim riots experienced by branch managers while resident in their hometowns, conditional on experiencing at least one death

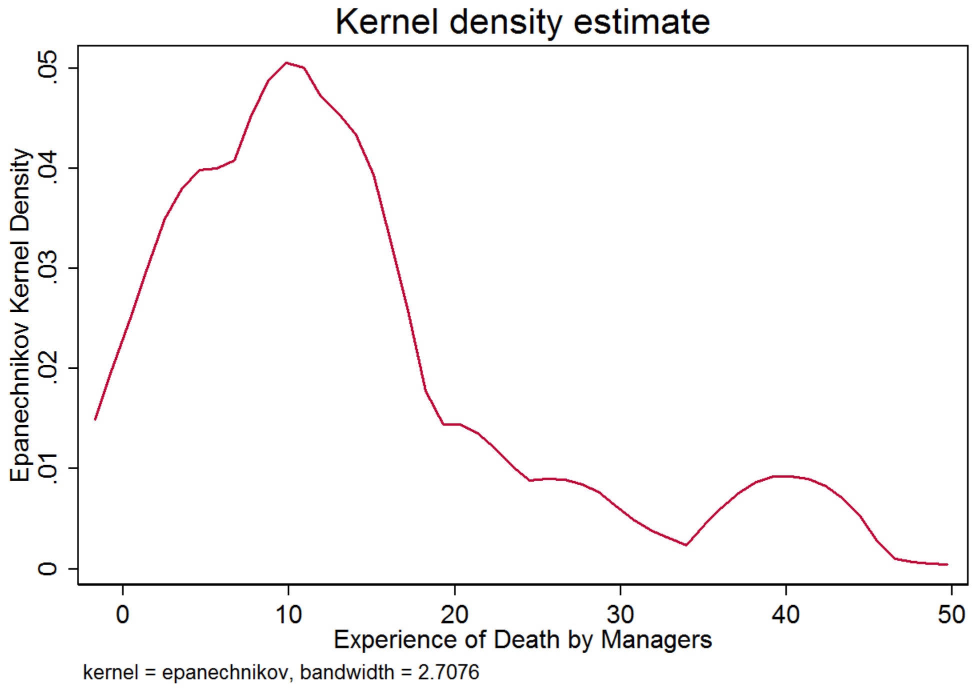
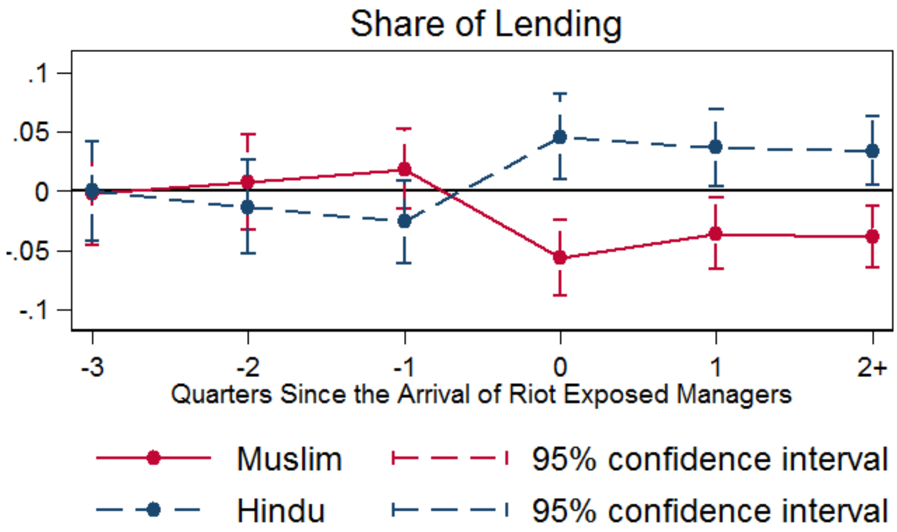


Figure 2: Share Of Lending to Muslims versus Hindu Borrowers Around Officer Transitions

The top figure shows the coefficients from a regression to capture shifts in the share of lending received by Muslims and Hindus around transitions to riot-exposed branch managers. The "whiskers" show 95 percent confidence intervals. The bottom figure provides a similar "event plot" using the share of loan contracts as the outcome variable.



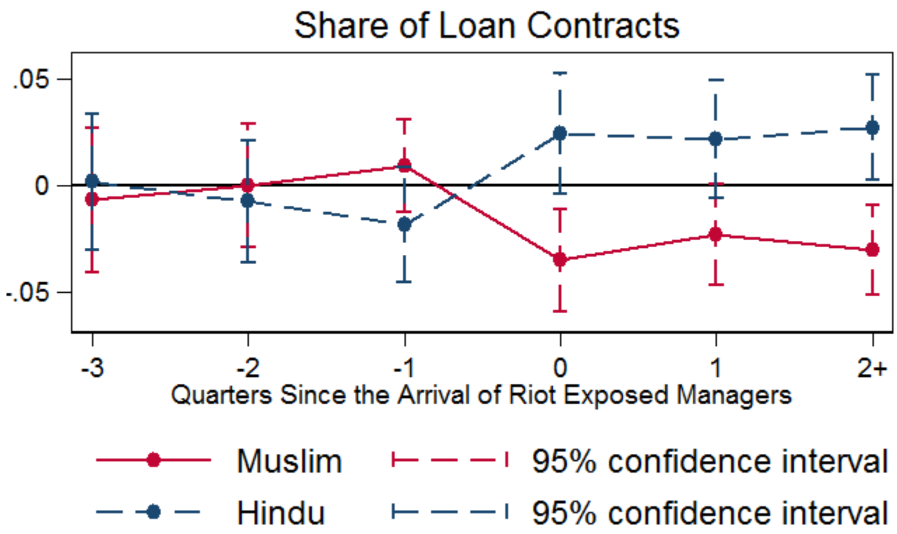


Figure 3: Riot Exposure and the Share Of Lending to Muslims Borrowers Across Branch Manager Tenure at the Bank

This figure provides regression coefficients from a specification that allows the impact of riot exposure on Muslim share of lending to vary as a function of the branch manager's years of employment at the bank. The "whiskers" show 95 percent confidence intervals.

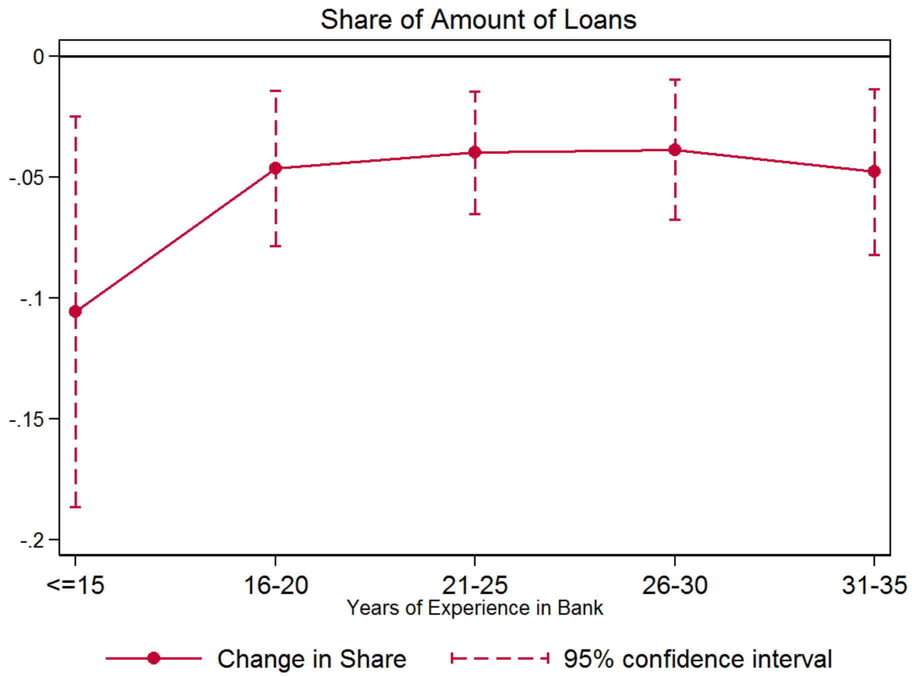


Table 1: Summary Statistics of Riots in India

The following table reports the summary statistics of the number of deaths, injuries and arrests in India due to Hindu-Muslim riots during 1950-1995.

STATE	Total Killed	Total Injured	Total Arrest	Total No. of Riots
Andhra Pradesh	339	1290	5936	51
Assam	478	224	228	22
Bihar	1005	805	2778	78
Delhi	91	739	1842	33
Gujarat	1657	4487	11542	244
Haryana	5	8	83	4
Karnataka	174	1082	1958	74
Kerala	16	290	111	20
Maharashtra	1450	5594	18432	201
Madhya Pradesh	339	1726	10050	68
Orissa	81	105	111	17
Punjab	0	4	12	2
Rajasthan	81	379	98	26
Tamil Nadu	32	209	277	16
Uttar Pradesh	1244	3158	35857	201
West Bengal	224	853	3916	70

Table 2: Religion of Borrowers and Lenders

The following table reports the percentages of borrowers and lenders belonging to each religion. Note that in our analysis, we focus on Hindu branch managers owing to the very small fraction of Muslim (and other) loan officers.

	Borrower (%)	Branch Manager (%)
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
Budhist	0.19	0.25
Others	0.23	0.25

Table 3: Summary Statistics on Branch-Group-Quarter Data

The following table reports the summary statistics of the primary variables employed in our analysis. We provide summary statistics separately for loan officers who experience at least 1 riot-related death in their hometown and loan officers who did not experience a fatal riot in their hometown. The data is at the branch-group-quarter level.

	Riot Exposed (N= 256)					Not Riot Exposed (N=1523)				
	Mean	Std Dev	p1	p50	p99	Mean	Std Dev	p1	p50	p99
No. of Killing Experienced	63.53	161.71	1.00	12.00	608.00	–	–	–	–	–
No. of Branches Worked	2.01	0.95	1.00	2.00	4.00	1.91	0.95	1.00	2.00	5.00
Age	47.59	4.31	33.00	48.00	55.00	46.70	4.43	34.00	47.00	55.00
Total Experience in Bank (Years)	24.23	5.53	8.00	25.00	35.00	21.66	4.65	10.00	24.00	33.00
Sum of New Credit (INR Mn)	1.07	4.89	0.00	0.12	10.00	1.04	2.87	0.00	0.12	10.30
Sum of New Credit to New Borrowers (INR Mn)	0.89	0.47	0.00	0.08	8.55	0.86	0.25	0.00	0.08	8.69
Sum of New Credit to Repeat Borrowers (INR Mn)	0.18	0.81	0.00	0.00	2.64	0.18	0.86	0.00	0.00	2.53
No. of New Loans	16.83	41.14	0.00	3.00	116.00	18.06	35.49	0.00	3.00	149.00
No. of New Loans to New Borrowers	14.10	36.44	0.00	2.00	101.00	15.24	30.85	0.00	2.00	124.00
Default	0.02	0.09	0.00	0.00	0.5	0.02	0.09	0.00	0.00	0.43

Table 4: Impact of Riot Experience on Lending Decisions

In this table we present the impact of riot experience on lending to borrowers belonging to different religions. Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-0.043***	0.040***	-0.012	-0.030***	0.028**	-0.007
	(0.013)	(0.014)	(0.013)	(0.010)	(0.012)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095

Table 5: Impact of Riot Experience on Amount of Lending

In this table we show the impact of riot experience on the quantity of lending to different religions. Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We include branch, district × quarter fixed effects and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is $log(1 + TotalLending)$ for each religion. In columns 4, 5 and 6 the dependent variable is $log(1 + NumberofLoanContracts)$ for each religion. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	Log(1 + Total Lending)			Log(1 + Number of Loan Contracts)		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-1.474*	0.187*	0.850	-0.238**	-0.073	0.069
	(0.789)	(0.102)	(1.253)	(0.103)	(0.077)	(0.122)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.681	0.784	0.690	0.785	0.804	0.801
Obs.	11799	12594	9095	11799	12594	9095

Table 6: Placebo - Caste-based differences in lending

In this table we present the impact of riot experience on lending to Hindu borrowers based on whether they are of the same caste as the branch manager. Data are at the branch-quarter-caste level. Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. Same Caste = 1 for the borrower group that is of the same caste as the branch manager in that quarter. We include branch × caste , district × caste × quarter, branch × quarter fixed effects and home district × quarter fixed effects. In columns 1 and 2 the dependent variable is the share of debt. In columns 3 and 4 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$		$\frac{\#NewDebt}{\Sigma \#NewDebt}$	
	(1)	(2)	(3)	(4)
Same Caste Dummy × Riot	0.009	0.009	0.012	0.012
	(0.020)	(0.019)	(0.021)	(0.020)
Same Caste Dummy	0.026***	0.026***	0.019**	0.019**
	(0.008)	(0.008)	(0.008)	(0.008)
Riot Dummy	-0.002		-0.003	
	(0.005)		(0.005)	
Controls	Yes	No	Yes	No
Branch × Caste	Yes	Yes	Yes	Yes
District × Caste × Time	Yes	Yes	Yes	Yes
Branch × Time FE	No	Yes	No	Yes
Home District × Time	Yes	No	Yes	No
R^2	0.858	0.858	0.860	0.860
Obs.	58660	58660	58660	58660

Table 7: Impact of Riot Experience on Loan Performance

In this table we investigate how riot exposure impacts loan performance. Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. Our analysis compares the default rates of loans disbursed to Muslim versus non-Muslim borrowers by riot-exposed managers versus those with no riot exposure. In columns 1 and 2 we include branch × borrower religion fixed effects, district × borrower religion × quarter fixed effects and lender home district × quarter fixed effects. In columns 3 and 4, we include branch × quarter fixed effects. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	Default (1)	Default (2)	Default (3)	Default (4)
Riot	-0.035*	-0.035*		
	(0.018)	(0.018)		
Non-Muslim Borrowers × Riot	0.025**		0.018*	
	(0.012)		(0.011)	
Hindu Borrowers × Riot		0.025**		0.019*
		(0.012)		(0.011)
Other Borrowers × Riot		0.024		0.008
		(0.020)		(0.016)
Controls	Yes	Yes	No	No
Branch × Religion	Yes	Yes	Yes	Yes
District × Religion × Time	Yes	Yes	Yes	Yes
Branch × Time FE	No	No	Yes	Yes
Home District × Time	Yes	Yes	No	No
R^2	0.608	0.608	0.770	0.770
Obs.	24531	24531	19500	19500

Table 8: Impact of Riot Experience on Lending Decisions Across Bank Experience

In this table we investigate whether the impact of riot experience varies based on a branch manager's tenure with the bank. Riot Experience = 1 for any loan officer who experienced 10 or more deaths while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of new debt. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

		$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$	
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
High Bank Experience × Riot Experience Dummy	-0.048***	0.044***	-0.013	-0.031***	0.028**	-0.008
	(0.014)	(0.015)	(0.014)	(0.010)	(0.012)	(0.013)
Low Bank Experience × Riot Experience Dummy	-0.029*	0.028*	-0.007	-0.028**	0.029**	-0.002
	(0.015)	(0.017)	(0.018)	(0.012)	(0.013)	(0.015)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095

Table 9: Impact of Riot Experience on Lending Decisions to New Borrowers

Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We include branch, district $\times$ quarter, and home district $\times$ quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of new debt to first-time borrowers. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts to first-time borrowers. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-0.045***	0.043**	-0.017	-0.030**	0.030**	-0.015
	(0.016)	(0.018)	(0.016)	(0.012)	(0.014)	(0.015)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District $\times$ Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.708	0.715	0.739	0.788	0.763	0.749
Obs.	11761	12550	9054	11761	12550	9054

Table 10: Impact of Riot Experience on Lending Decisions to Repeat Borrowers

Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of new debt to repeat borrowers. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts to repeat borrowers. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-0.045*	0.019	0.027	-0.054**	0.036	0.012
	(0.024)	(0.024)	(0.023)	(0.022)	(0.024)	(0.021)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.591	0.662	0.743	0.615	0.682	0.768
Obs.	8953	9613	7050	8980	9646	7068

Table 11: Impact of Riot Experience on Lending Decisions: Rural versus Urban Areas

Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. Urban is a dummy variable denoting that the branch is located in a semiurban, urban, or metropolitan area. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of new debt. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

		$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$	
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Urban × Riot Experience Dummy	-0.003	0.003	0.010	-0.002	-0.002	0.021*
	(0.016)	(0.016)	(0.014)	(0.013)	(0.013)	(0.011)
Riot Experience Dummy	-0.042***	0.039***	-0.016	-0.030***	0.029**	-0.016
	(0.013)	(0.015)	(0.014)	(0.011)	(0.012)	(0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11784	12577	9081	11784	12577	9081

Table 12: Impact of Riot Experience on Lending in Monopoly versus Competitive Branches

Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We define a branch as a Monopoly if there are no other branches (from the same bank or other banks) within a 10 kilometer radius. We include branch, district × quarter fixed effects and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of new debt. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Monopoly × Riot Experience Dummy	0.010	-0.009	-0.001	0.008	-0.002	-0.018
	(0.015)	(0.016)	(0.015)	(0.012)	(0.013)	(0.012)
Riot Experience Dummy	-0.048***	0.045***	-0.012	-0.035***	0.029**	0.001
	(0.016)	(0.016)	(0.015)	(0.013)	(0.013)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11784	12577	9081	11784	12577	9081

Table 13: Impact of Age of First Riot Experience on Lending Decisions

Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We group managers with riot exposure into three categories: (1) Managers who experienced their first riot at age ≤ 10; (2) Managers who experienced their first riot between the ages of 11 and 18; (3) Managers who experienced their first riot after the age of 18. We include branch, district $\times$ quarter, and home district $\times$ quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of new debt. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy $\times$ First Riot Experience (<10 Years)	-0.091***	0.083**	-0.020	-0.075***	0.053**	0.028
	(0.031)	(0.039)	(0.034)	(0.025)	(0.024)	(0.036)
Riot Experience Dummy $\times$ First Riot Experience ($10-18$ Years)	-0.059**	0.055	-0.014	-0.043*	0.023	0.032
	(0.029)	(0.037)	(0.034)	(0.023)	(0.024)	(0.035)
riot	0.015	-0.014	0.002	0.013	0.003	-0.038
	(0.026)	(0.036)	(0.035)	(0.022)	(0.022)	(0.036)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District $\times$ Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.812	0.796	0.794
Obs.	11799	12594	9095	11799	12594	9095

Table 14: Impact of Gujarat Riot Experience on Lending Decisions

This table examines how the 2002 Gujarat riots affected lending decisions. We restrict our sample to branches outside of Gujarat where Gujarat-exposed branch managers were posted following the riots. See the text for further details of the sample construction and analysis.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Gujarat Riot Experience Dummy	-0.081***	0.096***	-0.021	-0.038**	0.052***	-0.018*
	(0.026)	(0.029)	(0.015)	(0.016)	(0.016)	(0.010)
Branch	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.755	0.689	0.390	0.827	0.754	0.469
Obs.	324	331	227	324	331	227
Panel B						
Gujarat Riot Experience Dummy	-0.089**	0.079*	0.002	-0.051**	0.056**	-0.012
	(0.034)	(0.040)	(0.014)	(0.021)	(0.021)	(0.010)
Branch	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.869	0.804	0.497	0.908	0.861	0.515
Obs.	229	236	143	229	236	143

Table 15: TV Viewership and the 2002 Gujarat Riots

This table examines how TV viewership affected managers' lending decisions following the 2002 Gujarat riots. See the text for details of the sample and variable construction, and for information on the estimation. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

		$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \# NewDebt}$	
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Share of TV Viewers × Post	-0.060**	0.063***	-0.005	-0.036*	0.034*	0.001
	(0.023)	(0.024)	(0.018)	(0.020)	(0.020)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.777	0.787	0.794	0.848	0.837	0.820
Obs.	11552	12075	9360	11552	12075	9360

Table 16: Newspaper Circulation and the 2002 Gujarat Riot

This table examines how newspaper circulation affected managers' lending decisions following the 2002 Gujarat riots. See the text for details of the sample and variable construction, and for information on the estimation. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Newspaper Circulation × Post	-0.086	0.064*	-0.012	-0.064	0.035	-0.006
	(0.059)	(0.038)	(0.031)	(0.063)	(0.033)	(0.021)
Newspaper Circulation	0.084	-0.037	-0.028	0.050	-0.037	0.010
	(0.068)	(0.065)	(0.056)	(0.064)	(0.053)	(0.047)
Branch	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.689	0.714	0.745	0.789	0.785	0.796
Obs.	13518	14123	11532	13518	14123	11532

Appendix:

Table A.1: Impact of Riot Experience on Lending Decisions (Deaths Experienced >=5)

In this table we present the impact of riot experience on lending to borrowers belonging to different religions. Riot Experience = 1 for any loan officer who experienced 5 or more riot-related deaths while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

		$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$	
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-0.038***	0.037***	-0.015	-0.023**	0.022*	-0.010
	(0.012)	(0.013)	(0.012)	(0.010)	(0.011)	(0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095

Table A.2: Impact of Riot Experience on Lending Decisions (Deaths Experienced >0)

In this table we present the impact of riot experience on lending to borrowers belonging to different religions. Riot Experience = 1 for any loan officer who experienced at least one riot-related death while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-0.022**	0.013	0.001	-0.015*	0.008	-0.001
	(0.010)	(0.011)	(0.010)	(0.008)	(0.010)	(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095

Table A.3: Impact of Riot Experience on Lending Decisions — Log(1+Deaths Experienced)

In this table we present the impact of riot experience on lending to borrowers belonging to different religions, using a continuous measure of riot exposure based on the number of riot-related deaths experienced by a branch manager while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Log (1+Death)	-0.008**	0.007**	-0.003	-0.005**	0.004*	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095

Table A.4: Impact of Experiencing the 1969 Gujarat Riots on Lending Decisions

In this table we present the impact of riot experience on lending to borrowers belonging to different religions. Riot Experience = 1 for any loan officer who was in his hometown during the 1969 riots, and whose hometown had at least one fatality during the riots. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Experienced Riot of 1969	-0.042***	0.038***	-0.009	-0.027**	0.023*	-0.006
	(0.013)	(0.014)	(0.013)	(0.011)	(0.012)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.735	0.753	0.780	0.813	0.796	0.793
Obs.	10530	11212	8155	10530	11212	8155

Table A.5: Impact of Riot Experience on Lending Decisions, Controlling for Hometown Population

In this table we present the impact of riot experience on lending to borrowers belonging to different religions, controlling for hometown population. Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

		$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$	
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
Riot Experience Dummy	-0.040***	0.045***	-0.025	-0.023*	0.025*	-0.011
	(0.015)	(0.017)	(0.016)	(0.012)	(0.014)	(0.015)
Log(Population)	-0.001	-0.001	0.002	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095

Table A.6: Impact of Riot Experience on Lending Decisions Across Branch Experience

In this table we present the impact of riot experience on lending to borrowers belonging to different religions, controlling for hometown population, allowing the effect to differ based on whether the branch manager has above or below median tenure at the bank. Riot Experience = 1 for any loan officer who experienced 10 or more riot-related deaths while living in his hometown. We include branch, district × quarter, and home district × quarter fixed effects. In columns 1, 2 and 3 the dependent variable is the share of debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Standard errors are clustered at the branch manager level. ***, **, * denote statistical significance at the 1%, 5% and 10% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers	Hindu Borrowers	Other Borrowers	Muslim Borrowers	Hindu Borrowers	Other Borrowers
	(1)	(2)	(3)	(4)	(5)	(6)
High Branch Experience × Riot Experience Dummy	-0.045***	0.041***	-0.010	-0.034***	0.033***	-0.007
	(0.013)	(0.015)	(0.014)	(0.011)	(0.013)	(0.012)
Low Branch Experience × Riot Experience Dummy	-0.041***	0.039***	-0.013	-0.028***	0.025**	-0.007
	(0.013)	(0.015)	(0.013)	(0.011)	(0.012)	(0.013)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.793
Obs.	11799	12594	9095	11799	12594	9095