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Fisman, R, Sarkar, A, Skrastins, J and Vig, V

(2020)

Experience of communal conflicts and intergroup lending.

Journal of Political Economy, 128 (9).

ISSN 0022-3808

DOI: https://doi.org/10.1086/708856

University of Chicago Press
https://www.journals.uchicago.edu/doi/10.1086/7088...

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Experience of Communal Conflicts and Inter-group Lending *

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February 27, 2020

Abstract

We provide microeconomic evidence on ethnic frictions and market efficiency, using dyadic data on managers and borrowers from a large Indian bank. We conjecture that, if exposure to religion-based communal violence intensifies inter-group animosity, riot exposure will lead to lending decisions that are more sensitive to a borrower's religion. We find that riot-exposed Hindu branch managers lend relatively less to Muslim borrowers, and these loans are less likely to default, consistent with riot exposure exacerbating 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.

*This project has received funding from the European Research Council (ERC) under the European Unions Horizon 2020 research and innovation programme (grant agreement No. 679747)

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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, account for a third of Africa's economic under-performance. 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 branch managers 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 and Wilkinson (2006)), combined with data on each branch manager's year and city of birth, we can infer whether a branch manager's hometown experienced ethnic riots during his youth. (As explained in greater detail in Section II.A, we measure riot exposure based on riot fatalities in a manager's hometown that occur between his birth year and until the manager joins the bank, which is generally in his early 20s.) 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. 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. Finally, two features of our setting allow for the credible identification of the impact of riot exposure as distinct from (cross-sectional) differences in places or origin or branch locations. First, because of localized differences in the occurrence and timing of fatal riots, we can identify the effect of riots in our main regressions based on *within-district* differences in riot exposure (so identification comes from comparing managers from different hometowns within the same district, and/or from the same city but different age cohorts).[1] Second, the frequent rotation of officers – some with riot exposure and others not – allows us to distinguish the effect of riot exposure from branch location attributes and/or time trends, in particular via "event plots" of changes in lending patterns around 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).

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 minimum) one riot with 10 or more deaths (our main measure is 10 or more deaths summed across all riots), or use $log(1 + Deaths)$ as a continuous measure of riot intensity.[2]

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.[3] 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

[1]As we show in Section III.A, our main results are driven by within-hometown variation in the timing of riots.

[2]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 branch managers exposed to this event.

[3]Note that statistical discrimination makes ambiguous predictions – it depends on whether less precise information on borrowers leads to pooling among borrower types. See Fisman *et al.* (2017) for a brief discussion. Insofar as the results in this paper are concerned, the main point is that less information *can* lead to reduced borrowing, so we are required to consider credit quality to rule out this explanation.

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.[4]

We can rule out, to a large extent, alternative explanations for the patterns we observe by exploiting the granularity of our data. District $\times$ 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 particular group in a given branch.[5] Finally, fixed effects based on the manager's place of birth (Home District $\times$ Quarter fixed effects) ensure that we can distinguish the riot-exposure effect from broader in-group biases in geographies that are generally associated with religious animosities.

In summary, our main results provide evidence that differences in out-group animus based solely on early exposure to religious conflict leads to significant inefficiencies in loan allocation. Furthermore, our source of identification — local riots during branch managers' early 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

[4] 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.

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

the impact of riot exposure on lending decisions as a function of when the manager was first exposed to Hindu-Muslim violence, grouping managers 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. We find a similar effect of riot exposure for monopoly and competitive branches, which mitigates the concern that we are overestimating the overall impact of riot exposure on Muslims due to switching by borrowers facing discrimination.

In our final analysis we turn to a contemporaneous shock to branch managers' preferences resulting from the 2002 Gujarat riots that resulted in over 2000 fatalities. We find that, following these riots, lending to Muslims declined by 9.6 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 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 at 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 explored, for example, 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.* (2017) on CEOs' exposure to early life disaster and corporate risk-taking), and how exposure to different economic systems may affect attitudes toward gender roles the workplace (see Campa and Serafinelli (2019)). We similarly document long-lasting effects from early life experiences, focused on 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 and Wilkinson (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 branch manager'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 the very small number of 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 (2019)).[6]

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. Additionally, we omit branch-quarter-religion observations in which there are zero loans outstanding to that group. We do so because, we argue, this reflects a general absence of credit demand from that group such that an individual manager has little discretion to affect the level of credit. Furthermore, our credit quality measure is undefined in these cases.[7] In instances in which two groups (out of three)

[6] The branch level is determined by the level of the manager in charge of it. There are 6 levels; levels 4 and above focus primarily on corporate lending, and are run by very senior employees within the bank. Our results are virtually unchanged if we add higher-level managers, which is unsurprising given that almost all branches are level 3 and below. For example, adding level 4 branches increases our sample size by less than 0.1 percent.

[7] Our point estimates are similar though marginally smaller if we include all branch-quarter-religion observations, regardless of whether there are zero loans outstanding or zero lending to non-Hindus. If we generate a balanced panel by omitting branches that *ever* have zero loans outstanding, our point estimates are again similar but marginally larger in magnitude.

have zero loans outstanding (i.e., the stock of loans is zero) in a branch-quarter, the third group is also dropped from the sample.

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.[8] 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 each loan officer's religion, the personnel records also contain information on the hometown, year of birth and the year the officer joined the bank. Unfortunately, for many officers these data – particularly on city and year of birth – are missing. After dropping observations for individuals for whom the key entries are missing, our branch manager sample includes 1779 individuals (details on the number of observations dropped as a result of missing data for each variable are listed in Online Appendix Table A.1.

For the sample of 1779 branch managers, we use birth year and hometown information to link each individual 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 joined 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, branch managers (and loan officers more generally) experience frequent rotation among branches. By looking at shifts in lending around branch manager turnover, we will be able to identify the effect of managers' riot exposure on loan decisions, as distinct from other trends in borrowing that might vary across branches.

[8] 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.

Finally, we note 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 underdeveloped infrastructure and/or 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 weak relative to private banks).[9] Thus, to the extent that we observe favoritism in lending that worsens an officer's repayment rates, we may say that he faces a cost to obtain the utility benefits from prejudice.

II.B Conflict Data

Our conflict data come from Varshney and Wilkinson (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.[10] As Varshney and Wilkinson (2006) emphasize, the city (rather than a higher level of aggregation such as the state) is the"the most logical and significant level of analysis," because of the substantial

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

[10]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.

within-state variation in the extent of riots. Our measure of riot exposure is thus also constructed at the city-level: for each branch manager, 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 branch managers 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 branch manager 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 and/or few in number. We also present our main results with $log(1 + RiotDeaths)$ as a riot exposure measure; additionally, we present 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 branch managers exposed to at least a single riot with ten or more deaths.

In Table 3, we present summary statistics (at the branch-group-quarter level) for the main variables we employ in our analysis. We observe that only 16.8 percent of observations have branch managers with $RiotDeaths > 0$. 11.9 and 9.6 percent of branch managers 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 branch managers, 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. The data also indicate a high frequency of turnover among branch managers – the mean (median) spell in a branch is 8.03 (8) quarters, with standard deviation of 4.2. This churn generates a large number of transitions in our data – we observe an average of 38 head officer reallocations per quarter, and the median branch has one officer change during our sample period.

II.C Additional City- and State-Level Data

While most potential covariates 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, both overall and by religious affiliation, 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 branch managers 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 main specification takes the following form:

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

$ReligShare_{bq}$ 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. Thus, the identifying variation is *within*-district but across branches (there are, on average, 5.7 branches per district). Finally, we control for a range of manager attributes, including quarters of experience at the bank, quarters of experience at the branch, age, gender, and caste dummies. To allow for non-linear and/or non-monotonic effects of the experience and age controls, we include also the square of each variable.

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).

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 branch manager 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).[11] 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 a manager's riot exposure has a causal effect on lending across religions, we expect a discrete increase (decrease) 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

[11] The coefficients do not add up to precisely zero because of the small differences in sample across specifications. Recall that we omit branch-quarter observations for each column if loans outstanding are zero to that religion in a given quarter, which we suggest reflects a lack of any substantive credit demand from that group which could be affected by a manager's discretion.

shares of total lending:

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

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. Overall, these patterns are difficult to reconcile with the endogenous placement of branch managers to specific branches (within a district) on the basis of a growth or decline in Muslim credit demand: such explanations would not predict a well-defined change in credit provision precisely coincident with the arrival (or departure) of a riot-exposed manager.

In Online Appendix Table A.2, we provide the results of specifications that use $log(1 + AmountBorrowed)$ as the outcome variable, rather than the share of borrowing. 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 coefficients in these specifications are imprecisely estimated, but suggest that the shift in lending composition comes primarily from a reduction in Muslim borrowing rather than an increase in Hindu borrowing.

We now turn to a set of analyses that probe the robustness of our main results to alternative specifications, definitions of riot exposure, and further controls.

As noted earlier, because we include $HomeDistrict \times Quarter$ fixed effects, we identify the effect of riot exposure from both within-hometown variation in riot exposure of different cohorts, as well as cross-hometown variation in riot exposure within a district. We begin by assessing which of these sources of variation is driving our results. To capture the role of within-hometown variation, we implement a specification that includes $Hometown \times Quarter$ fixed effects.[12] To focus on cross-hometown variation within a district, we generate time-invariant measures of riot exposure based on riot fatalities over the entire 1950-1995 sample. Intuitively, this latter variable should capture the extent to which a town generally has Hindu-Muslim frictions.

We present these results in Online Appendix Tables A.3 and A.4 respectively. Focusing first on the within-hometown variation, we find that the estimated effect of riot exposure on Muslim credit share is larger, but very imprecisely estimated (p-value = 0.051). When we look at the relationship between the time-invariant measure of riot exposure, it is close to zero. Given the noisiness of these results, they should naturally be treated with considerable caution, but overall they suggest that our results are driven by within-hometown differences in riot exposure across managers from different birth cohorts.

To assess robustness to the definition of riot exposure, in Online Appendix Tables A.5 – A.7, we show the patterns for alternative definitions of $RiotExperience$, based on both more

[12]In this specification, while the effect of riot exposure is identified from variation in riots within a hometown across time, it is also possible that we are picking up the effects of (time-specific) efforts to exacerbate Hindu-Muslim animosities that in turn affect the timing of riots.

and less restrictive measures of exposure. The definition of riot exposure in Table A.5 is an indicator variable denoting the occurrence of at least one riot with 10 or more deaths in a branch manager's hometown during his youth. This definition – which requires the existence of at least one large riot, rather than several smaller ones – generates patterns that are very similar to those in Table 4. Tables A.6 and A.7 use cutoffs of 5 and 1 deaths during a branch manager's youth (across all riots). For the cutoff of 5 deaths we again observe results that are similar to those in Table 4, albeit marginally weaker. For a cutoff of 1 death, however, 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 assigning $RiotExperience = 1$ 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 Online Appendix Table A.8, we use a continuous measure ($log(1 + Deaths)$); results based on this measure are very similar to those reported in our main specification. In Online Appendix Table A.9 we use the most significant riot event during our sample period, the Gujarat riots of 1969, to define riot exposure (i.e., a branch manager 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 branch manager's riot exposure and Muslims' share of borrowing. Finally, we show that our main results are unaffected by controlling for hometown population (or other hometown attributes) — 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 $log(CityPopulation)$ and its square in Online Appendix Table A.10, as well as the Hindu and Muslim shares of the population (and also their squares). None of the coefficients of interest are affected, and in no case does the coefficient on city population approach significance.[13]

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 explore the effects of riot exposure on repayment in Table 5. In our first pair of regressions we include branch $\times$ religion (α_{br}) 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:

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

$Default_{bqr}$ 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 -0.023 (significant at the 5 percent level), indicating that loans issued to Muslim borrowers by riot-exposed branch managers have a default rate that is 2.3 percentage points lower than those issued by non-riot branch managers. As a benchmark, the default rate among non-riot branch managers to Muslim borrowers in the sample of branches with non-zero Muslim default is 6.3 percent, indicating that riot exposure leads to a 35 percent decline in Muslim default. The coefficient on the interaction term, β_2, is 0.023 (significant at the 5 percent level), indicating that the lower default rate for riot-experienced

[13] 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.

managers manifests itself only 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 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 Home District × Quarter fixed effects. The patterns are qualitatively very similar to those presented in the preceding specification.

An alternative interpretation is that riot-exposed managers are harsher in their enforcement of loan repayment by Muslim borrowers. To explore this possibility, we look at repayment of loans issued prior to a manager's arrival at a branch that are still outstanding in quarter q, and ask whether they go into default in that quarter. This captures the enforcement margin without clouding the interpretation by the inclusion of loans that the officer himself has issued. We present these results in columns (5) – (8), using specifications that exactly parallel those in the first half of the table, but with default on inherited loans as the outcome. We find that riot exposure has no effect on loan repayment of inherited loans, suggesting that favoritism in loan provision (rather than enforcement) is driving our main results.

III.C Branch manager and borrower experience

There are several ways that lender experience could attenuate the negative effects of riot exposure on Muslim credit provision. First, to the extent that the negative effect is driven by mistaken beliefs about Muslim creditworthiness, a branch manager 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. Exposure could also attenuate the effect of riot exposure if personal contact reduced animosity toward Muslims (as in the contact hypothesis of Allport (1954)).

To explore whether the effect of riot exposure varies over a branch manager's career, we augment equation 4 to included the interaction of riot exposure and an indicator variable denoting whether a branch manager's years with the bank is above the sample median of 24 years. We present these results in Table 6. If the effect of riot exposure declines with branch manager experience, we expect the interaction term to be positive in column (1) (and negative in column (2)). We find instead that the point estimate in column (1) 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 Online Appendix Table A.11, we consider a separate margin of exposure – the time that a branch manager has spent in a particular branch. We do so by defining an indicator variable denoting observations for which the branch manager 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.

We next consider whether borrower experience – in particular whether he or she has repaid loans in the past – mitigates a riot-exposed manager's negative priors on the borrower's creditworthiness. We do so by splitting our sample into lending to new versus repeat borrowers, and examining the effects of riot exposure in these two groups separately. We present these analyses in Tables 7 and 8, where we find that 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 does not vary with lender or borrower experience. We take this as further evidence that the relationship we document in our main results 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. Furthermore, these results indicate that, to the extent that our main results may be interpreted as taste-based discrimination, this animosity toward Muslim borrowers by Hindu branch managers does not dissipate with time.

III.D Competition, borrower demand, and the impact of riot exposure

In this section we examine heterogeneity in the effect of riot exposure as a function of a branch's location along a pair of dimensions that reflect the ability of potential borrowers to migrate across branches within the bank, or across banks.

We begin by comparing 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).

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 not biasing 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 9 we augment specification 4 with the interaction of $RiotExperience$ and $Monopoly$, an indicator variable which denotes monopoly branches.

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.

As a second approach to exploring displacement of borrowers across branches within the same bank, we present in Online Appendix Table A.12 results which look at whether lending at a branch is affected by the arrival of a riot-exposed Hindu manager at another nearby branch. If there were a displacement effect, then the arrival of a riot-exposed manager should increase Muslim borrowing at neighboring branches. We define the variable, $RiotExposedNearby_bq$, to denote the presence of a riot-exposed Hindu manager at a branch within a distance of 10km of b in quarter q (we obtain near-identical results if the radius is extended to 20 km). We do not find any such displacement effects, which again argues against the switching of borrowers across branches, in this case within the bank.

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).[14]

We group branch managers 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 10, we interact the riot exposure dummy variable with indicator variables for first exposure before age 10

[14]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.

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 branch managers 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 branch managers 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 in itself, 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 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 thus 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 were subsequently rotated, and examine whether lending patterns shifted around their arrival or departure at these subsequent placements. (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 11 we report the results from the following specification:

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

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, Hindus' share of lending increases by 10.3 percentage points, while the Muslim share declines by 9.6 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, and when we 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 branch managers 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 11) 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 $\times$ 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 A.13 and Table A.14 for television viewership and newspaper circulation respectively, suggest that branch managers 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 A.14 are also 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 branch managers 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 (2019), 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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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) branch managers.

	Borrower (%)	Branch Manager (%)
Hindu	89.40	95.68
Muslim	6.87	1.73
Christian	1.25	1.34
Sikh	1.84	0.77
Parsi	0.12	0.06
Budhist	0.28	0.18
Others	0.24	0.24

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 branch managers who experience at least 1 riot-related death in their hometown and branch managers 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	63.29	161.41	1.00	12.00	608.00	-	-	-	-	-
No. of Branches Worked	2.00	0.96	1.00	2.00	4.00	1.91	0.96	1.00	2.00	5.00
Age	47.59	4.31	33.00	48.00	55.00	46.70	4.44	34.00	47.00	55.00
Total Experience in Bank (Years)	24.24	5.36	8.00	25.00	35.00	23.29	4.66	10.00	24.00	33.00
Sum of New Credit (INR Mn)	1.07	4.88	0.00	0.12	10.02	1.05	2.87	0.00	0.12	10.33
Sum of New Credit to New Borrowers (INR Mn)	0.89	4.68	0.00	0.09	8.55	0.87	2.52	0.00	0.08	8.69
Sum of New Credit to Repeat Borrowers (INR Mn)	0.18	0.82	0.00	0.00	2.65	0.18	0.87	0.00	0.00	2.54
No. of New Loans	16.85	41.07	0.00	3.00	116.00	18.06	35.50	0.00	3.00	149.00
No. of New Loans to New Borrowers	14.12	36.38	0.00	2.00	101.00	15.25	30.86	0.00	2.00	124.00
Default	0.02	0.09	0.00	0.00	0.50	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 branch manager 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 debt. In columns 4, 5 and 6 the dependent variable is the share of the number of loans. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers (1)	Hindu Borrowers (2)	Other Borrowers (3)	Muslim Borrowers (4)	Hindu Borrowers (5)	Other Borrowers (6)
Riot Experience Dummy	-0.043[a]	0.040[a]	-0.011	-0.029[a]	0.028[b]	-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 $\times$ 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 5: Impact of Riot Experience on Loan Performance

In this table we investigate how riot exposure impacts loan performance. Riot Experience = 1 for any branch manager who experienced 10 or more riot-related deaths while living in his hometown. Our analysis in columns 1-4 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 5-8 we compare the default rates for Muslim versus non-Muslim borrowers, for loans that were inherited by riot-exposed managers versus those with no riot exposure. In columns 1, 2, 5, and 6 we include branch × borrower religion fixed effects and district × borrower religion × quarter fixed effects. In columns 3, 4, 7, and 8 we also include lender home district × quarter fixed effects. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	Default on Loans Extended				Default on Loans Inherited			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Riot	-0.023^{b}	-0.023^{b}	-0.036^{b}	-0.035^{b}	-0.002	-0.002	0.002	0.002
	(0.011)	(0.011)	(0.018)	(0.018)	(0.007)	(0.007)	(0.009)	(0.009)
Non-Muslim Borrowers × Riot	0.023^{b}		0.025^{b}		0.001		0.001	
	(0.011)		(0.012)		(0.007)		(0.008)	
Hindu Borrowers × Riot		0.023^{b}		0.025^{b}		0.001		0.000
		(0.011)		(0.012)		(0.007)		(0.008)
Other Borrowers × Riot		0.023		0.025		0.001		0.003
		(0.020)		(0.020)		(0.012)		(0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Branch × Religion	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District × Religion × Time	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.494	0.494	0.608	0.608	0.415	0.415	0.514	0.514
Obs.	25334	25334	24534	24534	35090	35090	34960	34960

Table 6: 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 branch manager 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. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers (1)	Hindu Borrowers (2)	Other Borrowers (3)	Muslim Borrowers (4)	Hindu Borrowers (5)	Other Borrowers (6)
High Bank Experience	-0.015	0.013	-0.005	-0.005	0.004	-0.010
× Riot Experience Dummy	(0.011)	(0.012)	(0.009)	(0.009)	(0.010)	(0.010)
Riot Experience Dummy	-0.033^{b}	0.031	-0.008	-0.026^{b}	0.025^{b}	0.000
	(0.015)	(0.017)	(0.015)	(0.011)	(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	0.812	0.796	0.794
Obs.	11799	12594	9095	11799	12594	9095

Table 7: Impact of Riot Experience on Lending Decisions to New Borrowers

Riot Experience = 1 for any branch manager 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 first-time borrowers. In columns 4, 5 and 6 the dependent variable is the share of new loan contracts to first-time borrowers. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% 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[a]	0.043[b]	-0.016	-0.029[b]	0.029[b]	-0.014
	(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 × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home District × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.708	0.715	0.739	0.789	0.763	0.749
Obs.	11761	12550	9054	11761	12550	9054

Table 8: Impact of Riot Experience on Lending Decisions to Repeat Borrowers

Riot Experience = 1 for any branch manager 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. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers (1)	Hindu Borrowers (2)	Other Borrowers (3)	Muslim Borrowers (4)	Hindu Borrowers (5)	Other Borrowers (6)
Riot Experience Dummy	-0.045	0.019	0.029	-0.054^{b}	0.037	0.014
	(0.024)	(0.024)	(0.023)	(0.022)	(0.024)	(0.020)
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	0.767
Obs.	8953	9613	7050	8953	9613	7050

Table 9: Impact of Riot Experience on Lending in Monopoly versus Competitive Branches

Riot Experience = 1 for any branch manager 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. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers (1)	Hindu Borrowers (2)	Other Borrowers (3)	Muslim Borrowers (4)	Hindu Borrowers (5)	Other Borrowers (6)
Monopoly Branch × Riot Experience Dummy	0.011	-0.010	-0.001	0.008	-0.002	-0.018
	(0.015)	(0.016)	(0.015)	(0.012)	(0.013)	(0.012)
Riot Experience Dummy	-0.049[a]	0.045[a]	-0.011	-0.034[a]	0.029[b]	0.001
	(0.016)	(0.017)	(0.015)	(0.012)	(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 Town × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.729	0.746	0.777	0.811	0.796	0.794
Obs.	11784	12577	9081	11784	12577	9081

Table 10: Impact of Age of First Riot Experience on Lending Decisions

Riot Experience = 1 for any branch manager 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. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers (1)	Hindu Borrowers (2)	Other Borrowers (3)	Muslim Borrowers (4)	Hindu Borrowers (5)	Other Borrowers (6)
Riot Experience Dummy	-0.091[a]	0.083[b]	-0.016	-0.071[a]	0.052[b]	0.029
× First Riot Experience (< 10 Years)	(0.031)	(0.039)	(0.034)	(0.025)	(0.024)	(0.036)
Riot Experience Dummy	-0.057[b]	0.052	-0.011	-0.041	0.022	0.032
× First Riot Experience (10 − 18 Years)	(0.028)	(0.037)	(0.034)	(0.023)	(0.024)	(0.035)
Riot Experience Dummy	0.013	-0.012	-0.000	0.011	0.004	-0.037
	(0.026)	(0.035)	(0.035)	(0.022)	(0.023)	(0.036)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Branch	Yes	Yes	Yes	Yes	Yes	Yes
District × Time	Yes	Yes	Yes	Yes	Yes	Yes
Home town × 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 11: 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. Manager controls include age, experience in the bank, experience in the branch, square of them, gender dummy and caste dummy. Standard errors are clustered at the branch manager level. a, b denote statistical significance at the 1% and 5% levels.

	$\frac{NewDebt}{\Sigma NewDebt}$			$\frac{\#NewDebt}{\Sigma \#NewDebt}$		
	Muslim Borrowers (1)	Hindu Borrowers (2)	Other Borrowers (3)	Muslim Borrowers (4)	Hindu Borrowers (5)	Other Borrowers (6)
Panel A						
Gujarat Riot Experience Dummy	-0.096^a	0.103^a	-0.014	-0.043^a	0.049^a	-0.011
	(0.025)	(0.027)	(0.012)	(0.013)	(0.016)	(0.008)
Branch	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.785	0.715	0.445	0.852	0.773	0.581
Obs.	324	331	227	324	331	227
Panel B						
Gujarat Riot Experience Dummy	-0.109^a	0.097	0.009	-0.063^a	0.069^a	-0.013
	(0.036)	(0.051)	(0.025)	(0.021)	(0.022)	(0.008)
Branch	Yes	Yes	Yes	Yes	Yes	Yes
State × Time	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.877	0.816	0.546	0.918	0.876	0.581
Obs.	229	236	143	229	236	143

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, which is nearly 17% of the sample of managers in our study.

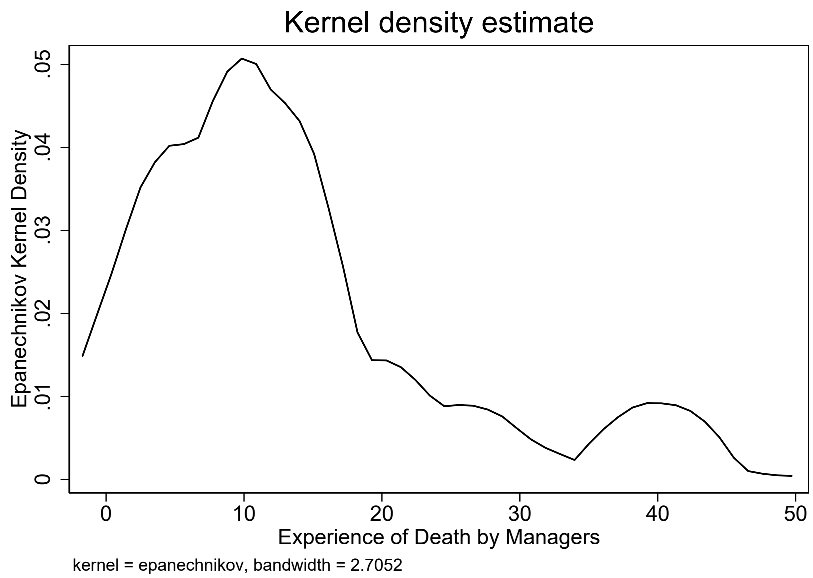

Figure 2: Share Of Lending to Muslims versus Hindu Borrowers Around Officer Transitions

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.

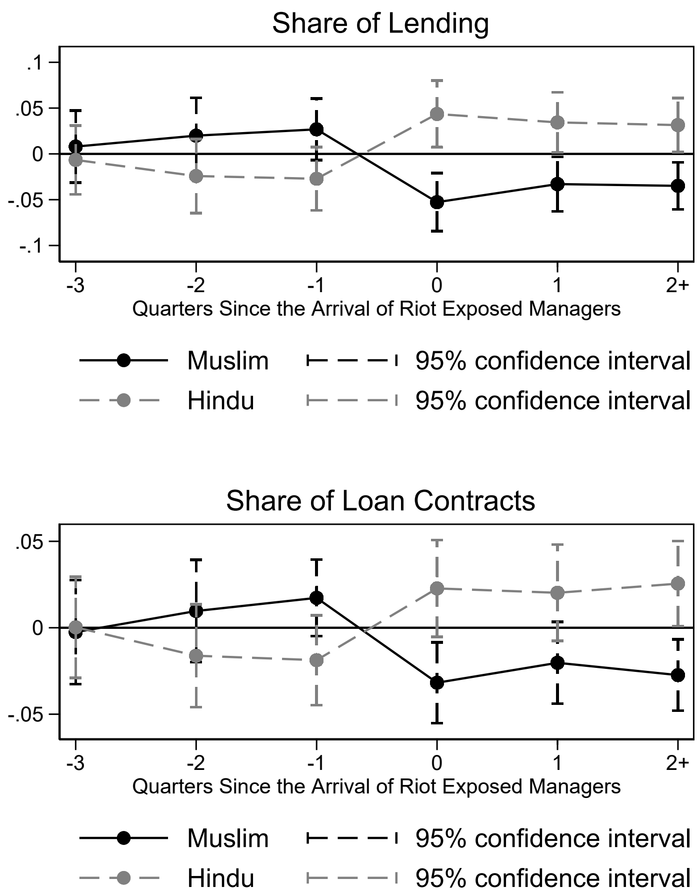

Figure 3: Riot Exposure and the Share Of Lending to Muslims Borrowers Across Branch Manager Tenure at the Bank

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 managers years of employment at the bank. The whiskers show 95 percent confidence intervals.