The Early Origins of Judicial Stringency in Bail Decisions: Evidence from early-childhood Exposure to Hindu-Muslim Riots in India

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Abstract. We estimate the causal effects of judge's exposure to communal violence during early childhood on the pretrial detention rates in their cases. We exploit novel administrative data on judgments and detailed resumes of judicial officers born during 1955-1991. Our baseline result is that judges exposed to communal violence between the ages 0- and 6 are 16 percent more prone to deny bail than the average judge. Heterogeneity analyses show that the impact is stronger for the experience of riots between the ages 3- and 6 years. We further find that the observed judicial stringency is driven by childhood exposure to riots with a higher duration of state-imposed lockdowns and low riot casualties. This result is consistent with the hypothesis that early childhood exposure to effective state intervention in social disorder generates a persistent effect on support for the state.

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1. Introduction

About three million people are held as pretrial detainees worldwide (Walmsley 2018). The use of pretrial detention as a crime policy tool is motivated by its potential to reduce pretrial flight (Kling 2006), and recidivism (Ribeiro and Ferraz 2019) via incapacitation, and deterrence (W. Dobbie, Goldin, and C. S. Yang 2018). However, pretrial detention typically lasts for several months and results in consequences for both the defendant and society (Kleinberg et al. 2018). Pretrial detention has been associated with an increase in the likelihood of being convicted and in the length of incarceration sentences (Stevenson 2018). It also has substantial economic costs in terms of the loss of formal employment (W. Dobbie, Goldin, and C. S. Yang 2018), increase in the accumulation of debts (Stevenson 2018), and nontrivial criminogenic effects (Kling 2006; Leslie and Pope 2017).

Recent studies have documented that a significant portion of the economic costs of pretrial detentions is borne by low-income and minority communities (W. Dobbie and C. Yang 2021; Henrichson et al. 2015). This finding is of particular concern since various studies have found evidence of judicial stringency and racial disparities in bail decisions (Kleinberg et al. 2018; Arnold, W. Dobbie, and C. S. Yang 2018). Nevertheless, although a significant number of studies have provided evidence of biases in courtroom decisions[1]), little is known about the origins of such judicial biases or stringency.

In our paper, we examine the origins of judicial stringency in bail decisions, which are decisions on pretrial detentions. Bail decisions affect millions worldwide, for example, in the United States (US) alone over 10 million people are impacted (Kleinberg et al. 2018) with roughly 450,000 people awaiting trial in jail on any given day (Minton and Zang 2015). The decision on whether a defendant should await trial in jail or at home potentially reflects the trade-offs that judges make between the perceived risks of new crimes that a defendant can commit while awaiting trial out of jail and the costs of incarceration (Kleinberg et al. 2018). Therefore, variations in judicial decisions could be driven by differences in either fundamental preference parameters or beliefs that define these trade-offs for judges. In this regard, a growing body of causal research on early-childhood in economics shows that early life exposure to a socio-political environment causes the development of fundamental parameters, such as later-life social preferences (Cappelen et al. 2020), preferences for honesty (Abeler, Falk, and Kosse 2021) and political identity (Billings, Chyn, and Haggag 2020) and inter-group behavior during adulthood (Couttenier et al. 2019; Fisman et al. 2020). Guided by this literature, we examine whether the early-childhood sociopolitical experiences of the judges explain variations in pretrial detentions. In particular, we test whether variations in judges' early-childhood exposure to social disorder explain their decisions on law and order. Motivated by Cappelen et al.

[1](Gazal-Ayal and Sulitzeanu-Kenan 2010; Shayo and Zussman 2011; Abrams, Bertrand, and Mullainathan 2012; Anwar, Bayer, and Hjalmarsson 2012; Depew, Eren, and N. Mocan 2017; Knepper 2018; Eren and N. H. Mocan 2020

2020, who show that early-childhood interventions between ages 3 and 4 affect later-life preferences for redistribution and views on fairness, we focus on examining the effects of exposure to religious violence during ages 0-6 years, controlling for exposure in later years.

Our study setting is the judicial system of India, which has one of the highest shares of pretrial detainees in the world: 70% of the total prisoners in India are under-trial prisoners, compared with 23% in the United States, 33% in France and 62% in Pakistan. Further, one-third of pretrial detainees are detained for more than one year. We focus on the early life experience of Hindu–Muslim riots in India, which has recently been examined in Fisman et al. 2020, to test for the impact of the exposure to violence on bank managers' lending decisions in India.[2] The Hindu-Muslim ethnic clashes occur throughout India and are recurrent events that continue to plague the country and potentially have unmeasured consequences in terms of social segregation, economic damages, and human capital depletion (Mitra and Ray 2014). Hindu-Muslim riots have reportedly claimed about 6,565 lives, injured 21,429 people, and resulted in 87,903 arrests in 1950–2000 with an average of 5 days of lockdown per riot.[3]

We analyze the bail decisions of 668 judges born in 1955–1991, who handled 323,380 bail cases in 2014–2018 in Uttar Pradesh (UP), which is the largest Indian state and has a population of 199.81 million (Census of India 2011). According to Prison Statistics of India[4], the percentage of pretrial detainees in UP increased from 70.6% in 2015 to 72.5% in 2019, in line with the national trend.[5] Owing to the high pendency rates of cases in courts, about 32% of pretrial detainees in UP remain incarcerated for more than a year, compared with the national average of 25%. This fact is striking, since "Bail is rule, jail is an exception" was established as a legal principle by the Supreme Court of India in a landmark judgment (State of Rajasthan v. Balchand alias Baliya) in 1978.

Our research setting and data have several unique features that allow us to identify causal effects on judicial decisions. First, our focus on Hindu–Muslim riots provides substantial within-region variations in early-childhood exposure to social disorder. Further, the recurrent nature of these riots allows us to test for the robustness of their impact across generations. Second, our analysis of exposure to conflict between ages 0 and 6 helps us to rule out self-selection into violence exposure. One concern could be about systematic relocation decisions by families due to the communal riots, for example, judges in non-exposed home districts could be affected by other families migrating to their districts in response to violence. This could lead to the violation of identifying Stable Unit Treatment Value Assumption (SUTVA) (Rubin 1980). We test for the possible violation of our identifying assumption using migration data. We find that the district-years affected by the communal riots do not have a differential rates of migration. Further, we use home-district fixed effects to account for family selection into location. Third,

[2]It would also be interesting to study political emergencies. However, these are usually aggregate shocks with not much within-country or within-state variations.

[3]Data is obtained from Varshney and S. Wilkinson 2006 and Mitra and Ray 2014

[4]These data are for 2014–2019 and are published by the National Crime Records Bureau.

[5]In India, the percentage of pretrial prisoners has increased from 67.6% in 2014 to 69.1% in 2019

we exploit an exogenous rotation policy for the judicial officers in UP, which generates plausibly exogenous spatial variations in the judicial postings of riot-exposed and not-exposed judges away from their home-districts during adulthood. This approach allows us to isolate the effects of riot exposure from the attributes of the district (where the judge is posted) and the home-district. It also rule out the self-selection of judges into less crime-prone districts. Fourth, since cases are exogenously assigned, it mitigates the possibility of judges selecting into particular types of cases. Fifth, bail is an ideal outcome to detect bias because it is purely discretionary and the judges are required to make decisions with limited information and almost no interaction with the defendants.

Our conflict dataset includes data from two sources. One is that of Mitra and Ray 2014 for the period 1950–2000.[6] The other is a novel dataset with data we collected on lockdowns during riots, which we sourced from the original historical newspaper articles that Varshney and S. Wilkinson 2006 used to prepare their dataset. We use a novel dataset on judiciary officers[7], which we retrieve from state administrative records containing information on judges' date of birth, home district, date of recruitment, judicial posting details, and academic qualifications. We combine the riot data with these administrative records to ascertain judges' exposure to conflict in early childhood. We obtain all the original bail judgment files from 2014–2018 from the judiciary website and extract case-level characteristics and bail decisions. Our extracted sample consists of 423,000 bail applications from the entire pool of cases that we downloaded (two million cases). We link these data with judiciary officers' data to arrive at judge-level panel data and the pretrial detention rate (which equals the total bail cases denied/the total bail cases assigned) as our primary outcome variable.

After controlling for unobserved heterogeneity (i.e., year of birth fixed effects and home district $\times$ quarter and district $\times$ quarter fixed effects), our main source of identification of the violence exposure (intent to treat) effect relates to variations in the bail decisions of judges across birth cohorts whose exposure to violence differs and who belong to the same home districts and of judges in the same birth cohort but who belong to different home districts.

We find that exposure to communal violence when aged 0–6 years causes an increase of 0.061 percentage points ($p < .01$) in the share of pretrial detentions, which is an increase of 16% compared to the mean. The effect is robust to the use of various estimation techniques, the inclusion and exclusion of controls, the use of placebo checks, and the removal of outliers. Further, we sort the judges by their influence on the regression coefficient and remove them one by one to test whether a few judges are driving our results, but do not find evidence in this regard. A key concern could be that it is difficult to assess whether exposure in early childhood to civil conflict directly affects any given judge's preferences or is associated with changes in the selection of who ultimately becomes a judge. To address this concern, we present the results of two exercises. First, using a representative sample of the working population from

[6]This dataset includes the dataset of Varshney and S. Wilkinson 2006 for the period 1950–1995, which has been used in several studies, such as in those by Fisman et al. 2020; Sarsons 2015.

[7]We use the terms judge and judiciary officer interchangeably.

an independent data source (the 66th round of the Employment and Unemployment Survey in 2011 by the National Sample Survey Organization (NSSO)), we find that the share of the early-childhood (0–6 years old) riot-exposed population in the entire working population in UP (in all sectors and all types of employment) is close to the share of the early-childhood riot-exposed judges in UP for details). We find no under- or over-representation of the early-childhood riot-exposed population in the sample of judges. Second, we test whether the number of judges or, the number of judges of a particular gender or religion are disproportionately, drawn from a riot-affected home district-year. We do not find any statistically significant difference between the proportions of early-childhood riot-exposed versus non-exposed judges by home district-year along gender or religion. Hence, we can rule out the possibility of selection into the judiciary due to childhood exposure to communal violence.

A part of the total effect on bail decisions could be driven by differences in the ability of the judges. We find that ability measured as the division[8] obtained in the Bachelor of Law (i.e., LLB) examination does not explain the increase in the pretrial detention rate. Guided by the active economics literature on endogenous preference formation, which shows early childhood as a formative period of social and political preferences,[9] we explore the behavioral explanations of the early-childhood exposure effect.

We find that high-intensity state interventions, such as a high duration of lockdowns and a high number of arrests, which are associated with limiting the riot casualties, explain the increase in the observed pretrial detention rates. This finding suggests that the early-childhood exposure to state-imposed lockdown measures which proved effective in containing violence possibly generated higher support for the institutions of the state in law-and-order matters. Further, we do not find any evidence of religious bias in the observed stringency in the bail decisions of early-childhood riot-exposed judges, which rules out the possibility of the inter-group hostility mechanism underlying the effect. In line with Cappelen et al. 2020, who found that early interventions between ages 3 and 4 years have lasting effects on social preferences, we find that exposure to violence between the ages of 3 and 6 years is the key driver of the observed judicial biases toward pretrial detentions.

This paper makes contributions to several strands of literature. Our first contribution is to provide the first causal evidence, to the best of our knowledge, on the early origins of judicial bias. We expand the rich literature on judicial bias by providing evidence on the long-term determinants of judicial decisions. Our focus on linking interventions during the formative years of the judiciary officers with stringency in their decisions relates particularly to the emerging evidence on the impact of early-childhood interventions on long-term social preferences, such as that found by Gould, Lavy, and Paserman 2011; Giuliano and Spilimbergo

[8]We classify the grades obtained in the LLB degree course into three divisions: I (Grades: $>= 60\%$), II (Grades: $>= 45\%$ and $< 60\%$), and III (Grades: $>= 33\%$ and $< 45\%$).

[9](Kohlberg 1984; Piaget 1997; Harbaugh, Krause, and Vesterlund 2002; Sutter and Kocher 2007; Fehr, Bernhard, and Rockenbach 2008; Almås et al. 2010; Bauer, Chytilová, and Pertold-Gebicka 2014; Angerer et al. 2015; Ben-Ner et al. 2017; Cappelen et al. 2020)

2014; Cappelen et al. 2020; Billings, Chyn, and Haggag 2020, more broadly, and on the impact of early-childhood exposure to violence on intergroup behavior, in particular (Couttenier et al. 2019; Fisman et al. 2020). Our study also adds to the robust empirical evidence on how early-childhood interventions influence various long-term outcomes, such as cognitive skills (J. J. Heckman 2006; Bleakley 2007; Almond, Edlund, and Palme 2009; Maccini and D. Yang 2009; Aizer and Cunha 2012; Bharadwaj, Løken, and Neilson 2013), health outcomes (Currie 2009; Maccini and D. Yang 2009; Almond and Currie 2011; Currie and Vogl 2013; Adhvaryu, Fenske, and Nyshadham 2019), and labor market outcomes (Almond 2006; Bleakley 2010; Gould, Lavy, and Paserman 2011).

Second, our analysis reveals that human capital achievements, as measured by the division achieved in the LLB examination, do not explain the observed pretrial release rates of early-childhood riot-exposed officers. Heterogeneity analyses indicate that these observed biases are possibly driven by behavioral effects. Our results add to the literature that demonstrates the importance of early investments during formative years in generating noncognitive outcomes (J. J. Heckman 2007; Cunha, J. J. Heckman, and Schennach 2010; J. Heckman, Pinto, and Savelyev 2013), such as motivation, dependability (J. J. Heckman 2006), and distributive preferences (Cappelen et al. 2020), which have economic consequences independent of cognitive achievements (J. J. Heckman and Rubinstein 2001; J. J. Heckman, Stixrud, and Urzua 2006).

Studies on judicial bias are now common in the literature on the economics of crime, which mostly focuses on the Organisation for Economic Co-operation and Development (OECD) countries—a large body of literature focuses on the criminal justice system in the United States (W. Dobbie and C. Yang 2021; W. Dobbie, Goldin, and C. S. Yang 2018; Kleinberg et al. 2018; Kling 2006; Stevenson 2018; Agan and Starr 2018; Arnold, W. S. Dobbie, and Hull 2020). Thus, our third key contribution is expanding this empirical examination using data from a country with a weak institutional context. Given the large share of pretrial detainees in Indian prisons, the examination of judicial bias in India is important in understanding the potential welfare consequences of institutional imperfections. Our study highlights the presence of bias in bail decisions in India, which adds to a recent study on India that found no in-group (by gender or religion) bias in judicial sentencing (Ash et al. 2021).

An understanding of the causal processes that shape social preferences is of interest to academics and policymakers alike. Our study reveals the importance of sociopolitical institutions early in life in shaping long-term outcomes. More crucially, we show that the impact of early-childhood exposure to institutions is robust across generations, that is, regardless of whether the judiciary officer was born in 1955 or 1980.

The remainder of the paper is organized as follows. Section 2 presents the context of the study and the data used. Section 3 explains the empirical strategy, and Section 4 presents the results of the balance test. Section 5 presents the results of the core analysis and a series of robustness tests. Section 6 explores potential mechanisms underlying riot effects. Section 7 concludes.

2. Context and Data

Our study setting includes the judiciary in UP, the largest Indian state, which has a population of about 199.81 million (Census of India, 2011)[10] and a demographic composition that is similar to that of India as a whole.[11] Next, we explain the unique features of the data and the study setting that allow us to estimate the causal effects of violence on bail decisions.

2.1. **Indian Judiciary System.** The Indian judiciary can be divided vertically into three levels. The apex court, the Supreme Court of India, is based in New Delhi. Its jurisdiction encompasses the entire country. Next in the hierarchy of courts are the High Courts. They are the highest court at the state level, and their jurisdiction is limited to the state boundaries. The third level is the district-level courts, at the district level, and their jurisdiction is restricted to the district.[12] Within district-level courts, the District and Session Court has the appellate jurisdiction over all the other courts, such as civil, criminal and family courts.

The district-level courts in UP have an average of 27 judges per district (i.e., 2,048 unique judges in 75 districts in August 2018). Districts are the smallest administrative division in India to which the authority of law and order are delegated.[13] The total number of judges per million population is 9.1, and the average age of judges is 43.84 years. UP has 22.6% female judges, and 6.9% of its judges are Muslims.

2.2. **Rotation Policy for Judicial Officers in UP.** The UP judiciary follows an explicit geographical rotation policy with the stated objectives of reducing corruption and collusion in the judiciary, which induces exogenous spatial variations in the distribution of officers across districts. Generally, the tenure of judicial officers is 3 years of service in the district.[14] District Judges are posted away from their hometown by the rotation policy, whereas the following norms guide the transfers of other judicial officers:

(1) Officers will not be posted to their hometown.
(2) They will not be posted within 6 years to a district in which they were earlier posted.
(3) They will not be posted within 3 years to any district falling in the zone[15] in which they were earlier posted.
(4) They will not be posted to any adjoining district of another zone.
(5) The constraints on re-posting of officers in the zone will not apply if they had been posted for a short period of less than 6 months.

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[10] see censusindia.gov.in

[11] Hindus and Muslims form 79.73% and 19.26% of UP's population, as against the national average of 79.8% and 14.2%, respectively.

[12] Some newly created districts do not have courts and rely on the services of courts in adjacent districts.

[13] District officials include an Indian Administrative Officer, tasked with administration and revenue collection, a Superintendent of Police, tasked with maintaining law and order, and a Deputy Conservator of Forests, tasked with maintaining environmental management. As per the Census of India, 2011, the country had 640 districts.

[14] The guidelines for transferring officer appear in circulars issued by the Registrar General of the High Court of Judicature at Allahabad. The tenure is of 2 years at an outlying court (courts far from district headquarters) or at Sonbhadra district.

[15] Zones are collection of districts. The entire state is split into 7 zones of contiguous districts.

The Assistant Registrar also collects information on the list of stations in UP where judiciary officers have close relatives and a statement of places where they were educated, as required under C.L. No. 25/Admin (A)/DR(S)/78 dated March 16, 1978.

We observe that officers are located away from the hometown. An average officer reallocation assigns a judge to a new district that is 325 km (std. dev. 165 km) away from the district of the previous assignment. The main advantage of the rotation policy is that it induces matching between judges and defendants that is plausibly uncorrelated with bail cases. In the next section, we explain the detailed data on judiciary officers, which we use to test the plausibility of the exogeneity of the judiciary rotation policies of the state.

2.3. **Data on Judiciary Officers.** We extract information on working and retired judges[16] from the Allahabad High Court website.[17] The collected data include details on judges' date of birth; their home-district; the dates on which they were promoted; their educational qualifications, dating back to the first school-leaving examinations; and the dates and locations of their postings and transfers. We use the data on home districts and the date of birth of the judges and match it with the data on riots to compute their exposure to riots at every age. Since some district boundaries in our sample have undergone changes over 50 years,[18] we first harmonize the districts in the two datasets by assigning every district to their parent (origin) district. We use the official census district (2011) records to trace the origin of every district in our data. Appendix Table B.I details the district formation and Appendix C.1 provides complete information on district harmonization.

2.4. **Bail Jurisprudence.** The fundamental right enumerated in Article 21 of the Constitution of India is that "No person shall be deprived of his life or personal liberty except according to procedure established by law." This right forms the basis of bail provision in India. Although bail is not defined legally in Indian codebooks,[19] it implies the release of a person detained by the police for a certain offence, by furnishing a guarantee of future attendance in the court for trial. The three categories of bail in India are as follows:

i) *Bail in bailable offences*: In Section 436 of Cr.P.C, bail is the right of a person who has been accused of committing an offence that is bailable in nature. This provision casts a mandatory duty for police officials as well the court to release the accused on bail if their alleged offence

[16]A few judges who judged cases during 2014–2018 retired during this period.

[17]`http://www.allahabadhighcourt.in/District/Officer/judge_id.html`, where the judges' ID is their unique identification. Since the information on retired judges has been removed from the Allahabad High Court website, we extract data from the archived web page.

[18]The total number of districts in UP is currently 75 and was 48 in 1950. Further, in 2000, a new state, Uttarakhand, was carved out of UP.

[19]The Criminal Procedure Code (Cr.P.C.) details the bail process but does not define bail. All the offences are categorized under bailable and nonbailable offences.

is bailable in nature.[20]

ii) *Bail in nonbailable offences*: When a person is charged with having committed a nonbailable offence(s), the court has to consider many factors: [21]

a) whether there is any prima facie or reasonable ground to believe that the accused had committed the offence; b) the nature and the gravity of the accusation; c) the severity of the punishment in the event of conviction; d) the danger of the accused absconding or fleeing; e) the character, behavior, means, position and standing of the accused; f) the likelihood of the offence being repeated; g) a reasonable apprehension of the witnesses being influenced; h) the danger of justice being thwarted by granting bail.

The subjective nature of the factors a judge must consider during bail decisions is evident.[22] When a court gives bail, the accused must sign a personal bond and usually two surety bonds (from relatives or others who can vouch for the defendant) for a certain amount. If the accused breaks the bail condition, the court is liable to recover the amount from the defendant. A granted bond can also be cancelled later if it is found that bail conditions are not complied with.

2.5. **Data on Defendants and Cases Registered.** We web-scraped all the case-level pdfs from the district e-court website by court establishment[23] in August 2018. We segregated about 423K bail cases [24] from the entire pool of two million downloaded cases. We performed optical character recognition, translated the documents to English and then extracted all the relevant variables at case level using text analysis. The primary details extracted are the bail decision (whether granted/denied), the name of the defendants (which is used to identify their religion, following Bhalotra et al. 2014), and the criminal section codes under which a case is registered. The criminal section codes pertain to either the Indian Penal Code (IPC: the comprehensive list of offences and associated punishments) or special laws (the Acts to augment the IPC). We created 11 crime categories from these criminal section codes, mostly following the chapters of the IPC codebook.[25] Appendix C.2 provides detailed information on the procedure we adopted. Since the lengthy process of text extraction could entail errors, we manually digitized all the variables for 60,000 cases—30,000 bail cases handled by Muslim judges and an equal number of

[20]In 2005, the Cr.P.C. was amended by adding section 436-A: A person who has undergone detention for a period that is half the maximum period of imprisonment imposed for a particular offence shall be released on his/her personal bond with or without sureties.

[21]State of U.P. through CBI v. Amarmani Tripathi, 2005 (8) SCC 21; Prahlad Singh Bhati v. NCT, Delhi Anr. 2001 (4) SCC 280; Ram Govind Upadhyay v. Sudarshan Singh Ors., 2002 (3) SCC 598.

[22]We do not examine Anticipatory Bail. Under Section 438 of the Cr.P.C., the High Court or Court of Sessions can issue bail before a person is arrested, which is known as Anticipatory Bail, if there is an apprehension or a reason to believe that a person may be arrested on an accusation of having committed a nonbailable offence. The court considers the same list of factors as mentioned in point (ii).

[23]Website: https://districts.ecourts.gov.in/up; the District Courts for Chandauli, Etawah, Hardoi, Kheri, Pratapgarh, and Sant Kabir Nagar districts have not uploaded judgments. In the district of Varanasi, very few bail cases have been uploaded.

[24]The bail cases are identified from the bail application marker provided with the case number.

[25]Arms and Explosives, Body Crime, Cow Slaughter, Electricity Theft, Gangster and Dacoity, Property Crime, Forgery, Criminal Intimidation, Public Tranquility, Public Health, and Other

randomly chosen cases handled by Hindu judges—and show the error rates for each variable extracted (see Appendix C.4 for the selection of the cases and associated error rates).

2.6. **Hindu-Muslim Communal Riots: 1950-2000.** The data on Hindu–Muslim riots are from two sources: the datasets of Varshney and S. Wilkinson 2006 and Mitra and Ray 2014. The combined dataset provides detailed information on the Hindu–Muslim riots in India as reported by a national English daily, *The Times of India*. We use the information on the district, month, and year to identify a unique riot.[26] For each recorded clash, the dataset also has information on the riot duration, the number of people killed or injured, and the number of people arrested. Further, we add the duration of lockdown for each riot from the source articles of Varshney and S. Wilkinson 2006.

In line with the literature (Couttenier et al. 2019), in our main specification, we use the extensive margin of exposure to conflict, which is a dummy variable of exposure to communal violence between the ages of 0 and 6 years that takes the value 1 if the home district of the judiciary officer experienced Hindu–Muslim communal clashes when the officer was 0–6 years old. Appendix Table B.II shows that 31.4% of the judges have been exposed to violence when aged 0–6 years.[27]

Since the judges in our sample have UP as their home state, our treatment variations in conflict exposure derive from the variations in communal clashes in this state. In all, 33 of the total of 48 home districts have experienced at least one riot during 1950–2000.[28] Within the districts experiencing at least one riot, the mean of the number of riots per year across districts is 0.12 with standard deviation 0.38. During 1950–2000, the average number of Hindu–Muslim riots per year per state was 7.6 in India as a whole and 8.2 in UP. The average number of deaths and injured per year per state are 50 and 140, respectively, for the entire country and 65 and 116 for UP. In terms of the intensity of violence in each riot, 6.7 people were killed, on average, in a riot in UP, which is similar to the average (6.3) for the whole of India. In terms of state response variables in UP, the average duration of lockdowns following a riot was 5 days and the average number of arrests was 144.

We set the following restrictions to arrive at our sample of judges. Since bail outcomes are our main outcome variable, we retain judges who are assigned to bail cases ($N = 1,268$), of which the names of the home districts and the home states of 35 judges and 67 judges, respectively, were not available in the administrative data. Following Arnold, W. S. Dobbie, and Hull 2020, we drop judges who were assigned too few cases (the bottom 5 percentile in terms of the number of cases (<97 cases) assigned to the judges, which amounts to 493 judges).[29] We also

[26]For some entries, district information is missing, but city/village names are provided. We use this to assign districts to a riot.

[27]The proportion of exposed bank managers in the study by Fisman et al. 2020, based on at least one death in the riot, is 14.4%

[28]Since the treatment in our case starts from 1950, we use the districts which were present in 1950 by merging the districts as detailed in C.1. Currently, there are 70 districts in Uttar Pradesh.

[29]Our results are robust to changing the threshold from the bottom 1 to the bottom 10 percentile; we have provided the results of the robustness checks in Section 7. The choice of the bottom 5 percentile as the threshold,, that is,

drop 10 judges because we did not have information on their LLB examination results. The LLB degree is the minimum requirement for a judiciary officer, and 29% of the judges had passed this course with first division and 71% with second division.The final sample consists of 668 bail judges handling 323,380 cases aggregated at the judge-district-quarter level, which yielded a sample size of 5,530. The descriptive statistics presented in Appendix Table B.III show that the analysis sample is similar to the total sample of judges, along observables in the data. Figure A.I reveals that the majority of the district judge transfers occurred mostly in the second quarter. Figure A.II reveals that the pattern is similar for the sample of bail judges. In Appendix Table B.II, we show that judges exposed to communal violence between the ages of 0 and 6 years are similar to judges without such exposure along the district covariates and the judge peers assigned to them. In terms of judges' characteristics, the judges exposed to communal clashes in early childhood are more likely to be females, older, and more experienced.

3. Empirical Strategy

3.1. **Specification.** We begin with our case-level data, where the unique identifier is a bail case. Each bail case is uniquely matched to a judge; that is, only one judge handles each bail case. We observe the bail decisions of a judge corresponding to each bail case. For each judge, we aggregate the pretrial detention rate at the district-quarter level. We conduct our analysis of pretrial decisions using the judge–district–quarter level data.[30] Our results are robust to case-level regression. Section 7, the robustness checks section, explains why we have judge-level regressions over case-level regressions as our preferred specification. Guided by our data depicted in Figure A.III in the Appendix, which shows that the proportion of bail cases trends in quarterly periods in the data, we aggregate bail decisions at the quarterly level. Our results are robust to aggregation at the monthly level.

Our key source of identification emerges from the variations in the bail decisions of judiciary officers, belonging to the same home district, who are assigned to the same district but whose exposure to communal riots differs. Given that judges from the same home district with the same exposure to riots may have different birth years, we control for the direct year of birth fixed effects.

Our key econometric specification is as follows:

$$B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{F}.\mathcal{E}_b + \beta \times kid[0-6]_j + \sigma X_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + \epsilon_{j,d,t} \quad (1)$$

judges handling a minimum of 97 cases in 4 years, is to maintain a balance between not dropping too many judges and not keeping too many judges who have handled very few cases.

[30] We address concerns about the clustering of bail decisions at the judge level by first aggregating case-level outcomes at the judge–district–quarter level (Bertrand, Duflo, and Mullainathan 2004). Noting that our treatment variation is at the judge level (Abadie et al. 2017) and that there may be correlations across outcomes for a judge across quarters and districts (Bertrand, Duflo, and Mullainathan 2004), we cluster our standard errors at the judge level.

where $B_{j,d,t}$ is the share of bail denied by judge "j" assigned to district "d" at quarter "t". The covariate in the regression, $kid[0-6]_j$, is the binary variable of exposure to communal riots when aged 0–6 years. The variable $expo(k)_j$ is the exposure to violence at the k th year of a judge j. α is an intercept. Spatial differences in the detection and registration of crimes in districts assigned to judges are accounted for through district-quarter fixed effects $\eta_{d,t}$. Some districts could have more cases registered in certain quarters because the police were more active and successful in those districts at those times. Districts could also vary by types of crime committed owing to seasonal weather shocks (Blakeslee and Fishman 2018). Further, to take into account unobserved differences in bail decisions that are driven by differences in the home districts of the judges in a particular quarter, we use $\delta_{h,t}$, which are the home-district-quarter fixed effects. The other reason is that home-district-quarter fixed effects filter out the current trends affecting judges' preferences from those affecting it through early-childhood exposure. $\mathcal{F}.\mathcal{E}_b$ are birth-cohort fixed effects, which control for unobserved differences by birth year. X_j is a vector of judge-level characteristics, such as religion, gender, division obtained in the LLB examinations, and on-the-job experience.

In our setting, the policy-induced exogenous rotation of judicial officers addresses endogeneity concerns related to judges selecting into districts and hence types of cases, as well as generates substantial heterogeneity across birth cohorts in their early-childhood exposure in all home districts such that the $kid[0-6]_j$ and $expo(k)_j$ dummies are not collinear with the home-district fixed effects, which allows us to separate unobserved confounders that vary at the home-district level. In addition, our focus on the exposure of judges to violence when aged 0–6 years alleviates the endogeneity concern of self-selection into conflict, whereas home-district fixed effects account for household selection into riot-exposed districts. Standard errors are clustered at the judge level to account for within-judge correlations in bail decisions over time and across the assigned districts.

In equation 1, our coefficient of interest β denotes the difference in bail decisions between judges exposed and not exposed to communal riots in early childhood (0–6 years old). We augment the equation by including a dummy variable that takes the value 1 if there was a riot 1–5 years before a judge was born, to control for the direct effects of pre-birth exposure to conflict.

The riot information is available until 2000, and in our sample, the youngest judge is born on October 7, 1991. Hence, we could calculate exposure to violence up to the first 9 years after birth for the full sample of judges in UP.[31] However, as a robustness check, we test for early-life exposure to violence by controlling for exposure to violence in later life for the subsample for which we can control for exposure to violence in later years.

[31]Controlling for the later years of riot experience is possible only at the cost of sample size reduction. For example, if we add exposure to violence up to the first 10 years, we will have to drop judges born in 1990 because we do not have information on riots in 2001.

4. Balance Test

In this section, we test whether the exogenous rotation policy of judiciary officers resulted in selection along observables. We estimate the following main specification equation, that is, equation-1 with covariates at the level of districts assigned to judges as the outcome variables.

$$Y_{j,d,t} = \alpha + \delta_{h,t} + \mathcal{F}.\mathcal{E}_b + \beta \times kid[0-6]_j + \sigma X_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + \epsilon_{j,d,t} \tag{2}$$

where $Y_{j,d,t}$ are the characteristics of the districts assigned to judge j at quarter t. The set of covariates $kid[0-6]_j$, $exposure(k)_j$, α, and $\delta_{h,t}$ are the same as those in the core econometric specification in equation-1.

Panel A of Table 1 reveals that the districts assigned to the judges exposed to communal violence during the ages of 0 to 6 years are not statistically different along caste, the proportion of Muslim population, and literacy rates, from the districts assigned to judges not exposed to such violence during early childhood. Although the districts assigned to the riot-exposed judges have more males and a higher proportion of the working male population, the difference is very small in magnitude. Columns 1–6 in Panel B present the results of a leave-one-out regression we run to test whether the peers assigned to judges differ by early-childhood exposure to riots. We find that there is no statistically significant difference between peer groups assigned to judges by early-childhood exposure along dimensions such as religion, age, and on-the-job experience. However, the group of early-childhood exposure judges do have 10% fewer female peer judges than the group of judges with no such exposure to violence. Female judges are correlated with high pretrial detention rates. Column 7 of Panel B shows that there is no statistical difference in the total number of cases assigned to the early-childhood riot-exposed judges and not-exposed judges. One likely concern is that riot-exposed judges select into cases involving specific types of crimes. To address this concern, first, we define crime categories using the IPC, which is the official criminal code of India, and the special Acts passed by the central and state governments (see Appendix-C.2.iii for details). We show in Appendix Table B.IV that case assignment (based on the 11 types of crime categories explained in the data section) is not correlated to the exposure variable. Considering the potential concern about measurement error due to errors in the data extraction of crime categories, we test for selection in a manually digitized random sample of judges (Appendix Table-B.V) and find no evidence of the selection of judges into crime types.

Next, we apply the method used by Couttenier et al. 2019 to demonstrate the exogeneity in the allocation of judges determined by the rotation policy. The notion is to test whether the judicial postings across different district-quarters is non-random. More specifically, we test whether the average characteristics of the judges from the same home district are similar to those of the judges (from the same home district) posted in different district-quarters.[32] We test

[32] Suppose X judges are from home district A and the average age of these judges is 46 years. Out of these X judges, say x1 judges are posted in district B and (X-x1) judges are posted in district C at a given time. If the judges are posted randomly, then the average age of x1 and (X-x1) judges would also be 46 years.

for the difference in means along the judge-level treatment and non-treatment covariates across district-quarters. Formally, we estimate the following equation separately for judges from each home district for every quarter:

$$J_{h,b,q,d} = \sum_{d=1}^{75} \beta_{d,q} \times \mathcal{I}_{h,b,q,d} + \epsilon_b \tag{3}$$

where $J_{h,b,q,d}$ are the judge-level characteristics of judges from home district "h", birth cohort "b", at quarter "q" in district "d". $\beta_{d,q}$ are the district-quarter specific coefficients corresponding to the indicator dummy for judges, denoted as $\mathcal{I}_{h,b,q,d}$, that takes the value 1 if the judge from the home district "h", birth cohort "b", is allocated to district "d" at quarter "q". The dependent variables are judge-level characteristics, such as exposure to communal conflict when aged 0–6 years, gender, religion, age, time to promotion, and age when joining the judiciary. For each home district, we examine the number of district-quarters for which the F-test of the null hypothesis $\hat{\beta_{d,q}} = \hat{\mu_h}$ is rejected where $\hat{\mu_h}$ are the average characteristics of the judges at the home-district level. If the allocation is exogenous, then the district-quarter specific coefficient $\hat{\beta_{d,q}}$ should not differ from the home-district average, and the F-test should not be rejected for this district-quarter. If there is no selection in the spatial allocation of the judges, then the observable judge characteristics in some districts with respect to the home-district average should not be over- or under-represented. Each row in Table B.VI represents the share of home districts for which the F-test is rejected at the 10% cutoff in at most 0, 1, 5, and 10 districts. For instance, it shows that for 95% of the home districts, we do not have any district-quarter specific coefficients that differ from the home-district average of $Kid[0-6]$ and for 100% of the home districts, less than five district-quarter coefficients differ from the home-district average. We observe similar results for the home-district averages related to the average of female judges, Muslim judges, and judges with first division in their LLB examination. Regarding the home-district average age of judges, time to promotion, and joining age, for almost all home districts, less than 10 districts have district-quarter specific coefficients that differ from the home-district averages. Therefore, we do not find any evidence of selection along observables in the spatial allocation of the judges.

5. Main Impact of Exposure to Communal Violence

We present the results of our key econometric specification as represented by equation-1 in Panel A of Table 2. We report our coefficient of interest β controlling for exposure to violence in later years. The coefficient of interest demonstrates the causal effect of exposure to communal violence when 0–6 years old on the shares of bail denied (that is, pretrial detention rates), where the control group consists of judiciary officers with either no experience of violence or who have not been exposed to violence when 0–6 years old. Column (1) of Panel A provides the treatment effect estimates using a variant of regression equation-1, which is a specification without controls for judge-level characteristics. The treatment effects of exposure to riot are positive and statistically significant at the 1% level of significance. The shares of bail denied by judiciary officers exposed to violence in early years are 0.063 percentage point higher, which is an increase of 17% (= 0.063/.37) compared with the baseline mean, than the shares of bail denied by judiciary officers without such exposure. In Column (2), we add controls for judge-level characteristics, such as experience, gender, religion, and LLB examination grades. The treatment effect estimates show a 0.061 percentage point increase in pretrial detention rates, which is an increase of 16.4% (= 0.061/.37) compared with the baseline mean, which is statistically significant at the 1% level of significance. In Column (3), we add controls for the occurrence of communal riots 5 years before birth. Our coefficient of interest remains almost unaffected, with 17% (= 0.062/.37) increase in detention rates. The effects are statistically significant at the 1% level of significance.

In Column (4), we add birth-year-quarter fixed effects (and exclude birth-year fixed effects) to flexibly account for the unobserved current time trends by judges' birth cohort. The coefficient remains stable at 0.06 and is statistically significant at the conventional level. Even though we have shown that riot-exposed judges do not select into types of crime, we perform one more check to alleviate the concern. We change the specification in Column (5) where we aggregate the data at the judge-crime type-district-quarter level (which increases the number of observations) and include crime-type fixed effects explicitly. The size of the coefficient estimate relative to the mean is 16.87% (=.054/0.32) which is very similar to the estimates from Column(2).

Recent literature on the causal effects of exposure to violence during early-childhood in the context of asylum seekers in Switzerland (Couttenier et al. 2019) and bank managers in India (Fisman et al. 2020) have used ages 0–12 and 0–10 years old (for early-childhood), respectively. In light of this evidence, we estimate our main regression equation 1 and add controls for exposure to violence in the years after age 6 in Panel B. In Column (1) of Panel B, we control for exposure until age 14 years, in Column (2) of Panel B we control for exposure until age 18 years, and last, in Column (3), we control for exposure until age 22 years. Adding controls for exposure reduces our sample from Column (1) to Column (3) in Panel B, but our results remain positive with a similar magnitude and are statistically significant at the 5% level of significance.

In our main table, we also report various estimates of the standard errors of the treatment effect of early-childhood exposure to communal violence. The Moulton-corrected standard errors and the wild bootstrap standard errors are both stable and demonstrate that the coefficients of interest across specifications are significant at the 5% level of significance.

6. Interpretation: Selection or Exposure Effect

A key concern about the interpretation of the impact coefficient is that the coefficient could also include sorting into the judiciary. We adopt two empirical strategies to show that impact of early-childhood exposure to violence is not driven by the selection of exposed individuals into the judiciary.

In the presence of the selection effect, we would observe either under- or over-representation in the judiciary of judges exposed to riots in early-childhood. In the first method, we compare the share of the early-childhood riot-exposed population in the entire working population (in all sectors and all types of employment) in UP with the share of early riot-exposed judges in this state. To this end, we exploit the data from the NSSO Employment and Unemployment Survey of the National Sample Survey Organization (66th round, 2011). We restrict our analysis to the riot-affected UP districts. This survey captures information about individuals' age (but not their birth date) and the district in which they were residing at the time of the survey (i.e., their current district, but not their birth district), which we use to ascertain their riot exposure. One constraint is that no all-India survey captures information on survey respondents' birthplace (or even birth district). However, the migration literature has shown a low migration rate (5–6%) for India. Further, 99% of the migration is within a district. Hence, for this exercise, we assume that the current district is the birth district. Next, we select the sample born after 1950 (since our riot data start from 1950) who are employed (all types of employment). In Appendix Table-B.VII, we find that the percentage of the total working population exposed to riots when aged 0–6 years is 39.2%, whereas the percentage of riot-exposed judges in the total population of judges in UP is 38.64%, in the sample of bail judges is 38.88%, and in our analysis sample is 38.8%. It is reassuring to note that there is no over- or under-representation of riot-exposed judges in the judiciary compared with the representation of the riot-exposed population in the total working population.

In the second approach, we ask whether different numbers and types of judges, where type is defined by gender and religion, are drawn from different riot-affected districts. In particular, we test whether the home-districts that experience a riot in a given year are more likely to have different total number and types of judges using the following specification at the home-district-riot-year level.

$$Judges_{h,y} = \alpha + \eta_h + \delta_y + \beta Exposed_{h,y} + \epsilon_{h,y} \tag{4}$$

where h and y denote home-district and riot-year. η_h is the home-district fixed effect, and δ_y is the riot-year fixed effect. The outcome variables $Judges_{hy}$ are the total number of judges, the proportion of females, and the proportion of Muslim judges. Since our focus is on the first 6 years of exposure, the outcome variable includes judges born 6 years before any given home-district riot-year. For instance, if the district Agra had a riot in 1970, the total number of judges affected by this riot (in their early-childhood, 0-6 years) would be the judges born in Agra between 1965-1970.

Appendix Table B.VIII shows that there is no selection of the type or total number of judges by riot-affected home-districts in any given year.

7. Robustness

7.1. **Few Judges per District Concern.** Our analytical sample has 668 judges over several districts, possibly resulting in a small number of judges per district, thereby raising the concern that the impact is attributable to riot exposure among a few judges. To address this concern, we sort the judges by their influence on the regression coefficient, where Cook's distance measures the influence. Figure 2 plots the coefficients from the estimates of our main specification (Column (2) of Panel A of Table 2), by excluding one judge at a time—starting with the judge having the highest influence on the regression coefficient—and ultimately excluding 300 judges. The coefficient is stable and statistically significant at the 5% level of significance until the exclusion of the first 280 judges (out of 668 judges), alleviating the concern that a few judges may be driving our result.

7.2. **Case-level regressions.** Our outcome variable is aggregated at the judge–district–quarter level. It may be argued that using case-level outcome data could account for the differences in workload by judges within a court-quarter (i.e., across courtrooms in the same District Court in a given quarter), which are not addressed by the district-quarter fixed effect, especially for larger districts with multiple police stations and multiple courtrooms adjudicating criminal trials. In Appendix Table B.IX, we test the regression at the case level instead of aggregating at the judge–district–quarter level. Column (1) has the same controls and fixed effects as in our baseline results (i.e., Column (2) of Table 2). In Column (2), we add the crime-type fixed effect, and in Column (3), we further refine our specification by adding two more controls—a dummy for whether the defendant is a Muslim and a dummy for the nonbailable nature of the case. The coefficients range from 0.038 to 0.043, which is 11 to 12% over the mean, and are close to our main result.

Although case-level data account for the different workloads per judge, there are concerns about correct inference owing to the clustering of outcomes. Following the design-based uncertainty approach of Abadie et al. 2017, since the random variation of treatment is at judge level, the data should be clustered at judge level. However, if each judge has a different number of cases, case-level data lead to misleading inferences because of the varying cluster sizes (MacKinnon and Webb 2017). Assuming a sampling-based approach to clustering, in line with (Cameron and Miller 2015), then the level at which the data should be clustered because of correlation is ambiguous. It can be suggested that with case-level outcomes, since the same defendant(s) can be represented across cases assigned to judges, the correct inference would require accounting for serial correlation across cases with the same defendant in addition to clustering at the judge level. In a similar quasi-random judge assignment study, W. Dobbie, Goldin, and C. S. Yang 2018 account for two-way clustering by including the defendant- and the judge-level clusters. This approach is not feasible for our data because we do not have a unique defendant ID.

The benefit of adding case level controls are very limited due to data limitations. However, there are inference issues as mentioned above arising from clustered data in case-level regressions. Hence, our preferred specification aggregates the outcome data at the judge level using judge-level clustering for inference.

7.3. **Sample Selection.** We follow Arnold, W. S. Dobbie, and Hull 2020 and exclude the bottom 5 percentile judges (i.e., judges handling less than 97 cases) from our primary analysis sample, to allay concerns related to judges dealing with very few cases driving our outcomes. In

Appendix Table B.X, we present the results using alternative thresholds for the exclusion of judges from analysis samples. From Column (1) to Column (10), we change the threshold of exclusion from the bottom 1 to 10 percentile. The coefficients are very stable and close to our main result in all the specifications.

7.4. **High-rank Judges.** Another concern is that high-ranked judges may influence the cases assigned to them. In Appendix Table B.XI, we exclude District and Session Judges and Chief Judicial Magistrates—the two most influential judges in the district-level judiciary—and find that our coefficient magnitudes range from 0.054 to 0.084 percentage points and are statistically significant at the 95% confidence interval.

7.5. **Outlier Tests.** The next set of robustness tests is to check for potential outliers in our baseline results. In Appendix Table B.XII, we show that judges from home districts exposed to a high number of riots do not drive our results. Column (1) in Table B.XII presents the results after the exclusion of judges from the home districts that have experienced the highest number of Hindu–Muslim riots, Column (2) presents the results after the exclusion of judges from the home districts with the second-highest number of riots, and so on. We observe that the effect of early-childhood exposure to riot is positive and statistically significant at the conventional levels, with its magnitude ranging from an increase of 0.057 to 0.075 percentage point in pretrial detention rates. In Appendix Table B.XIII, we remove the home districts with the highest number of riots cumulatively. Here, again we find that the treatment effect of exposure to riots when in the age group of 0–6 years is positive, and its magnitude ranges from 0.068 percentage point to 0.088 percentage point, significant at the 1% level of significance. Last, we test our baseline results by removing observations that are 3, 2, and 1 standard deviation away from the residual mean in Column (1), Column (2), and Column (3) in Appendix Table B.XIV, respectively. In addition, we remove observations with high leverage, which shift estimates to at least one standard error and to at least $4/N$. The results are positive, with magnitudes ranging from 0.053 percentage point to 0.064 percentage point, and are significant at the conventional level of significance across all specifications.

7.6. **Placebo Test.** In our placebo check, we follow a Monte Carlo approach and randomly reassign our treatment variable $kid[0-6]$ following a binomial distribution, based on the observed distributions of $kid[0-6]$, keeping all other characteristics unchanged. We estimate our main specification (Column (2) of Panel A of Table 2) on the simulation data. We implement 1,000 simulations. The sampling distribution of the treatment effects of $kid[0-6]$ Monte Carlo draws is centered around zero. Figure A.IV demonstrates that the probability of the treatment effect found in our main specification being spurious is negligible.

8. Threats to Identification: Migration

The migration of households from riot-hit districts to districts less likely to experience Hindu-Muslim riots would violate SUTVA (Rubin 1980), which is our identifying assumption. Therefore, we test whether the migration rates are affected by the communal riots. We use the NSSOs' microdata from the Employment and Unemployment Survey 1983, which captures migration information.[33] The important migration-related information we exploit are the age at which migration occurs, the district from where migration takes place (i.e., the origin district), and whether migration occurs within the district or to another district. The data allow us to perform analysis only for the migrating population. Since the violation of SUTVA in our setting occurs in case of migration from a riot-hit district to another district, and not within a riot-hit district, we show that the share of out-migration from the district in the total migration is not correlated with the riots.

We build the data at the district-year level and run the following regression.

$$MigrationRate_{h,y} = \alpha + \eta_h + \delta_y + \beta Exposed_{h,y} + \epsilon_{h,y} \tag{5}$$

where h and y denote the origin of migration district and the year of migration. η_h is the district fixed effect, and δ_y is the year fixed effect. The outcome variable $MigrationRate_{hy}$ is the ratio of migration across districts to the total migration. The coefficient to focus upon is β associated with the explanatory variable $Exposed_{h,y}$ that captures whether the district-year cell had a riot. The β coefficient as shown in Table 3 is close to zero and statistically insignificant.

9. Mechanisms

A growing body of economics literature on endogenous preference formation emphasizes early childhood as a period in which fundamental preference parameters and character skills develop [34]. More importantly, recent studies have highlighted that the social environment during early childhood can have persistent causal effects on preferences, such as the preference for honesty (Abeler, Falk, and Kosse 2021), risk (Giuliano and Spilimbergo 2014), and redistribution (Cappelen et al. 2020). In particular, seminal recent studies on exposure to violence in early childhood, such as those by Couttenier et al. 2019; Fisman et al. 2020), have found that high casualties resulting from intergroup conflict produce lasting intergroup hostility. One possibility of social environment affecting children is through parental influence. Parental traits can shape preferences of their children, for example children have been shown to become long-term oriented when observing a long-term oriented adult (Bandura and Mischel 1965). It is possible that parents experiencing effective state-intervention in civil clashes develop positive attitude towards state institutions. The children observing their parents confidence in the state

[33] We could not find any nationally representative survey capturing both the origin and destination districts. The later rounds of the NSSO's Employment and Unemployment Surveys do not provide data on origin districts.

[34] Kautz et al. 2014; Alan, Boneva, and Ertac 2019; Falk et al. 2021; Kohlberg 1984; Piaget 1997; Harbaugh, Krause, and Vesterlund 2002; Sutter and Kocher 2007; Fehr, Bernhard, and Rockenbach 2008; Ben-Ner et al. 2017; Almås et al. 2010; Bauer, Chytilová, and Pertold-Gebicka 2014

may develop greater support for state. We follow the emerging literature on early childhood and explore whether there is intergroup hostility in the observed judicial stringency or whether judicial stringency potentially represents support for the state; we use our data to rule out other non-behavioral channels, such as differences in cognitive abilities.

9.1. **No Intergroup Bias Behavior.** Early-life exposure to an intergroup conflict could generate animosity between groups, as evidenced in the high-intensity Hindu–Muslim violence in the Indian context in the case of bank managers (Fisman et al. 2020). To estimate the intergroup hostility effect, we would need to identify the religion of the judges and the defendants but do not have such administrative data. Following Bhalotra et al. 2014, who use names to infer the religion of electoral candidates in India, we use names to infer the religion of the judges and the defendants.

We manually assign each judge to a religious group using the judge's name and their father's name. For defendants, first, we use the "Stanford Named Entity Algorithm" to extract their names from the judgments (see Appendix C.3 for details). Then, we use the Nilabhra name2community algorithm to identify Urdu-sounding names, which we classify as Muslim names. To address the concern about the likely scope for error in identifying Muslim names, we test it on the dataset of Bhalotra et al. 2014. We find that this algorithm predicts the religion from names with a 6% error rate. However, the error rates in the classification of the defendants' religion are higher (20%) owing to additional errors in the process of extracting names from the judgment pdfs.

In our sample, only 51 out of the 668 judges are Muslims, out of which only 13 Muslim judges were exposed to communal violence when 0–6 years old. In comparison, we have 617 Hindu judges in our sample, out of which 197 Hindu judges were exposed to religious riots between the ages of 0 and 6 years.[35] However, about 20% of cases involve only Muslim defendants, as measured by the algorithm.

We perform a subsample analysis to test whether the bail decisions of early-childhood riot-exposed Hindu judges differ in cases where all the defendants are Hindus from their decisions in cases where all the defendants are Muslims. Columns (2) and (3) of Table 6 reveal that the coefficient measuring the causal effect on early-childhood exposure to communal riots remains positive for both Hindu and Muslim defendants, with the coefficient for Hindu defendants being 0.051 percentage point (14% increase in pretrial detention rates) and 0.073 percentage point (20% increase in pretrial detention rates), both statistically significant at the 5% level of significance.

9.2. **Riot Intensity and State Lockdowns.** Hindu–Muslim religious clashes in India affect socioeconomic outcomes not only through riot casualties or social segregation but also through state-imposed lockdowns. S. I. Wilkinson 2006 argues that the state response to Hindu–Muslim riots in the form of arrests, lockdowns, and increased police presence plays a huge role in

[35] The sparse presence of Muslim judges is not surprising, and several studies have shown the under-representation of Muslims, including Fisman et al. 2020 who considered the exposure to violence of bank managers.

determining riot damages. In other words, an effective state response can prevent the escalation of a riot. The early-childhood exposure of individuals to a sociopolitical environment in which strong state action resulted in fewer riot-related deaths can potentially generate in them support for, or confidence in, the state relative to the individual. Therefore, we hypothesize that judicial stringency could be driven by judges with a positive childhood experience of state intervention to curb civilian misconduct.

To examine this hypothesis, we analyze the heterogeneity impact of riot casualties interacted with state lockdowns. First, we compute casualties per land area (instead of population size to avoid reverse causation bias) experienced by each judge between ages 0 and 6 years. We select the median value of casualty experienced by the riot-exposed judges as the threshold below which we term a riot as a low-severity riot. Similarly, a state response measure, such as a high lockdown, is defined as a lockdown that lasts for more than 5 days, which is the median days of lockdown experienced by our sample of riot-exposed judges. High arrests are defined as arrests that exceed 170, the median value of arrests that occurred in riots experienced by judges between the ages of 0 and 6 years.

We compare the bail decisions of non-exposed judges with that of judges experiencing high state action in terms of high lockdowns or arrests, but varying levels of riot casualties, using the following equation:

$$\begin{gathered} B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{F}.\mathcal{E}_b + \beta_1 \times high - casualty - high - state - action[0-6]_j + \\ \beta_2 \times low - casualty - high - state - action[0-6]_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + X_j + \epsilon_{j,d,t} \end{gathered} \tag{6}$$

where $high - casualty - high - state - action[0-6]_j$ denote high casualty and a higher period of lockdowns or police arrests, and $low - casualty - high - state - action[0-6]_j$ denote low levels of casualty and higher period of lockdowns or police arrests. The remaining variables are same as in our main specification in equation- 1.

Table-4 presents the heterogeneity impact by intensities of lockdown and riot-related casualties. We observe that among the judges who experienced early-childhood riots with intense state response measured by the total number of police arrests or the total number of days of state-imposed lockdowns, it is the judges with an early-childhood experience of riots resulting in low casualties who drive judicial stringency in bail decisions.[36]

The above pattern is consistent with the hypothesis that the early-life experiences of judges regarding effective lockdowns or police arrests that have effectively controlled civilian violence generate in them persistent confidence in the state relative to the individual.

[36]We report riot severity results using an alternative specification in Table B.XV. We find that by all measures of riot intensity, low-intensity riots explain a high share of bail denied by early-childhood riot-exposed judges, which is statistically significant at the 1% level of significance (compared with non-exposed judges). Further the results are robust to using alternative methods of calculating the intensity thresholds, as presented in Appendix Table B.XVIII

9.3. **Heterogeneity by Age of Exposure.** Motivated by Cappelen et al. 2020, who show that interventions during ages 3 to 4 years have a long-term impact on social preferences, we examine the heterogeneity by riot exposure to test whether our interpretation of the interaction term of conflict severity is driven by changes in the support for the state.

We group judicial officers based on their exposure to riots into three mutually exclusive age groups of 0–3, 3–6, and 6–9 years to test whether the effects are determined by exposure between the ages of 3 and 6 [37]. We estimate the following regression specification, which is a variant of our main specification in equation 1:

$$\begin{aligned} B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{F}.\mathcal{E}_b + \beta \times pre - birth[0-3]_j + \beta_1 \times kid[0-3]_j + \\ \beta_2 \times kid[3-6]_j + \beta_3 \times kid[6-9]_j + X_j + \epsilon_{j,d,t} \end{aligned} \tag{7}$$

In our data, a similar number of judges in each age bin were exposed to communal riots. The families of 125 judges (approximately 19% of the total judges in the estimation sample) were exposed to violence between 0 and 3 years before the judges' birth. Further, 131 judges were exposed to violence when 0–3 years old, and 132 judges when 3–6 years old (which is approximately 20% of the total judges in the estimation sample). Last, 163 judges were exposed to communal clashes when 6–9 years old (which is approximately 24% of the total judges in the estimation sample). In Appendix Table B.XVI, we show the results on estimating the above equation for older cohorts of judiciary officers for whom we can control for potential exposure to violence up to age 22 years. We find a statistically significant ($p < .05$) positive treatment effect of exposure for the age group of 3–6 years. Figure 1 plots the coefficient estimate of each age group. We note that the effects are primarily driven by exposure between the ages of 3 and 6 years.

Next, we test the impact on bail decisions by the age of first exposure to communal conflicts. We construct conditional extensive margins by estimating the effects on bail decisions by the age at first exposure, denoted by $firstexposure(k)$. Our regression equation is as follows:

$$B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{F}.\mathcal{E}_b + \beta \sum_{k=1}^{n} firstexposure[k]_j + \sigma X_j + \epsilon_{j,d,t} \tag{8}$$

where $firstexposure(k)$ is defined as the age at first exposure at k. All other variables are the same as specified for equation 1.

Figure 3 and Appendix FigureA.V plot the coefficient estimates with 95% confidence intervals of equation 8 for the full sample, for which the effects of the age at first exposure can be estimated only up to year 9, and for the subsample, for which the effects of the age at first exposure can be estimated up to year 22. Both plots demonstrate the causal effect on bail decisions of the age at first exposure being 4 and 5 years. Appendix Table-B.XVII shows the

[37] The term mutually exclusive means that if a judge is exposed when aged 0–3 years as well as when aged 3–6 years, then (s)he will be categorized into the 0–3 age category.

effect of age at first exposure to communal violence for the full analysis sample. We observe that the effect of the age at first exposure being 4 years is 0.081 percentage point, and the effect of the age at first exposure being 5 years is 0.118 percentage point, which is statistically significant at the 5% and 1% levels of significance, respectively. Given the findings in the literature on early childhood, this result provides further support for our interpretation in the above section that there is a causal link between early-childhood riot exposure to the support for the state in controlling civilian misconduct.

9.4. **Judicial Education and Judicial Stringency.** Next, we test whether differences in cognitive skills as measured by performance in the mandatory LLB examination explain differences in bail decisions across judges. Table-5 reports that the inclusion of LLB examination results do not affect our coefficient estimates of the early-childhood riot exposure effect. Therefore, we conclude that heterogeneity in skills in law training does not explain our results.

10. Conclusion

In this study, we examine the population of judges and show that their exposure to communal violence at ages 0–6 years has persistent economic and statistically significant effects on pretrial detention rates. Unlike studies that have focused on estimating bias and discrimination in judicial decisions, we investigate the origins of judicial bias. We show that early-childhood exposure to the sociopolitical environment has robust effects on adult decisions across generations. We show that judges exposed to communal violence between the ages of 0 and 6 years are 16% more prone to deny bail than the average judge. The effect is driven by exposure to a low number of riot-related deaths and injuries and a low riot duration, as well as by exposure when between 3 and 6 years of age.

We provide some evidence in support of our interpretation that the experience of riot de-escalation efforts by the state that result in low riot-related damages during formative years has long-term effects on judicial outcomes. Further research on how preferences and beliefs are formed owing to sociopolitical events during the formative years of childhood would provide decision-makers with insights for designing effective policy tools.

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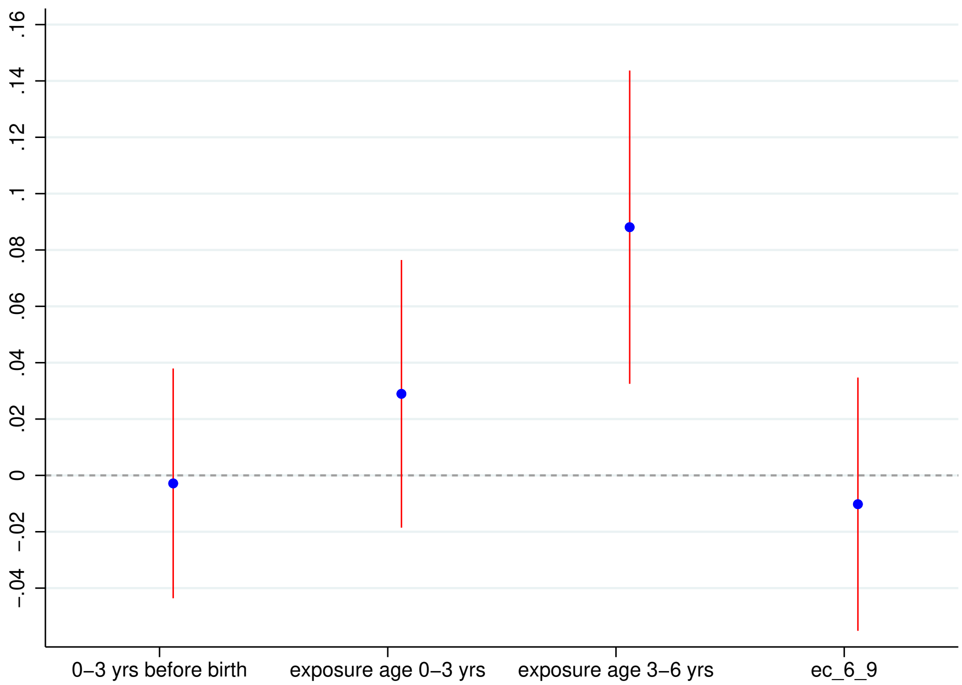
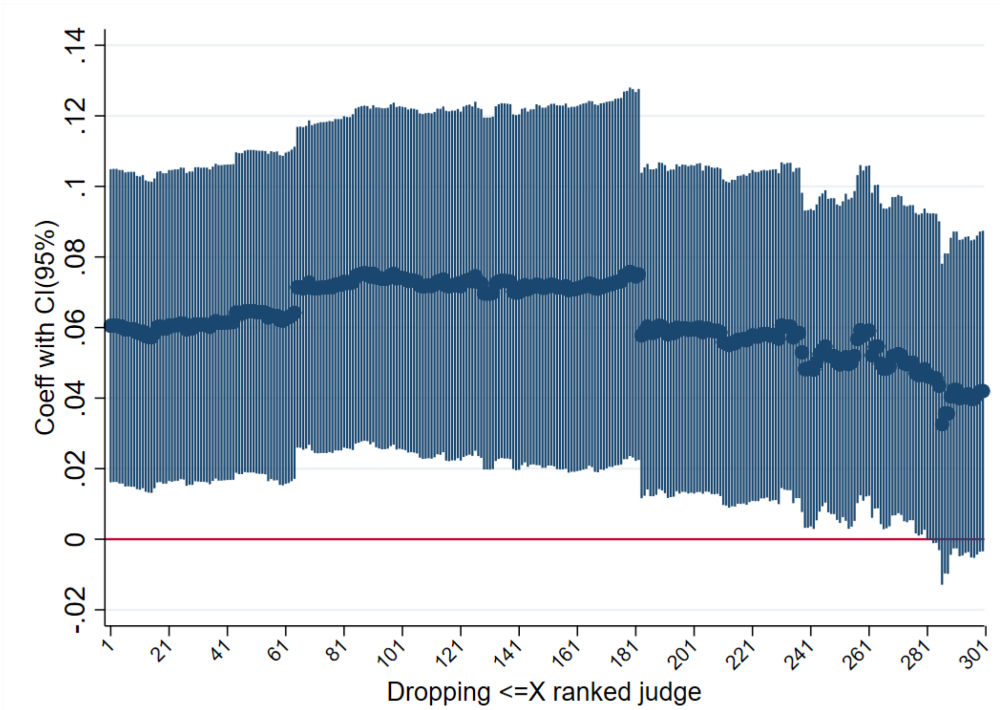
11. Figures

Figure 1. Treatment Effects Estimates of Riot-Exposure by Age



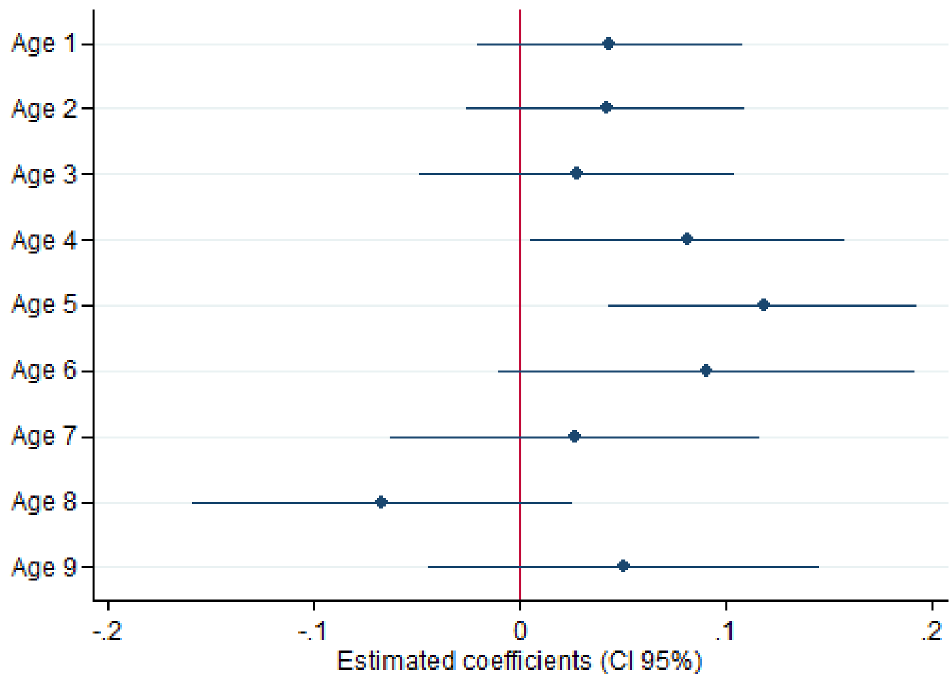
Notes: The figure reports the coefficients from equation-7, where we create four mutually exclusive age bins of 3 years each starting from 3 years before birth up to 9 years of age for exposure to riots. The figure illustrates that exposure to riots when aged 3-6 years of age is statistically significant.

FIGURE 2. Removing Judges serially with high Influence



Notes: The figure reports the coefficients from running the main equation-7, 300 times by removing one by one judges with highest contribution to the early-childhood exposure effect. The contribution is measured by Cook's distance. The result goes away only after removing half of the judges from the sample.

Figure 3. Treatment Effects Estimates of Age at First Exposure



Notes: Estimated coefficients and confidence interval at 95% are reported through ordinary least squares estimation using equation-8 on the total sample of 668 judges. The main dependent variable is the pretrial detention rate at the judge X district X quarter level. The estimation includes home district X quarter, birth year, and district X quarter fixed effects.

12. Tables

Table 1. BALANCE TESTS

PANEL A: DISTRICT LEVEL	(1)	(2)	(3)	(4)	(5)	(6)
Proportion	Male	Muslim	SC/ST	Working Pop	Not Literate	Male Primary Ed
***K*id[0-6]**	0.001**	-0.014	0.005	0.008***	-0.004	0.000
	(0.000)	(0.009)	(0.005)	(0.002)	(0.006)	(0.002)
Observations	38,034	38,034	38,034	38,034	38,034	38,034
R-squared	0.092	0.114	0.102	0.123	0.130	0.092
Mean Dep Var	0.53	0.19	0.21	0.33	0.32	0.34
home-district X Quarter F.E	yes	yes	yes	yes	yes	yes
Year of Birth F.E	yes	yes	yes	yes	yes	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge
Judge	668	668	668	668	668	668

PANEL B: PEERS AND CASES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
At Dist-Qtly level	Total	Muslim	Prop. Muslim	Avg. Age	Avg. Exp	Female	Tot Bail Cases
***K*id[0-6]**	-0.354	-0.136	-0.004	0.118	0.140	-0.018**	-.921
	(2.016)	(0.173)	(0.005)	(0.198)	(0.218)	(0.009)	(6.754)
Observations	5,369	5,369	5,369	5,369	5,369	5,369	5530
R-squared	0.199	0.197	0.175	0.207	0.211	0.226	0.359
Mean of Dep Var	34.72	2.05	0.06	46.32	14.25	0.17	55.28
home-district X Quarter F.E	yes	yes	yes	yes	yes	yes	yes
Year of Birth F.E	yes	yes	yes	yes	yes	yes	yes
District X Quarter F.E	no	no	no	no	no	no	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge	Judge
Judges	642	642	642	642	642	642	660

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges with home districts in UP. Robust standard errors in parentheses are clustered at the judge level. The dependent variable in Panel A (District-level Characteristics in terms of Proportion) are: Male (Column 1), Muslims (Column 2), Scheduled Caste (SC)/ Scheduled Tribe (ST) (Column 3), Working Population (Column 4), Illiteracy (Column 5), and Male Primary Education (Column 6). The dependent variable in Panel B (Peers of Judges with leave-me-out approach): Total Judges (Column 1), Number of Muslim Judges (Column 2), Proportion of Muslim Judges (Column 3), Average Age (Column 4), Average Experience (Column 5), Proportion of Female Judges (Column 6), and Total Bail Cases (Column 7). The sample mean and the standard deviation of the dependent variables are reported. The explanatory variable is exposure to communal conflict between the ages of 0 and 6 years. All estimations include home district X quarter, year of birth fixed effects. Exposure to riots until 9 years of age is included as control with a set of binary measures. The reduction from 668 is due to the dropping of singleton observations.

Table 2. BAIL DECISIONS AND EARLY EXPOSURE TO RIOTS

PANEL A: FULL SAMPLE DEPENDENT VARIABLE: SHARE DENIED	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
1-5 yrs ***P*re Birth**			0.008		
			(0.020)		
***K*id[0-6]**	0.063***	0.061***	0.062***	0.060**	0.054***
	(0.022)	*(0.023)*	*(0.023)*	*(0.024)*	*(0.017)*
Observations	5,530	5,530	5,530	5,443	29,925
R-squared	0.327	0.332	0.332	0.391	0.235
Mean Dep Var	0.37	0.37	0.37	0.37	0.32
Controls	no	yes	yes	yes	yes
home-district X Quarter F.E	yes	yes	yes	yes	yes
Date of Birth F.E	yes	yes	yes	no	yes
District X Quarter F.E	yes	yes	yes	yes	yes
Date of Birth X Quarter F.E	no	no	no	yes	no
Crime Type F.E	no	no	no	no	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge
Total Number of Clusters	660	660	660	657	668
Standard errors of *K*id[0-6]					
Moulton-corrected	0.0219	0.0220	0.0225		
Wild Bootstrap errors	0.0231	0.0232	0.0237		

PANEL B: SUBSAMPLES DEPENDENT VARIABLE: SHARE DENIED	**(1)**	**(2)**	**(3)**
***K*id[0-6]**	0.059**	0.058**	0.056**
	(0.023)	(0.024)	(0.024)
$\sum_{k=7}^{N} expo(k)_{h,b,t}$: **N**	14	18	22
Cluster Level	Judge	Judge	Judge
No of Judges	651	637	604
Observations	5,488	5,425	5,248
R-squared	0.335	0.339	0.343
Mean Dep Var	0.37	0.37	0.37
home-district X Quarter F.E	yes	yes	yes
Date of Birth F.E	yes	yes	yes
District X Quarter F.E	yes	yes	yes

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges (Panel A) and the subsample of judges (Panel B) from home districts in UP. Robust standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at the judge–district–quarter level. Kid[0–6] is a dummy of childhood exposure to communal conflict. Column 1 is without controls; Column 2 includes controls such as a dummy for Muslim, for female, first division in the Bachelor of Law examination, and the total tenure as a judge at the time of judgment, Column 3 further adds a binary measure of pre-birth exposure of judges' families to communal conflict. All estimations include a set of binary variables coding for exposure up to 9 years of age (since our conflict data are up to the year 2000 and the youngest judge in our sample is born in 1991). In Panel B, we extend the exposure control for later years: up to 14 years of age (Column 1: keeping judges born before 1986), 18 years of age (Column 2: keeping judges born before 1982), and 22 years of age (Column 3: keeping judges born before 1978) on a subsample of judges. All estimations include home district X quarter, year of birth, and district X quarter fixed effects.

Table 3. Does Migration rate affected by Riots ?

SHARE DENIED	
Riot (1/0)	0.00246
	(0.0289)
Observations	1,530
R-squared	0.361
Mean Dep Var	0.31
Origin District FE	yes
Year of Migration FE	yes

Notes: This table reports ordinary least squares estimation based on district-year level data prepared from the National Sample Survey Organization's Employment and Unemployment Survey 1983, following the specification in equation-5. Robust standard errors are provided in parentheses. The dependent variable is inter-district migration rate at the district-year level. The main explanatory variable is a dummy capturing whether the district-year cell has experienced a communal riot. The estimation includes district and year fixed effects. The main coefficient of interest captures whether the district-year cells affected by communal riots have more migration at the across-district level.

Table 4. CONFLICT INTENSITY AND HIGH STATE RESPONSIVENESS

CONFLICT INTENSITY SHARE DENIED	(1)	(2)
***K*id[0-6]**High Curfew*High Riot-Casualty	0.009 (0.031)	
***K*id[0-6]**High Curfew*Low Riot-Casualty	0.0774* (0.0418)	
***K*id[0-6]**High Arrests*High Riot-Casualty		-0.0173 (0.0331)
***K*id[0-6]**High Arrests*Low Riot-Casualty		0.134*** (0.0444)
Observations	4,573	4,609
R-squared	0.362	0.371
Mean Dep Var	0.36	0.36
Controls	yes	yes
home-district X Quarter F.E	yes	yes
Date of Birth F.E	yes	yes
District X Quarter F.E	yes	yes
Cluster Level	Judge	Judge
Total Number of Clusters	549	551

Notes:We report ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at the judge–district–quarter level. All estimations include home district X quarter, year of birth, and district X quarter fixed effects, and a set of binary variables coding for past exposure up to 9 years of age. The main explanatory variable (Kid[0–6] is a binary measure of childhood exposure to communal conflict at 0–6 years of age) where high state action is interacted with low and high conflict severity. The median of the variable under consideration defines the threshold for severity to split treated judges equally into two groups. The median district areas' threshold value is 23 casualties (killed and injured). The median curfew is of 5 days duration and median arrest is 170. In Columns 1 and 2 we compare the judges experiencing high state action in terms of curfew and arrests respectively with judges not experiencing riots in their first 6 years of life.

Table 5. Judicial Stringency and Cognitive Skills

DEPENDENT VARIABLE: SHARE DENIED	(1)	(2)
***K*id[0-6]**	0.0605***	0.0595***
	(0.0226)	(0.0223)
LLB Division	0.00669	
	(0.0163)	
Observations	5,530	5,530
R-squared	0.332	0.332
Mean Dep Var	0.37	0.37
home-district X Quarter F.E	yes	yes
Date of Birth F.E	yes	yes
District X Quarter F.E	yes	yes
Number of Judges	660	660

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 660 judges who are from home districts within UP. Robust standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at the judge–district–quarter level. Kid[0–6] is a dummy of childhood exposure to communal conflict. The controls include a dummy for Muslim, for female, and the experience of the judge at the time of judgment. Column 1 includes the dummy variable indicating whether the judge attained first division in the Bachelor of Law examination. Column 2 reports the estimates without including performance in this examination as control. All estimations include home district X quarter, year of birth, and district X quarter fixed effects.

Table 6. Heterogeneity by Religion of the Defendants

	(1) Hindu Judge	(2) Hindu Judge-Hindu Defendant	(3) Hindu Judge-Muslim Defendant
***K*id[0-6]**	0.057**	0.051**	0.073**
	(0.025)	*(0.025)*	*(0.037)*
Observations	5,079	4,925	3,078
R-squared	0.348	0.321	0.399
Mean Dep Var	0.37	0.37	0.35
home-district X Quarter FE	yes	yes	yes
D.O.B Year FE	yes	yes	yes
District X Quarter FE	yes	yes	yes
Cluster	Judge	Judge	Judge
No of Judges	609	608	591

Notes: This table reports OLS estimations based on the judge-district-quarter level. Robust standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at the judge-district-quarter level. Kid[0-6] is a dummy for exposure to communal conflict between 0-6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home-district X quarter, year of birth, and district X quarter fixed effects. Column 1 includes cases handled by only the Hindu Judges. Column 2 includes the cases handled by Hindu Judges when all the involved defendants are Hindu. Column 3 includes the cases handled by Hindu Judges when all the involved defendants are Muslims.

Appendix A. Figures

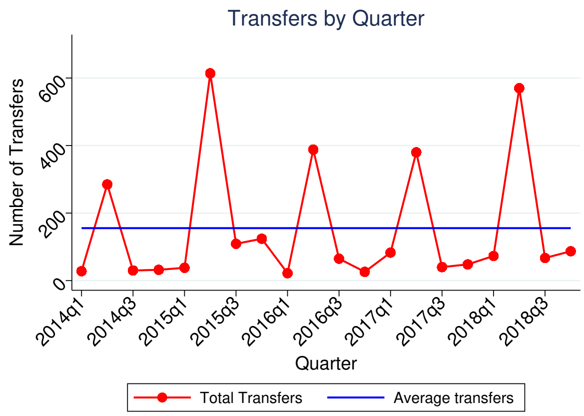


Figure A.I. All Judges

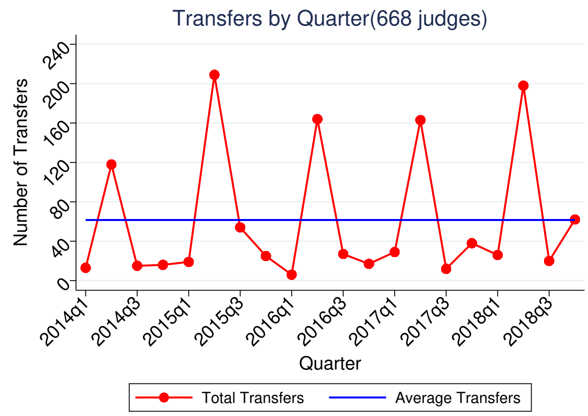
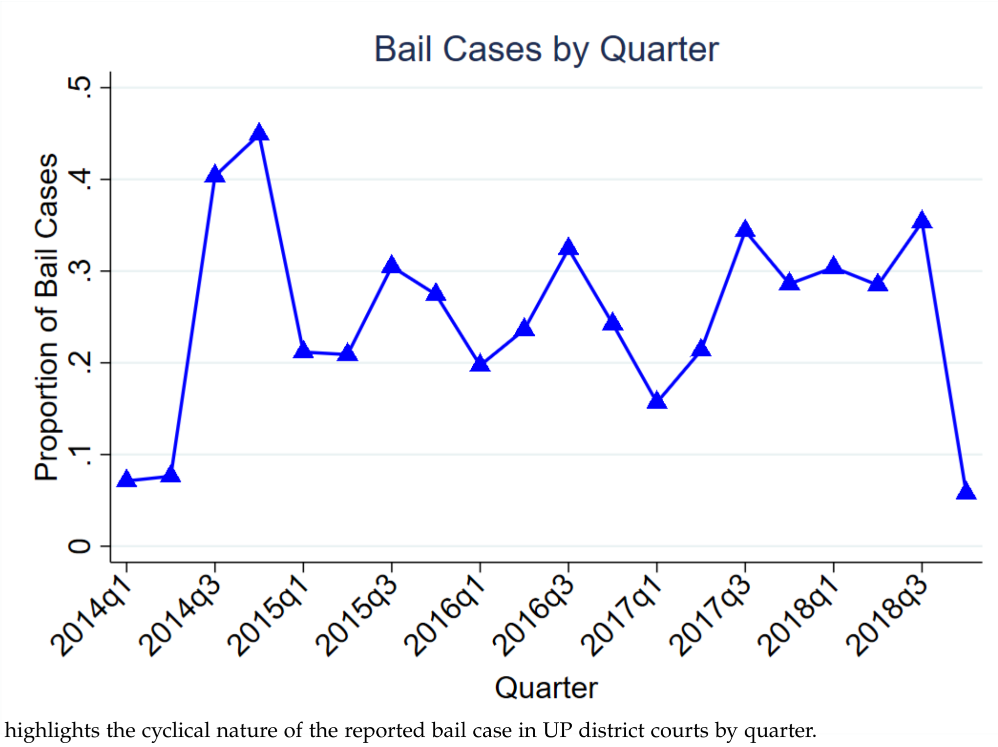


Figure A.II. Sample Judges

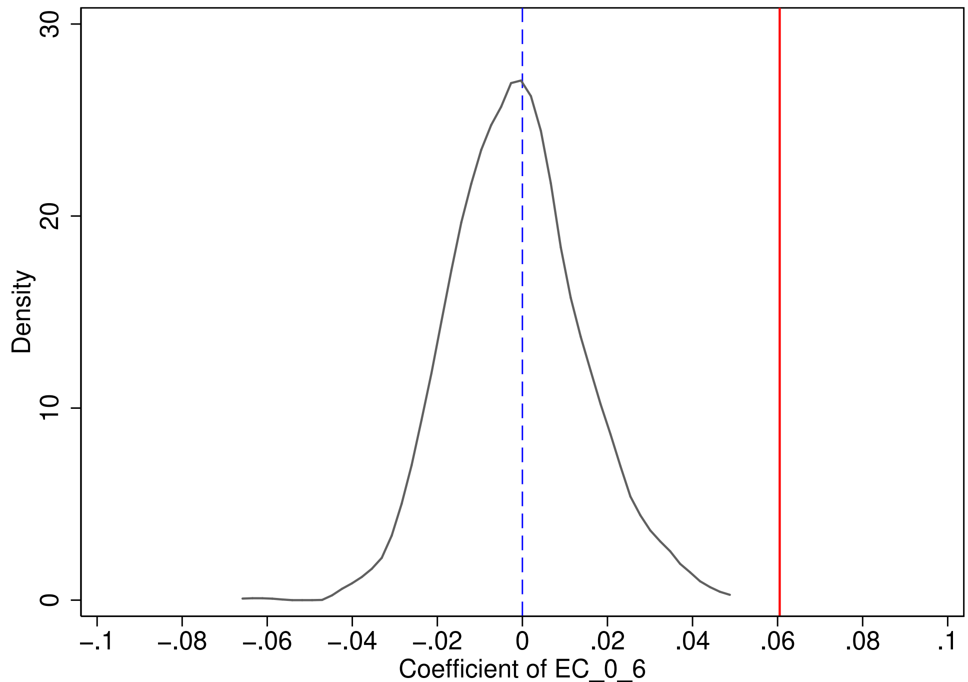
Notes: The figure reports the total and the average number of transfers of judges in the state of Uttar Pradesh for the full sample of judges and the 668 bail judges in our analysis sample. On average, 195 judges (7% of all judges) are transferred every quarter, with most transfers concentrated in the second quarter. Our analysis sample has a similar trend, with an average of 8.5% judges transferred every quarter.

Figure A.III. Distribution of Bail Cases by Quarter



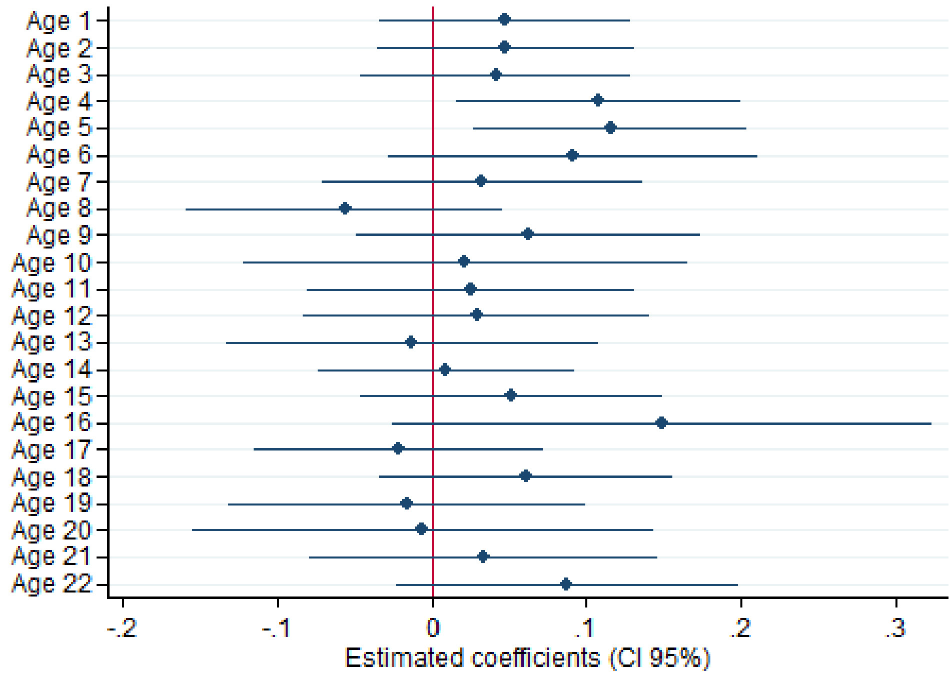
Notes: The figure highlights the cyclical nature of the reported bail case in UP district courts by quarter.

Figure A.IV. Distribution of Monte Carlo Treatment Effect Estimates



Notes: The figure displays the distribution of coefficients from our main regression (Column 2 of Table 2) obtained from 1,000 random draws of our variable Kid[0–6], keeping the proportion of judges (210/668) experiencing a communal conflict between 0 to 6 years the same as in our original dataset.

Figure A.V. Treatment Effects Estimates of Age at First Exposure



Notes: Estimated coefficients and confidence interval at 95% are reported from ordinary least squares estimation using equation 8 on a sample of 612 judges born before 1979. The main dependent variable is the pretrial detention rate at the judge X district X quarter level. The estimation includes home district X quarter, birth year, district X quarter fixed effects.

Appendix B. Tables

Table B.I. Creation of New Districts in Uttar Pradesh

State	New Districts	Parent Districts	Date of Formation	Comments	Source(link)
UP	Ambedkar Nagar	Faizabad	29-09-1995	Tehsils: Akbarpur, Tanda	https://ambedkarnagar.nic.in/
UP	Amethi	Sultanpur/Rae Bareilly	01-07-2010	Sultanpur Tehsils: Amethi, Gauriganj and Musafirkhana; Rae Bareilly Tehsils: Salon and Tiloi	https://amethi.nic.in/
UP	Amroha (Jyotiba Phule Nagar)	Moradabad	24-04-1997	Tehsils: Separating Amroha, Dhanora and Hasanpur	https://amroha.nic.in/
UP	Auraiya	Etawah	17-09-1997	Tehsils: Auraiya and Bidhuna	https://auraiya.nic.in/
UP	Balrampur	Gonda	25-05-1997		https://balrampur.nic.in/
UP	Baghpat	Meerut	01-05-1997	Tehsil: Baghpat	https://baghpat.nic.in/
UP	Bhadohi	Varanasi	30-06-1994		https://bhadohi.nic.in/
UP	Chandauli	Varanasi	20-05-1997		https://chandauli.nic.in/
UP	Chitrakoot	Banda	06-05-1997	Tehsils: Karwi and Mau	https://chitrakoot.nic.in/
UP	Gautam Buddha Nagar	Ghaziabad/ Bulandshahar	06-09-1997	Ghaziabad Blocks: Dadri and Bisrakh; Bulandshahar Blocks: Danakur and Jewar	https://gbnagar.nic.in/
UP	Ghaziabad	Meerut/ Bulandshahar	14-11-1976		https://censusindia.gov.in/2011census/dchb/ 0909_PART_B_DCHB_GHAZIABAD.pdf
UP	Hapur	Ghaziabad	28-09-2011	Tehsils: Hapur, Garhmukteshwar and Dhaulana	https://hapur.nic.in/
UP	Haridwar	Saharanpur	1988		https://haridwar.nic.in/
UP	Hathras (Mahamaya Nagar)	Aligarh/Mathura/ Agra	03-05-1997	Hathras tehsil came from Aligarh. Further it shares the Jail of Aligarh.	https://hathras.nic.in/
UP	Kannauj	Farrukhabad	18-09-1997		https://kannauj.nic.in/
UP	Kasganj (Kanshi Ram Nagar)	Etah	15-04-2008		https://kasganj.nic.in/
UP	Kaushambi	Allahabad	04-04-1997		https://kaushambi.nic.in/
UP	Kushinagar (Padrauna)	Deoria	13-05-1994		https://kushinagar.nic.in/
UP	Kanpur Dehat	Kanpur	23-04-1981		https://kushinagar.nic.in/
UP	Kanpur Nagar	Kanpur	23-04-1981		https://kanpurdehat.nic.in/
UP	Lalitpur	Jhansi	1974	Exact date unknown	https://lalitpur.nic.in/
UP	Maharajganj	Gorakhpur	02-10-1989		https://maharajganj.nic.in/
UP	Mahoba	Hamirpur	11-02-1995	Tehsil: Mahoba	https://mahoba.nic.in/
UP	Mau	Azamgarh	14-11-1988		https://mau.nic.in/
UK	Rudraprayag	Chamoli	18-09-1997		https://rudraprayag.gov.in/
UP	Sambhal	Moradabad	28-09-2011		https://sambhal.nic.in/
UP	Sant Kabir Nagar	Basti/ Siddarth Nagar	05-09-1997	Formed by carving : complete Khalilabad tehsil (from Basti), 131 villages of Basti tehsil and 161 villages of Santha block (Bansi tehsil, Siddarth Nagar)	https://sknagar.nic.in/
UP	Shamli	Muzaffarnagar	28-09-2011	Tehsils: Shamli and Kairana	https://shamli.nic.in/
UP	Shravasti	Bahraich	01-05-1997		https://shravasti.nic.in/
UP	Siddarth Nagar	Basti	29-12-1988		https://siddharthnagar.nic.in/
UP	Sonbhadra	Mirzapur	04-03-1989		https://sonbhadra.nic.in/
UK	Uddham Singh Nagar	Nainitaal	29-09-1997		https://usnagar.nic.in/

Notes: The table outlines the formation of new districts from parent (origin) districts in alphabetical order, their date/year of formation, and the source of this information. We assign all districts to their origin districts. With this harmonization, we arrive at 47 unique home districts with our 668 sample of judges. The details of district merging are explained in Appendix notes C.1.

Table B.II. DESCRIPTIVE STATISTICS BY EARLY EXPOSURE TO RIOTS

SUBSAMPLE:	NON EXPOSURE 0-6 YRS.	EXPOSURE 0-6 YRS.
TOTAL:	458	210
	A. JUDGES' CHARACTERISTICS	
Proportion of Females	0.0196	0.157
(std. dev.)	*(0.014)*	*(0.025)*
Proportion of Muslims	0.083	0.062
(std. dev.)	*(0.013)*	*(0.017)*
Age	52.41	50.01
(std. dev.)	*(0.328)*	*(0.605)*
Experience	19.5	17.5
(std. dev.)	*(0.416)*	*(0.671)*
LLB division	1.74	1.65
(std. dev.)	*(0.021)*	*(0.033)*
	B. PEERS CHARACTERISTICS IN DISTRICT	
Number of Judges	34.96	33.78
(std. dev.)	*(0.278)*	*(0.421)*
Fraction of Female Judges	0.172	0.165
(std. dev.)	*(0.001)*	*(0.002)*
Fraction of Muslim Judges	0.063	0.058
(std. dev.)	*(0.001)*	*(0.001)*
Age of Peer judges	46.25	46.44
(std. dev.)	*(.034)*	*(0.051)*
Experience of Peer judges	14.16	14.39
(std. dev.)	*(0.038)*	*(0.054)*
	C. SOCIO-ECONOMICS OF DISTRICTS ASSIGNED TO JUDGES	
Proportion of Male	0.526	0.526
(std. dev.)	*(0.001)*	*(0.001)*
Proportion of Muslims	0.191	0.186
(std. dev.)	*(0.004)*	*(0.006)*
Proportion of SC/ST	0.209	0.215
(std. dev.)	*(0.002)*	*(0.003)*
Illiteracy Rate	0.325	0.325
(std. dev.)	*(0.003)*	*(0.003)*
Working Population	0.334	0.337
(std. dev.)	*(0.001)*	*(0.002)*

Notes: Subsection A presents the average observed characteristics of judges experiencing no riots in the first 6 years of childhood (458 judges) and judges experiencing riots in the first 6 years of childhood (210 judges). Subsection B shows a subset of judge characteristics, as explained in Table B.III. Subsection B presents average peers' characteristics (using the leave-me-out approach, i.e., the judge is excluded when peers' characteristics are computed) at the district-quarter level. Subsection C presents the average district characteristics (computed from Census 2011 district-level data) of judges exposed to, and judges not exposed to, Hindu–Muslim riots when aged 0–6 years.

Table B.III. Descriptive Statistics of UP District Court Judges

	All Judges			Bail Judges			Our Sample		
VARIABLES	N	mean	sd	N	mean	sd	N	mean	sd
Muslim Judge	2,434	0.061	0.240	1,236	0.067	0.250	668	0.0763	0.266
Female Judge	2,434	0.198	0.398	1,236	0.137	0.344	668	0.115	0.320
Age	2,434	46.55	11.29	1,236	49.58	8.537	668	51.66	7.687
Joining Age	2,434	31.81	4.687	1,236	32.76	4.920	668	32.81	5.114
Experience	2,434	14.74	11.26	1,236	16.82	9.283	668	18.85	9.202
Promotion Time Taken	1,651	7.471	2.260	974	7.169	2.356	542	7.560	2.352
Grade 10 Division	1,405	1.460	0.567	549	1.526	0.571	235	1.579	0.575
Grade 10 Age	1,446	14.99	0.984	579	14.93	0.970	246	14.91	0.963
Grade 12 Division	1,413	1.537	0.580	553	1.627	0.598	238	1.689	0.599
Grade 12 Age	1,446	17.11	1.080	579	17.06	1.056	247	17.05	1.085
LLB Division	2,385	1.621	0.488	1,206	1.697	0.460	668	1.711	0.454
LLB Age	2,405	23.72	2.134	1,219	23.64	2.119	668	23.42	2.031
Masters	2,434	0.386	0.487	1,236	0.360	0.480	668	0.356	0.479
PhD	2,434	0.015	0.121	1,236	0.016	0.126	668	0.0120	0.109
Number of Bachelors	2,431	1.863	0.363	1,234	1.938	0.272	668	1.958	0.253

Notes: The table presents the characteristics of all judges (including the judges who retired during 2014–2018 but had handled bail cases), bail judges, and our sample of judges (arrived after removing judges handling less than 97 cases and judges with no information on the division obtained in the Bachelor of Law (LLB) examination). We notice that our sample of judges is very similar to the full sample of judges along different observable characteristics, such as the proportion of Muslim judges, the proportion of female judges, age (on December 31, 2018), joining age (age in years at which a judge enters into judiciary), experience, time to promotion (in years to become a Class I rank of judge), Grade 10 division (3 divisions: I ($\geq$ 60% marks), II ($\geq$ 45% and $<$ 60%), and III($\geq$ 33% and $<$ 45%), Grade 12 Division (3 divisions: I ($\geq$ 60% marks), II ($\geq$ 45% and $<$ 60%), and III($\geq$ 3% and $<$ 45%), LLB Division(I ($\geq$ 60% marks), II ($\geq$ 45% and $<$ 60%), and III($\geq$ 33% and $<$ 45%), Grade 10 Age (age at which a judge completes Class 10), Grade 12 Age (age at which a judge completes Class 12/Intermediate), LLB Age (age at which a judge obtains the LLB degree), Masters (dummy = 1 if a judge has a master's degree), PhD (dummy = 1 if a judge has a doctorate), and Number of Bachelors (number of bachelor's degrees of a judge). Only graduates were eligible to take the LLB exam earlier; currently, some integrated courses, such as BA + LLB, have started).

TABLE B.IV. Balance Table : Crime Categories (FULL SAMPLE)

	Body Crime	Prop Crime	Crim Intim	Cow Slaught	Elec. Theft	Others
***K*id[0-6]**	-0.107	-0.770	-0.434	0.0624	-0.102	1.012
	(2.867)	(2.418)	(0.439)	(0.280)	(0.639)	(1.783)
Observations	5,530	5,530	5,530	5,530	5,530	5,530
R-squared	0.325	0.336	0.374	0.327	0.190	0.327
Mean Dep Var	15.47	14.16	4.52	1.21	1.25	10.82
Home Dist X Quarter F.E	yes	yes	yes	yes	yes	yes
Birth Year F.E	yes	yes	yes	yes	yes	yes
Dist X Quarter F.E	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge
No. of Clusters	660	660	660	660	660	660

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level on the full sample of judges. Standard errors in parentheses are clustered at the judge level. The dependent variable is the different categories of crime: Body Crime (Column 1), Property Crime (Column 2), Criminal Intimidation (Column 3), Cow Slaughter (Column 4), Electricity Theft (Column 5), and Others (Column 6). Kid[0–6] is a dummy of childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home district X quarter, year of birth, and district X quarter fixed effects. The classification of crime categories is based on Appendix Tables B.XIX and B.XX

Table B.V. Balance Table : Crime Categories (MANUAL SAMPLE)

	Body Crime	Prop Crime	Crim Intim	Cow Slaught	Elec. Theft	Others
***K*id[0-6]**	-1.930	-1.002	-0.159	0.00155	0.587	-0.106
	(1.518)	(0.852)	(0.439)	(0.115)	(0.399)	(0.206)
Observations	3,811	3,811	3,811	3,811	3,811	3,811
R-squared	0.421	0.473	0.440	0.443	0.311	0.433
Mean Dep Var	5.14	4.34	1.76	0.40	0.32	0.82
Home Dist X Quarter F.E	yes	yes	yes	yes	yes	yes
Birth Year F.E	yes	yes	yes	yes	yes	yes
Dist X Quarter F.E	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge
No. of Clusters	525	525	525	525	525	525

This table reports ordinary least squares estimations based on the judge–district–quarter level manually entered sample of 535 judges. Standard errors in parentheses are clustered at the judge level. The dependent variable is the different categories of crime: Body Crime (Column 1), Property Crime (Column 2), Criminal Intimidation (Column 3), Cow Slaughter (Column 4), Electricity Theft (Column 5), and Others (Column 6). Kid[0–6] is a dummy of childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home district X quarter, year of birth, and district X quarter fixed effects. The classification of crime categories is based on Appendix Tables B.XIX and B.XX

Table B.VI. F-TEST OF ROTATION POLICY

Number of Districts X Quarter	**0**	**1**	**5**	**10**
***K*id[0-6]**	95	98.57	100	100
Female	92.45	98.67	100	100
Muslim	97.24	99.69	100	100
LLB First Division	93.37	98.57	100	100
Age	36.63	49.59	79.39	94.9
Joining Age	37.45	51.43	78.88	93.78
Time to Promotion	39.32	60.37	87.58	98.67

Notes: This table is based on the estimation results of Equation 3 on the sample of 668 judges at the district-quarter level. The null hypothesis $\hat{\beta_{d,q}} = \mu_h$ where μ_h refers to home district-level average of the judges' characteristics under consideration, such as childhood exposure to conflict when aged 0–6 years (Kid[0-6]), gender, religion, the division obtained in the Bachelor of Law examination, age on December 31, 2018, age at joining the judiciary and time to promotion as Class I officer. Each row represents the share of home districts for which the F-test of this null hypothesis is rejected at the 10% cutoff in at most 0, 1, 5, and 10 district-quarters.

Table B.VII. Selection into Occupation: Share of Riots Exposed Population

	Share of population exposed to Riots in 0-6 years of age			
	Total Working Population	**Total Judges**	**Total Bail Judges**	**Our Sample Judges**
% Exposed	39,20%	38,64%	38,88%	38,8%
# Population	26 993 272	2 197	890	500

Notes: The table reports the representation of early-childhood riot-exposed judges in the representative Indian sample (the National Sample Survey Organization's Employment-Unemployment Survey, 2011) and the sample of judges in Uttar Pradesh.

Table B.VIII. Does riot affected district-year sends different types of judges ?

	Total Judges	Total Judges	Prop Female Judges	Prop Muslim Judges
Exposed	-0.129	0.00603	0.0196	-0.0178
	(0.498)	(0.346)	(0.0215)	(0.0160)
Observations	694	609	694	694
R-squared	0.811	0.581	0.526	0.508
Mean Dep Var	9.57	5.42	0.24	0.10
Home Dist F.E	yes	yes	yes	yes
Year F.E	yes	yes	yes	yes
Riot Exposure	0-6 yrs	3-6 yrs	0-6 yrs	0-6 yrs

Notes: This table reports ordinary least squares estimations based on the home district-year level sample using the following equation

$$Num\_judges_{h,y} = \alpha + \eta_h + \delta_y + \mathbf{fi} \times \mathbf{Exposed_{h,y}} + \mathbf{ffl_{h,y}}$$

η_h and δ_y are home-district and year fixed effects. Robust standard errors are in parentheses. Exposed is a dummy that indicates whether the home district-year had a riot. The dependent variables are the number of judges (Columns 1 and 2), Proportion of Female Judges (Column 3), and Proportion of Muslim judges (Column 4) from the exposed home district-year in the first 6 years (after riot) in Columns 1, 3, and 4 and in 3–6 years in Column 2.

TABLE B.IX. Bail Decisions and Early Exposure to Riots (Case Level Regressions)

	(1)	(2)	(3)
***K*id[0-6]**	0.0381**	0.0435**	0.0409**
	(0.019)	(0.017)	(0.017)
Muslim Defendant			-0.018***
			(0.005)
Non Bailable			0.155***
			(0.014)
Observations	323,194	323,194	196,300
R-squared	0.081	0.160	0.186
Mean Dep Var	0.34	0.34	0.34
Home Dist X Quarter F.E	yes	yes	yes
Birth Year F.E	yes	yes	yes
Dist X Quarter F.E	yes	yes	yes
Crime Type FE	no	yes	yes
Controls	yes	yes	yes
Cluster Level	Judge	Judge	Judge
No. of Clusters	668	668	668

Notes: This table reports ordinary least squares estimations based on the case level. Standard errors in parentheses are clustered at the judge level. The dependent variable is a dummy for whether the bail is rejected. Kid[0–6] is a dummy of childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age and our usual controls. All estimations include home district quarter, year of birth, and district quarter fixed effects. Columns 2 and 3 further include crime-type fixed effects. Column (3) also adds a dummy for whether the defendant is Muslim and whether the case is nonbailable in nature (i.e., booked crimes are nonbailable according to the Indian Penal Code).

Table B.X. Changing threshold of Judges Handling few cases

SHARE DENIED	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
***Kid*[0-6]**	0.0378*	0.0466**	0.0508**	0.0525**	0.0605***	0.0601**	0.0647**	0.0667**	0.0681**	0.0760***
	(0.0206)	(0.0211)	(0.0216)	(0.0224)	(0.0226)	(0.0236)	(0.0250)	(0.0263)	(0.0269)	(0.0280)
Observations	6,427	6,119	5,889	5,695	5,530	5,370	5,179	5,020	4,877	4,706
R-squared	0.306	0.316	0.315	0.326	0.332	0.331	0.341	0.345	0.350	0.364
Mean Dep Var	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.36
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
home-district X Quarter F.E	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Date of Birth F.E	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
District X Quarter F.E	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Bottom Threshold percentile	1	2	3	4	5	6	7	8	9	10
Num of Bail cases in 4yrs	32	53	69	87	97	109	118	130	143	167
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge	Judge	Judge	Judge	Judge
No. of Clusters	875	794	737	694	660	627	595	570	545	522

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges from home districts within UP. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate. Kid[0-6] is a dummy of childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables, coding for past exposure up to 9 years of age. All estimations include home district X quarter, year of birth, and district X quarter fixed effects. From Columns 1–10, we exclude the judges falling in the bottom 1 to 10 percentiles. The threshold in terms of the number of bail cases handled during 2014–2018 is also provided.

Table B.XI. Excluding High Ranked Judges

	(1)	(2)	(3)
***K*id[0-6]**	0.0835***	0.0538**	0.0768***
	(0.0255)	(0.0231)	(0.0259)
Observations	4,772	5,290	4,538
R-squared	0.358	0.330	0.358
Mean Dep Var	0.37	0.36	0.36
Home Dist X Quarter F.E	yes	yes	yes
Birth Year F.E	yes	yes	yes
Dist X Quarter F.E	yes	yes	yes
Controls	yes	yes	yes
Excluding	**DSJ**	**CJM**	**DSJ+CJM**
Cluster Level	Judge	Judge	Judge
No. of Clusters	597	646	583

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level. Standard errors in parentheses are clustered at the judge level. The dependent variable is pretrial detention rates. Kid[0–6] is a dummy of childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home district X quarter, year of birth, and district X quarter fixed effects. We exclude District and Session Judges (DSJs) in Column 1; Chief Judicial Magistrates (CJMs) in Column 2; and both DSJs and CJMs in Column 3.

Table B.XII. OUTLIER TEST-1

SHARE DENIED	(1)	(2)	(3)	(4)	(5)	(6)	(7)
***K*id[0-6]**	0.0745***	0.0567**	0.0590**	0.0675***	0.0707***	0.0579**	0.0622***
	(0.0236)	(0.0229)	(0.0231)	(0.0247)	(0.0252)	(0.0230)	(0.0235)
Observations	5,283	5,231	5,416	5,201	5,243	5,430	5,276
R-squared	0.341	0.341	0.328	0.341	0.334	0.337	0.339
Mean Dep Var	0.37	0.37	0.37	0.37	0.37	0.37	0.37
Controls	yes	yes	yes	yes	yes	yes	yes
home-district X Quarter F.E	yes	yes	yes	yes	yes	yes	yes
Date of Birth F.E	yes	yes	yes	yes	yes	yes	yes
District X Quarter F.E	yes	yes	yes	yes	yes	yes	yes
Home Dist Removed	Aligarh	Meerut	Moradabad	Bulandshahr	Varanasi	Lucknow	Allahabad
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge	Judge
No. of Clusters	632	629	645	618	628	646	628

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges from home districts within UP. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate. Kid[0-6] is a dummy of childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home district X quarter, year of birth, and district X quarter fixed effects. From Column 1 to Column 6, from the full sample we exclude judges from one of the top six home districts *one at a time* (Aligarh(26), Meerut(24), Moradabad(18), Bulandshahr(15), Varanasi(13), Lucknow(13), and Allahabad (13)) that have the maximum number of riots during 1950–2000.

Table B.XIII. OUTLIER TEST-2

SHARE DENIED	(1)	(2)	(3)	(4)	(5)	(6)	(7)
***Kid*[0-6]**	0.0745***	0.0693***	0.0679***	0.0727***	0.0880***	0.0855***	0.0875***
	(0.0236)	(0.0240)	(0.0246)	(0.0266)	(0.0298)	(0.0304)	(0.0329)
R-squared	0.341	0.350	0.347	0.358	0.360	0.366	0.375
Observations	5,283	4,984	4,871	4,542	4,248	4,147	3,892
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge	Judge
No. of Clusters	632	601	586	544	512	498	466
Mean Dep Var	0.37	0.37	0.37	0.37	0.37	0.37	0.37
Controls	yes	yes	yes	yes	yes	yes	
Home Dist X Quarter F.E	yes	yes	yes	yes	yes	yes	yes
Birth Year F.E	yes	yes	yes	yes	yes	yes	yes
Dist X Quarter F.E	yes	yes	yes	yes	yes	yes	yes

This table reports OLS estimations based on the judge-district-quarter level sample of 668 judges coming from home-districts within UP. Standard errors in parentheses are clustered at the judge level. The dependent variable is pretrial detention rate at judge-district-quarter level in Column 1 and Column 2. The dependent variable is log of bail amount at judge-district-quarter level in Column 3. Kid[0-6] is a dummy of childhood exposure to communal conflict between the age of 0-6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home-district X quarter, year of birth and district X quarter fixed effects. From Column 1 to Column 6 we exclude judges *cumulatively* coming from one of the top six home-districts (Aligarh(26), Meerut(24), Moradabad(18), Bulandshahar(15), Varanasi(13), Lucknow(13) and Allahabad (13)) having maximum number of riots in between 1950-2000 from full sample.

Table B.XIV. OUTLIER TEST-3

Excl.	1σ	2σ	3σ	Leverage	Influence measure (dfbeta)	Influence measure (Cook's distance)
***K*id[0-6]**	0.0532***	0.0577**	0.0605***	0.0639***	0.0605***	0.0578***
	(0.0171)	(0.0225)	(0.0226)	(0.0230)	(0.0226)	(0.0210)
Observations	5,252	5,519	5,530	4,992	5,524	4,991
R-squared	0.457	0.341	0.332	0.288	0.330	0.413
Mean Dep Var	0.35	0.37	0.37	0.36	0.37	0.35
Home Dist X Quarter F.E	yes	yes	yes	yes	yes	yes
Birth Year F.E	yes	yes	yes	yes	yes	yes
Dist X Quarter F.E	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge
No. of Clusters	660	660	660	646	660	657

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges from home districts within UP. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate. Kid[0–6] is a dummy for childhood exposure to communal conflict when aged 0–6 years. All estimations include a set of binary variables coding for past exposure up to 9 years of age. All estimations include home district quarter, year of birth, and district quarter fixed effects. In Column 1, Column 2, and Column 3, we remove observations that are 3, 2, and 1 standard deviation away from the residual mean. In Column 4, we remove observations with high leverage. In Column 5, we remove observations that shift estimates to at least one standard error. In Column 6, we remove observations that shift estimates at least to 4/N.

Table B.XV. CONFLICT INTENSITY

PANEL A: CONFLICT INTENSITY SHARE DENIED	**(1)**	**(2)**	**(3)**	**(4)**
***K*id[0-6]**: Killed in Riots (High)	0.021			
	(0.031)			
***K*id[0-6]**: Killed in Riots (Low)	0.079***			
	(0.026)			
***K*id[0-6]**: Casualties in Riots (High)		-0.026		
		(0.028)		
***K*id[0-6]**: Casualties in Riots (Low)		0.096***		
		(0.026)		
***K*id[0-6]**: No of Riots (High)			0.016	
			(0.028)	
***K*id[0-6]**: No of Riots (Low)			0.076***	
			(0.024)	
***K*id[0-6]**: Duration of Riots (High)				0.018
				(0.033)
***K*id[0-6]**: Duration of Riots (Low)				0.070***
				(0.025)
Observations	5,530	5,530	5,530	5,530
R-squared	0.335	0.338	0.336	0.335
Mean Dep Var	0.37	0.37	0.37	0.37
Controls	yes	yes	yes	yes
home-district X Quarter F.E	yes	yes	yes	yes
Date of Birth F.E	yes	yes	yes	yes
District X Quarter F.E	yes	yes	yes	yes
Cluster Level	Judge	Judge	Judge	Judge
Total Number of Clusters	660	660	660	660

Notes: We report ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at the judge–district–quarter level. All estimations include home district X quarter, year of birth, and district X quarter fixed effects, and a set of binary variables coding for past exposure up to 9 years of age. The main explanatory variable (Kid[0-6]—a binary measure of childhood exposure to communal conflict when aged 0–6 years) is interacted with high and low conflict severity. The median of the variable under consideration defines the threshold for severity to split treated judges equally into two groups. The median district areas' threshold values are 4 persons killed for Column 1, 23 casualties (killed + injured) for Column 2, 1 number of riot exposed in Column 3, and 3 days of riots in Column 4.

Table B.XVI. IMPACT OF EARLY VIOLENCE EXPOSURE: SPLIT IN 3 YEARS GROUPS

SHARE DENIED	**(1)**	**(2)**	**(3)**	**(4)**
0-3 years **Pre Birth**	-0.00283	0.00440	0.00189	-0.00961
	(0.0208)	(0.0204)	(0.0210)	(0.0237)
Kid[0-3]	0.0289	0.0270	0.0281	0.0181
	(0.0242)	(0.0247)	(0.0258)	(0.0280)
Kid[3-6]	0.0881***	0.0905***	0.0858***	0.0905***
	(0.0283)	(0.0294)	(0.0293)	(0.0312)
Kid[6-9]	-0.0102	-0.00384	-0.00801	-0.0156
	(0.0229)	(0.0236)	(0.0250)	(0.0275)
Observations	5,530	5,488	5,425	5,248
R-squared	0.334	0.337	0.340	0.345
Mean Dep Var	0.37	0.37	0.37	0.37
home-district X Quarter FE	yes	yes	yes	yes
Year of Birth FE	yes	yes	yes	yes
District X Quarter FE	yes	yes	yes	yes
Controls	yes	yes	yes	yes
exp[N]	9	14	18	22
Cluster Level	Judge	Judge	Judge	Judge
Total Number of Clusters	660	651	637	604

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at judge–district–quarter level. 0–3 years Pre-Birth, Kid[0–3], Kid[3–6], and Kid[6–9] are mutually exclusive binary measures of childhood exposure to communal conflict. All estimations include home district X quarter, year of birth, and district X quarter fixed effects. Column 2 extends the past exposure control for later years: up to age 14 years (Column 2: keeping judges born before 1986), age 18 years (Column 3: keeping judges born before 1982), and age 22 years (Column 4: keeping judges born before 1978) on a subsample of judges.

Table B.XVII. AGE AT FIRST EXPOSURE

SHARE DENIED	
Age at First Exposure at 1yr	0.043
	(0.033)
Age at First Exposure at 2 yrs	0.042
	(0.034)
Age at First Exposure at 3 yrs	0.028
	(0.039)
Age at First Exposure at 4 yrs	0.081**
	(0.039)
Age at First Exposure at 5 yrs	0.118***
	(0.038)
Age at First Exposure at 6 yrs	0.090*
	(0.052)
Observations	5,530
R-squared	0.336
Mean Dep Var	0.37
home-district X Quarter FE	yes
Year of Birth FE	yes
District X Quarter FE	yes
Controls	yes
Cluster	Judge
Number of Judges	660

Notes: This table reports ordinary least squares estimation specification of equation-8. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at judge–district–quarter level. The controls in the specification are gender, religion, performance in the Bachelor of Law examination, experience, and age at first exposure at 7, 8, and 9 years.

Table B.XVIII. HETEROGENEITY BY CONFLICT INTENSITY USING ALTERNATIVE THRESHOLDS

CONFLICT INTENSITY SHARE DENIED	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
***K*id[0-6]**: Killed in Riots (High)	0.025			-0.005	-0.004
	(0.036)			*(0.038)*	*(0.037)*
***K*id[0-6]**: Killed in Riots (Low)	0.068***			0.075***	0.073***
	(0.024)			*(0.024)*	*(0.024)*
***K*id[0-6]**: Casualties in Riots (High)		-0.004			
		(0.033)			
***K*id[0-6]**: Casualties in Riots (Low)		0.077***			
		(0.026)			
***K*id[0-6]**: Duration of Riots (High)			0.003		
			(0.039)		
***K*id[0-6]**: Duration of Riots (Low)			0.066***		
			(0.024)		
Observations	5,530	5,530	5,530	5,530	5,530
R-squared	0.333	0.335	0.334	0.335	0.335
Mean Dep Var	0.37	0.37	0.37	0.37	0.37
Controls	yes	yes	yes	yes	yes
home-district X Quarter F.E	yes	yes	yes	yes	yes
Date of Birth F.E	yes	yes	yes	yes	yes
District X Quarter F.E	yes	yes	yes	yes	yes
Cluster Level	Judge	Judge	Judge	Judge	Judge
Total Number of Clusters	660	660	660	660	660
Threshold Type	p75	p75	p75	***K*id[0-6]**: Killed per sq km >.002	***K*id[0-6]**: Killed > 10

Notes: This table reports ordinary least squares estimations based on the judge–district–quarter level sample of 668 judges. Standard errors in parentheses are clustered at the judge level. The dependent variable is the pretrial detention rate at the judge–district–quarter level. All estimations include home district X quarter, year of birth, and district X quarter fixed effects and a set of binary variables coding for past exposure up to 9 years of age. The main explanatory variable (Kid[0–6]—a binary measure of childhood exposure to communal conflict when aged 0–6 years) is interacted with high and low conflict severity. The threshold in Columns 1 to 3 is defined by the 75 percentile values from the distribution of variables under consideration at the district month level. Column 4 uses the threshold of killed per sq. km greater than 0.002, which implies a median area of the district having 10 persons killed. Column 5 uses an absolute threshold of 10 persons killed in the riot.

Table B.XIX. IPC Categorization

IPC Chapters	Topic	Sections	Categories
	Of Offences Affecting Human Body		
16	Offences affecting Life	299-318	Body_crime
	Hurt	319-338	
	Wrongful restraint/Confinement	339-348	
	Criminal Force and Assault	349-358	
	Kidnapping, Abduction, Slavery, Forced Labour	359-374	
	Sexual Offences	375-377	
	Offences Against Property		
17	Theft	378-382	Property Crime
	Extortion	383-389	
	Robbery and Dacoity	390-	
	Criminal Misappropriation of property	403-404	
	Criminal Breach of Trust	405-409	
	Receiving of Stolen Property	410-414	
	Cheating	415-420	
	Fraudelent deeds and disposition of property	421-424	
	Mischief	425-440	
	Criminal Trespassing	441-462	
18	Forgery	463-489	Forgery
11	Of False Evidence and Offences against public justice	191-229	
12	Of Offences relating to Coin and Government Stamps	230-263	
22	Criminal Intimidation	503-510	Criminal Intimidation
8	Offences against Public Tranquility	141-160	Public Tranquility
15	Of offences relating to religion	295-298	
14	Of Offences Affecting Public health, safety, convenience, decency and morals	268-294	Public Health
20	Offences Relating to Marriage	493-498	Other
20A	Cruelty by Husband	498A	
5	Of Abetment	107-120	
5A	Criminal Conspiracy	120A, 120B	
6	Offences against State	121-130	
7	Offences relating to the Army, Navy and Air Force	131-140	
9	Of Offences by or relating to public servants	161-171	
10	Of Contempts of the Lawful authority of public servants	172-190	
13	Of Offences relating to Weights and measures	264-267	
19	Criminal Breach of Contracts of Service	490-492	
21	Defamation	499-502	
23	Attempts to Commit Offences	511	

Notes: This table presents the list of offences included in our crime categories based on the Indian Penal Code (IPC). The crime categories are created using the IPC chapters—in a way, following the legal classification. The cases are lodged under one or more IPC sections, which are retrieved from the judgment documents.

TABLE B.XX. Acts Categorization

Act	Section(s)	Categories
Arms Act	25	Arms and Explosives
Explosive Substances Act	5	
Cow Slaughter Act	8	Cow Slaughter
Prevention of Cruelty to Animals Act	11	
Electricity Act	135	Electricity
Gangsters Act	3	Gangs and Dacoits
UP Goonda Act	10	
UP Dacoity Affected Areas	12	
Copyright Act	63	Other
Dowry Prohibition Act	3	
Essential Commodities Act	7	
Examination Act	9/10	
Forest Conservation Act	26	
Gambling Act	3	
Immoral Traffic Prevention Act	3/5/6	
Indian Forest Act	26	
Indian Medical Council Act	15	
IT Act	66	
Mines and Minerals, Development and Regulation Act	21	
Motor Vehicle Act	207	
Narcotic Drugs and Psychotropic Substances Act	20	
Negotiable Instruments Act	138	
Petroleum Act	15	
Prevention of Atrocities Act (SC ST)	3	
Prevention of Children from Sexual Offences Act	4/8	
Prevention of Corruption Act	13	
Prevention of Damage to Public Property Act	3	
Prevention of Food Adulteration Act	16	
Railway Act	143	
Railway Property, Unlawful possession Act	3	
Representation of the People Act	136	
Trademark Act	103	
UP Excise Act	60/63	
UP Excise Act	63	

Notes: This table presents the list of offences included in the crime categories based on the special Central and State Acts. States have formed special laws from time to time to include different types of offences that are not dealt with in the Indian Penal Codebook (IPC) in detail or require special attention. If a case is filed under both IPC and some Acts, we first use IPC to create crime categories; otherwise, we use this table to create additional crime categories. The Arms and Explosives category includes offences relating to illegal carrying/selling/use of arms or explosives. Cow Slaughter includes offences relating to the slaughtering of cows and the infliction of unnecessary suffering on animals. Electricity thefts are included under the Electricity category. Gangs- and Dacoity-related Acts are passed to handle very severe crimes/criminals. Others include all the Acts that are not categorized above.

Table B.XXI. ROBUSTNESS CHECKS: CLASSIFICATION TO ALTERNATIVE ORIGIN DISTRICTS

SHARE DENIED	(1)	(2)	(3)
Kid[0-6]	0.069***	0.070***	0.072***
	(0.0237)	(0.0239)	(0.0236)
home-district X Quarter FE	yes	yes	yes
Year of Birth FE	yes	yes	yes
District X Quarter FE	yes	yes	yes
Controls	yes	yes	yes
Observations	5,531	5,537	5,539
R-squared	0.333	0.334	0.334
Mean Dep Var	0.37	0.37	0.37
Judge	660	660	660
Allocations to Parent Districts	Ghaziabad to Meerut	Firozabad to Mainpuri	Hathras to Agra

Notes: This table reports the impact of exposure to communal violence between the ages of 0 and 6 years under the alternative classification of the new districts that were carved out of the origin districts. We examine alternative assignments of the newly formed district to origin districts that were not tested in our main analysis in the main text. We find the effects of exposure to communal violence between the ages of 0 and 6 are similar across different district allocations and statistically significant at the 1% level of significance. Here we sequentially change the home districts. Column 1 assigns Ghaziabad to Meerut, Column 2 further assigns Firozabad to Mainpuri (keeping Ghaziabad with Meerut), and Column 3 assigns Hathras to Agra (maintaining the changes in Columns 1 and 2).

Appendix C. Appendix Notes

C.1. **Merging of Districts in Uttar Pradesh.** We use the information on the judges' home-district and birth year from their resumes and merge it with the information on years and districts of riot incidents from the conflict data to identify the judges' exposure to communal violence.

After independence, several changes have been made to the district boundaries in the state of Uttar Pradesh. In Appendix Table B.I, we provide a list that tracks the formation of new districts from their origin districts. We adopt the following approach to provide a consistent measure of being a treated/control judge. The first case in our study context relates to districts that are broken down into smaller districts. Then, these smaller districts that were a part of this origin district are assigned to the origin district. For example, if district D is split into D1 and D2, we treat both D1 and D2 as D. The logic is that if D1 and D2 at time "t" were treated as district D, then D1 and D2 at time "t + k" will also be treated as district D. The second case is when a new district is carved out by merging several other districts. For example, the district Hathras was formed from Aligarh, Mathura, and Agra in 1997. In this case, we have three possible ways to rename Hathras. We randomly choose one of the three origin districts. For our analysis, we assign Hathras to Aligarh. The third case is when districts are disintegrated sequentially over time. For example, a new district D1 is carved out of a district D at time "t." Then, at time "t + k" district D2 is carved out of D1. Here, we assign D1 and D2 to D. For example, the district Ghaziabad was formed from Bulandshahar and Meerut in 1976 and Hapur was created from Ghaziabad in 2011. We categorize Hapur as Ghaziabad, and then can reassign Ghaziabad in two ways: as Bulandshahar or as Meerut. In our main analysis, we assign Ghaziabad to Bulandshahar.

Further, we implement sensitivity checks by using alternative origin-district allocations and find that the results on the impact of early-childhood exposure to conflict between the ages of 0 and 6 are stable, robust, and statistically significant at the 1% level of significance across assignments to these districts, as demonstrated in Appendix Table-B.XXI.

C.2. **Creation of Variables.** The first step involves identifying the bail cases from the universe of all the downloaded cases. For every downloaded case, we use the information on whether the case is a "Bail Application" to identify whether the case is a bail case. We also extract the case number and year. Our extracted sample consists of 423,000 bail applications from the entire pool of two million downloaded cases.

In Uttar Pradesh, Hindi and English are the official languages for the district-level judiciary. However, since Hindi is predominantly spoken in the state, most of the judgments are written in the Hindi (Devanagari) script. We translate all the Hindi judgments using Google Translate (via the inbuilt libraries of Python) into English to extract relevant information. Further, some of the judgments are uploaded in a format with different encoding to avoid duplication. For those judgments, we use optical character recognition to convert Hindi judgments (pdfs) into correctly encoded texts before translating them into English. We use these translated judgments to extract our essential variables.

The written judgments can be divided into three parts. The first part contains information about the case, such as the defendant(s)'s name (often accompanied by the father's name, age, and address), the criminal section under which they have been charged, the date of the judgment, and the name of the judge who delivered the judgment. The second part details the event that led to the filing of the case. Sometimes, it also contains the legal precedents followed in arriving at the decision. The last part contains the judge's decision on the bail application, either granting or denying bail and the bail amount if granting bail. We use the first and the last part of the written judgments in extracting our variables. The main content we obtain is through text extraction and is as follows.

i) Outcome of the bail decisions: The outcome of the bail decisions is present in the last part of the judgment, and we use negative words to identify whether bail was denied[38] and positive words to identify whether bail was granted.[39]

ii) Name of the defendant(s): The name of defendants is used to identify their religion. The information is present in the first part of the judgment. We use the "Stanford Named Entity Algorithm."[40] One concern about using this algorithm is that it is not perfectly suitable for Hindi names. It may have missed some names (or some part of the names). Sometimes, it extracts the names of places (partly because, in a few cases, it is difficult to differentiate between the names of places and people. For instance, from the name Gautam Buddha Nagar district, it picked Gautam Buddha, which can be the name of a person). We filter out the names of places using the names of districts, subdistricts, and cities. The algorithm also selected the judges' names (and their variants, given that it was translated using Google Translate), which we remove carefully. Further, we screen all non-names that the algorithm selected. Even after all these checks, some scope for error remains.

After extracting the names, we use the Nilabhra name2community algorithm to identify the defendants' religion. We discuss the efficiency of the Nilabhra algorithm in a later subsection. In some cases, only nicknames are provided (e.g., Bhura and Kaalu). In such cases, we use the name/surname of the defendant's father to identify their religion.

iii) We use the text extraction method to extract information on the Acts[41] and the Indian Penal Code (IPC) sections from the judgment documents. Criminal cases are registered either

[38]That is, "not granted," "not released," "not accepted," "not acceptable," "not approved," "not freed," "denied," "unacceptable," "cancelled," "canceled," "aborted," "dismissed," "rejected," "abrogated," "abortable," "to be canceled," "terminated," "cancellation," "suspended," and "revoked."

[39]That is, "released on bail," "granted," "released," "accepted," "acceptable," "approved," "freed," "acquitted," "surrender," "personal bond," "bond," "bondage," "security bond," "bond," "suretages bond," "interim," "amount," "money," "acceptance," "sureties," and "collateral."

[40]It associates each word to four tags: person (PER), location (LOC), organization (ORG), and miscellaneous (MISC). We consider the words tagged as PER.

[41]We perform text extraction on these judgment files, searching for the word "Act" at the top of the document.

through a First Information Report or a Complaint Register by the police. The IPC is the official criminal code of India. It provides a comprehensive list of offences and associated punishments and states whether the offence is bailable or not. In addition to the IPC, special Acts passed by the central and the state government guide the categorization of crimes .Appendix Table B.XIX and B.XX provide the list of all the sections/Acts under each crime category. A case can be lodged under one or more IPC sections and/or under special Acts. For every case, we use the offence (IPC/Act section) carrying maximum punishment to categorize the case. When there is a tie, we are indifferent and randomly pick one section. We create 11 crime categories: Arms and Explosives, Body Crime, Cow Slaughter, Electricity Theft, Gangster and Dacoity, Property Crime, Forgery, Criminal Intimidation, Public Tranquility, Public Health, and Other.

C.3. **Accuracy of Algorithm in Classification of Religion.** The religion assignment algorithm is crucial since we use it to assign the religion of the defendants and the judges. We do it in two steps; first, we use the free Nilabhra algorithm to assign the religion, and then, we check all the names (along with fathers' names) manually to correct the religion. Next, we describe in detail this procedure and the accuracy of the algorithm:

i) Judges' religion assignment: We manually classify Urdu sounding names[42] of all the judges (using not only their names, but also their fathers' name) as Muslim following Bhalotra et al. 2014. To test the algorithm further, we run the algorithm on the judges' names. The error rate of the Nilabhra algorithm is 1.9% (20 out of 1,150 Hindu judges are incorrectly classified as Muslims by the algorithm, and four out of 83 Muslim judges are incorrectly classified as Hindu). This gives us confidence that the algorithm is relatively sound in predicting the religion from the names.

ii) Testing on a different dataset: We test our algorithm on a completely different dataset from Bhalotra et al. 2014, where names are categorized as Muslim and Non-Muslim. Our algorithm predicts with 6% error rate (880 out of 18,118 non-Muslim names are wrongly classified as Muslims by the algorithm, and 442 out of 3,791 Muslim names are wrongly classified as non-Muslims).

iii) Defendants' religion assignment : We use the Nilabhra algorithm first to assign a religion to the defendants using their names. In case the defendants' name is neutral, we use the fathers' name to categorize the defendant as Muslim or Hindu. We test for measurement error in the defendants' religion classification using a subsample of manually digitized entries. In the next section, we explain how we arrive at the subsample from which we retrieve information manually.

C.4. **Selection of Sample for Manual Entry.** We manually extract all the aforementioned variables from a subsample of cases to compute approximate error rates. We include all cases

[42]Most of the names have clear first names, such as Mohammad and Begum

(30,000) handled by all Muslim bail judges in our analysis sample. Muslim judges comprise around 7.6% (51 judges out of 668 judges) of the total number of judges and handle 9.43% (30,727/325,944) cases. We randomly select an equal number of cases, that is, 30,000 cases, handled by Hindu judges. To arrive at our random sample of cases handled by Hindu judges, we first randomly select Hindu judges and then randomly choose cases handled by them.

In selecting Hindu judges, our objective is to retain judges similar to Muslim judges along key covariates. First, we ensure that the Hindu judges are from the same pool of home-districts as the Muslim judges. Second, we restrict the age range of the pool of Hindu judges to the same age range (30–63 years) as that of Muslim judges. We stratify the random sample by crime types of the bail cases. Within each crime category, we select a sample of Hindu judges, such that Hindu judges' median experience is the same as that of Muslim judges.

After randomly selecting the Hindu judges based on these conditions, we randomly select the cases for every judge to ensure that the total number of cases is approximately 30,000. It should be noted that the text extraction of IPC codes and Acts yielded an error rate of approximately 22%.[43] However, the measurement error for the crime category is random, and therefore, the sample of cases handled by Hindu judges that is selected for manual digitization is random.

C.4.1. *Approximate Error Rates in Text Analysis.* The overall measurement error on bail outcomes is 5% (1559 bail cases are incorrectly classified as denied out of 38,947 bail cases granted, and similarly 1,358 cases are incorrectly classified as granted out of 19,687 bail cases denied.). The measurement error in religion assignment for the defendants is due to two reasons: the improper extraction of names from the pdfs and incorrect religion classification. To obtain an approximation of error rates, we compare the religion of the names extracted using the algorithm with the manually assigned religion. The overall error rate in the total manually extracted sample is 18% (6,771 out of 38,456 Hindu names are incorrectly classified as Muslim, and 2,158 out of 11,510 Muslim names are incorrectly classified as Hindu).

[43] This error rate was computed using the manually digitized data set.