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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 judges' exposure to communal violence during early childhood on pretrial detention rates by exploiting novel administrative data on judgments and detailed resumes of judicial officers born during 1955–1991. Our key result is that judges exposed to communal violence between ages 0 and 6 years are 16% more prone to deny bail than the average judge, with the impact being stronger for the experience of riots between ages 3 and 6 years. The observed judicial stringency is driven by childhood exposure to riots with a higher duration of state-imposed lockdowns and low riot casualties.

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), recidivism via incapacitation (Ribeiro and Ferraz, 2019), and deterrence (Dobbie et al., 2018). However, pretrial detention has been associated with an increase in the likelihood of being convicted and in the length of incarceration sentences (Stevenson, 2018), loss of formal employment (Dobbie et al., 2018), accumulation of debts (Stevenson, 2018), and nontrivial criminogenic effects (Kling, 2006; Leslie and Pope, 2017). Decisions on pretrial detention have consequences for both the defendant and society (Kleinberg et al., 2018) as such detentions typically last for several months, with low-income and minority communities bearing a significant portion of the economic costs of pretrial detentions (Dobbie and Yang, 2021; Henrichson et al., 2015). This is of particular concern since several studies under various settings have documented evidence of judicial stringency and racial disparities in bail decisions (Kleinberg et al., 2018; Arnold et al., 2018). However, little is known about the origins of such judicial biases.

In this paper, we examine the origins of judicial stringency in bail decisions. In particular, we test whether variations in judges' early childhood exposure to communal violence explain their decisions on bail. 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 may commit while awaiting trial out of jail and the incarceration costs (Kleinberg et al., 2018). Therefore, variations in judicial

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decisions could be driven by differences in either fundamental preference parameters or beliefs that define these trade-offs for judges.

In this regard, research in psychology argues that brain growth is a sequential process: the environmental stimuli during early childhood matter critically for later-life social attitudes.[1] A growing body of causal early childhood research in economics shows that early life exposure to a sociopolitical environment engenders the development of fundamental parameters, such as later-life social preferences (Cappelen et al., 2020), preferences for honesty (Abeler et al., 2021) and political identity (Billings et al., 2020), and intergroup behavior during adulthood (Couttenier et al., 2019; Fisman et al., 2020). Childhood memories, which are known to begin from the age of 3,[2] have been shown to affect prosocial behaviors during adulthood(Gino and Desai, 2012). Further, research in psychology shows that children aged 5–6 years develop perceptions toward state institutions due to both indirect or direct exposure to civilian violence (Cohrssen et al., 2022).

Guided by the psychology and economics literature on early childhood, we focus on examining the effects of early childhood (0–6 years) exposure to Hindu-Muslim riots, controlling for exposure in later years, on bail decisions.[3] Hindu-Muslim conflicts are a uniquely dominant form of community violence across India that has a deep historical legacy that predates the massacre of the 1947 partition that led to the formation of the Indian republic. Since India is a collectivist society where parents rely on the wider community to help in child-rearing,[4] violence that disrupts trust and causes fear within communities could potentially have a persistent impact on children. Following Fisman et al., 2020, we evaluate Hindu-Muslim riots that are salient enough to have had national media coverage claiming 6,565 lives, injuring 21,429 people, and resulting in 87,903 arrests between 1950 and 2000, with an average of five days of lockdown per riot. Our conflict dataset includes data from Mitra and Ray, 2014 for the period 1950–2000[5] and a novel dataset on lockdowns during riots.

Our study setting is the Indian judicial system, which has one of the highest shares of pretrial detainees in the world: 70% of the total prisoners in India are under trial, compared with 23% in the United States, 33% in France, and 62% in Pakistan. In particular, we analyze the decisions on pretrial detentions of 668 judges born between 1955 and 1991, who handled 323,380 bail cases from 2014 to 2018 in Uttar Pradesh (UP), the largest Indian state with a population of 199.81 million (Census of India 2011). According to Prison Statistics of India, the percentage of pretrial detainees in UP increased from 70.6% in 2015 to 72.5% in 2019, in line with the national trend.[6] 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 the 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. We link conflict and bail data with detailed data on judiciary officers[7] 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.

We adopt the empirical approach used in the recent seminal empirical studies on the impact of early exposure to violence that has research settings identical to ours: the work of Couttenier et al., 2019, who estimate the causal impact of early childhood exposure to conflict on asylum seekers' criminal behavior, and Fisman et al., 2020, who provide causal estimates of the exposure to Hindu-Muslim riots in India on bank managers' lending decisions. Following the literature mentioned above, we identify the effect of riot exposure using two key variations. The first variation is based on the variations in the early childhood riot exposure of judges who were born in the same home districts but belong to different birth cohorts as well as on cross home district variations in early childhood riot exposure within the same birth cohorts. The second variation relates to the exogenous rotation policy of judiciary officers—some judges with riot exposure and others without—during adulthood. The second variation experienced by judges during adulthood allows us to distinguish the effect of riot exposure from the location attributes of the district assigned to the judges as well as to control for unobserved time-varying differences in the districts.

One concern about our empirical strategy could be about systematic relocation decisions by families owing 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 a violation of the Stable Unit Treatment Value Assumption (SUTVA) (Rubin, 1980). Using detailed migration data of the judges and micro data on employment, we show that possible violation of the SUTVA is less likely in our setting. Next, our identification strategy assumes that the rotation policy of the judicial officers and exogenous assignment of cases are implemented on average to rule out possible sorting of judges into districts and cases. We implement several empirical strategies and show there is no evidence of differential selection along a wide range of case characteristics, peer characteristics, and judge characteristics.

There could be additional concerns about current factors directly affecting the judges' decisions. Current trends in judges' home districts could directly affect their decisions on bail. We therefore include home-district-quarter trends to filter out current trends in their home districts affecting judicial decisions from those affected through early riot exposure. Further, there could be time-varying covariates correlated with riot exposure that could drive our effects. We demonstrate that early childhood exposure to riots does not capture trending factors that could potentially be correlated with riots.

We find that exposure to communal violence when aged 0–6 years causes an increase of 6 percentage points ($p < 0.01$) in the share of pretrial detentions, which is an increase of 16% compared with the mean. The effect is robust to using various estimation techniques; including and excluding controls such as judge-level covariates (e.g., gender, religion, performance in the Bachelor of Law or LLB examination, and on-the-job experience), using placebo checks, and removing outliers. More importantly, we control for the time-varying covariates at the home-district level comprising the share of the Muslim population, the share of the urban population, the share of Muslims in the urban population, and the log of the total population, which are shown to be predictors of occurrences of Hindu-Muslim ethnic violence (Corbridge et al., 2012). 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 we do not find evidence in this regard.

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

[1] See, for example, Tierney and Nelson III, 2009; Sale et al., 2009; and Brito and Noble, 2014.

[2] See, for example, Fehr et al., 2008; Malmendier and Nagel, 2011.

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

[4] Sinha et al., 2001

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

[6] In India, the percentage of pretrial prisoners has increased from 67.6% in 2014 to 69.1% in 2019. The prison statistics of India report is published by the National Crime Records Bureau.

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

as the division[8] obtained in the 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 for social and political preferences,[9] we explore the behavioral explanations of the early childhood exposure effect. We find that high-intensity state interventions (measured by lockdowns or arrests) associated with limiting the riot casualties explain the higher pretrial detention rates. This finding suggests that early childhood exposure to state-imposed lockdown measures that proved effective in containing violence possibly generated higher support for state institutions in law-and-order matters.

Further, we do not find 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 find that early interventions between ages 3 and 4 years have lasting effects on social preferences, we find that exposure to violence between ages 3 and 6 years, robust to multiple hypothesis testing, is the key driver of the observed judicial biases toward pretrial detentions. A key interpretation aspect is that it is difficult to assess whether exposure to conflict in early childhood directly affects judges' preferences or is associated with changes in the selection of who ultimately becomes a judge. We address this concern and find that our results are less likely to be explained by the possibility of selection into the judiciary due to childhood exposure to communal violence.

This paper makes contributions to several strands of literature. It's 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 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 et al., 2011; Giuliano and Spilimbergo, 2014; Cappelen et al., 2020; and Billings et al., 2020, more broadly, and on the impact of early childhood exposure to violence on inter-group behavior, in particular Couttenier et al., 2019 and Fisman et al., 2020. We contribute to this literature by examining the effects of bureaucrats' early childhood exposure to violence on their public service decisions. Our study also adds to the robust empirical evidence on the influence of early childhood interventions on various long-term outcomes, such as cognitive skills (Heckman, 2006; Bleakley, 2007; Almond et al., 2009; Maccini and Yang, 2009; Aizer and Cunha, 2012; Bharadwaj et al., 2013), health outcomes (Currie, 2009; Maccini and Yang, 2009; Almond and Currie, 2011; Currie and Vogl, 2013; Adhvaryu et al., 2019), and labor market outcomes (Almond, 2006; Bleakley, 2010; Gould et al., 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 detention 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 the formative years in generating noncognitive outcomes (Heckman, 2007; Cunha et al., 2010; Heckman et al., 2013), such as motivation, dependability (Heckman, 2006), and distributive preferences (Cappelen et al., 2020), which have economic consequences independent of cognitive achievements (Heckman and Rubinstein, 2001; Heckman et al., 2006).

Studies on the 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 countries. A large body of literature focuses on the criminal justice system in the United States (Dobbie and Yang, 2021; Dobbie et al., 2018; Kleinberg et al., 2018; Kling, 2006; Stevenson, 2018; Agan and Starr, 2018; Arnold et al., 2020; Doleac, 2021). 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).

Last, this paper also contributes to a growing body of evidence on extraneous factors in judicial decision-making such as judicial distortions due to media (Lim, 2015), presidential selection (Mehmood, 2021), religion (Shayo and Zussman, 2011, gift-giving (Bakhtawar and Mehmood, 2022), rituals (Mehmood et al., 2021), and race (Bielen 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 rest of the paper is organized as follows. Section 2 presents the conceptual framework, and Section 3 details the institutions. Section 4 presents the data used, Section 5 explains the empirical strategy, and Section 6 presents the main results. Section 7 explores threats to identification, and Section 8 examines whether the effect is driven by the sorting effect. Section 9 presents a series of robustness tests. Section 10 explores potential mechanisms underlying riot effects, and Section 11concludes.

2. Conceptual framework

The central principle of life course theory is that the impact that events have on life trajectories depend crucially on timing within the life cycle (Elder and Glen, 1998). Brain development is shown to be a sequential process where the foundations of systems crucial to later-life social perceptions and behavior are formed in the early years and are determined by experiences during this time (Tierney and Nelson III, 2009; Sale et al., 2009; Brito and Noble, 2014). In line with the literature in psychology, economics literature on endogenous preference formation emphasizes early childhood as a period in which fundamental preference parameters and character skills develop.[10].

Within the critical period research, studies in psychology have investigated how childhood (direct and indirect) exposure to communal violence has significant effects on the developing brain (Berens et al., 2017; Benjet et al., 2020). In line with the literature in psychology, causal studies in economics provide evidence of the persistent effects of early life exposure to a sociopolitical environment on socio-political outlooks[11]

We focus on the impact of Hindu-Muslim communal violence during early childhood. The conflict between Hindus and Muslims,

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

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

[10] Kautz et al., 2014; Alan et al., 2019; Falk et al., 2021; Kohlberg, 1984; Piaget, 1997; Harbaugh et al., 2002; Sutter and Kocher, 2007; Fehr et al., 2008; Ben-Ner et al., 2017; Almås et al., 2010; Bauer et al., 2014

[11] Billings et al., 2020; Abeler et al., 2021; Cappelen et al., 2020; Couttenier et al., 2019; Fisman et al., 2020

two historically, socially, and politically influential communities, have deep historical roots in India that predates the massacre of the 1947 partition. Due to the socially disruptive nature of the violence, the administrative response is also drastic and unlike the response to any other forms of crimes such as murders, robberies, rapes, and arson. Apart from mass arrests, the state responds to Hindu-Muslim violence by imposing district-wide curfews/lockdowns, closing schools and businesses, and deploying paramilitary forces and riot police, at times for days (e.g., in our sample, on average, five days of lockdowns per riot is imposed). The lockdowns and the presence of riot police or the army are widely publicly observable and goes beyond the localities where the riot was first triggered. The response (and non-response) of the administration to mob attacks of Hindu-Muslim riots have immediate consequences in terms of community-wide mass displacements and losses in livelihoods that directly impact even those individuals who did not personally experience the violent attacks.

Core community violence has a crucial role in child-rearing in the Indian context. India is a collectivist society where mothers or parents do not rear children alone; rather they are connected to wider communities that help in child-rearing tasks (Kim et al., 1994; Sinha et al., 2001). Anecdotally, one unique aspect of Hindu-Muslim violence is that it disrupts social ties within a well-functioning community. For instance, in the case of the Muzaffarnagar (in UP) riots in 2013, it was reported that government officials had to evacuate Muslims from 147 villages where Muslims and Hindus lived peacefully for decades.[12] In the latest Delhi riots (2020), *Guardian* reports[13] that "in streets where Hindus and Muslims had lived peacefully side by side, bodies lay bloodied alongside discarded and burned-out cars, bikes, shattered glass and smouldering shopfronts." Distinct from crimes like homicides, arson, rapes, burglaries, or financial theft, the riots cause lasting social segregation, mistrust, and fear within social networks that shared neighborhoods (Mitra and Ray, 2014; Field et al., 2008).

In this paper, we ask whether early childhood exposure to Hindu-Muslim riots explains judicial stringency in bail decisions. Bail decisions are primarily driven by considerations of how much trust a judge has in individuals versus state institutions. Early childhood experience of civilian conflict could generate biases in favor of state institutions, preference for strong law and order, or even specific demographic groups. Research has shown that children in their early years who directly or indirectly experience civilian violence form their own perceptions about institutions. For instance, Cohrssen et al., 2022 show that preschool children aged from 5 to 6 years attending kindergartens in areas both directly and less directly impacted by the Hong Kong protests (June 2019 to February 2020) became critical of police conduct and saw protesters as needing protection from the police.

There are many other potential channels by which early childhood adversity could impact later-life attitudes. Studies in psychology suggest that childhood exposure to conflict psychologically scars individuals, increasing their risk for depression and anxiety, among other mental health disorders (Summerfield, 2000; Chapman et al., 2004). One study finds that even indirect childhood exposure to conflict, such as the occurrence of two or more beatings within 1 km, is associated with later-life mental health disorders (Benjet et al., 2020). Therefore, observed judicial decisions may reflect anxiety disorders experienced by early riot-exposed judges. Alternatively, early riot exposure or in utero conditions can affect cognitive outcomes, resulting in a bias in bail decisions.

Another possibility of the social environment affecting children is through parental influence. Parental traits can shape the 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 also possible that parents experiencing effective state intervention in civil clashes develop positive attitude toward state institutions. The children who observe their parents' confidence in the institutions and the functioning of the state may develop greater support for such institutions. In this regard, age of exposure is a critical determinant of later-life world perceptions. Memory formation in humans begins largely at age 3 onward, shaping long-term beliefs and attitudes (Fehr et al., 2008; Malmendier and Nagel, 2011). Childhood memories have been shown to affect later-life prosocial behaviors and punishment of ethically questionable behavior of others (Gino and Desai, 2012). In this paper, we will not be able to disentangle all possible channels by which childhood experiences (direct or indirect) of civilian riots affect judicial decisions. However, we will provide suggestive evidence of some of the potential channels.

3. Institutions

This section provides an overview of the institutional features of our study's setting, highlighting the factors that allow estimating the causal effects of exposure to violence on bail decisions. The fundamental right enumerated in Article 21 of the Constitution of India is the following: "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.[14] A judge has to consider several factors—like the severity of the offence, character of the accused, and danger of influencing witnesses in case of granting bail, which makes granting or denying the bail a subjective decision. Bail decisions are an ideal outcome to detect bias not only due to the discretion involved but also because they are undertaken with limited information and almost no interaction with the defendants.[15]

Our study's setting is the largest Indian state, UP, which has a population of about 199.81 million as per Census 2011, and a religious composition that is similar to that of India as a whole.[16] We evaluate bail decisions at the district-level courts, the lowest tier of courts in the Indian judiciary system.[17] 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). The total number of judges per million population is 9.1, and their average age is 43.84 years. UP has 22.6% female judges, and 6.9% of its judges are Muslim.

UP judiciary follows an explicit geographical rotation policy for its judges with the stated objectives of reducing corruption and collusion in the judiciary, which induces exogenous spatial variations in the distribution of judges across districts. According to the policy, a judge cannot be posted in their hometown district and will be transferred after completing a tenure of three years in any given district.[18] We find that, on average, a judge spends

[12] https://www.reuters.com/article/muzaffarnagar-riots-muslims-displaced-fe-idINDEE98I0CA20130919

[13] https://www.theguardian.com/world/2020/mar/01/india-delhi-after-hindu-mob-riot-religious-hatred-nationalists

[14] Bail 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 Criminal Procedure Code (Cr.P.C.) details the bail process by the type of offence.

[15] See Appendix Section C.1.1 for details on bail jurisprudence.

[16] 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.

[17] Districts are the smallest administrative division in India to which law and order authority are delegated. 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.

[18] The guidelines for transferring officers appear in circulars issued by the Registrar General of the High Court of Judicature at Allahabad. The tenure is two years at an outlying court (far from district headquarters) or at the Sonbhadra district. See Appendix C.1.2 for details on the rotation policy of the judges.

2.3 years in any assigned district. Fig. A.I reveals that the majority of the district judge transfers occurs the second quarter and the pattern is similar for the sample of bail judges. Further, an average 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.

4. Data

4.1. Data on judiciary officers

We extract information on working and retired judges from the Allahabad High Court website.[19] 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 judges' home districts and date of birth and match them 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,[20] 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.2 provides complete information on district harmonization.

4.2. Data on defendants and cases registered

We web-scraped all the case-level PDFs from the district e-court website by court establishment[21] in August 2018. We segregated about 423,000 bail cases[22] from the entire pool of two million downloaded cases. The primary details we 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),[23] and the criminal section codes under which a case is registered.[24] We created 11 crime categories from these criminal section codes, mostly following the chapters of the IPC codebook.[25] Appendix C.3 provides detailed information on the procedure we adopted.[26]

4.3. Hindu-Muslim communal riots: 1950–2000

The data on Hindu-Muslim riots are from two sources: the datasets of Varshney and 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.[27] 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 Wilkinson, 2006. Appendix Table B.II shows that 31.4% of the judges have been exposed to violence when aged 0–6 years.[28]

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.[29] Within the districts experiencing at least one riot, the mean of the number of riots per year across districts is 0.12 with a standard deviation of 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 state response variables, the average duration of state-imposed lockdowns following a riot was 5 days both in UP and in India as a whole, and the average number of arrests was 144 in 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.

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 et al., 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).[30] We also 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 Table 1 show that the analysis sample is similar to the total sample of judges, along observables in the data.

In Appendix Table B.II, we present the unconditional differences between the riot-exposed and non-exposed judges. We find that the judges exposed to communal violence between the ages of 0 and 6 years are similar to judges without such exposure along most covariates. However, we find that the judges exposed to communal clashes in early childhood are more likely to be female, younger, and less experienced. Our identification strategy does not rely on the unconditional differences between the early riot exposed (trea-

[19] See http://www.allahabadhighcourt.in/District/Officer/judge_id.html, for the active judges, where the judges' ID is their unique identification. A few judges who judged bail cases during 2014–2018 retired during this period. Since the information on retired judges is removed from the Allahabad High Court website, we extract data from the archived web page.

[20] 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.

[21] See 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.

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

[23] The accuracy of the algorithm is in the range of 5%–6%. Details are provided in Appendix C.4

[24] The criminal section codes pertain to either the Indian Penal Code (IPC): the comprehensive list of offenses and associated punishments or special laws (the acts to augment the IPC).

[25] These are arms and explosives, body crime, cow slaughter, electricity theft, gangster and dacoity, property crime, forgery, criminal intimidation, public tranquility, public health, and other.

[26] 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 randomly chosen cases handled by Hindu judges—and show the error rates for each variable extracted (see Appendix C.5 for the selection of the cases). The measurement error is 5% in the bail outcome, which is the main outcome variable, and we show that it is not correlated to our main explanatory variable (see Appendix C.6 for details).

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

[28] 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%.

[29] Since the treatment in our case starts from 1950, we use the districts that were present in 1950 by merging the districts as detailed in Appendix Table C.2. Currently, there are 70 districts in UP.

[30] Our results are robust to changing the threshold from the bottom 1 to the bottom 10 percentile; we provide the results of the robustness checks in Section 9. The choice of the bottom 5 percentile as the threshold, that is, judges handling a minimum of 97 cases in four years, is to maintain a balance between not dropping too many judges and not keeping too many judges who have handled very few cases.

Table 1
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 at after removing judges handling less than 97 cases and judges with no information on the division obtained in the 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 ($\geqslant 60\%$ marks), II ($\geqslant 45\%$ and $< 60\%$), and III ($\geqslant 33\%$ and $< 45\%$), Grade 12 Division (3 divisions: I ($\geqslant 60\%$ marks), II ($\geqslant 45\%$ and $< 60\%$), and III ($\geqslant 3\%$ and $< 45\%$), LLB Division (I ($\geqslant 60\%$ marks), II ($\geqslant 45\%$ and $< 60\%$), and III ($\geqslant 33\%$ 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 examination earlier; currently, some institutions offer integrated courses such as BA + LLB.

ted) and non-exposed (control) judges. Below, we explain our empirical strategy and the identifying assumptions.

5. Empirical strategy

To identify the early riot exposure effect on bail decisions, ideally we require exogenous variation across two levels. First, we require exogenous variation in the assignment of judges to riots between 0–6 years of age in order to rule out the selection of judges into conflict. Second, we require exogenous assignment of bail cases to judges during their adulthood in order to rule out the sorting of judges into cases. We combine two plausibly exogenous variations to achieve these two levels of variations.

Following recent empirical studies estimating the causal impact of early childhood exposure to violence,[31] we identify the effect of riot exposure based on two variations. Our first source 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. Second, we exploit the variation generated by the exogenous rotation policy of judiciary officers during adulthood to rule out the selection of judges into specific types of bail cases.

5.1. Specification

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 equals one if the home district of the judiciary officer experienced Hindu-Muslim communal clashes when the officer was 0–6 years old.

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.[32] Our results are robust to case-level regression. Section 9, the robustness checks section, explains why we have judge-level regressions rather than case-level regressions as our preferred specification. Guided by our data depicted in Appendix Fig. A.III, 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 baseline econometric specification is as follows:

$$B_{jhbdt} = \alpha + \eta_{dt} + \delta_h + \mathcal{FE}_b + \beta \times kid[0-6]_j + \sigma X_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + \epsilon_{jhbdt}, \quad (1)$$

where B_{jhbdt} is the share of bail denied by judge j born in home district h belonging to birth cohort b assigned to district d at quarter t. The covariate in the regression, $kid[0-6]_j$, is the binary variable of exposure to riots when aged 0–6 years, and the associated β, our coefficient of interest, denotes the difference in pretrial detention rates between exposed and non-exposed judges. $\delta_h, \mathcal{FE}_b$, and η_{dt} are home district, birth-year cohort, and district-quarter fixed effects, respectively. The variable $exposure(k)_j$ is the exposure to violence at the kth year of a judge j. α is an intercept. X_j are judge-level controls, such as gender, religion, division obtained in LLB examinations, and on-the-job experience. Standard errors are clustered at the judge level to account for within-judge correlations in bail decisions over time and across the assigned districts.

Our identification strategy has three key identifying assumptions. First, conditional on home district fixed effects and birth

[31] Couttenier et al., 2019; Fisman et al., 2020

[32] 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 et al., 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 et al., 2004), we cluster our standard errors at the judge level.

cohort fixed effects, the assignment of judges into Hindu-Muslim riots between 0–6 years of age is as good as random. Riots may be triggered in certain years when certain political parties are in power. Thus, judges born in riot-years may pick up not only riot exposure effects but also the effects of unmeasured socio-political preconditions correlated with religious riots during that birth year. Birth cohort fixed effects, $\mathcal{FE}_b$, control for unobserved differences by birth year. Home districts of the riot-exposed may be culturally different from that of the non-exposed judges. Therefore, we control for time-invariant home-district-specific factors through δ_h. In our preferred specification, we augment our baseline specification to include home-district-quarter fixed effects to make a less restrictive identification assumption. This augmented specification filters out the current trends in home districts that could directly affect judges' preferences from those affected through early childhood exposure. Additionally, our focus on the exposure of judges to violence when aged 0–6 years alleviates the endogeneity concern of self-selection into conflict.

Our second identifying assumption is that there is no systematic migration of households from riot-hit districts to districts that are less likely to be affected by communal violence. This is the SUTVA (Rubin, 1980) assumption for identification. In Section 7, using both the National Sample Survey Organization's (NSSO) microdata from the Employment and Unemployment Survey 1983, and administrative data on schooling districts of the judges in our sample, we show that possible violation of the SUTVA is less likely in our setting.

Our third identifying assumption is that the judge rotation policy exogenously assigns the judges into districts. This allows us to distinguish the effect of early childhood riot exposure from location attributes of the districts assigned to the judges and to control for time-varying unobservables affecting bail outcomes, such as variation in crimes registered in districts. 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 assigned to judges could also vary by types of crime committed due to seasonal weather shocks (Blakeslee and Fishman, 2018). If non-exposed judges could systematically select into less crime-prone districts, they would likely get bail cases that are petty crimes and more likely grant bail. Therefore, the observed differences in the share of bail denied between early riot-exposed and non-exposed judges would be confounded by the differences in the types of cases assigned to the judges.

Moreover, although we establish in Section 7 that judge rotation policy yields the randomization of judges into districts and cases on average, spatial and time-varying differences in the detection and registration of crimes in districts assigned to judges are additionally accounted for through district-quarter fixed effects $\eta_{d,t}$. In our setting, the policy-induced exogenous rotation of judicial officers generates substantial heterogeneity across birth cohorts in their early childhood exposure in all home districts such that the $kid[0-6]_j$ and $exposure(k)_j$ dummies are not collinear with the home district fixed effects, which allows us to separate unobserved confounders that vary at the home district level.

We control for later years' exposure to riots up to the first 9 years after birth in our preferred specification. We could not control for exposure beyond age 9 in the full sample of judges due to data limitation. The riot information is available until 2000, and in our sample, the youngest judge is born on October 7, 1991.[33] However, as a robustness check, we test for early life exposure to violence by controlling for exposure to violence for up to age 22 for the subsample for which we can control for exposure to violence in later years. We implement additional robustness checks by augmenting our preferred specification by including a dummy variable that equals one if there was a riot one to five years before a judge was born to control for the direct effects of pre-birth exposure to conflict. Additionally, we use alternative specifications with birth-year-quarter fixed effects to account for unobserved current trends that affect judges born in certain birth cohorts. We also implement specifications where we include crime fixed effects.

We observe unconditional differences between early riot-exposed and non-exposed judges along some characteristics dimensions of gender, division achieved in LLB exams, on-the-job experience, and religion (Appendix Table B.II). Conditioning on home district and birth cohort fixed effects, we find that riot-exposed and non-exposed judges have no statistically significant difference along many dimensions except religion and division achieved in LLB exams (Appendix Table B.III). It is reassuring to find that our early riot exposure coefficient estimates remain stable after controlling for religion, academic achievement in LLB exam, gender, and on-the-job experience, implying that the dimensions that vary unconditionally and conditionally across treated and control judges are unlikely to confound our treatment effect estimates in our fixed effects model.

Last, the early exposure effect estimates may be confounded by time-varying home district characteristics. We address this concern by controlling for the time-varying share of the Muslim population, share of the urban population, share of Muslims in the urban population, and log of the total population, which are shown to be predictors of the occurrences of Hindu-Muslim ethnic violence (Corbridge et al., 2012).[34]

6. Main impact of exposure to communal violence

In this section we present our main results. Panel A of Table 2 shows the main results. The coefficient of interest, β, controls for exposure to violence in later years. This coefficient demonstrates the causal effect of exposure to communal violence when 0–6 years old on the 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 baseline regression Eq. 1 without controls for judge-level characteristics (X_j). The treatment effects of exposure to riots 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 5.6 percentage points higher, which is an increase of 15% ($= 0.0566/0.37$) 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 5.46 percentage point increase in pretrial detention rates, which is an increase of 15% ($= 0.0546/0.37$) compared with the baseline mean, which is statistically significant at the 5% level of significance.

Column (3) presents our preferred specification, where we account for home-district-quarter fixed effects. The treatment effect of early exposure to riots is a 16% increase compared with the baseline mean. In Column (4), we add controls for the occurrence of communal riots five years before birth. This specification controls for household effects for sorting into potential riot-affected districts and also captures in utero effects. The coefficient

[33] 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.

[34] These variables are at the home district year level, which come from the decennial census data for 1961, 1971, 1981, 1991, and 2001. The yearly level values are interpolated from these values.

Table 2
Bail decisions and early riot exposure.

PANEL A: FULL SAMPLE Dependent Variable: Share Denied	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
1–5 Yrs ***Pre Birth***				0.008			0.003
				(0.020)			*(0.020)*
***K*id[0–6]**	0.0566***	0.0542**	0.061***	0.062***	0.060**	0.054***	0.062**
	(0.0214)	*(0.0214)*	*(0.023)*	*(0.023)*	*(0.024)*	*(0.017)*	*(0.025)*
Observations	5,708	5,708	5,530	5,530	5,443	29,925	5,530
R-squared	0.229	0.233	0.332	0.332	0.391	0.235	0.335
Mean Dep Var	0.37	0.37	0.37	0.37	0.37	0.32	0.37
Controls	no	yes	yes	yes	yes	yes	yes
Home District	yes	yes	no	no	no	no	no
Home District X Quarter FE	no	no	yes	yes	yes	yes	yes
Date of Birth FE	yes	yes	yes	yes	no	yes	yes
District X Quarter FE	yes	yes	yes	yes	yes	yes	yes
Date of Birth X Quarter FE	no	no	no	no	yes	no	no
Crime Type FE	no	no	no	no	no	yes	no
Cluster Level	Judge	Judge	Judge	Judge	Judge	Judge	Judge
Total Number of Clusters	667	667	660	660	657	668	660
Standard Errors of *K*id[0–6]							
Moulton-corrected	0.0209	0.0209	0.0220	0.0225	0.0236	0.0160	0.0235
Wild Bootstrap Errors	0.02	0.01	0.0232	0.0237	0.0253	0.0173	0.0170
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 FE	yes	yes	yes				
Date of Birth FE	yes	yes	yes				
District X Quarter FE	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 baseline specification without controls; Column (2) includes controls such as a dummy for Muslim, female, first division in the Bachelor of Law examination, and the total tenure as a judge at the time of judgment. Column (3) is the preferred specification replacing the home district fixed effects with the home district interacted with current quarter fixed effects. Column (4) further adds a binary measure of pre-birth exposure of judges' families to communal conflict. Column (7) further adds extra controls: share of urban population, share of the Muslim population, share of Muslims in urban areas, and log of the total population. Column (5) adds cohort-quarter fixed effects and Column (6) aggregates the data at the judge-district-crime-quarter level and adds crime-fixed effects in the regression. 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.

on pre-birth exposure is statistically insignificant with very small magnitude. Our coefficient of interest remains almost unaffected, with a 17% (= 0.062/0.37) increase in detention rates. The effects are statistically significant at the 1% level of significance.

In Column (5), 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 will show later 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 (6), where we aggregate the data at the judge-district-quarter-crime type 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% (= 0.054/0.32), which is very similar to the estimates from Column (4) and our preferred specification in Column (3). Finally, in Column (7), we add potential predictors of riots as controls—share of the urban population, share of the Muslim population, share of Muslims in urban areas, and log of the total population at the home-district-year level—and our result remains unchanged.

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.[35] In light of this evidence, we estimate our main regression with home-district-quarter effects 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), we control for exposure until age 18 years; and in Column (3), we control for exposure until age 22 years. Adding controls for exposure reduces our sample size from Columns (1) to (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. Further, in Appendix Table B.IV, we show that our results are robust to clustering at the home-district-

[35] Appendix Table B.V shows that exposure to riots between the age of 0–9, 0–10, or 0–12 do not cause statistically significant effects on the share of bail denied. The coefficient is positive, consistent with the effect of riot exposure between the age of 0–6. However, the riot exposure coefficients are imprecise.

year-of-birth and home district level. The coefficients are stable across specifications.

7. Threats to identification

In this section we explain several empirical strategies that we implement to test our key identifying assumptions that we stated in Section-5.

7.1. Time-varying covariates correlated with riot exposure

One concern related to our fixed effects approach is that it does not account for time-varying covariates that could be correlated with riot exposure. In Appendix A.IV-A.VIII, we present a comparison of pre-trends in variables such as the share of the urban population, the share of literate people, the share of the Muslim population, and the share of Scheduled Castes (SC), which could be correlated with riots. We find that exposed and non-exposed judges experienced similar trends along these variables for at least two years before the riots and up to seven years after the riots. Since these covariates could potentially measure political factors (Wilkinson, 2006), the absence of pre-trends partly removes concerns about the effect being driven due to the political environment rather than to riots.[36]

Additionally, we implement another empirical strategy to examine whether the early riot-exposure effect measures political factors unrelated to riots. We use the reported cause of the riot data to define politically motivated riots. If the reported cause had the words "political," "election," "allegations of unpatriotic acts," "legal," "protest against police action," "leader," "chief," "terrorist," "Hindu nationalist," or "police firing," we group it as a politically motivated riot. This amounts to only 15% of the riots being political in our sample. We show in Appendix Table B.VI that our coefficient estimates remain unchanged if we exclude politically triggered riots. Further, our specification with home district current trends picks up any confounding effect of current trends, including political events, specific to each home district.

7.2. Tests for exogenous assignment of judges in districts

In this section, we test whether the exogenous rotation policy of judiciary officers resulted in selection along observables. One likely concern is that riot-exposed judges select into cases involving specific types of crimes. We check for differences between riots-exposed and non-exposed judges across a host of case attributes using two empirical strategies. Our first empirical specification is as follows:

$$Y_{jbhdt} = \alpha + \eta_{dt} + \delta_{ht} + \mathcal{FE}_b + \beta \times kid[0-6]_j + \sigma X_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + \epsilon_{jbhdt}, \quad (2)$$

where Y_{jbhdt} are the characteristics of cases at the level of judge j from birth cohort b born in home district h assigned to district d at quarter t. The set of covariates $kid[0-6]_j, exposure(k)_j, \alpha$, and δ_{ht} are the same as those in the preferred econometric specification with home-district-quarter trends.

Table B.VII shows there are no treatment effects on case characteristics; conditional on fixed effects, there is no selection of riot-exposed judges into cases. Column (1) shows no statistical difference in the total number of cases assigned to the early childhood riot-exposed judges and non-exposed judges. Columns 2–10 show no treatment effects along the following case characteristics: share of cases with Muslim defendants, share of non-bailable cases, share of cases booked under special acts, share of cases booked under one IPC section, share of cases booked under two IPC sections, share of cases booked under three IPC sections, share of cases booked under four IPC sections, share of cases booked under five IPC sections, and share of cases booked under six or more IPC sections. All the coefficients are small and insignificant, showing that exposed and non-exposed judges handle similar type of cases.

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.3.iii for details). We show in Appendix Table B.VIII 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.IX) and find no evidence of the selection of judges into crime types.

A more convincing way is to show that there are no pre-trends in the case composition corresponding to the timing of judge rotation. Our second empirical specification is as follows:

$$Y_{jbhdt} = \alpha + \eta_d + \delta_h + \mathcal{FE}_b + \sum_{k=-12}^{12} \beta_k \times kid[0-6]_j \times months_k + \sigma X_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + \epsilon_{jbhdt}, \quad (3)$$

where Y_{jbhdt} are the characteristics of cases at the level of judge j from home district h of birth cohort b assigned to district d in month t, where the month is relative to the transfer date of the judge in the district. The set of covariates $kid[0-6]_j, exposure(k)_j, \alpha$, and δ_{ht} are the same as those in the preferred econometric specification, which is an augmented version of the baseline specification in Eq. 1. Though we are adding 12 months before and after the event of judge rotation, the table reports the coefficients only for 6 months before and after. Appendix Tables B.X and B.XI present the results. We find no pre-trends around the transfer date of a judge in terms of case characteristics.

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.[37] We test 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:

$$J_{hbqd} = \sum_{d=1}^{75} \beta_{dq} \times \mathcal{I}_{hbqd} + \epsilon_b, \quad (4)$$

where J_{hbqd} are the judge-level characteristics of judges from home district h and birth cohort b at quarter q in district d. β_{dq} are the district-quarter-specific coefficients corresponding to the indicator dummy for judges, denoted as $\mathcal{I}_{hbqd}$, which equals one if the judge from the home district h and birth cohort b is allocated to district

[36] Since data on political variables are available at the constituency level and not at the level of our analysis, we cannot account for political factors in our specifications directly.

[37] 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.

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 $\hat{\beta_{dq}} = \hat{\mu_h}$ 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 $\hat{\beta_{dq}}$ should not differ from the home district average and the F-test should not be rejected for this district-quarter.

Suppose there is no selection in the spatial allocation of the judges. In that case, the observable judge characteristics in some districts with respect to the home district average should not be over- or under-represented. Each row in Appendix Table B.XII 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 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.

Eren and Mocan, 2020 show that peers of judges influence judicial decisions. To test whether the peer groups assigned to judges differ by early childhood exposure to riots, we run a leave-one-out regression (Columns 1–6 in Appendix Table B.XIII present the results). We estimate the following equation (our preferred specification without district-quarter fixed effects, with covariates at the level of district-quarter assigned to judges as the outcome variables):

$$Y_{jbhdt} = \alpha + \delta_{ht} + \mathcal{FE}_b + \beta \times kid[0-6]_j + \sigma X_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + \epsilon_{jbhdt}, \quad (5)$$

where Y_{jbhdt} are the peer characteristics assigned to judge j from birth cohort b born in home district h assigned to district d at quarter t. The set of covariates $kid[0-6]_j, exposure(k)_j$, and α are the same as those in the baseline econometric specification in Eq. 1, and δ_{ht} is the home-district-quarter fixed effect. We find 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 non-exposed judges. Female judges are correlated with high pretrial detention rates. Therefore, the actual treatment effect of early riot exposure could be higher than our estimated value if female peers influence judicial decisions. We address this to an extent by accounting for the differences in peer attributes with district-quarter fixed effects in our empirical specification measuring the causal impact of early riot exposure.

7.3. Migration

The migration of households from riot-hit districts to districts less likely to experience Hindu-Muslim riots would violate the 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.[38] The important migration-related information we exploit is the age at which migration occurs, the district from where migration occurs (i.e., the origin district), and whether migration occurs within the district or in another district. The data allow us to perform analysis only for the migrating population. Since the violation of the SUTVA in our setting occurs in case of migration from a riot-hit district to a district that is not riot hit, 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:

$$MigrationRate_{hy} = \alpha + \eta_h + \delta_y + \beta Exposed_{hy} + \epsilon_{hy}, \quad (6)$$

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 $Exposed_{hy}$ 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.

Next, we collect administrative data on the districts where the judges completed their secondary schooling, higher secondary schooling, and undergraduate studies for a subsample of judges. In Table 4 we establish there is no early childhood riot exposure effect on migration away from home districts to the districts where they completed their secondary schooling (at age 15), higher secondary schooling (at age 17), and undergraduate studies (at age 21).

8. 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. If early exposure to religious riots causes individuals of a certain type to join the judiciary when they are adults, then the impact coefficient captures both the exposure effect and the effect of the unobservables that caused them to choose judiciary as an occupation. For example, as a thought experiment, if early riot exposure makes women more likely to join the judiciary and if females have lower levels of tolerance of misconduct independent of exposure, then our impact coefficient would measure both the exposure effect and the gender effect. On the other hand, if there is no sorting into judiciary, then the impact coefficient would measure the persistent effect that riot exposure during early childhood has on attitudes toward civilian misconduct and state institutions.

We adopt two empirical strategies to show that impact of early childhood exposure to violence is less likely to be driven by the selection of early riot-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 approach, using data from the Employment and Unemployment Survey of the NSSO (66th round, 2011), 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. We show that the share of early riot exposed population working in salaried jobs is similar to the share of early riot exposed judges in judiciary. We restrict our analysis to the riot-affected UP districts. The NSSO survey captures information about individuals' age (but not their birth date) and the dis-

[38] 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.

Table 3
Riot exposure effect on migration.

Inter-district migration rate	
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 Eq. 6. Robust standard errors are provided in parentheses. The dependent variable is the 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 higher inter-district migration rate.

trict 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. We next select the sample born after 1950 (since our riot data start from 1950) who are employed (all types of employment). In Appendix Table B.XIV, 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 other words, we test whether we have a disproportionate number of judges or a disproportionate share of female or Muslim judges who originate from a given home district and belong to a particular birth cohort. In particular, we examine 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:

$$Judge_{h(y-5,y)} = \alpha + \eta_h + \delta_y + \beta Exposed_{hy} + \epsilon_{h(y-5,y)}, \quad (7)$$

where h and y denote home district and riot-year. Our outcome variable is the total number of current (2014–2018) judges born in home district h between the years y and y–5. In other words, $Judge_{h(y-5,y)}$ is the total number of judges born in home district h during the period y–5 and y. Eq. 7 is effectively testing whether riots in a particular home district in a given year is causing a disproportionate number of people to become judges today (i.e., in the year 2014–2018).

We create a balanced panel of district-years (i.e., 47 home districts and 1955–1991 birth years). The exposure variable captures whether there was a riot in a given home-district-year. There are three outcome variables. The first outcome variable is the total number of (current) judges from district h born in between y–5 and y. For instance, if Agra district had a riot in 1970, the total number of judges in the year 2014–2018 affected by this riot in their early childhood (0–6 years) would be the number of judges born in Agra in 1965–1970. Similarly, our second and third outcome variables are the proportion of female (current) judges and the proportion of Muslim (current) judges from district h born in between y–5 and y. η_h is the home district fixed effect, and δ_y is the riot-year fixed effect. Appendix Table B.XV shows that there is no selection by gender, religion, or total number of judges by riot-affected home districts in any given year. There may be selection along other dimensions, such as income, for which we do not have the data.

9. Robustness

9.1. Few judges per district concern

Our analytical sample has 668 judges over several districts, possibly resulting in a small number of judges per district and 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. Fig. 1 plots the coefficients from the estimates of our main specification (Column (3) 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 excluding the first 280 judges (out of 668 judges), alleviating the concern that a few judges may be driving our result.

9.2. 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

Table 4
Treatment effect of exposure on migration of judges.

Class	(1) X	(2) XII	(3) LLB	(4) X	(5) XII	(6) LLB
***K*id[0–6]**	0.00351	0.0281	−0.000214	0.0365	0.0473	−0.0571
	(0.051)	(0.049)	(0.041)	(0.073)	(0.068)	(0.060)
Observations	651	651	651	351	351	351
R-squared	0.167	0.165	0.436	0.190	0.234	0.443
Mean Dep Var	0.40	0.33	0.39	0.39	0.33	0.40
Home District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All Judges	All Judges	All Judges	Bail Judges	Bail Judges	Bail Judges

Notes: This table tests for the effect of judges' exposure to riots during early childhood (0–6 years) on migration away from their home district using the information on the districts where the judges completed their Class X (secondary schooling at age 15), XII (higher-secondary schooling at age 17) and LLB (undergraduate studies at age 21). The outcome variable is a dummy variable that equals 1 if the district where the judge completed a given degree is different from her home district. The estimation includes home district and year fixed effects. The main coefficient of interest captures whether the exposed judges' migration for studies differ from that of non-exposed judges.

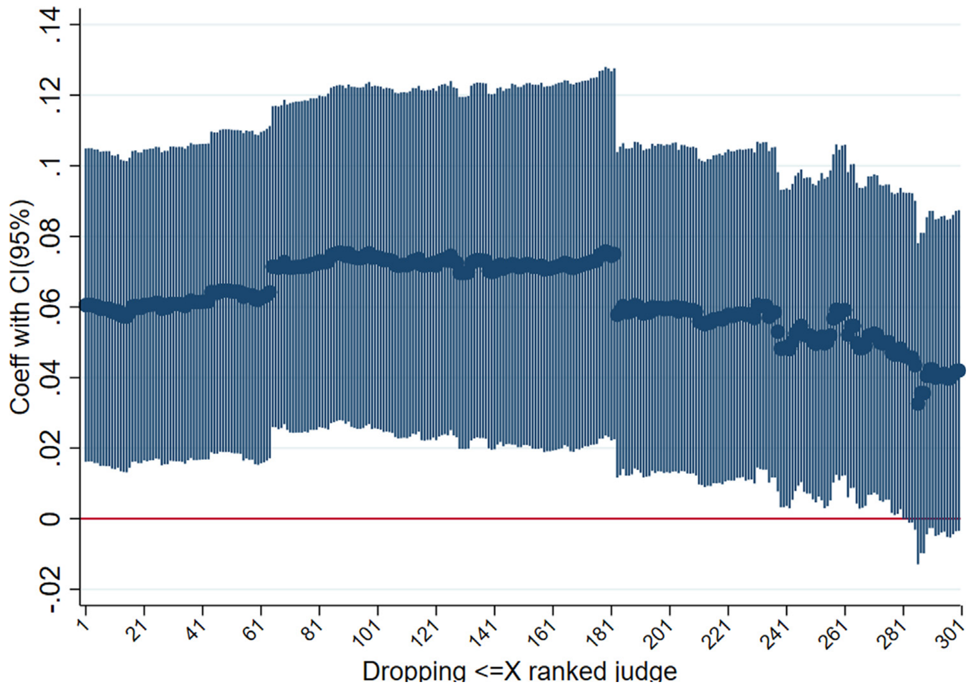

Fig. 1. Removing judges serially with high influence. Notes: The figure reports the coefficients from running the main Eq. 9, 300 times by removing one by one the judges with the highest contribution to the early childhood exposure effect. The contribution is measured by Cook's distance. The result goes away only after removing half of the judges from the sample..

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.XVI, 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 preferred specification (i.e., Column (3) of Table 2). In Column (2), we add the crime type fixed effects, and in Column (3), we further refine our specification by adding two more controls—a dummy for whether the defendant is 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 the judge level, the data should be clustered at the 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), 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, Dobbie et al., 2018 account for two-way clustering by including the defendant- and judge-level clusters. This approach is not feasible for our data because we do not have a unique defendant ID.

The benefits of adding case-level controls are very limited owing 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.

9.3. Sample selection

We follow Arnold et al., 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.XVII, 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.

9.4. High-rank judges

Another concern is that high-ranked judges may influence the cases assigned to them. In Appendix Table B.XVIII, 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 5.4 to 8.4 percentage points and are statistically significant at the 95% confidence interval.

9.5. Outlier tests

The next set of robustness tests is to check for potential outliers in our baseline results. In Appendix Table B.XIX, we show that judges from home districts exposed to a high number of riots do not drive our results. Column (1) presents the results after excluding judges from home districts that have experienced the highest number of Hindu-Muslim riots, Column (2) presents the results after excluding judges from home districts with the second-highest number of riots, and so on. The effect of early childhood exposure to riots is positive and statistically significant at the conventional levels, with its magnitude ranging from an increase of 5.7 to 7.5 percentage point in pretrial detention rates.

In Appendix Table B.XX, 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 6.8 to 8.8 percentage points, significant at the 1% level of significance. Last, we test our baseline results by removing observations that are 3, 2, and 1 standard deviations away from the residual mean in Column (1), Column (2), and Column (3) in Appendix Table B.XXI, respectively. In addition, we remove observations with high leverage, which shifts estimates to at least one standard error and to at least $4/N$. The results are positive, with magnitudes ranging from 5.3 to 6.4 percentage points, and are significant at the conventional level of significance across all specifications.

9.6. 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 (3) of Panel A of Table 2) on the simulation data and implement 1,000 simulations. The sampling distribution of the treatment effects of the $kid[0-6]$ Monte Carlo draws is centered around zero. Fig. A.IX demonstrates that the probability of the treatment effect found in our main specification being spurious is negligible.

9.7. Dropping home districts with no riots

It is possible that home districts that have never had any riot are culturally different from home districts that have had at least one riot. Analyzing judges who originate from home districts that have had at least one Hindu-Muslim riot, we find our coefficient treatment effect unchanged, presented in Appendix Table B.XXII.

10. Mechanisms:

Critical period research in psychology and in the early childhood literature in economics point toward various mechanisms through which communal violence could potentially have long-lasting effects, as explained in our conceptual framework in Section 2. Disentangling each mechanism will not be feasible with our dataset. Therefore, we provide suggestive evidence on some of the mechanisms highlighted in the growing literature of early childhood. We begin with examining whether observed judicial stringency reflects inter-religious hostility due to exposure to religious violence. Early life exposure to an inter-group 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).

10.1. Inter-group bias behavior

To estimate the inter-group hostility effect, we would need to identify the religion of the judges and the defendants, but we 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 judges' name and their fathers' name. For defendants, we first use the "Stanford Named Entity Algorithm" to extract their names from the judgments (see Appendix C.4 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 rate in the classification of the defendants' religion is higher (20%) owing to additional errors in the process of extracting names from the judgment PDFs.

In our sample of 668 judges, only 51 are Muslim, 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 were exposed to religious riots between ages 0 and 6 years.[39] 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 7 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 5.1 percentage points (14% increase in pretrial detention rates) and 7.3 percentage points (20% increase in pretrial detention rates), both statistically significant at the 5% level of significance. We do a Chow test from a pooled regression and find that the early riot exposure coefficient for Hindu defendants is not statistically different from the early riot exposure coefficient for Muslim defendants (F-stat = 0.15). Additionally, we subsample cases with at least one Muslim defendant. In Appendix B. XXIII, we do not find any statistically significant effect on the share of bail denied for cases with at least one Muslim defendant.

Since judicial stringency in our data is not reflecting long-lasting inter-group animosity from childhood experiences of inter-religious riots, we explore other possible channels highlighted in the literature of early childhood. Early life circumstances at birth (Adhvaryu et al., 2019) and, in particular, in utero nutrition (Almond and Currie, 2011) have been shown to impact later-life mental health and, in general, adult health and cognitive outcomes. We next explore whether biases in judicial decisions are driven by in utero conditions or by differences in cognitive outcomes.

10.2. In-utero effect

Appendix Table B.XXIV shows that observed judicial stringency is not explained by in utero effects. The inclusion of a dummy that captures exposure one year before birth does not affect our coefficient estimates of the early childhood riot exposure effect, and the coefficient on the dummy is very small and insignificant.

10.3. Judicial education

Early life exposure to civil conflict could have an impact on cognitive skills. 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 including LLB examination results does 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.

10.4. Riot intensity and state lockdowns

Cohrssen et al., 2022 demonstrates that both indirect and direct exposure to civilian violence and reactions of the state can cause very young children to develop perceptions about state institutions. To shed some light on this possibility, we implement heterogeneity in treatment effects by the experience of riots, where we

[39] 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 consider bank managers' exposure to violence.

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 FE	yes	yes
Date of Birth FE	yes	yes
District X Quarter FE	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.

focus on the experience of high versus low intensity riots that have had high versus low state reactions.

Wilkinson, 2006 argues that the state response to Hindu-Muslim riots in the form of arrests, lockdowns, and increased police presence plays a significant role in 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 their 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.

It is possible that low casualties in a riot is indicative of an effective administrative response to civil disorder. It could also be possible that low casualties reflect low levels of inter-group conflict independent of state efficiency. Therefore, the hypothesis that early experience of effective state response to civil riots causes later-life support for state administration is difficult to test using only riot intensity data. To examine the hypothesis, we use data on state-imposed lockdowns and arrests along with measures of riot casualties as riot intensity.

We interact state action with riot intensity to examine whether a specific type of riot drives our results. However, we acknowledge that the link between state action and riot causality is tenuous. We do not have granular data to measure riot casualty and state action by hour or day. Thus, we do not know whether low casualties resulted from strong state action at the start of the riot. We conjecture that high state action is more likely to be indicative of a state that has reacted to the riots. Therefore, to understand whether the experience of an effective state response explains later-life judicial stringency, we first condition on high-state action by sub-sampling high-state action riots. Then, we estimate the exposure effects by riot intensity. We assume that low riot intensity conditional on high state action would measure an effective administrative response.

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.[40] 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 five 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 ages 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:

$$B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{FE}_b + \beta_1 \times high - casualty - high - state - action[0-6]_j + \beta_2 \times low - casualty - high - state - action[0-6]_j + \sum_{k=7}^{9} \gamma(k) \times exposure(k)_j + X_j + \epsilon_{j,d,t}, \quad (8)$$

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 a higher period of lockdowns or police arrests. The remaining variables are the same as in our preferred specification.

Table 6 presents the heterogeneity impact by intensities of lockdown and riot-related casualties. 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. 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.

However, if we do not subsample high-state action and implement our analysis on the full sample, 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). We report riot severity results using an alternative specification in Appendix Table B.XXVI. Further, the results are robust to using alternative methods of calculating the intensity thresholds, as presented in Appendix Table B.XXVII. All riots analyzed in our sample are local riots with national media coverage, pointing toward the fact that even riots with low casualties in our sample were politically and socially important enough to have had national media coverage.

10.5. Heterogeneity by media coverage

The exposure of riots could still vary by the importance given by the national media. To test if riots that have had higher importance given by national media explains our result, we measure the importance of media exposure by the total number of newspaper articles written on any given riot. For the weighted exposure variable, we weigh by inverse of the page number where the news of the riot appears, assuming that a low page number indicates higher importance as most salient news are written on the first few pages of newspapers. Appendix Table B.XXVIII shows that there is no differential effect of the extent of coverage of riots by the national media on the share of bail denied by the judges. In other words, conditional on the riot being already covered on national media, any additional increase in media exposure does not seem to have any additional effect.

Research in early childhood has highlighted the age of exposure as a key parameter in understanding underlying mechanisms by which early life interventions produce lasting effects. Memory for-

[40] Our results are robust to defining intensity of riots as casualties per capita, as demonstrated in Appendix Table B.XXV)

Table 6
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 FE	yes	yes
Date of Birth FE	yes	yes
District X Quarter FE	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 bail 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, 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 interacts 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 number of arrests 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 7
Heterogeneity by defendants' religion.

	(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
DOB 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 defendants are Hindu. Column (3) includes the cases handled by Hindu Judges when all the defendants are Muslims.

mation has been shown to start at the age of 3 (Fehr et al., 2008), and childhood memories have been associated with adult prosocial behaviors (Gino and Desai, 2012). Cappelen et al., 2020 show that interventions during ages 3 to 4 years have a long-term impact on social preferences. The literature in psychology shows that children aged 5 and 6 years develop their own perceptions of state institutions when they have both direct and indirect exposure to civilian protests (Cohrssen et al., 2022). We therefore examine the heterogeneity by the age of riot exposure to test whether our effects are driven by the age of exposure that is associated with the development of social behaviors.

10.6. Heterogeneity by age of exposure

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 ages 3 and 6 years.[41] We estimate the following regression specification, which is a variant of our preferred specification:

$$B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{FE}_b + \beta \times pre - birth[0-3]_j + \beta_1 \times kid[0-3]_j + \beta_2 \times kid[3-6]_j + \beta_3 \times kid[6-9]_j + X_j + \epsilon_{j,d,t}. \quad (9)$$

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 (approximately 20% of the total judges in the estimation sample). Last, 163 judges were exposed to communal clashes when 6–9 years old (approximately 24% of the total judges in the estimation sample).

Appendix Table B.XXIX shows 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. We find a statistically significant ($p < 0.05$) positive treatment effect of exposure for the age group of 3–6 years. Fig. 2 plots the coefficient estimate of each age group. We note that the effects are primarily driven by exposure between ages 3 and 6 years. Our results are robust to multiple hypothesis testing, with the Bonferroni p-value for the treatment effect of exposure for the age group of 3–6 years being 0.0058. Additionally, for further robustness, we estimate the effects of single age coefficients without grouping. Appendix A.X-A.XIII shows that the exposure at age 5 drives our effect. This result aligns with the findings in psychology (Cohrssen et al., 2022) and is consistent with what we show next on the effect of age of first exposure.

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:

$$B_{j,d,t} = \alpha + \eta_{d,t} + \delta_{h,t} + \mathcal{FE}_b + \beta \sum_{k=1}^{n} firstexposure(k)_j + \sigma X_j + \epsilon_{j,d,t}, \quad (10)$$

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

Fig. 3 and Appendix Fig. A.XIV plot the coefficient estimates with 95% confidence intervals of Eq. 10 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.XXX shows the effect of age at first exposure to communal violence for the full analysis sample. The effect of the age at first exposure being 4 years is 8.1 percentage points, and the effect of the age at first exposure being 5 years

[41] 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 they will be categorized into the 0–3 age category.

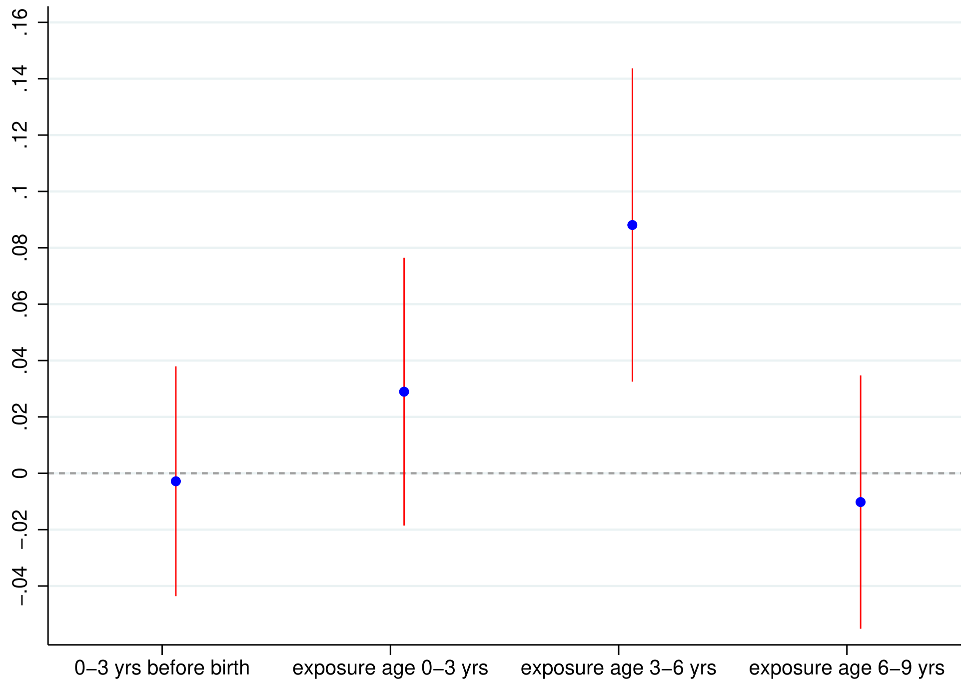

Fig. 2. Treatment effects estimates of riot exposure by age. Notes: The figure reports the coefficients from Eq. 9, where we create four mutually exclusive age bins of 3 years each starting from 3 years before birth up to 9 years of age for exposure to riots. The figure illustrates that exposure to riots when aged 3–6 years of age is statistically significant. .

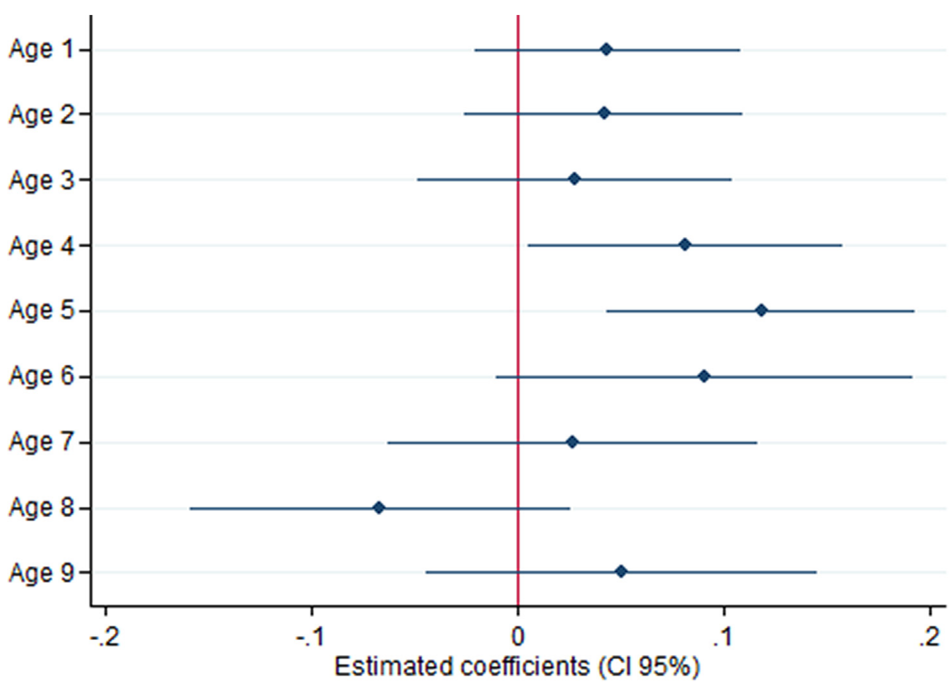

Fig. 3. Treatment effects estimates of age at first exposure. Notes: Estimated coefficients and the confidence interval at 95% are reported through ordinary least squares estimation using Eq. 10 on the total sample of 668 judges. The main dependent variable is the pretrial detention rate at the judge-district-quarter level. The estimation includes home-district-quarter, birth-year, and district-quarter fixed effects..

is 11.8 percentage points, which is statistically significant at the 5% and 1% levels of significance, respectively. Given the findings in the early childhood literature, this result provides further support for our interpretation in the above section that there is a causal link between early childhood riot exposure to support for the state and controlling civilian misconduct.

10.7. Recent riot exposure

In Fig. 4, we plot the post-period effects of current riots (2014–2016)[42] at the weekly level for up to 12 weeks separately for early

[42] The data on Hindu-Muslim riots for 2014–2016 are from Lehne, 2022.

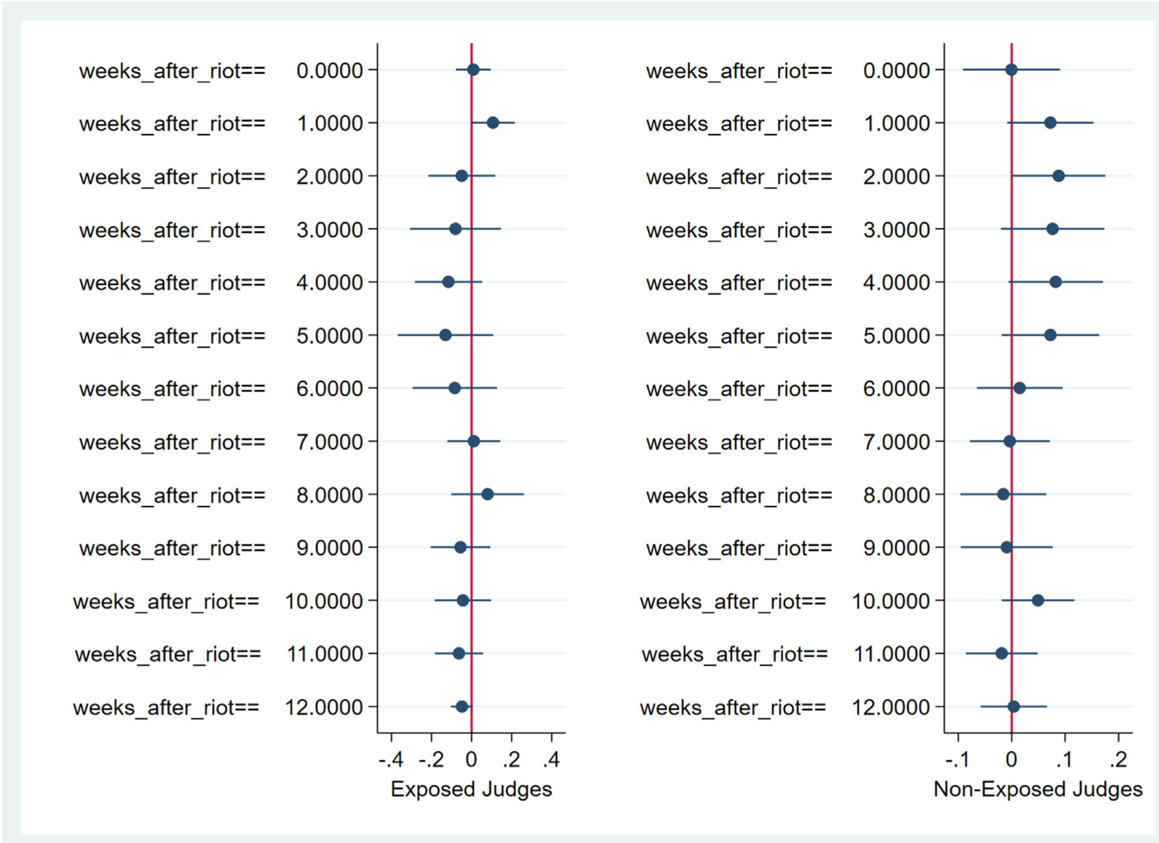

Fig. 4. Impact of current riots. Notes: This figure presents event study plots of share of bail denied up to 12 weeks post-riot exposure for riots that occurred in 2014–2017. The figure presents estimates of the trajectory of the share of bail denied over time separately for early exposed and non-exposed judges following the specification: $Y_{jdt} = \beta * T + \eta_j + \delta_{dt} + \epsilon_{jdt}$; where, Y_{jdt} is the share of bail denied by judge j, posted at district d in week t; T is the time dummy for the post and pre-exposure periods, η_j is judge fixed effect, δ_{dt} is district-weekly trends, and ϵ_{jdt} is the error term clustered at the judge level..

exposed and non-exposed judges. The effects of current riots are not persistent, and there is no effect of riots on the shares of bail denied for early exposed judges. For the non-exposed judges, there are marginal positive effects up to five weeks post-riots, with most post-period effects being statistically insignificant.

10.8. Effect by crime type

It is likely that observed judicial stringency by early riot-exposed judges is driven by their attitudes toward violent crimes, indicating that violence experienced during childhood reduces tolerance for only violent crimes. However, we find no such effect in Table B.XXXI. The treatment effect coefficient is positive for all crimes. However, early exposure to the riot effect is statistically significant for body crime, forgery, and property crimes and is imprecise for arms and explosives, criminal intimidation, and cow slaughter.

11. 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 also 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 resulted in low riot-related damages during judges' 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.

Data availability

We will share the link to our data and codes in the next step.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2023.104846.

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