September 15, 2026

Dear Professors Breza, Viviano, and Chandrasekhar,

I am a Ph.D. candidate in Economics at the University of California, Santa Barbara, and an applied microeconomist with research interests in family economics, development economics, and the economics of conflict. I am writing to apply to the NBER Workshop on Development Economics and Networks for Early-Career Researchers.

I am submitting my job market paper, "The Long Shadow of Communal Violence: Childhood Exposure and Later-Life Mental Health," for consideration at the workshop. The paper studies the long-run consequences of childhood exposure to Hindu–Muslim riots on women's mental health in India.

If I am shortlisted, Professor Kelly Bedard of the Department of Economics at UC Santa Barbara can provide a recommendation:

Professor Kelly Bedard
Department of Economics
University of California, Santa Barbara
Email: Kelly.bedard@ucsb.edu

Thank you for considering my application.

Sincerely,
Reetika Sindhi
Ph.D. Candidate in Economics
University of California, Santa Barbara
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Reetika Sindhi

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Education

Expected 2027 Ph.D. in Economics, University of California, Santa Barbara
2022 M.A. in Economics, University of California, Santa Barbara (CGPA 3.74/4)
2017 M.Sc. in Economics, Shiv Nadar University (CGPA 9.07/10)
2014 B.A.(Honors) in Economics, University of Delhi (First Division)

Ph.D. Coursework

Core Courses: Econometrics I–III; Microeconomic Theory I–III; Macroeconomic Theory I–III.
Field Courses: Population Economics; Behavioral Development Economics; Clustering and Multiple Comparisons; Field Experiments; Economic Data Wrangling; Economics of Education; Health Economics II; Nonparametric Econometrics; Human Capital; Labor Supply; Causal Machine Learning; Panel Data and Difference-in-Differences; Demography.

Research Fields

Applied Microeconomics: Labor Economics, Family Economics, Economics of Crime and Conflict, and Development Economics.

Published Papers (Abstract at the end)

Prakash, N., Sahoo, S., Saraswat, D., & **Sindhi, R.** (2024). "When Criminality Begets Crime: The Role of Elected Politicians in India." *Journal of Law, Economics, and Organization.*

Working Papers (Abstracts at the end)

Sindhi, R., Dhamija, G., & Gupta, S. K. (2026). "The Long Shadow of Communal Violence: Childhood Exposure and Later-Life Mental Health." *Available at SSRN: 7355444. (Job Market Paper)*

Sindhi, R. (2026). "Intergenerational Spillovers of Birth Order: Evidence from the United States." *Available at SSRN: 7102018. (Preparing for Submission)*

Sindhi, R. "The Impact of Red Flag Laws on Intimate Partner Homicide." *(Draft available soon)*

Ladkat, S. S., Dhamija, G., & **Sindhi, R.** (2026). "Who Fills the Domestic Deficit? Maternal Employment and Adolescent Gender Inequality in India." *(Draft available soon).*

Policy Articles

2024 "When Criminality Begets Crime: The Role of Elected Politicians in India", with Nishith Prakash, Soham Sahoo, and Deepak Saraswat, *Ideas for India*

Teaching Experience

2026–2027 **Instructor, UC Santa Barbara**
Economics 5: Statistics for Economics
(*Spring 2026, scheduled for Fall 2026 and Winter 2027)*

2025 **Head Teaching Assistant, UC Santa Barbara**
Coordinated a team of 7–8 TAs for courses with 500+ students per quarter.
Economics 100B: Intermediate Microeconomic Theory II (*Winter 2025)*
Economics 10A: Intermediate Microeconomic Theory I (*Spring 2025, Summer 2025)*

2021–2026 **Teaching Assistant, UC Santa Barbara**
Led discussion sections and office hours; graded assignments and exams.
Economics 5: Statistics for Economics (*Winter 2026, Summer 2025)*
Economics 100B: Intermediate Microeconomic Theory II (*Winter 2024, Spring 2024)*
Economics 10A: Intermediate Microeconomic Theory I (*Fall 2021, Fall 2023, Summer 2024)*
Economics 1: Principles of Microeconomics (*Fall 2022, Winter 2023)*
Economics 2: Principles of Macroeconomics (*Winter 2022, Spring 2022, Spring 2023)*
Economics 101: Intermediate Macroeconomic Theory I (*Summer 2023)*

2015–2017 **Teaching Assistant, Shiv Nadar University**
Led discussion sections for undergraduate courses.
Economic Development, Logic and Reasoning

2011–2015 **Private Tutor**
Undergraduate: Microeconomics, Macroeconomics, and Statistics
High School: Economics, Accountancy, and Mathematics

Research Experience

2019–2021 Research Assistant to Soham Sahoo, Indian Institute of Management Bangalore

- Worked extensively with Indian survey and census datasets conducting data cleaning, standardization, and district harmonization.
- Conducted meta-analysis for "Does female economic empowerment promote development?", screening studies, computing standardized mean differences, and estimating meta-regression models.

Other Work Experience

2017–2018 Team Leader, Bank of America Merrill Lynch
Investment Banking division – prepared, substantiated, and analyzed profit and loss on collateral exchanged under Dodd–Frank for Over the Counter trades.

Conference Presentations & Seminars

2026	All-California Labor Economics Conference, UC Berkeley
	USC/UCLA Workshop on Families and Inequality, UC Los Angeles
	All-UC Demography Conference (Poster), UC Santa Barbara
2025	Graduate Student Lab Lunch, Broom Center Seminar Series, UC Santa Barbara
	Applied Microeconomics Lunch, UC Santa Barbara
2021	International Conference on Globalization & Development (Poster), University of Göttingen
	16th Annual Conference on Economic Growth and Development, Indian Statistical Institute

Awards and Fellowships

2026–2027	Guru Gobind Singh Fellowship (UC-wide)
2026	Conley Research Accelerator Fellowship, UC Santa Barbara
	Best Poster Award, All-UC Demography Conference
	Graduate Student Research and Training Grant, Broom Center for Demography
	Travel Grant, USC/UCLA Workshop on Families and Inequality, UC Los Angeles
2025	Research Quarter Fellowship, UC Santa Barbara
	Outstanding Undergraduate Course TA Award, UC Santa Barbara
2022	Summer Fellowship, UC Santa Barbara
2021–2026	Doctoral Fellowship, UC Santa Barbara
2018	Global Recognition Award, Bank of America Merrill Lynch
2017	Gold Award, Bank of America Merrill Lynch
2015–2017	Graduate Fellowship, Shiv Nadar University

Certifications

Ongoing	Certificate in College and University Teaching (CCUT), UC Santa Barbara
Winter 2025	Leadership Certificate, UC Santa Barbara

Affiliations

Graduate Student Fellow, Broom Center for Demography, UC Santa Barbara

Technical Skills

Stata, R, Python, LaTeX, GradeScope, Canvas.

Academic Service at UC Santa Barbara

2026	Round II Judge, Graduate Research Presentation Competition (Grad Slam)
2025	Panelist, Graduate Success Series for Incoming Graduate Students, Graduate Division
2023–2025	Mentor, First-Year Economics Ph.D. Students

Languages

English (*native proficiency*), Hindi (*native*), Punjabi (conversational)

Abstract (Published Paper)

"When Criminality Begets Crime: The Role of Elected Politicians in India"

This article examines the effect of electing criminally accused politicians on crime in India, considering the severity of their alleged offenses. Employing an instrumental variable approach that exploits the quasi-random variation in outcomes of close elections between candidates with and without criminal accusations, we find that a standard deviation (SD) increase in the share of criminally accused leaders in institutionally weaker states leads to a 0.05 SD rise in yearly reported crimes. Leaders accused of serious crimes have a more pronounced effect on crime outcomes, including crimes against women. Crucially, leaders accused of serious crimes also exert a negative influence on female labor force participation, underlining their detrimental impact on socioeconomic welfare. Our findings highlight the significance of considering the nature and severity of criminal accusations when evaluating the impact of criminally accused politicians on crime and society.

Abstracts (Working Papers)

"The Long Shadow of Communal Violence: Childhood Exposure and Later-Life Mental Health" *(Job Market Paper)*

We study the long-run relationship between childhood exposure to communal violence and women's mental health in India. Using a newly constructed crosswalk spanning seven decades, we link historical Hindu–Muslim riot locations to respondents' childhood districts in the Longitudinal Ageing Study in India (LASI). Exploiting variation in exposure across childhood districts and birth cohorts, we find that exposure to at least one riot between birth and age 14 is associated with a 29 percent higher probability of major depressive disorder relative to the sample mean. Greater cumulative exposure is similarly associated with worse later-life mental health. The association is strongest for exposure at ages 0–4 and 10–14 and weaker for exposure at ages 5–9. Childhood riot exposure is also associated with earlier marriage, pointing to changes in life-course trajectories as a potential channel linking childhood violence to later-life mental health. The findings show that even localized, short-lived episodes of communal violence can have mental-health consequences that persist for decades, despite being far less sustained than the more commonly studied armed conflicts.

"Intergenerational Spillovers of Birth Order: Evidence from the United States"
(Preparing for Submission)

Birth order shapes life outcomes, but do these differences persist across generations? This paper examines whether a mother's birth order is associated with her children's developmental trajectories. Using mother-child panel data from the National Longitudinal Survey of Youth, I find that children of later-born mothers score lower on cognitive tests than those of firstborn mothers. These gaps emerge early, persist into adolescence, and are associated with lower college completion. In magnitude, these intergenerational differences are about one-third the size of canonical first-generation birth order gaps. Suggestive evidence indicates that children of later-born mothers receive less cognitive and emotional stimulation at home than those of firstborn mothers.

"The Impact of Red Flag Laws on Intimate Partner Homicide" *(Draft available soon)*

Extreme Risk Protection Order (ERPO) laws, or "red flag" laws, allow law enforcement and family members to petition a court to temporarily remove firearms from individuals who pose a credible risk of harm, on the basis of observable behavior rather than a prior conviction. Unlike most firearm policies, they are prospective, individualized, and civil, and by empowering those closest to a potential perpetrator to act, they are especially relevant to domestic violence, where danger is often visible within the household before any crime occurs. I estimate the causal effect of ERPO adoption on intimate partner homicide by exploiting the staggered rollout of these laws across U.S. states from 1999 to 2024 and applying modern difference-in-differences methods designed for staggered treatment adoption. Using agency-year homicide data from the FBI Supplementary Homicide Reports, I find that firearm intimate partner homicides decline by 10–15% following adoption, with no evidence of differential pre-trends.

"Who Fills the Domestic Deficit? Maternal Employment and Adolescent Gender Inequality in India" *(Draft available soon)*

Adolescent girls in India perform substantially more unpaid domestic work than boys. Using nationally representative time-use data, we examine how this gender gap changes when mothers are employed. Controlling flexibly for adolescent, parental, and household characteristics and a rich set of fixed effects, we find that maternal employment is associated with about 26 additional minutes of domestic work per day for girls, while boys are largely unaffected. The gap increases with mothers' work hours, is largest among mothers in casual wage employment, and widens as girls move through adolescence. Girls primarily accommodate this additional work by reducing leisure and personal care. The association is also stronger in more patriarchal districts and persists across most socioeconomic groups. Results are robust to an instrumental-variables strategy using a shift-share measure of district-level female labor demand.

Last Updated: September 13th, 2026

Statement of Research Interests and Motivation

I am a Ph.D. candidate in Economics at the University of California, Santa Barbara, and an applied microeconomist focused on development economics, family economics, and the economics of conflict. My research examines how exposure to social and institutional environments shapes human capital, family decisions, and well-being, with a particular interest in how these effects persist across the life course and across generations.

My job market paper, *"The Long Shadow of Communal Violence: Childhood Exposure and Later-Life Mental Health,"* studies the long-run mental health consequences of childhood exposure to Hindu–Muslim riots in India. We find that women exposed to Hindu-Muslim riots during childhood have substantially worse mental health decades later, with particularly large associations for exposure in early childhood and early adolescence. We also find that childhood exposure is associated with child marriage, suggesting that violence may alter life-course trajectories in ways that contribute to persistent effects on well-being.

My broader research agenda asks how families respond to economic, social, and institutional shocks, and how these responses can reinforce or mitigate inequality. In related work, I study how maternal employment affects the allocation of unpaid household labor between adolescent girls and boys in India, and how family circumstances in one generation shape human-capital outcomes in the next.

I am particularly interested in attending the NBER Workshop on Development Economics and Networks because studying how violence reshapes social cohesion and social networks is a natural next extension of my current research. My job market paper establishes that childhood exposure to communal violence is associated with worse mental health decades later, providing a foundation for asking why these effects persist. One important possibility is that episodes of communal violence alter the social environment itself by weakening social cohesion, changing patterns of trust and interaction, and reshaping ties within and across communities. These changes in social relationships may, in turn, have lasting consequences for mental health and well-being. I am therefore interested in extending my work to study how conflict affects social networks and whether changes in those networks help explain the persistence of its effects on mental health. The workshop would help me develop the conceptual and empirical tools needed to pursue this next stage of my research agenda.

The Long Shadow of Communal Violence: Childhood Exposure and Later-Life Mental Health*

The draft is updated frequently. The latest version is available here.

Reetika Sindhi†

Gaurav Dhamija‡

Sagnik Kumar Gupta§

September 2026

Abstract

We study the long-run relationship between childhood exposure to communal violence and women's mental health in India. Using a newly constructed crosswalk spanning seven decades, we link historical Hindu–Muslim riot locations to respondents' childhood districts in the Longitudinal Ageing Study in India (LASI). Exploiting variation in exposure across childhood districts and birth cohorts, we find that exposure to at least one riot between birth and age 14 is associated with a 2.4 percentage point higher probability of major depressive disorder in later life, or 29 percent of the sample mean. Greater cumulative exposure is similarly associated with worse later-life mental health. The association is strongest for exposure at ages 0–4 and 10–14, consistent with differences in vulnerability across developmental stages. Childhood riot exposure is also associated with a higher likelihood of child marriage, pointing to changes in life-course trajectories as a potential channel. These findings show that even localized, short-lived episodes of communal violence can have mental-health consequences that persist for decades.

Keywords: Communal violence; childhood exposure; mental health; older women; India

JEL Codes: D74, I12, I31, J12, J14, J16, N35

* We thank Abhiroop Mukhopadhyay for providing a supplementary dataset containing district information that we merged with the publicly available LASI data. This information allowed us to recover childhood districts for many respondents for whom this information is missing in the publicly available data. We also thank Abhiroop Mukhopadhyay and Kelly Bedard for their valuable feedback.We thank participants at the 2026 All California Labor Economics Conference for helpful comments and suggestions. We thank Kriti Dutta Majumdar for his excellent research assistance on this project. Reetika Sindhi acknowledges support from the Guru Gobind Singh Fellowship, a University of California system-wide fellowship, for the 2026–2027 academic year.

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"For a bewildered child, the violence of identity was extraordinarily hard to grasp."

— Amartya Sen, Identity and Violence (2006)

1 Introduction

Violent conflict remains one of the greatest threats to human well-being worldwide. In 2025 alone, more than 204,000 conflict events were recorded globally, resulting in over 240,000 deaths (ACLED, 2025).[1] One persistent form of such violence is communal violence organized around group identities such as religion or ethnicity, which can deepen social divisions and disrupt communities long after the violence itself has subsided. While the immediate impact of communal violence are relatively well documented in economics (Iyer and Shrivastava, 2018; Fisman et al., 2020; Iyer et al., 2026; Werner and Skali, 2025), much less is known about its long-run consequences. These consequences may be particularly severe for children, for whom exposure to violence during critical stages of development can have lasting effects across the life course (Almond et al., 2018). Furthermore, evidence on the consequences for mental health remains especially limited, despite mental health being a central component of well-being (Beck et al., 2011; Ridley et al., 2020; Abramson et al., 2024) and potentially one of the dimensions most affected by exposure to violent episodes. We address this gap by providing the first evidence in economics on the long-run effects of childhood exposure to Hindu–Muslim riots on women's mental health in India.

South Asia, with its population nearing two billion and remarkable religious, ethnic, and linguistic diversity, offers a natural laboratory for the study of communal violence. India, the region's most populous country, stands out for two central reasons. First, the economic cost of poor mental health is staggering: the World Health Organization estimates cumulative losses of roughly $1.03 trillion between 2012 and 2030 (Organization et al., 2017), equivalent

[1] The Armed Conflict Location & Event Data (ACLED) is an independent global monitor that collects data on conflict, protests, and other forms of political violence worldwide.

to about 26 percent of India's projected GDP in 2025. The burden of depression is especially acute among older adults in low- and middle-income settings, with women experiencing rates twice as high as men (Banerjee et al., 2023).

Second, Hindus and Muslims have lived alongside one another on the Indian subcontinent for more than a millennium (Jha, 2013), yet this long history of coexistence has repeatedly been disrupted by communal violence. Between 1950 and 1995, more than 1,100 Hindu–Muslim riots were recorded, resulting in approximately 7,000 deaths. Unlike sustained armed conflict or civil war, these riots were typically localized, episodic, and short-lived, making childhood exposure a salient and well-defined shock.[2] Such exposure may be particularly consequential for girls, as safety concerns may restrict their mobility and schooling or lead families to marry them at younger ages, shaping subsequent life trajectories (Saikia, 2011; Ghose and Pandey, 2025; Borker et al., 2021). These gender differences in both the burden of depression and the consequences of communal violence make women a population of interest for our analysis.

We study the long-run effects of childhood exposure to Hindu–Muslim riots on women's mental health later in life by combining the Longitudinal Ageing Study in India (LASI) with the Varshney–Wilkinson database of Hindu–Muslim riots (Varshney et al., 2002). LASI is particularly valuable for our analysis because respondents report the district in which they spent most of their childhood through age 14, allowing us to measure exposure using childhood residence rather than current location or place of birth. It also contains the Composite International Diagnostic Interview–Short Form (CIDI-SF), from which we construct a standardized measure of major depressive disorder (MDD). We link respondents' childhood districts to the timing and location of Hindu–Muslim riots recorded between 1950 and 1995 and reconstruct each woman's exposure history from birth through age 14.

Linking these two data sources required overcoming an important challenge. Most childhood exposure in our sample occurred between 1950 and 1990, whereas LASI records respondents'

[2] Most Hindu–Muslim riots lasted five days or fewer, with many lasting only a single day (Iyer and Shrivastava, 2018).

childhood district retrospectively when they were surveyed in 2017–2018. Because India's district geography changed substantially over this period, historical riot locations cannot be matched directly to LASI districts. We therefore construct a hand-coded crosswalk that traces changes in district boundaries over time and assigns riots recorded under historical administrative boundaries to the 2018 geography used by LASI. To our knowledge, this is the first crosswalk linking the historical Hindu–Muslim riot data to LASI, allowing us to recover consistent childhood exposure histories across birth cohorts and districts.

Prior work in political science and economics shows that Hindu–Muslim violence in India was shaped by political mobilization, electoral incentives, and the response of local and state institutions (Varshney et al., 2002; Wilkinson, 2004; Iyer and Shrivastava, 2018). Because these forces operated largely above the level of individual households, the timing of communal violence was plausibly outside families' immediate control. Our empirical strategy exploits variation in the timing and location of riots across childhood districts and birth cohorts. Women who grew up in the same district but were born in different years can therefore differ in exposure because violence occurred during the childhood years of some cohorts but not others; likewise, women from the same birth cohort can differ in exposure because riots occurred in some districts but not in others. We capture exposure in two ways: an indicator for whether a woman experienced at least one riot between birth and age 14, and a measure of the cumulative intensity of riot exposure over the same period. The resulting estimates compare women across district–cohort cells while absorbing persistent differences across childhood districts and shocks common to birth cohorts.

We find that childhood exposure to communal violence has substantial and persistent consequences for later-life mental health. Exposure to at least one Hindu–Muslim riot between birth and age 14 increases the probability of major depressive disorder (MDD) in older adulthood by 2.4 percentage points, approximately 29 percent of the sample mean. Results are similar when we measure exposure using the cumulative number of riots experienced during

childhood, with more frequent exposure associated with a higher probability of depression later in life. The estimates remain stable across a range of identification and robustness checks, including state- and district-specific cohort trends, controls for childhood rainfall shocks and changes in district demographics, placebo exposure outside the childhood window, alternative treatments of extreme exposure, sample restrictions, leave-one-state-out specifications, and nonlinear models. Moreover, childhood riot exposure is associated with increases in the individual components of the MDD measure, including loss of interest, loss of appetite, difficulty concentrating, feelings of worthlessness, trouble sleeping, low energy, and thoughts of death.

The effects vary with age at exposure- estimates are largest at ages 0–4 and 10–14 and substantially smaller at ages 5–9. This non-monotonic pattern is consistent with evidence that both early childhood and adolescence can represent sensitive periods. Shocks in early childhood may disrupt foundational skill formation, while exposure during adolescence coincides with rapid socio-emotional development and important educational and life-course transitions, making shocks during both periods potentially persistent (Carneiro et al., 2021; Gong et al., 2020; Nikolova and Nikolaev, 2021; Akresh et al., 2023; Hidalgo-Aréstegui et al., 2025). Finally, women exposed to communal riots during childhood are more likely to marry before ages 18 and 15. This higher likelihood of child marriage points to a potential life-course pathway through which childhood exposure to communal violence may have lasting consequences for women's mental health (Field and Ambrus, 2008; Roychowdhury and Dhamija, 2021; Chari et al., 2017).

This paper makes two main contributions. First, we contribute to the economics literature on conflict and mental health. Existing work documents substantial psychological costs of exposure to war and violence: combat exposure increases post-traumatic stress and suicidal ideation (Cesur et al., 2013), violence-induced psychological trauma can alter economic behavior (Moya, 2018), and early-life exposure to bombing during the Vietnam War has

persistent effects on mental health in adulthood (Singhal, 2019). Related work documents long-run effects of childhood exposure to war on physical health and other dimensions of human capital (Kesternich et al., 2014; Akbulut-Yuksel, 2017). We extend this literature by showing that persistent mental-health consequences are not confined to large-scale or sustained armed conflict: exposure during childhood to localized and relatively short-lived episodes of communal violence is associated with major depressive disorder decades later.

Second, we contribute to the literature on communal violence. A prominent economics literature studies the origins of religious conflict, including the roles of residential and economic conditions (Field and Ambrus, 2008; Mitra and Ray, 2014), while more recent work documents consequences for electoral behavior, intergroup discrimination, and subsequent decision-making (Iyer and Shrivastava, 2018; Fisman et al., 2020; Bharti and Roy, 2023). In particular, existing evidence shows that exposure to Hindu–Muslim riots can have persistent effects on intergroup lending and on adult judicial behavior following childhood exposure. We add to this literature by showing that the consequences of communal violence also extend to mental health: childhood exposure to Hindu–Muslim riots is associated with major depressive disorder in older adulthood, several decades after the violence occurred.

The remainder of the paper is organized as follows. The next section provides historical background on Hindu–Muslim communal violence in India and discusses why childhood exposure may have lasting consequences. We then describe the data and construction of our historical district crosswalk, followed by our empirical strategy. The subsequent section presents the main results, identification and robustness checks, heterogeneity analyses, and evidence on marriage timing as a potential channel. The final section concludes.

2 Background

This section provides the historical context for our analysis of childhood exposure to Hindu–Muslim communal violence in India. We begin with a brief history of Hindu–Muslim relations under British rule and the rupture created by Independence and the 1947 Partition of British India. We then describe the scale and character of violence surrounding Partition before turning to Hindu–Muslim riots in post-1947 independent India. Finally, we discuss why even short-lived exposure to communal violence during childhood may have consequences that persist into later life.

2.1 Hindu–Muslim Relations in Historical Perspective (Pre-1947)

For centuries, Hindus and Muslims coexisted across the Indian subcontinent, often connected through trade, occupational complementarities, and local institutions that structured everyday interaction.[3] Jha (2013), for example, shows that medieval trading towns in which Hindu and Muslim merchants developed complementary economic roles also developed institutions that facilitated inter-group exchange and remained less prone to communal violence centuries later. Religious conflict was therefore not simply a consequence of religious heterogeneity; it also depended on the economic and institutional conditions governing inter-group interaction. Although episodes of religious violence occurred, they were not a regular feature of provincial life during much of the nineteenth century (Iyer and Shrivastava, 2018). By the late nineteenth and early twentieth centuries, however, religious identity had become increasingly intertwined with political competition under British colonial rule. Colonial institutions progressively organized political representation around communal categories, most notably

[3] Hindus are followers of Hinduism, a diverse religious tradition originating in the Indian subcontinent and commonly associated with beliefs in *dharma* (moral and social duty), karma (the consequences of one's actions), rebirth, and *moksha* (liberation from the cycle of rebirth), as well as devotion to different manifestations of the divine. Muslims are followers of Islam, a monotheistic Abrahamic religion centered on belief in one God, the Holy Book Quran, and the teachings of the Prophet Muhammad.

through separate electorates, which allowed members of designated religious communities to elect their own representatives. These arrangements increased the political salience of religious affiliation and the scope for mobilization along communal lines (Pandey, 2006). Communal violence intensified in the final years of British rule, with major Hindu–Muslim clashes occurring across several regions of British India in 1946–47.[4]

2.2 Partition and Hindu-Muslim Violence (1947)

When British rule ended in August 1947, British India was divided into Muslim-majority Pakistan and Hindu-majority India, which remained religiously plural and adopted a secular constitution in 1950.[5] The Partition plan was announced in June 1947 and enacted by the British Parliament in the following months. Its most difficult task was dividing Punjab in the northwest and Bengal in the east, two religiously mixed provinces where Hindus and Muslims had long lived alongside one another. In Punjab, alongside Hindus and Muslims, Sikhs also constituted an important religious community. [6] Two Boundary Commissions, chaired by Cyril Radcliffe—a British lawyer who had never previously visited India—were given only a couple of weeks to draw the borders. Pakistan and India became independent on August 14 and 15, respectively, but the final boundaries were not announced until August 17. Many households therefore entered independence unsure which country their town or village would belong to and could suddenly find themselves on the "wrong" side of the border

[4] Major episodes occurred in Calcutta (now Kolkata, in the present-day Indian state of West Bengal), Noakhali (in present-day Bangladesh, then part of British India and later East Pakistan), Bombay (now Mumbai, in the Indian state of Maharashtra), Ahmedabad (in the Indian state of Gujarat), and Patna (in the Indian state of Bihar), among other places (Iyer and Shrivastava, 2018). The violence was therefore geographically dispersed and was not confined to Punjab and Bengal, the two provinces subsequently divided between India and Pakistan at Partition.

[5] Partition followed years of disagreement over the political structure of an independent India. The Indian National Congress generally favored a united India, while the Muslim League, led by Muhammad Ali Jinnah, demanded a separate Muslim-majority state.

[6] Sikhs are followers of Sikhism, a distinct monotheistic religion founded in Punjab in the fifteenth century by Guru Nanak, who was born into a Hindu family. Sikhism subsequently developed as a separate religious tradition through the teachings of Guru Nanak and nine succeeding Gurus, with its own scripture and institutions. Sikh and Hindu communities nevertheless shared substantial linguistic, cultural, social, and familial ties in Punjab.

as local conditions deteriorated (Bhattacharya and Mukhopadhyay, 2022).

For ordinary Hindus, Muslims, and Sikhs, decisions made at the highest levels of politics abruptly transformed everyday life. Families that had lived for generations in the same communities had little direct influence over Partition or the placement of the borders, yet bore much of its cost. Partition did not initiate communal violence but sharply raised its stakes by tying religious identity to *territory, citizenship, property, migration, and security.* Whether a family could remain in its home, retain its property, or safely live among its neighbors could now depend on which side of a newly drawn international boundary it fell.

The resulting population movement was extraordinary in scale. Bharadwaj et al. (2008) estimate that roughly 18 million people moved across the new borders, making Partition one of the largest and most rapid population movements of the twentieth century. Muslims moved predominantly toward Pakistan and Hindus and Sikhs toward India, abruptly reshaping the religious composition of many communities. Mass displacement unfolded alongside widespread violence: families were separated; homes, land, and businesses were abandoned, destroyed, or seized; and neighborhoods, markets, and social networks were disrupted. Estimates of the death toll range from several hundred thousand to roughly one million (Iyer and Shrivastava, 2018). Women and girls faced particularly severe violence because women's sexuality and marital status were closely tied to notions of family and communal "honor," making attacks on women a means of humiliating rival communities. Historical accounts document rape, abduction, forced conversion, forced marriage or cohabitation, and honor-related killings (Menon and Bhasin, 1998; Butalia, 2017); an estimated 75,000–100,000 women were abducted. Within weeks, Partition had transformed a political division into a profound rupture in household, economic, and community life.

2.3 Hindu–Muslim Violence Post Independence (Post 1947)

Independence ended colonial rule, but it did not end communal violence. Hindu–Muslim riots continued across India after 1947, with substantial variation across both place and time. The period from 1950 to 1980 was comparatively less violent, averaging about 16 reported riots per year, whereas the period from 1981 to 2001 saw nearly three times as many incidents annually (Iyer and Shrivastava, 2018). Although riots occurred across a broad geographic area, their incidence was highly uneven across cities and districts: some localities experienced repeated outbreaks, while others with sizeable Hindu and Muslim populations remained comparatively peaceful. A large political science literature shows that such variation cannot be explained by religious heterogeneity alone. Varshney et al. (2002) emphasizes the role of inter-communal civic networks in containing violence; Wilkinson (2004) shows that state incentives to prevent riots depend partly on electoral competition and the political value of minority votes; and Brass (2011) highlights the role of political actors and local organizations in mobilizing communal tensions. Complementing this work, Iyer and Shrivastava (2018) provides economic evidence that electoral incentives also shape the incidence of Hindu–Muslim violence in post-Independence India.

This combination of political mobilization and localized violence is central to our empirical setting. Riot occurrence may reflect broader political and institutional conditions, but exposure from the perspective of an individual child is largely determined by whether violence happens to occur in her district during the years in which she is growing up. Children do not choose the timing of communal conflict, and households have limited ability to anticipate the precise location and onset of a riot. Because riots varied sharply across districts and years, women born in different places or cohorts could therefore experience very different levels of communal violence during childhood. We exploit this spatial and temporal variation by linking each woman's childhood district and birth cohort to the history of Hindu–Muslim riots during her first fourteen years of life.

2.4 Why Short-Lived Childhood Exposure May Have Lasting Consequences

For our analysis, the significance of communal violence lies not only in why riots occurred, but also in what it meant to *grow up through one*. A riot appears in historical data as an event occurring in a particular place and time, but the shock experienced by children may extend well beyond recorded deaths and injuries. Communal violence can expose households to bereavement, physical injury, property and livelihood losses, displacement, restrictions on mobility, disruptions to schooling and local economic activity, and uncertainty about future violence. Even children who are not directly victimized may experience the sudden transformation of familiar neighborhoods and social relationships into sources of insecurity.

Hindu–Muslim riots were generally localized and episodic (Iyer and Shrivastava, 2018). Unlike prolonged civil wars, an outbreak could be short-lived, with markets, schools, and other aspects of daily life eventually resuming. Yet exposure during childhood may have consequences that outlast the violence itself. A large literature on early-life conditions shows that temporary but salient shocks during formative stages of development can leave effects that remain visible well into adulthood (Almond et al., 2018). Three channels are particularly relevant in our setting. First, exposure may have a direct psychological effect. Witnessing violence, experiencing bereavement or displacement, or living through a period in which familiar surroundings become unsafe may generate lasting psychological distress and alter perceptions of personal safety and social relationships. Consistent with this channel, Singhal (2019) documents long-run effects of early-life exposure to wartime bombing in Vietnam on mental health in adulthood.

Second, communal violence may disrupt human-capital accumulation. School closures, restrictions on mobility, displacement, and losses in household resources can interrupt children's education. Such disruptions during formative years may reduce educational attain-

ment and, in turn, shape employment, income, and economic security later in life. More broadly, evidence from conflict settings documents long-run effects of childhood exposure on education, physical health, and other dimensions of human capital (Leon, 2012; Minoiu and Shemyakina, 2014; Akresh et al., 2012).

Third, for girls, household responses to insecurity may alter the timing of marriage. Economic shocks can affect marriage timing when households use marriage to respond to changes in resources and risk (Corno et al., 2020). More directly, evidence from India shows that communal violence can accelerate women's marriage Ghose and Pandey (2025) find that exposure to religious riots reduces women's age at marriage and increases the likelihood of child marriage. Earlier marriage can then shape female human-capital investment and subsequent economic and social outcomes. Field and Ambrus (2008), for example, shows that delaying marriage increases female schooling and literacy, while other evidence links marriage timing to women's labor-market outcomes and exposure to domestic violence (Chari et al., 2017; Field and Ambrus, 2008; Roychowdhury and Dhamija, 2021).

3 Data

We base our estimation sample on Wave I of the Longitudinal Ageing Study in India (LASI), the first and, at present, only nationally representative longitudinal survey of adults aged 45 years and older in India.[7] LASI stands at the forefront of global ageing research, modeled closely on the U.S. Health and Retirement Study (HRS), the Survey of Health, Ageing and Retirement in Europe (SHARE), the English Longitudinal Study of Ageing (ELSA), and the China Health and Retirement Longitudinal Study (CHARLS). Conducted between April 2017 and December 2018, LASI reached respondents across nearly all Indian states and union territories, producing uniquely comprehensive data for the Indian context.

[7] Wave I is the only publicly available wave of LASI at the time of writing. Wave II is currently underway.

This dataset has several unique advantages for applied economic research. First, unlike earlier nationally representative surveys in India, LASI directly records respondents' childhood district of residence. Respondents were asked where they lived for most of their childhood up to age 14, allowing us to assign riot exposure using their reported childhood district of residence. We therefore do not assume that respondents remained in their birth district throughout childhood. Instead, the data allow us to identify the district in which they actually spent most of their childhood and link that location directly to historical riot exposure. Second, as India's first nationally representative longitudinal ageing survey, it enables a rigorous examination of the long-run consequences of childhood shocks on later-life outcomes in a large, representative sample from a low- and middle-income country—a research design that remains comparatively limited in developing countries as compared to the abundance of such studies in high-income settings. Third, LASI includes detailed, validated measures of mental health, which remain scarce in nationally representative surveys in developing countries, particularly India. Together these features make LASI an ideal data source for investigating the long-run relationship between childhood exposure to communal violence and later-life mental health.

Our analysis focuses on women for two main reasons. First, women bear a disproportionately high burden of depression: depression is nearly twice as common among women as among men (Baranov et al., 2020), with gender disparities particularly pronounced at older ages in low- and middle-income countries (Banerjee et al., 2023). Second, as discussed above, women and girls may be particularly vulnerable to the consequences of communal violence. Evidence from South Asia documents the gendered nature of such violence, including heightened risks of sexual violence, displacement, and restrictions on mobility (Saikia, 2011; Ghose and Pandey, 2025). Importantly, these consequences need not be confined to those directly victimized by a riot. Recurrent local violence may also change how households respond to perceived risks, particularly for girls. Concerns about safety may lead families to restrict girls' mobility, schooling, social interactions, or participation in activities outside the home,

thereby narrowing the opportunities available to them during formative years (Ghose and Pandey, 2025; Borker et al., 2021). Such restrictions may also increase dependence on the household and shape subsequent decisions about education, work, and marriage. Together, these considerations make women a particularly important population for examining the persistent mental-health consequences of childhood exposure to communal violence.

We begin with all 35,567 women aged 45 years and older interviewed in LASI Wave I and exclude 7,273 women born before 1950, leaving 28,294 respondents. This restriction is motivated by the historical and institutional context of post-Independence India. Although Independence and Partition occurred in 1947, the immediate years that followed were marked by substantial demographic upheaval, as large-scale migration and resettlement across the newly drawn borders continued into the early 1950s (Bhattacharya and Mukhopadhyay, 2022). The 1950 cutoff therefore limits overlap with cohorts born during Partition and its immediate aftermath. It is also institutionally meaningful: the Constitution of India came into force in 1950, establishing a new constitutional framework grounded in religious equality and freedom and marking the beginning of India's post-Independence democratic order. We thus focus on cohorts whose childhood exposure to communal violence occurred in post-Partition India.

We further exclude 1,313 women for whom either childhood district of residence or birth year cannot be identified, since both are required to construct district-by-cohort riot exposure, leaving 26,981 respondents. We then construct our primary outcome, major depressive disorder (MDD), using the Composite International Diagnostic Interview–Short Form (CIDI-SF) depression algorithm (Figure 1). The CIDI-SF first screens respondents based on the duration, intensity, and frequency of depressive symptoms. Respondents who satisfy all three screening criteria complete a seven-item symptom battery and are classified as having MDD if they endorse at least three symptoms. Finally, we exclude 327 women with missing information on the mental-health outcome, yielding a final analytical sample of 26,654

women. Figure 2 summarizes the construction of the analytical sample. Finally, the fixed-effects estimator drops 27 singleton observations that provide no within-group variation for identification, leaving an estimation sample of 26,627 women.

We then link each woman to the communal-riot history of her childhood district. The riot data are drawn from the Varshney–Wilkinson database on Hindu–Muslim violence (Varshney et al., 2002), which records 1,194 communal riot events between 1950 and 1995 based on contemporaneous newspaper reports, predominantly from the *Times of India*. For each event, the data record the date and location of the riot, casualty and arrest figures, the reported cause, and a verbatim excerpt from the original news report. Figures 3 and 4 illustrate the substantial temporal and geographic variation in communal violence in our estimation sample. The temporal pattern is relevant for interpreting our estimates. Our birth cohorts span roughly 1950 to the mid-1970s. Cohorts born before 1950s were more directly exposed to Partition and cohorts born in late 1970s were more exposed to the sharp rise in Hindu–Muslim riots beginning in the mid 1980s. Our sample therefore largely captures a comparatively low-violence post-Partition period. Because our estimation sample largely excludes cohorts whose childhood coincided with the sharp escalation in Hindu–Muslim violence beginning in the mid-1980s, our estimates are identified primarily from variation during a comparatively low-violence period. If the long-run effects of communal violence are larger under more frequent or intense exposure, our estimates may therefore understate the effects experienced by cohorts exposed during periods of extreme violence.

Linking the historical riot records to LASI requires a substantial geographic harmonization exercise. The riot data report locations using the administrative district boundaries in force at the time of each event, beginning in 1950, whereas respondents' childhood locations in LASI are coded to the district geography used in Wave I, conducted in 2017–2018. Over this period, India's administrative geography changed considerably: districts were repeatedly split, merged, renamed, and, in some cases, transferred across states. As a result, a

historical district recorded in the riot data may correspond to multiple districts in the LASI geography, while some LASI districts were formed from territory belonging to more than one historical district. Historical riot locations therefore cannot be linked to respondents' childhood districts using a simple name or code match.

We address this problem by constructing a hand-coded historical district crosswalk. To the best of our knowledge, no existing crosswalk systematically maps the historical districts recorded in the riot data to the district geography used in LASI Wave I. We first recover the 636 districts represented in LASI and treat this 2017–2018 district coding as the fixed target geography. We then reconstruct the administrative history of every historical district appearing in the riot data using the India State and District Evolution Database (Jolad et al., 2026). Our assignment procedure is conservative. Historical districts with unchanged boundaries are matched directly. When a historical district subsequently split into multiple districts, we assign the riot to all descendants formed entirely from the historical district's territory. We exclude descendants formed by combining territory from multiple historical districts. When the historical district cannot be matched directly, we proceed sequentially using the reported town or city, then the village, and, where necessary, geographic information contained in the underlying newspaper report.

This harmonization is an important component of the data construction rather than a mechanical merge. Of the 1,290 riot-location records, 1,109 can be mapped using direct or clean historical district relationships, while 181 require some form of manual resolution. Among these, 55 are resolved using contextual geographic information in the underlying newspaper reports and 45 involve the exclusion of a mixed-origin descendant district. Importantly, 35 records cannot be located with sufficient confidence even after exhausting the district, town or village, and newspaper information. We leave these observations unassigned rather than impose an arbitrary geographic match. Every non-standard decision is recorded with a coding flag and written justification. Appendix B describes the harmonization procedure

in detail.

Table 1 reports summary statistics for the estimation sample. Major depressive disorder is present among 8.2 percent of women in the sample. Childhood exposure to communal violence is relatively common: 38 percent of women experienced at least one Hindu–Muslim riot in their childhood district between birth and age 14. The sample also reflects the predominantly rural and low-education environments in which these cohorts grew up. About 80 percent of women lived in rural areas during childhood, while 87 percent of mothers and 71 percent of fathers had never attended school. Hindus constitute 81 percent of the sample and Muslims 12 percent.

4 Empirical Strategy

We exploit childhood-district-by-birth-year variation in exposure to Hindu–Muslim riots to estimate the effect of childhood communal violence on mental health later in life. Women who grew up in the same district but were born in different years may experience different levels of exposure because a riot can fall within the childhood years of one birth cohort but outside those of another. Similarly, women from the same birth cohort may differ in exposure because riots occurred in some childhood districts but not others. Similar district-by-cohort variation has been used in previous work studying the long-run consequences of Hindu–Muslim riots in India (Fisman et al., 2020; Bharti and Roy, 2023).

Let r_{dt} denote the number of Hindu–Muslim riots occurring in district d in year t. For a woman born in year b, we define cumulative childhood riot exposure as

$$N_{db} = \sum_{a=0}^{14} r_{d,b+a}, \tag{1}$$

where ages 0 through 14 correspond to the period for which LASI identifies the district in

which the respondent spent most of her childhood. Our primary measure of exposure is the extensive margin,

$$D_{db} = \mathbf{1}\{N_{db} \geq 1\}, \tag{2}$$

which equals one if at least one Hindu–Muslim riot occurred in the respondent's childhood district between birth and age 14. Our main econometric specification is

$$Y_{idb} = \alpha + \beta D_{db} + X_i'\gamma + \delta_d + \lambda_b + \varepsilon_{idb}, \tag{3}$$

where Y_{idb} is an indicator equal to one if woman i, who spent most of her childhood in district d and was born in year b, meets the criteria for major depressive disorder (MDD). D_{db} is an indicator equal to one if the woman was exposed to at least one Hindu–Muslim riot in her childhood district between birth and age 14. The vector X_i includes controls for religion, caste, mother's education, father's education, and type of residence (urban/rural) during childhood. δ_d denotes childhood-district fixed effects, and λ_b denotes birth-cohort fixed effects. β is our coefficient of interest capturing the the average difference in the probability of later-life MDD between women who were exposed to communal riots during childhood and those who were not, conditional on the included covariates and fixed effects. Because treatment varies at the childhood-district-by-birth-cohort level and exposure windows overlap across nearby birth cohorts, we cluster standard errors at the childhood-district level. All regressions are weighted using the LASI population weights.

The childhood-district fixed effects absorb persistent differences across places that may be related to both communal violence and later-life mental health. This is particularly important in our setting because Hindu–Muslim violence is highly geographically concentrated and cannot be explained simply by differences in local religious composition. For example, Varsh-

ney et al. (2002) documents cities with similar Hindu–Muslim population shares but sharply different histories of communal violence, emphasizing the importance of persistent differences in local inter-communal institutions and civic networks. District fixed effects therefore absorb these and other time-invariant differences across places, including persistent differences in economic conditions, institutions, demographic composition, and the underlying propensity for communal violence. Birth-year fixed effects absorb factors common to women born in the same year, including nationwide economic, political, and public-health conditions that may independently affect mental health later in life. Identification therefore comes from differences in childhood riot exposure across birth cohorts within the same district and across districts within the same birth cohort, net of persistent district-level heterogeneity and factors common to a given birth cohort.

This research design relies on the assumption that, conditional on childhood-district and birth-year fixed effects and the covariates in X_i, district-by-cohort variation in childhood riot exposure is uncorrelated with other determinants of later-life mental health. In particular, there should be no unobserved district-by-cohort factors that both affect the likelihood of communal violence and independently shape the later-life mental health of the cohorts growing up at the time. The design also requires that exposure in one district does not generate economically meaningful spillovers to individuals assigned to other districts, and that households do not systematically sort across districts in anticipation of future communal violence in ways related to potential mental-health outcomes. The next section presents several validity checks related to these concerns.

Equation (3), uses the binary exposure indicator D_{db}. This specification captures the extensive margin of childhood exposure—whether a woman experienced at least one riot between birth and age 14—but does not distinguish between exposure to a single riot event and repeated episodes of communal violence. We therefore also examine whether later-life mental health varies with the cumulative intensity of childhood riot exposure. To do so, we define

$$L_{db} = \log(1 + N_{db}), \tag{4}$$

and estimate

$$Y_{idb} = \alpha + \beta_L L_{db} + X_i'\gamma + \delta_d + \lambda_b + \varepsilon_{idb}. \tag{5}$$

Because the distribution of cumulative riot exposure is highly right-skewed, we use $\log(1 + N_{db})$ rather than the raw number of riots. The logarithmic transformation reduces the influence of the relatively small number of district-cohort cells with very high riot exposure and allows the marginal association of an additional riot to diminish as cumulative exposure increases. Adding one before taking logs allows us to retain women with zero childhood exposure. The coefficient β_L therefore captures how later-life MDD varies with the intensity of cumulative childhood riot exposure.

5 Results

5.1 Main Results

Table 2 reports our main estimates of the relationship between childhood exposure to Hindu–Muslim riots and major depressive disorder (MDD) later in life. Panels A and B report separate regressions for the extensive and intensive margins of childhood exposure, respectively. Panel A uses an indicator for exposure to at least one riot between birth and age 14, while Panel B uses the log of one plus cumulative riot exposure over the same period. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are weighted using LASI population weights, and standard errors are clustered at the childhood-district level.

Panel A shows a sizeable association between childhood riot exposure and later-life mental health. Exposure to at least one Hindu–Muslim riot during childhood is associated with a 2.4 percentage point higher probability of MDD, with the estimate statistically significant at the 5 percent level. Relative to the weighted MDD prevalence of 8.22 percent in the estimation sample, this corresponds to approximately 29 percent of the outcome mean. The inclusion of childhood-district and birth-year fixed effects means that the estimate is based on differences in exposure across birth cohorts within districts, after accounting for persistent differences across districts and factors common to birth cohorts.

Panel B shows that the association also varies with the intensity of exposure. The coefficient on $\log(1 + \text{Cumulative Riots})$ is 0.020 and is statistically significant at the 1 percent level. Given the logarithmic specification, the marginal association declines with prior exposure. Moving from zero to one riot is associated with a 1.4 percentage point higher probability of MDD, or about 17 percent of the outcome mean; moving from one to two riots is associated with a further 0.8 percentage point increase.[8] The results therefore point to an adverse relationship not only with whether a woman was exposed to communal violence during childhood, but also with the amount of violence to which she was exposed.

5.1.1 Alternative Definition of Treatment

We also consider a more flexible definition of treatment that does not impose the logarithmic functional form in equation (5). Among women exposed to at least one riot during childhood, we divide cumulative childhood riot exposure, N_{db}, into terciles and construct indicators for low, medium, and high exposure. Women who experienced no riots during childhood form the comparison group. All other estimation details follow the baseline specification in

[8] With an estimated coefficient of $\hat{\beta} = 0.020$, the predicted change in MDD associated with moving from R_0 to R_1 cumulative riots is $0.020[\log(1 + R_1) - \log(1 + R_0)]$. Thus, moving from zero to one riot implies an increase of $0.020 \log(2) = 0.0139$, or approximately 1.4 percentage points. Relative to an outcome mean of 0.082, this corresponds to approximately 17 percent. Moving from one to two riots implies an additional increase of $0.020[\log(3) - \log(2)] = 0.020 \log(3/2) = 0.0081$, or approximately 0.8 percentage points.

equation (3). Results are reported in Appendix Table A1.

Relative to women with no childhood riot exposure, the probability of MDD is 2.4 percentage points higher for women in the low-exposure group, 2.3 percentage points higher for those in the medium-exposure group, and 3.3 percentage points higher for those in the high-exposure group. Although the estimates for the low- and medium-exposure groups are similar, the largest association is observed among women with the greatest cumulative exposure. These results show that the intensive-margin finding is not driven by the logarithmic transformation and that greater childhood exposure to communal violence is associated with worse mental health later in life.

To put the magnitude of these estimates in perspective, the 2.4 percentage point effect of binary treatment corresponds to approximately 29 percent of the mean prevalence of MDD in our sample. This magnitude is comparable to estimates in the small literature examining the long-run mental-health effects of childhood exposure to conflict. Singhal (2019), using an instrumental-variables strategy based on bombing intensity during the Vietnam War, finds that a 1 percent increase in bombing intensity increases the probability of severe mental distress in adulthood by 16 percentage points. Relative to a sample mean of 31 percent, this represents an increase of approximately 52 percent. Akbulut-Yuksel et al. (2025), using variation in Allied bombing intensity during World War II, finds that a one-standard-deviation increase in bombing exposure increases the probability of clinical depression later in life by 3.3 percentage points, or 16.2 percent relative to the outcome mean. Our estimate of 29 percent of the MDD mean lies between these two benchmarks. Although the nature and intensity of conflict, measures of exposure, and mental-health outcomes differ across these settings, the comparison indicates that the magnitude of the effect we estimate is economically meaningful and within the range documented in the existing literature.

5.2 Identification Checks

Our identifying assumption is that, conditional on childhood-district and birth-year fixed effects and the individual-level controls, variation in childhood riot exposure across childhood district-by-birth-cohort cells is unrelated to other determinants of later-life mental health. The main concern is therefore that districts experiencing riots at particular points in time may also be experiencing other changes that affect the cohorts growing up there. We address this concern in four ways in Table 3. Column (1) reports the baseline specification. Column (2) adds state-specific linear cohort trends, allowing states to follow different trajectories across birth cohorts. Column (3) goes further and adds district-specific linear cohort trends, allowing each childhood district to have its own underlying trend. Column (4) controls for the mean annual rainfall shock experienced between birth and age 14, defined as the district-year z-score of total annual rainfall relative to a trailing 50-year district-specific baseline. This is relevant in India because rainfall shocks have been shown to predict communal violence and may also affect childhood economic conditions through channels other than riots (Sarsons, 2015; Pailler and Tsaneva, 2018). Finally, column (5) allows local demographic conditions to evolve differently across the 1950–1976 birth cohorts by interacting birth year with cohort-specific district sex ratios, Hindu and Muslim population shares, and Scheduled Caste and Scheduled Tribe population shares. For each birth cohort, these district characteristics are drawn from the most recent Census available prior to that cohort's birth.

The estimates are stable across all four checks. For the binary measure of riot exposure, the coefficient ranges from 0.024 in the baseline specification to 0.028 with state- or district-specific trends, and returns to 0.024 when we control for childhood rainfall shocks or cohort-specific district characteristics. For cumulative exposure, the corresponding coefficients range from 0.020 to 0.024. All estimates remain positive and statistically significant. Thus, the main results are not sensitive to allowing for differential state or district trends, rainfall shocks over the childhood period, or changes in observable district composition across cohorts.

While these exercises cannot rule out all unobserved district-by-cohort shocks, they address the main concern that the timing of riot exposure is proxying for other changes in the local childhood environment.

As a falsification exercise, we examine riot activity in the respondent's childhood district during the 14 years preceding her birth. Because these riots occurred before the respondent was born, they cannot represent direct childhood exposure. This test addresses the concern that childhood riot exposure may proxy for persistent, time-varying conditions in the district that extend across nearby cohorts and are independently related to later-life mental health. If such local conditions, rather than exposure during childhood itself, were driving the baseline relationship, riot activity immediately preceding birth should also predict later-life MDD. Table 4 provides little evidence of this pattern. Pre-birth riot activity is not significantly associated with MDD. Moreover, when pre-birth riot activity and childhood riot exposure are entered jointly, the coefficient on childhood exposure remains positive and statistically significant, while the coefficient on pre-birth riot activity is small and statistically insignificant. These results weigh against the interpretation that our baseline estimates are driven by persistent time-varying local conditions surrounding the respondent's birth cohort rather than by exposure to communal violence during childhood.

As an additional falsification exercise, we randomly permute the MDD outcome and, separately, the riot-exposure measures and re-estimate the baseline specifications. These permutations break the systematic relationship between childhood riot exposure and later-life mental health while preserving the empirical distributions of the variables being randomized. Appendix Figure A1 shows that the coefficients are insignificant in most of permutations. This exercise provides additional evidence that the baseline results are not driven by spurious correlations generated by the empirical specification.

5.3 Robustness Checks

We next address several concerns about the measurement of childhood riot exposure and the definition of the estimation sample. One concern is that the intensive-margin estimate may be disproportionately influenced by the small number of women with very high cumulative exposure. We therefore first winsorize cumulative riot exposure at the 90th and 95th percentiles, which limits the influence of extreme observations while retaining the full estimation sample. A related concern is that these observations may represent unusually exposed district–cohort cells for which the estimated relationship is not representative of the rest of the sample. We therefore also trim the exposure distribution, excluding observations outside the 1st–99th and 5th–95th percentiles. Table 5 shows that the results are largely unchanged under both approaches. The intensive-margin coefficient is 0.023 when exposure is winsorized at the 90th percentile and 0.020 at the 95th percentile, compared with 0.020 in the baseline. Trimming yields coefficients of 0.022 and 0.019, while the extensive-margin estimate remains 0.024 under both trimming rules. These results indicate that the main findings are not driven by a small number of observations with unusually high levels of childhood riot exposure.

A second concern is selective mortality. Because LASI surveys individuals in later adulthood, women most adversely affected by childhood violence may be less likely to survive into the estimation sample, potentially attenuating or otherwise distorting the estimated effect. We therefore restrict the sample to women ages 45–60, who have experienced substantially less cumulative mortality than older respondents. The coefficients remain positive and are somewhat larger than in the full sample: 0.035 at the extensive margin and 0.029 at the intensive margin, compared with baseline estimates of 0.024 and 0.020. Although this exercise cannot eliminate survivor-selection concerns, the persistence of the effect among younger respondents makes selective mortality an unlikely explanation for the main result.

A third concern is that the estimates may be disproportionately influenced by the riot history

of a particular state. We therefore re-estimate the baseline specification repeatedly, excluding one state or union territory at a time. Appendix Table A2 shows that the extensive-margin coefficient remains positive in all 36 leave-one-out samples, ranging from 0.013 to 0.034. The intensive-margin estimates range from 0.015 to 0.024 and remain statistically significant in every specification. The results are therefore not driven by any single state.

Finally, because MDD is a binary outcome, we examine whether the results depend on estimating a linear probability model. Appendix Table A3 reports average marginal effects from probit and logit specifications. The extensive-margin estimates are 0.024 and 0.025, respectively, while the intensive-margin estimates are 0.021 and 0.023. These estimates closely match the baseline results, indicating that the findings are not sensitive to the functional form used for the binary outcome.

5.4 Depressive Symptoms

Our primary outcome, major depressive disorder (MDD), combines several symptoms into a single diagnostic indicator. We therefore examine the seven underlying symptoms separately to assess whether the estimated effect of childhood riot exposure is concentrated in a particular component of the MDD measure or reflects a broader deterioration in mental health. Table 6 reports the results. Each column replaces MDD with an indicator for one of the seven symptoms, while leaving the baseline specification unchanged. Panel A uses the extensive-margin measure of childhood riot exposure and Panel B uses the intensive-margin measure.

The estimates are positive for all seven symptoms. In Panel A, childhood riot exposure is significantly associated with loss of interest, loss of appetite, difficulty concentrating, feelings of worthlessness, and trouble sleeping. The coefficients for low energy and thoughts of death are also positive, though imprecisely estimated. The pattern is stronger in Panel B:

cumulative riot exposure is positively and significantly associated with all seven symptoms. The MDD result therefore does not appear to be driven by a single component of the diagnostic measure. Instead, childhood exposure to communal violence is associated with a broad-based increase in depressive symptoms later in life.

5.5 Age at Exposure

The effect of exposure to communal violence may depend on when during childhood the exposure occurs. The birth-to-age-14 window spans distinct stages of development, and a large literature shows that shocks experienced at different ages can have different consequences for subsequent outcomes (Cunha and Heckman, 2007; Currie and Almond, 2011; Almond et al., 2018). This possibility is especially relevant for communal violence, since exposure during different stages of childhood may affect individuals at different points in their physical, cognitive, and socio-emotional development. We therefore examine whether the later-life mental-health effects of communal violence vary with age at exposure.

To do so, we divide the birth-to-age-14 exposure window into three five-year intervals: ages 0–4, 5–9, and 10–14. Let $\mathcal{A}_1 = \{0, \ldots, 4\}$, $\mathcal{A}_2 = \{5, \ldots, 9\}$, and $\mathcal{A}_3 = \{10, \ldots, 14\}$. For a woman born in year b and raised in district d, cumulative riot exposure during age interval $\mathcal{A}_j$ is

$$N_{db}^{(j)} = \sum_{a \in \mathcal{A}_j} r_{d,b+a}, \tag{6}$$

where r_{dt} denotes the number of riots occurring in district d in year t. For each interval, we construct the same two measures of exposure used in the main analysis:

$$D_{db}^{(j)} = \mathbf{1}\{N_{db}^{(j)} \geq 1\}, \qquad L_{db}^{(j)} = \log\left(1 + N_{db}^{(j)}\right). \tag{7}$$

Rather than entering a single measure of riot exposure accumulated between birth and age 14, we enter the three age-specific measures jointly:

$$MDD_{idb} = \sum_{j=1}^{3} \beta_j E_{db}^{(j)} + X_i'\gamma + \alpha_d + \lambda_b + \varepsilon_{idb}, \tag{8}$$

where $E_{db}^{(j)}$ denotes either $D_{db}^{(j)}$ or $L_{db}^{(j)}$. The specification is otherwise identical to the baseline: we include the same individual-level controls and childhood-district and birth-year fixed effects, use LASI population weights, and cluster standard errors at the childhood-district level. The only change is that the aggregate measure of riot exposure between birth and age 14 is replaced by three age-specific measures.

In addition to the joint specification, we also estimate the effect of exposure in each age interval separately. The two approaches use different comparison groups and therefore answer slightly different questions. In the joint specification, exposure in all three age intervals enters simultaneously, so the coefficient for a given interval compares women who were exposed during that interval with women who were not, holding exposure during the other two intervals fixed. For example, the coefficient on exposure at ages 0–4 captures the association between early-childhood exposure and MDD conditional on exposure at ages 5–9 and 10–14. In the separate specifications, by contrast, each age-specific exposure measure enters on its own. The comparison group therefore consists of all women who were not exposed during the interval under consideration, including women who may have experienced riots at other ages. The joint specification more directly isolates exposure during a particular developmental window, while the separate specifications provide a complementary estimate that does not condition on exposure elsewhere in childhood.

Table 7 reports the results. Column (1) includes exposure during all three age intervals jointly. At the extensive margin, exposure at ages 0–4 is associated with a 2.2 percentage point increase in the probability of later-life MDD, while exposure at ages 10–14 is associated

with a 1.8 percentage point increase; both estimates are statistically significant at the 5 percent level. In contrast, the coefficient for exposure at ages 5–9 is smaller, at 0.7 percentage points, and is not statistically significant. Panel B shows a similar ordering at the intensive margin. The coefficients on log cumulative exposure are 0.017 for ages 0–4, 0.008 for ages 5–9, and 0.013 for ages 10–14. The estimates for exposure at ages 0–4 and 10–14 are statistically significant at the 10 percent level, while the estimate for ages 5–9 is smaller and statistically insignificant.

Columns (2)–(4) estimate the three age intervals separately. The extensive-margin coefficients remain largest for exposure at ages 0–4 and 10–14: 0.020 and 0.015, respectively, compared with 0.004 for ages 5–9. The estimates for the youngest and oldest age groups are statistically significant at the 10 percent level. The intensive-margin coefficients exhibit the same ordering, although they are less precisely estimated when the age intervals are considered separately. The similarity in the ranking of the coefficients across the joint and separate specifications suggests that the pattern is not driven solely by how exposure during the other childhood periods is treated.

The estimates do not suggest a monotonic relationship between age at exposure and later-life mental health. Instead, the coefficients are consistently larger for exposure during early childhood and at ages 10–14, and smaller for exposure at ages 5–9. This pattern is consistent with evidence emphasizing early childhood as an important period for subsequent development (Cunha and Heckman, 2007; Currie and Almond, 2011; Almond et al., 2018), as well as work documenting persistent mental-health consequences of adversity experienced around adolescence (Gong et al., 2020; Bharadwaj et al., 2021; Rossin-Slater et al., 2020). We interpret these differences as suggestive rather than as evidence of sharply defined critical periods. The results nevertheless point to early childhood and the transition into adolescence as periods in which exposure to communal violence may have particularly persistent consequences for mental health.

5.6 Heterogeneity

We next examine whether the relationship differ across religion, caste, childhood residence, and parental education. Table 8 reports pooled interaction specifications for each dimension. In each column, the treatment coefficient gives the estimated association for the reference group ($M = 0$), while the interaction term measures whether the association differs for the group indicated by $M = 1$. The row labeled "Coefficient for $M = 1$" reports the implied treatment association for that group.

Columns (1) and (2) provide little evidence of heterogeneity by religion or caste. The interaction terms are statistically insignificant in both panels, indicating no meaningful differences in the effect of childhood riot exposure between Hindu and Muslim women or between marginalized- and upper-caste women.

Columns (4) and (5) likewise show no statistically significant differences by parental schooling. The interaction terms for father's and mother's schooling are statistically insignificant under both treatment definitions. Although the implied association are relatively large for women whose mothers had any schooling—0.045 at the extensive margin and 0.041 at the intensive margin—the interaction terms themselves are not statistically significant. We therefore do not interpret these differences in subgroup point estimates as evidence of systematic heterogeneity by parental education.

Column (3) shows clear heterogeneity by childhood residence. The association is positive and significant for women raised in rural areas, while the interaction with urban residence is negative and significant in both panels. One possible explanation is that rural households had fewer economic and institutional resources to buffer the disruptions caused by communal violence. We interpret this rural–urban difference as suggestive, since our data do not currently allow us to examine the sources of this heterogeneity in greater detail.

5.7 Potential Channels

We examine marriage timing as one potential pathway through which childhood exposure to communal violence may shape women's subsequent life-course trajectories. Early marriage can have persistent consequences for women and their children. Field and Ambrus (2008) show that earlier marriage in Bangladesh reduces women's schooling, literacy, and use of preventive health care. In India, Chari et al. (2017) find that earlier maternal marriage is associated with higher fertility and worse health and educational outcomes for children, while Roychowdhury and Dhamija (2021) document an increase in women's exposure to physical domestic violence.

Table 9 reports the results. At the extensive margin, exposure to at least one riot during childhood increases the probability of marrying before age 18 by 3.2 percentage points and before age 15 by 2.6 percentage points. The estimated effect on age at marriage is also negative, although it is not statistically significant. At the intensive margin, greater cumulative riot exposure increases the probability of marriage before age 18 by 3.8 percentage points and before age 15 by 4.7 percentage points. It is also associated with marrying approximately 0.31 years, or 3.7 months, earlier.[9]

These results suggest that childhood exposure to communal violence can alter women's subsequent life-course trajectories by increasing the likelihood of child marriage and, with greater cumulative exposure, leading to earlier marriage more generally. Given the documented consequences of early marriage for women's human capital, health, and well-being, changes in marriage timing represent one potential channel through which childhood communal violence may have persistent later-life consequences.

[9] Ghose and Pandey (2025) provide related evidence from the 2002 Gujarat riots, showing that riot exposure led women to marry earlier. Their analysis focuses on a single episode of communal violence in one state, whereas we examine childhood exposure to Hindu–Muslim riots across India over several decades.

6 Conclusion

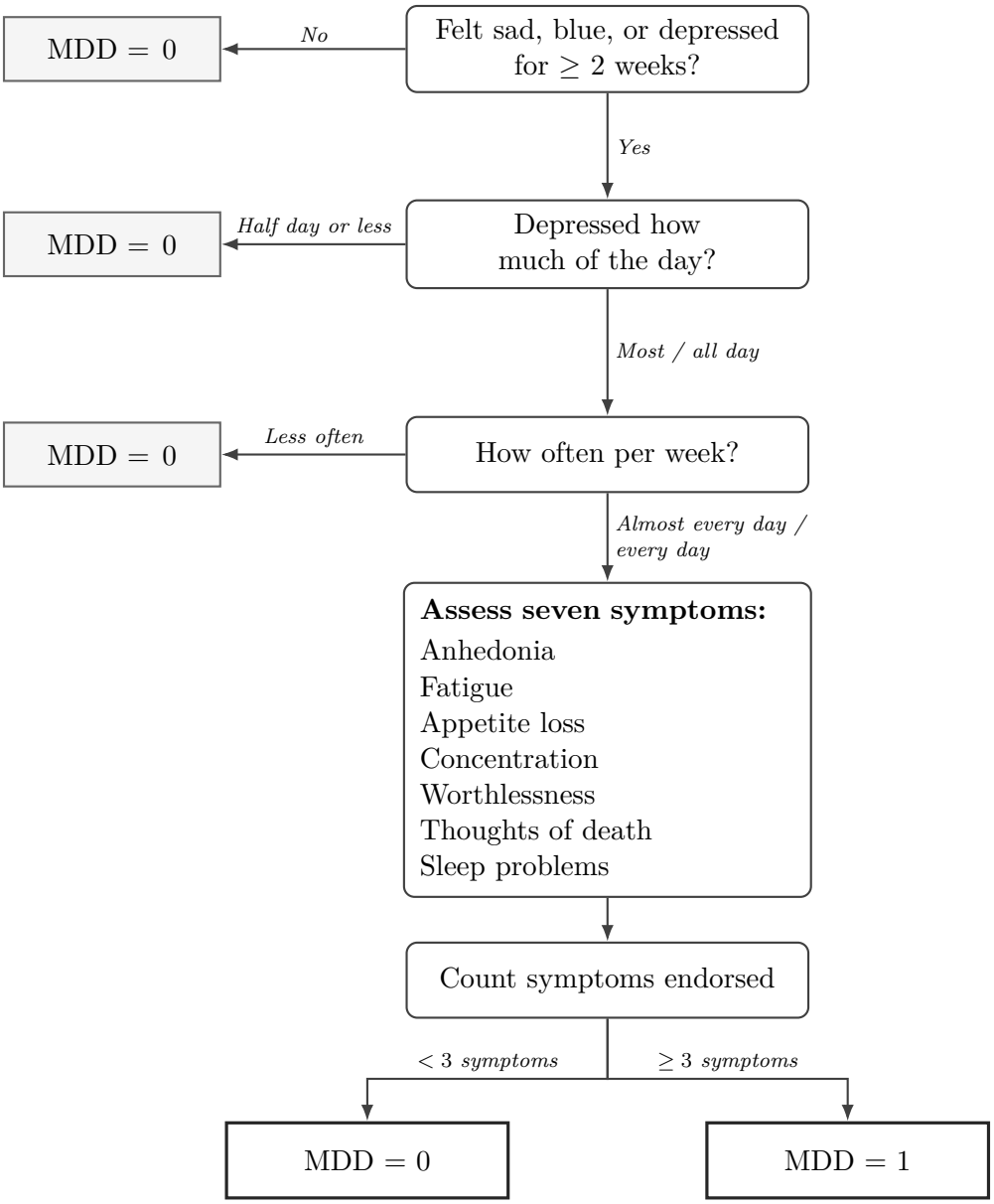
This paper examines whether exposure to Hindu–Muslim communal violence during childhood is associated with women's mental health decades later. We combine LASI with historical riot records and construct a geographic crosswalk that allows us to assign riot exposure using the district in which respondents spent most of their childhood. Our empirical design exploits variation in the timing of riots across childhood districts and birth cohorts, while controlling for childhood-district fixed effects, birth-year fixed effects, and individual-level characteristics.

We find a substantial and persistent relationship between childhood communal violence and major depressive disorder in older adulthood. Exposure to at least one riot between birth and age 14 is associated with a 2.4 percentage point higher probability of MDD, approximately 29 percent of the weighted outcome mean. Greater cumulative exposure is also associated with worse mental health, and the result is visible across a broad range of depressive symptoms rather than being concentrated in a single component of the diagnostic measure. The estimates remain similar across specifications that allow for differential state and district cohort trends, account for childhood rainfall shocks, and control for changes in observable district characteristics across cohorts.

The results also point to potentially important differences in the timing and consequences of exposure. Estimates are largest for violence experienced during early childhood and at ages 10–14 and smaller for exposure during ages 5–9, suggesting that developmental timing may matter for the persistence of childhood shocks. We also find that women exposed to communal riots during childhood tend to marry earlier. Although these estimates do not establish marriage timing as a causal mediator, they suggest that communal violence may shape later-life outcomes through changes in both psychological well-being and subsequent life-course trajectories.

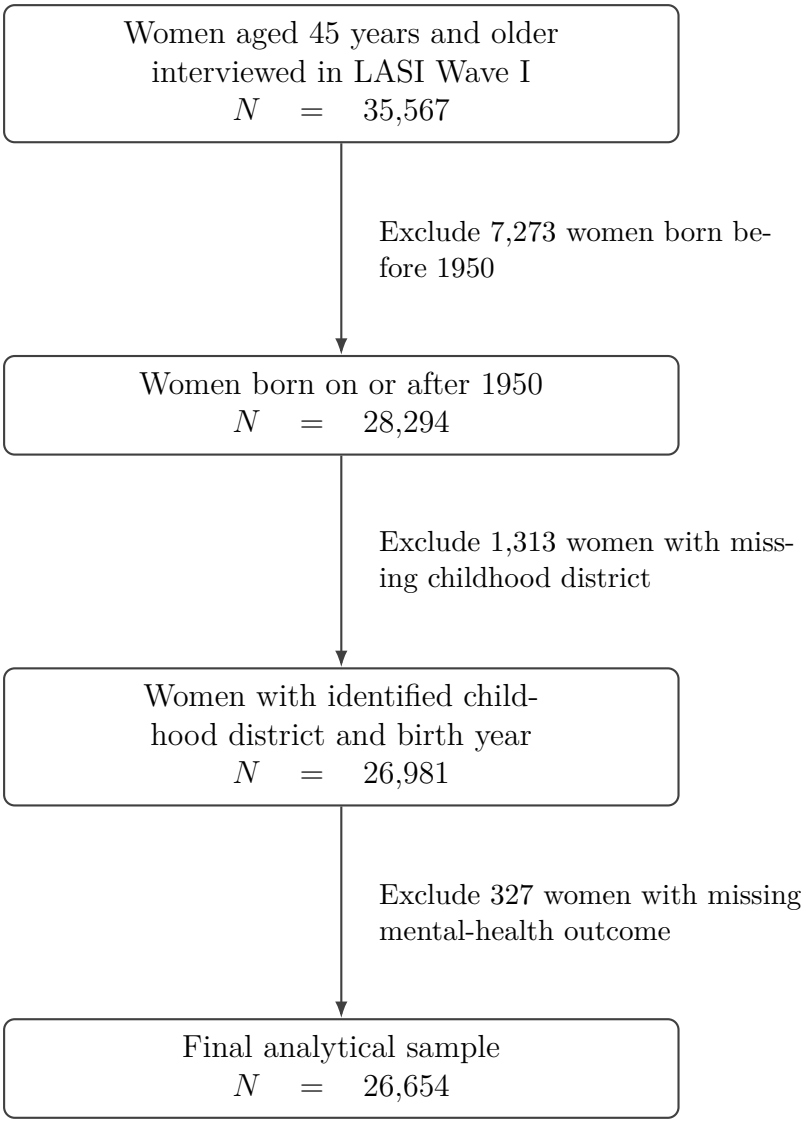
These findings broaden the way the costs of communal violence are typically measured. Deaths, injuries, displacement, and property destruction capture important immediate consequences of violence, but they may substantially understate its total social cost if exposure during childhood remains associated with mental health many decades later. This is particularly important in settings such as India, where communal violence is often localized and short-lived and may therefore appear transitory in conventional measures of conflict. Our results suggest instead that even brief episodes of violence can cast a long shadow: the consequences experienced by children may remain visible well into older adulthood.

Figure 1: CIDI-SF Depression Algorithm in LASI



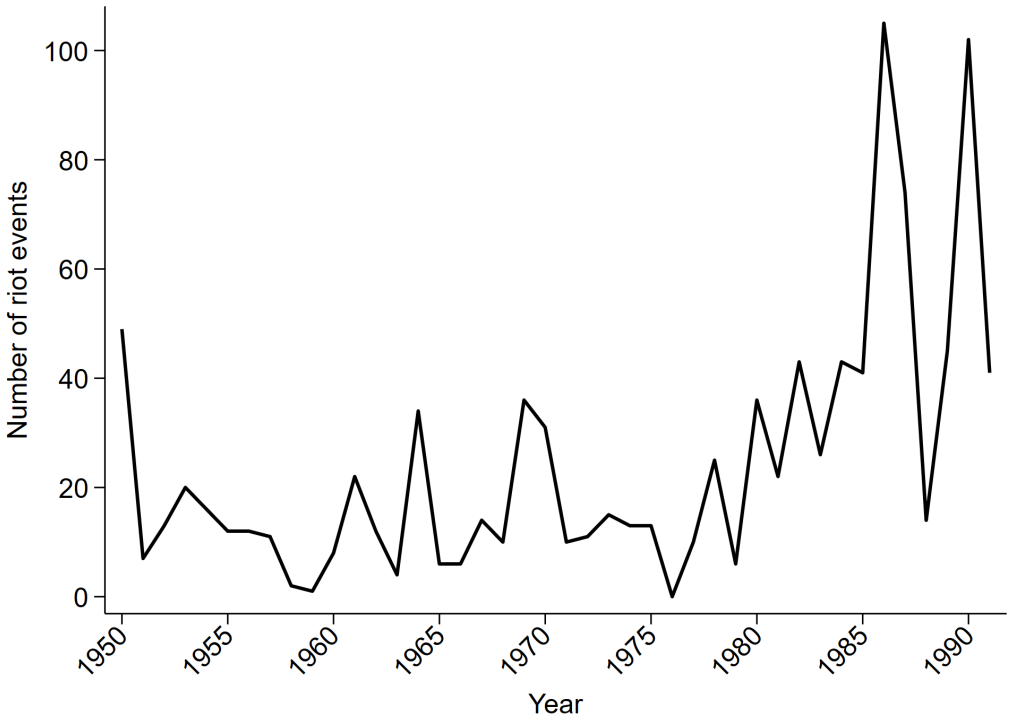
Notes: MDD denotes major depressive disorder. The figure summarizes the CIDI-SF branching structure used to construct the MDD indicator in LASI. Respondents who fail any of the three screening questions are coded MDD = 0 and are not asked the symptom battery. Among those who clear all three screens, endorsement of at least three of the seven symptoms is coded MDD = 1.

Figure 2: Sample Construction



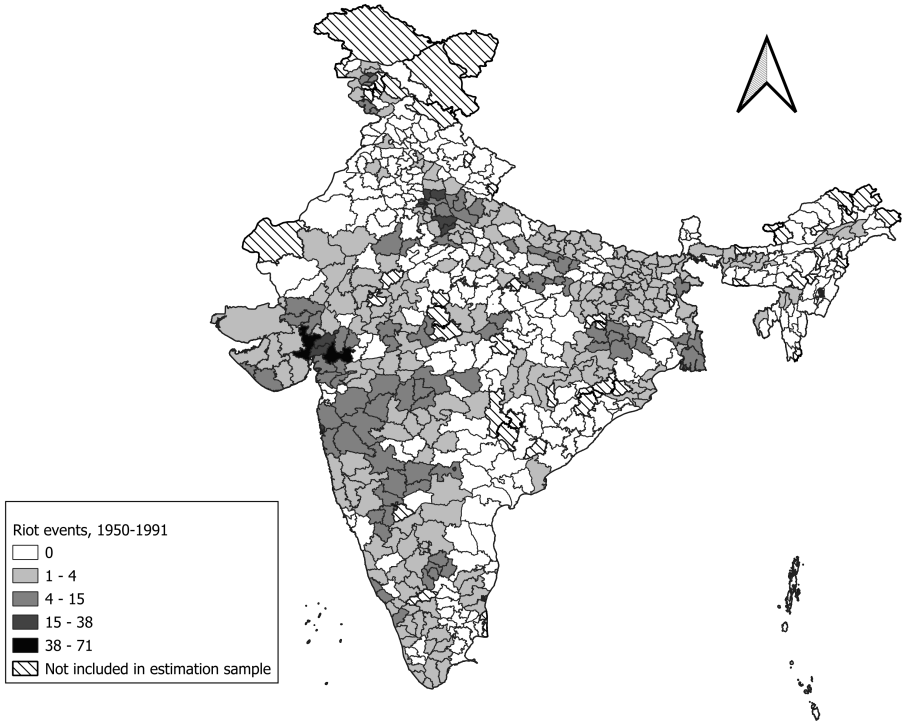
Notes: The sample begins with all women aged 45 years and older interviewed in LASI Wave I. Women born before 1950 are excluded to focus on post-Partition birth cohorts. Respondents are further excluded if childhood district cannot be identified, or if information required to construct the mental-health outcome is missing.

Figure 3: Temporal Variation in Hindu–Muslim Riots



Notes: The figure plots the annual number of recorded Hindu–Muslim riots between 1950 and 1991 that contribute to childhood riot exposure in the estimation sample. Riot data are drawn from the Varshney–Wilkinson database.

Figure 4: Geographic Variation in Hindu–Muslim Riots



Notes: The figure shows the geographic distribution of Hindu–Muslim riots between 1950 and 1991 across districts represented in the estimation sample. Darker shading indicates a greater cumulative number of recorded riots over the period. Riot data are drawn from the Varshney–Wilkinson database.

Table 1: Summary Statistics

Variable	N	Mean / %	Std. Dev.	Min	Max
Panel A. Outcome					
Major depressive disorder					
Yes	1,847	8.22	27.46	0	1
No	24,780	91.78	27.46	0	1
Panel B. Childhood Riot Exposure					
Extensive margin: Riot exposure					
Yes	7,674	37.92	48.52	0	1
No	18,953	62.08	48.52	0	1
Intensive margin: Cumulative Riots	26,627	1.14	2.44	0	25
Panel C. Demographic and Socioeconomic Characteristics					
Mother's education					
Never attended school	22,830	87.14	33.48	0	1
Primary (Grade 1–7)	2,296	7.47	26.29	0	1
Middle/Secondary (Grade 8–11)	661	2.67	16.12	0	1
Higher secondary and above	123	0.36	5.99	0	1
Missing	717	2.36	15.19	0	1
Father's education					
Never attended school	18,425	70.62	45.55	0	1
Primary (Grade 1–7)	4,377	15.32	36.02	0	1
Middle/Secondary (Grade 8–11)	2,159	7.66	26.60	0	1
Higher secondary and above	776	3.66	18.78	0	1
Missing	890	2.73	16.30	0	1
Religion					
Hindu	19,601	81.26	39.02	0	1
Muslim	3,145	11.74	32.19	0	1
Other	3,879	6.99	25.51	0	1
Missing	2	0.00	0.59	0	1
Caste					
Scheduled Tribe (ST)	4,521	8.58	28.01	0	1
Scheduled Caste (SC)	4,568	19.60	39.70	0	1
Other Backward Class (OBC)	10,125	45.19	49.77	0	1
General	6,485	23.83	42.61	0	1
No caste/tribe	714	2.27	14.91	0	1
Don't know/Refused	214	0.53	7.24	0	1
Panel D. Childhood Characteristics					
Childhood residence: rural					
Yes	21,276	80.44	39.67	0	1
No	5,351	19.56	39.67	0	1

Notes: The sample consists of women born between 1950 and the mid-1970s. All means and standard deviations are population weighted. Childhood riot exposure is measured from birth through age 14 (before turning 15). The extensive-margin treatment indicates exposure to at least one riot, while the intensive-margin treatment is $\log(1+N_{db})$, where N_{db} is the cumulative number of riots experienced during childhood.

Table 2: Main Results: Childhood Exposure to Riots and Depression in Later-Life

	Major Depressive Disorder
Panel A. Extensive Margin	
Riot exposure	0.024^{**}
	(0.012)
Panel B. Intensive Margin	
$\log(1 + \text{Cumulative Riots})$	0.020^{***}
	(0.007)
Observations	26,627

Notes: The dependent variable is an indicator for major depressive disorder (MDD). The childhood exposure window is defined from birth to age 14 (before turning 15). Panels A and B report separate regressions using different treatment definitions. Panel A defines treatment at the extensive margin: *Riot Exposure* is an indicator equal to one if the respondent was exposed to at least one Hindu–Muslim riot during the childhood exposure window. Panel B defines treatment at the intensive margin as $\log(1 + N_{db})$, where N_{db} denotes the cumulative number of riots during the childhood exposure window. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Identification Checks

	Major Depressive Disorder				
	(1)	(2)	(3)	(4)	(5)
Panel A: Extensive Margin					
Riot Exposure	0.024**	0.029**	0.028**	0.025**	0.024**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Panel B: Intensive Margin					
log(1 + Cumulative Riots)	0.020***	0.024***	0.024**	0.021***	0.020***
	(0.007)	(0.008)	(0.013)	(0.007)	(0.007)
Observations	26,627	26,627	26,627	26,198	24,985
Childhood-district FE	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	No	Yes	No	No	No
District-specific linear trends	No	No	Yes	No	No
Childhood annual rainfall shock	No	No	No	Yes	No
Cohort-specific district characteristics × birth year	No	No	No	No	Yes

Notes: The dependent variable is an indicator for major depressive disorder (MDD). Each panel–column combination reports estimates from a separate regression. In Panel A, *Riot Exposure* is an indicator equal to one if the respondent was exposed to at least one Hindu–Muslim riot day in her childhood district between birth and age 14. In Panel B, treatment is $\log(1 + N_{db})$, where N_{db} is the cumulative number of riot days in the respondent's childhood district over the same exposure window. Column (1) reports the baseline specification, which includes religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Column (2) adds state-specific linear trends by interacting state indicators with birth year. Column (3) instead adds district-specific linear trends by interacting childhood-district indicators with birth year. Column (4) controls for childhood rainfall conditions. For each district-year, annual rainfall is standardized relative to the district's mean and standard deviation of annual rainfall over the preceding 50 years; the resulting standardized shocks are then averaged over ages 0–14. Districts whose centroids are more than 100 km from their nearest rainfall grid cell are excluded from this specification. Column (5) controls flexibly for pre-existing district characteristics by interacting birth year with the district sex ratio and the Hindu, Muslim, Scheduled Caste, and Scheduled Tribe population shares measured in the Census immediately preceding the respondent's birth cohort. The 1951 Census is assigned to women born in 1950–1960, the 1961 Census to those born in 1961–1970, and the 1971 Census to those born in 1971–1980. All regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Placebo Test: Riot Activity in the Pre-Birth Window

	Major Depressive Disorder	
Panel A. Extensive Margin		
Riot Activity in Pre-Birth Window	-0.020	0.000
	(0.019)	(0.014)
Childhood Riot Exposure		0.031**
		(0.015)
Panel B. Intensive Margin		
$\log(1 + \text{Riot Activity in Pre-Birth Window})$	-0.028	-0.002
	(0.018)	(0.011)
$\log(1 + \text{Cumulative Childhood Riot Exposure})$		0.025***
		(0.009)
Observations	11,270	25,820

Notes: The dependent variable is an indicator for major depressive disorder (MDD). Childhood riot exposure is measured from birth through age 14. The placebo measure captures riot activity in the respondent's childhood district during the 14 years immediately preceding her birth. In Panel A, the placebo measure is an indicator equal to one if at least one Hindu–Muslim riot occurred during the pre-birth window; childhood riot exposure is defined analogously over ages 0–14. In Panel B, the corresponding measures are the log of one plus cumulative riot activity over each window. Column (1) includes only the placebo measure, while Column (2) jointly includes the placebo measure and childhood riot exposure. The pre-birth analysis is restricted to cohorts for which the full 14-year pre-birth window is observed. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Robustness Checks

	Major Depressive Disorder					
	(1) Baseline	(2) Winsorized 90th pct.	(3) Winsorized 95th pct.	(4) Trimmed 1%	(5) Trimmed 5%	(6) Age 45–60
Panel A. Extensive Margin						
Riot Exposure	0.024^{**}	–	–	0.024^{**}	0.024^{**}	0.035^{*}
	(0.012)			(0.012)	(0.012)	(0.019)
Panel B. Intensive Margin						
$\log(1 + \text{Cumulative Riots})$	0.020^{***}	0.023^{**}	0.020^{***}	0.022^{***}	0.019^{**}	0.029^{**}
	(0.007)	(0.010)	(0.007)	(0.007)	(0.009)	(0.011)
Observations	26,627	26,627	26,627	26,361	25,536	19,624

Notes: The dependent variable is an indicator for major depressive disorder (MDD). Each panel–column combination reports estimates from a separate regression. Column (1) reports the baseline specification. Columns (2) and (3) winsorize cumulative childhood riot exposure at the 90th and 95th percentiles, respectively; because winsorization changes only the intensive-margin treatment, these specifications are reported only in Panel B. Columns (4) and (5) restrict the sample to observations between the 1st–99th and 5th–95th percentiles of the cumulative childhood riot-exposure distribution, respectively. Column (6) restricts the sample to women ages 45–60 to reduce concerns about selective mortality among older respondents. In Panel A, *Riot Exposure* equals one if the respondent was exposed to at least one Hindu–Muslim riot in her childhood district between birth and age 14. In Panel B, treatment is $\log(1 + N_{db})$, where N_{db} denotes cumulative riot exposure over the same window. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Childhood Exposure to Riots and Depressive Symptoms

	(1) Lost interest	(2) Low energy	(3) Lost appetite	(4) Trouble concentrating	(5) Feeling worthless	(6) Thoughts of death	(7) Trouble sleeping
Panel A. Extensive Margin							
Riot Exposure	0.023**	0.014	0.019**	0.021*	0.022**	0.012	0.022**
	(0.012)	(0.009)	(0.009)	(0.011)	(0.011)	(0.007)	(0.011)
Panel B. Intensive Margin							
log(1 + Cumulative Riots)	0.016*	0.015**	0.018***	0.014*	0.015**	0.010**	0.019***
	(0.008)	(0.006)	(0.006)	(0.007)	(0.007)	(0.005)	(0.007)
Observations	26,627	26,627	26,627	26,627	26,627	26,627	26,627

Notes: Each panel–column combination reports estimates from a separate regression, with the dependent variable indicated at the top of the column. The outcomes are indicators for individual depressive symptoms. Panels A and B report separate regressions using the extensive- and intensive-margin treatment definitions, respectively. In Panel A, *Riot Exposure* is an indicator equal to one if the respondent was exposed to at least one Hindu–Muslim riot day in her childhood district between birth and age 14. In Panel B, treatment is $\log(1 + N_{db})$, where N_{db} is the cumulative number of riot days over the same exposure window. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 7: Age at Childhood Exposure

	Joint Specification	Separate Specifications		
	(1)	(2)	(3)	(4)
Panel A. Extensive Margin				
Riot exposure, ages 0–4	0.022**	0.020*		
	(0.010)	(0.010)		
Riot exposure, ages 5–9	0.007		0.004	
	(0.008)		(0.008)	
Riot exposure, ages 10–14	0.018**			0.015*
	(0.008)			(0.008)
Panel B. Intensive Margin				
log(1 + Riot Exposure), ages 0–4	0.017*	0.015		
	(0.010)	(0.010)		
log(1 + Riot Exposure), ages 5–9	0.008		0.007	
	(0.007)		(0.008)	
log(1 + Riot Exposure), ages 10–14	0.013*			0.012
	(0.008)			(0.008)
Observations	26,627	26,627	26,627	26,627

Notes: The dependent variable is an indicator for major depressive disorder (MDD). Panel A measures childhood riot exposure at the extensive margin. Each treatment variable is an indicator equal to one if the respondent experienced at least one Hindu–Muslim riot in her childhood district during the indicated age window. Panel B measures exposure at the intensive margin using the log of one plus cumulative riot exposure during the corresponding age window. Column (1) divides childhood into three developmental windows—ages 0–4, 5–9, and 10–14—and includes exposure in all three windows jointly. Columns (2)–(4) estimate exposure during each developmental window separately. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Heterogeneity

	Major Depressive Disorder				
	(1) Religion	(2) Caste	(3) Childhood Residence	(4) Father's Education	(5) Mother's Education
Panel A. Extensive Margin					
Riot Exposure	0.027**	0.033*	0.029**	0.019*	0.021*
	(0.012)	(0.017)	(0.013)	(0.011)	(0.012)
Riot Exposure $\times M$	-0.009	-0.011	-0.024*	0.008	0.024
	(0.016)	(0.013)	(0.012)	(0.012)	(0.017)
Coefficient for $M = 1$	0.019	0.022	0.004	0.027	0.045
p-value	[0.317]	[0.062]	[0.723]	[0.080]	[0.019]
Panel B. Intensive Margin					
log(1 + Cumulative Riots)	0.021***	0.019**	0.026***	0.019**	0.015**
	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)
log(1+Cumulative Riots)$\times M$	-0.010	0.001	-0.019**	-0.004	0.026
	(0.010)	(0.009)	(0.009)	(0.008)	(0.016)
Coefficient for $M = 1$	0.012	0.020	0.007	0.015	0.041
p-value	[0.209]	[0.017]	[0.379]	[0.068]	[0.009]
$M = 1$	Muslim	Marginalised	Urban	Any schooling	Any schooling
Observations	22,736	25,695	26,627	25,736	25,909

Notes: The dependent variable is an indicator for major depressive disorder (MDD). Each panel–column combination reports estimates from a separate pooled interaction regression. Panels A and B use the extensive- and intensive-margin treatment definitions, respectively. M denotes the moderator indicated in each column and equals one for Muslim in Column (1), marginalised caste (Scheduled Tribes/Scheduled Caste/Other Backward Class) , urban childhood residence, father with any schooling, and mother with any schooling, respectively. The corresponding $M = 0$ groups are Hindu, General caste, rural childhood residence, father with no schooling, and mother with no schooling. The treatment coefficient is for $M = 0$, while the interaction coefficient measures the difference in the treatment effect between $M = 1$ and $M = 0$. The row "Coefficient for $M = 1$" reports the sum of the treatment and interaction coefficients, with the corresponding p-value shown in brackets. The childhood exposure window is defined from birth to age 14. All specifications include the baseline controls, excluding the characteristic used to define the corresponding moderator, together with childhood-district and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 9: Potential Channels

	(1) Child Marriage (Before Age 18)	(2) Child Marriage (Before Age 15)	(3) Age at Marriage
Panel A. Extensive Margin			
Riot Exposure	0.032**	0.026*	-0.170
	(0.015)	(0.015)	(0.137)
Panel B. Intensive Margin			
$\log(1 + \text{Cumulative Riots})$	0.038**	0.047***	-0.308**
	(0.016)	(0.015)	(0.156)
Observations	26,483	26,483	26,483

Notes: Each panel–column combination reports estimates from a separate regression, with the dependent variable indicated at the top of the column. In Column (1), child marriage is defined as marriage before age 18, while in Column (2), it is defined as marriage before age 15. Panels A and B use the extensive- and intensive-margin treatment definitions, respectively. In Panel A, *Riot Exposure* is an indicator equal to one if the respondent was exposed to at least one Hindu–Muslim riot during the childhood exposure window. In Panel B, treatment is $\log(1 + N_{db})$, where N_{db} denotes cumulative riot exposure over the same window. The childhood exposure window is defined from birth to age 14. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A Additional Result

Table A1: Dose–Response: Childhood Exposure to Riots and Depression in Later Life

	Major Depressive Disorder
Low exposure	0.024*
	(0.014)
Medium exposure	0.023**
	(0.011)
High exposure	0.033**
	(0.013)
Observations	26,627

Notes: The dependent variable is an indicator for major depressive disorder (MDD). Childhood riot exposure is measured from birth through age 14. Among women with positive childhood riot exposure, we divide the cumulative number of riot days experienced during childhood into terciles, defining *Low*, *Medium*, and *High* exposure. Women with no childhood riot exposure constitute the reference category. Indicators for the three exposure categories are entered jointly, so each coefficient is interpreted relative to women with no childhood riot exposure. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

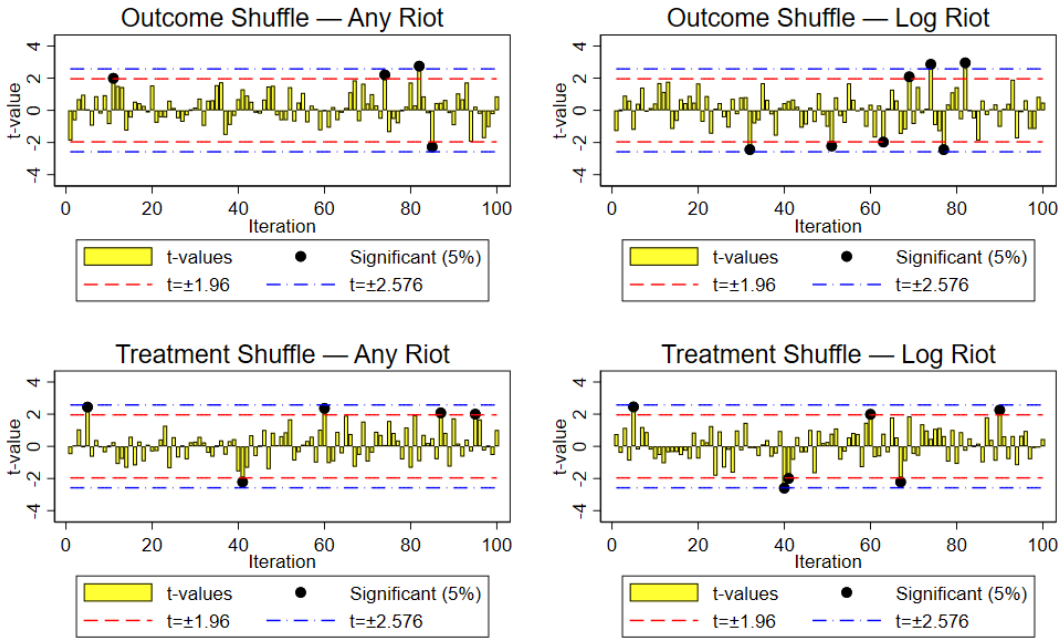


Figure A1: Randomization-Based Falsification Tests

Notes: The figure reports the distribution of t-statistics from 100 randomized falsification regressions. The top panels randomly permute the MDD outcome across respondents while holding childhood riot exposure fixed; the bottom panels randomly permute childhood riot exposure while holding the observed MDD outcome fixed. The left panels use the indicator for exposure to at least one riot between birth and age 14, while the right panels use $\log(1 + \text{Cumulative Riots})$ over the same period. Each bar represents the t-statistic from one randomized regression. Black dots identify placebo estimates that are statistically significant at the 5 percent level. Red dashed lines denote the 5 percent critical values ($t = \pm 1.96$), and blue dash-dotted lines denote the 1 percent critical values ($t = \pm 2.576$). All regressions use the same controls, childhood-district and birth-year fixed effects, population weights, and childhood-district-clustered standard errors as the baseline specification.

Table A2: Leave-One-State-Out Robustness

State omitted	Extensive margin	Intensive margin	N	State omitted	Extensive margin	Intensive margin	N
Jammu & Kashmir	0.025^{**}	0.021^{***}	26,029	West Bengal	0.018	0.015^{*}	25,232
	(0.012)	(0.007)			(0.012)	(0.009)	
Himachal Pradesh	0.024^{**}	0.020^{***}	26,104	Jharkhand	0.026^{**}	0.022^{***}	25,735
	(0.012)	(0.007)			(0.012)	(0.007)	
Punjab	0.025^{**}	0.020^{***}	25,677	Odisha	0.027^{**}	0.021^{***}	25,557
	(0.012)	(0.007)			(0.012)	(0.007)	
Chandigarh	0.024^{**}	0.020^{***}	26,584	Chhattisgarh	0.026^{**}	0.021^{***}	25,845
	(0.012)	(0.007)			(0.012)	(0.007)	
Uttarakhand	0.024^{**}	0.020^{***}	26,119	Madhya Pradesh	0.013	0.015^{**}	25,610
	(0.012)	(0.007)			(0.009)	(0.007)	
Haryana	0.025^{**}	0.020^{***}	25,929	Gujarat	0.026^{**}	0.022^{***}	25,590
	(0.012)	(0.007)			(0.012)	(0.008)	
Delhi	0.025^{**}	0.020^{***}	26,389	Daman & Diu	0.024^{**}	0.020^{***}	26,432
	(0.012)	(0.007)			(0.012)	(0.007)	
Rajasthan	0.025^{**}	0.020^{***}	25,651	Dadra & NH	0.024^{**}	0.020^{***}	26,386
	(0.012)	(0.007)			(0.012)	(0.007)	
Uttar Pradesh	0.022^{*}	0.016^{**}	24,442	Maharashtra	0.034^{***}	0.024^{***}	25,046
	(0.012)	(0.007)			(0.013)	(0.008)	
Bihar	0.023^{*}	0.019^{**}	25,041	Andhra Pradesh	0.025^{**}	0.020^{***}	25,513
	(0.013)	(0.008)			(0.012)	(0.007)	
Sikkim	0.024^{**}	0.020^{***}	26,347	Karnataka	0.022^{*}	0.022^{***}	25,725
	(0.012)	(0.007)			(0.012)	(0.008)	
Arunachal Pradesh	0.024^{**}	0.020^{***}	26,345	Goa	0.024^{**}	0.020^{***}	26,201
	(0.012)	(0.007)			(0.012)	(0.007)	
Nagaland	0.024^{**}	0.020^{***}	26,207	Lakshadweep	0.024^{**}	0.020^{***}	26,291
	(0.012)	(0.007)			(0.012)	(0.007)	
Manipur	0.024^{**}	0.020^{***}	26,171	Kerala	0.025^{**}	0.021^{***}	25,604
	(0.012)	(0.007)			(0.012)	(0.008)	
Mizoram	0.024^{**}	0.020^{***}	26,216	Tamil Nadu	0.029^{**}	0.022^{***}	25,072
	(0.012)	(0.007)			(0.012)	(0.007)	
Tripura	0.025^{**}	0.020^{***}	26,206	Puducherry	0.024^{**}	0.020^{***}	26,260
	(0.012)	(0.007)			(0.012)	(0.007)	
Meghalaya	0.025^{**}	0.020^{***}	26,274	Andaman & Nicobar	0.024^{**}	0.020^{***}	26,383
	(0.012)	(0.007)			(0.012)	(0.007)	
Assam	0.026^{**}	0.021^{***}	25,725	Telangana	0.025^{**}	0.020^{***}	26,006
	(0.012)	(0.007)			(0.012)	(0.007)	

Notes: Each row re-estimates the baseline specification after excluding the indicated state or union territory. The extensive-margin column reports the coefficient on *Riot Exposure*; the intensive-margin column reports the coefficient on $\log(1+\text{Cumulative Riots})$. Standard errors are shown in parentheses within each cell. The full-sample baseline coefficients are 0.024 for the extensive margin and 0.020 for the intensive margin. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. Regressions are population weighted and standard errors are clustered at the childhood-district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A3: Robustness to Nonlinear Probability Models

	Major Depressive Disorder	
	(1) Probit AME	(2) Logit AME
Panel A. Extensive Margin		
Riot Exposure	0.024^{**}	0.025^{**}
	0.024^{**}	0.025^{**}
Panel B. Intensive Margin		
$\log(1 + \text{Cumulative Riots})$	0.021^{***}	0.023^{***}
	0.021^{***}	0.023^{***}
Observations	23,877	23,877

Notes: The dependent variable is an indicator for major depressive disorder (MDD). The table reports average marginal effects (AMEs). Column (1) estimates probit models and column (2) estimates logit models. Panel A uses *Riot Exposure*, an indicator for exposure to at least one Hindu–Muslim riot between birth and age 14. Panel B uses $\log(1 + N_{db})$, where N_{db} denotes cumulative riot exposure over the same window. All specifications include religion, caste, parental education, rural childhood residence, childhood-district fixed effects, and birth-year fixed effects. The nonlinear models are estimated on the subsample remaining after excluding fixed-effect cells that perfectly predict the binary outcome. Regressions are population weighted. Standard errors are clustered at the childhood-district level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B Historical District Harmonization

The riot data record the district, town or city, and, in some cases, village associated with each communal-riot event . Because India's district boundaries changed substantially between 1950 and the period covered by LASI Wave 1, historical district names cannot in general be matched mechanically to the district geography observed in LASI. Districts were repeatedly divided, renamed, merged, and transferred across administrative boundaries. Consequently, the same geographic area may appear under different district names at different points in time, while a single historical district may correspond to several districts in the LASI geography.

We therefore construct a district crosswalk that harmonizes the historical geography of the riot data to the district coding used in LASI Wave 1. The procedure is designed to preserve, as closely as possible, the geographic footprint of the district in which a riot could have occurred without assigning an event to territory that cannot be linked to that historical district with reasonable confidence.

B.1 Target Geography

We first construct the set of districts to which historical riot locations can be assigned. Using the district identifiers available in LASI Wave 1, we recover 636 unique district names and their corresponding LASI district codes. These districts form the fixed target geography for the harmonization. Thus, the object of the crosswalk is not India's current administrative map, but the district geography represented in LASI Wave 1.

This distinction matters because some districts created relatively recently are not separately represented in LASI. A district can therefore have a well-defined administrative history but nevertheless be excluded from the final crosswalk if it does not appear in the LASI district

list. Conversely, when a historical district corresponds to several districts represented in LASI, the crosswalk may associate a single historical riot observation with multiple LASI districts.

For every historical district appearing in the riot data, we manually reconstruct its administrative history. We determine the district boundaries in effect at the time of the riot and trace subsequent splits, mergers, and renamings to the LASI geography. Boundary histories are reconstructed using the India State Stories District Dashboards, which document when districts were created and the historical districts from which they were formed. Because many changes involve partial transfers of territory rather than simple one-to-one renaming, we conduct these checks individually rather than relying on a mechanical name crosswalk.

B.2 Assignment Rules

The historical-to-LASI crosswalk follows three principal assignment rules.

B.2.1 Rule 1: Direct Assignment

If the historical district experienced no relevant boundary change and maps uniquely to a district represented in LASI, we assign the riot directly to that district.

For example, a riot recorded in *Lucknow* in 1950 maps directly to Lucknow in the LASI geography. No additional district is created from the relevant historical territory for purposes of this observation, and the observation is therefore assigned only to Lucknow.

These direct matches require no discretionary geographic assumption and receive no manual coding flag.

B.2.2 Rule 2: Clean Splits

A more important case arises when a historical district subsequently splits into multiple districts. Because the riot data generally identify the district rather than the exact point within the historical district at which the event occurred, we cannot determine which later descendant contains the precise riot location. When the historical district is subsequently partitioned into multiple *clean descendants*—that is, districts formed entirely from territory belonging to that historical district—we therefore assign the riot to each of those descendants.

This rule preserves the geographic coverage of the historical district. To assign the riot to only one descendant would implicitly assume that the event occurred in a particular portion of the historical district, even though the underlying data do not provide sufficient information to make that distinction.

Aligarh. The March 1950 riot recorded in *Aligarh* illustrates this case. Historical Aligarh subsequently gave rise to the present-day districts of Aligarh and Hathras. Hathras was carved from Aligarh without incorporating territory from another historical district. We therefore map the historical riot to both Aligarh and Hathras in the LASI geography.

Mahesana. A riot recorded in *Mahesana* in 1982 provides another example. Mahesana subsequently split to form Patan. Because Patan represents a clean descendant of Mahesana, the riot is mapped to both Mahesana and Patan. Importantly, Gandhinagar is not added to this particular observation merely because some territory historically associated with Mahesana had previously contributed to Gandhinagar. Gandhinagar had already been formed before the 1982 riot. The relevant geographic unit at the date of the riot was therefore Mahesana, with Patan added only because it was subsequently carved from that district.

Thus, the assignment rule is explicitly date-specific: we first determine the district boundaries that existed when the riot occurred and then trace the subsequent evolution of that

historical district forward to the LASI geography.

B.2.3 Rule 3: Mixed-Origin Descendants

Clean splitting is not always possible. Some present-day districts were formed by combining territory from two or more historical districts. We refer to these as *mixed-origin districts*. When such a district appears as a descendant of the historical district being harmonized, we exclude it because its present-day territory cannot be uniquely attributed to the historical district in which the riot was recorded.

Suppose, for example, that historical district d later gives rise to districts d_1, d_2, and d_3. If d_1 and d_2 are formed entirely from territory belonging to d, but d_3 combines territory from d and another historical district k, we assign the riot to d_1 and d_2 but not to d_3. Including d_3 would incorrectly expose territory inherited from district k to a riot known only to have occurred somewhere in historical district d.

Several cases in the riot data illustrate why this restriction matters.

Ahmadabad, Gandhinagar, and Botad. For a riot recorded in *Ahmadabad* in 1950, the historical district subsequently contributed territory to both Gandhinagar and Botad. Gandhinagar was formed using territory from Ahmadabad together with territory from Mehsana, while Botad incorporated territory originating in Ahmadabad and Bhavnagar. Neither descendant can therefore be uniquely attributed to historical Ahmadabad. The riot is assigned to Ahmadabad itself, while the mixed-origin descendants are not treated as clean descendants of Ahmadabad.

The Gandhinagar–Botad case also illustrates the interaction between historical harmonization and LASI coverage. Gandhinagar appears in the LASI district list and is excluded because of its mixed origin. Botad, by contrast, is not separately represented in the LASI

Wave 1 target geography. Its absence from LASI is therefore already sufficient for exclusion.

Agra and Firozabad. A 1950 riot recorded in *Agra* provides a second example. Firozabad subsequently emerged from territory associated with Agra together with territory from another historical district(Mainpuri). Firozabad is therefore not treated as a clean descendant of Agra for this observation, and the riot is assigned to Agra rather than mechanically propagated to Firozabad.

Mahesana, Patan, and Gandhinagar. A 1955 riot in *Mahesana* illustrates a case containing both clean and mixed descendants. Mahesana subsequently produced Patan through a clean split, while some Mahesana territory also contributed to the formation of Gandhinagar together with territory from Ahmadabad. We therefore assign the riot to Mahesana and Patan, but not to Gandhinagar. This example makes clear that the procedure operates at the descendant level: the presence of one mixed-origin descendant does not cause us to discard otherwise clean descendants of the same historical district.

Goalpara. The historical evolution of *Goalpara* in Assam provides a more extensive example. A riot recorded in Goalpara in 1950 can be linked to Goalpara itself and to the clean descendant districts Kokrajhar, Bongaigaon, Dhubri, and Chirang. These districts are therefore included in the harmonization. Goalpara also contributed, through subsequent administrative changes, to Baksa and Tamulpur. Because these districts were formed using territory originating in multiple historical districts, they cannot be uniquely attributed to Goalpara and are excluded. This example illustrates why a simple rule assigning riots to every eventual descendant of a historical district would generate substantial geographic measurement error.

B.3 Matching Historical Locations

The assignment rules above presume that the historical district itself can be identified. In practice, the raw riot records are not always sufficiently standardized to permit an immediate district match. We therefore apply a fixed matching hierarchy.

The historical district reported in the riot data is always used first. If that field is missing, ambiguous, or found not to correspond to an actual district, we next use the reported town or city. When the town field is unavailable and a village is reported, we use the village. Finally, when the administrative fields are insufficient by themselves, we consult geographic information contained in the underlying newspaper notes.

The hierarchy can be summarized as

$$\text{District} \longrightarrow \text{Town/City} \longrightarrow \text{Village} \longrightarrow \text{Context in the newspaper report.}$$

Town- and village-level information is used conservatively because settlement names are substantially more likely than district names to occur in multiple locations. Once a town or village is used to identify the relevant historical district, that district is still subjected to the same boundary-history rules described above.

Orai. A 1956 observation records *Orai* in the district field. Orai is a city, rather than a district, and was located in Jalaun district. The observation is therefore reassigned to historical Jalaun, after which the standard district harmonization rules are applied.

Hubli. Similarly, a 1960 observation records *Hubli* as though it were a district. Hubli is a city in Dharwad district. We first correct the historical district to Dharwad. Because Dharwad subsequently split into Dharwad, Gadag, and Haveri, the resulting riot assignment

includes all three clean descendant districts. Thus, correcting an erroneous district field is only the first step; the recovered historical district must subsequently be harmonized to the LASI geography.

B.4 Use of Newspaper Context

In some observations, neither the reported district nor the settlement name is sufficient to identify the location uniquely. In these cases, we use geographic information contained in the newspaper report underlying the riot record. Relevant information includes references to nearby cities, distances, police jurisdictions, administrative units, or historically identifiable events.

Naduvaltam. For a 1954 observation reporting *Naduvaltam*, multiple present-day locations with the same or similar name are possible. The newspaper report, however, refers to the Malabar police force and describes the location as approximately three miles from Calicut. These contextual details identify the relevant location as belonging to the historical Malabar district and allow the observation to be mapped to Kozhikode.

Chhamb. A 1952 observation reporting *Chhamb* similarly requires contextual historical information to determine the appropriate geographic assignment. The location is resolved using information on the historical Chhamb area and its subsequent administrative geography, producing assignments to Jammu and Samba.

The purpose of these contextual assignments is not to infer a location from weak geographic clues. They are used only when the information in the underlying report resolves an otherwise ambiguous place name sufficiently to support a defensible district assignment.

B.5 Locations That Cannot Be Identified

When the full matching hierarchy does not yield a unique or sufficiently confident location, the observation is left geographically unassigned rather than assigned probabilistically or based on an arbitrary choice among possible districts.

For example, one 1953 riot identifies the village as *Budhi*. Villages with this name could be located in both Kathua and Doda districts, and the newspaper report does not contain enough information to determine which location is intended. The observation is therefore left unassigned. Similarly, a 1950 observation reporting the town *Saugor* could not be matched confidently to a district in the reported state and is not assigned to a LASI district.

This conservative treatment ensures that uncertainty in the historical location is not mechanically converted into measurement error in childhood riot exposure.

B.6 Spelling and Historical Place Names

Historical newspaper records also contain spelling variants, older romanizations, and typographical errors. We correct such cases only when the underlying location can be independently established.

For example, the village recorded as *Temghur* in a 1969 observation is identified as Temghar and mapped to historical Poona (present-day Pune). Similarly, the village recorded as *Kapus Talani* in a 1987 observation is verified as a location in Amravati district. These corrections are distinguished from contextual geographic inference because the intended location is otherwise unambiguous; the intervention concerns only the spelling or romanization of the place name.

For less widely recognized settlements, we independently verify the geographic match. For example, the historical location of Dohad is checked against records showing that it belonged

to Panch Mahals before becoming a separate district in 1997. Similarly, the administrative history of Bangalore Rural is used to determine whether a location near Ramanagaram belonged to Bangalore or Bangalore Rural at the exact date of the riot. These date-specific checks are important because the appropriate historical district may differ for two riots occurring at the same physical location but on opposite sides of an administrative boundary change.

B.7 Historical Back-Tracing

A distinct problem arises when the location reported in the riot data corresponds to a present-day district that did not yet exist as a district at the date of the riot. If that district was subsequently formed from multiple historical parent districts, simply assigning the riot to the modern district would impose a district geography that did not exist when the event occurred.

For such observations, we identify the historical parent districts from which the recorded location was later formed and trace the observation back to the administrative geography in effect at the riot date. We refer to this procedure as *historical back-tracing*.

Importantly, this situation differs from the mixed-origin exclusion described under Rule 3. Under Rule 3, a mixed-origin district appears as a later descendant while we are tracing a known historical district forward; because that descendant contains territory from another historical district, it is excluded. Under historical back-tracing, by contrast, the mixed-origin district is itself the recorded location of the riot. We therefore retain that district as information about the modern location and supplement it with the historical parent districts that existed when the riot occurred.

Firozabad. A riot in August 1960 is reported in the town of *Firozabad*, but Firozabad did not yet exist as a district in 1960. The later district was formed using territory from Agra and Mainpuri. We therefore retain Firozabad as the present-day location and add Agra and Mainpuri as the historical parent districts relevant to the riot date.

This example also illustrates the difference between forward harmonization and back-tracing. For a 1950 riot known to have occurred somewhere in historical Agra, Firozabad is not added because its later territory cannot be uniquely attributed to Agra. But when the riot location itself is identified as Firozabad, as in the 1960 observation, the modern location provides additional information. We therefore use the later location to trace the observation back to Agra and Mainpuri.

Ghaziabad. A January 1954 observation reports *Ghaziabad*, which likewise did not yet exist as a district at that date. Ghaziabad was subsequently formed from territory belonging to Meerut and Bulandshahr. The historical parents are therefore added to the assignment. Applying the clean-descendant rule to those historical parents yields a broader set of LASI districts: Meerut, Baghpat, Bulandshahr, Ghaziabad, Hapur, and Gautam Buddh Nagar.

Sambhal. The same logic applies to a 1978 riot reported in *Sambhal*. Sambhal was formed from territory associated with Moradabad and Budaun. We therefore trace the location to those historical parents and their relevant descendants, producing an assignment that includes Moradabad, Jyotiba Phule Nagar, Budaun, and Sambhal.

These examples highlight why both the date of the riot and the direction of the administrative mapping matter. A district that is excluded when encountered as an ambiguous future descendant can nevertheless provide useful geographic information when it is the location explicitly recorded for the riot.

B.8 Locations Outside Present-Day India

A small number of observations refer to districts that lie outside the borders of present-day India. Such observations cannot be linked to LASI respondents and are therefore excluded from the harmonized district panel.

For example, riots recorded in *Khulna* and *Jessore* in 1961 refer to districts located in what was then East Pakistan and is now Bangladesh. Because LASI's target geography contains only districts within present-day India, these observations receive no LASI district assignment.

B.9 State Corrections

We also verify whether the state reported in the raw riot data is consistent with the identified district. When the district can be confidently established but the state field is erroneous, we correct the state rather than discard the geographic information.

For example, a 1987 observation records *Etah* as belonging to Bihar, although Etah is in Uttar Pradesh. A 1990 observation similarly records *Gonda* as belonging to Gujarat rather than Uttar Pradesh. In both cases, the district is identifiable and the state field is corrected.

B.10 Documentation and Coding Flags

Every manual intervention is recorded using a numeric coding flag and a free-text justification. The flags document why manual review was necessary and make it possible to distinguish ordinary direct or clean-split assignments from observations requiring additional geographic information or judgment. The codes are not necessarily mutually exclusive: an observation may receive more than one flag when more than one intervention is required.

Table B1: Coding Flags Used in Historical District Harmonization

Code	Reason	Obs.	Illustrative case
1	Mixed-origin descendant	45	Gandhinagar is excluded when harmonizing historical Ahmadabad or Mahesana because its territory originates in both districts.
2	Town or settlement recorded as district	15	Orai is reassigned to Jalaun; Hubli is reassigned to Dharwad before applying subsequent split rules.
3	Location inferred from contextual information	55	Naduvaltam: reference to the Malabar police force and proximity to Calicut identifies the relevant location as Kozhikode.
4	Location unidentifiable	35	Budhi: same-named villages in Kathua and Doda cannot be distinguished from the available information.
5	Spelling or transliteration correction	3	Temghur is identified as Temghar in historical Poona; Kapus Talani is verified in Amravati.
6	Historical backtracing	10	Firozabad is traced to Agra and Mainpuri for a 1960 riot; Ghaziabad is traced to Meerut and Bulandshahr for a 1954 riot.
7	Outside present-day India	10	Khulna and Jessore: locations now in Bangladesh and therefore outside the LASI target geography.
8	State mismatch	11	Etah is corrected from Bihar to Uttar Pradesh; Gonda is corrected from Gujarat to Uttar Pradesh.

Notes: The table summarizes manual coding decisions in the historical riot-location crosswalk. The underlying file contains 1,290 riot-location observations. Of these, 181 receive at least one coding flag and 1,109 are matched without a manual coding flag. Two observations receive more than one flag, so the number of code assignments exceeds the number of flagged observations.

B.11 Quality Assurance

Several quality-control procedures are used throughout the harmonization. First, each riot observation is manually inspected, including the underlying newspaper notes where available. Second, we examine the administrative history of every historical district appearing in the riot data, rather than checking boundary changes only when a district name fails to match mechanically. Third, non-obvious town and village assignments are independently verified using external geographic sources, including official district government records where available. Finally, every non-standard decision is documented through the coding flags described above together with a written explanation of the assignment.

The resulting crosswalk therefore imposes a common LASI Wave 1 geography on historical riot locations while retaining the information contained in the historical administrative boundaries. Clean descendants are included so that the geographic footprint of historical districts is not artificially narrowed. Mixed-origin descendants are excluded when they cannot be attributed uniquely to the historical district. Observations involving districts that did not yet exist at the riot date are explicitly traced back to the relevant historical administrative units.

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