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Childhood Exposure to Communal Violence and Long-Run Religious Inequality in Psychological and Economic Well-Being

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Childhood Exposure to Communal Violence and Long-Run Religious Inequality in Psychological and Economic Well-Being*

Abstract

Can childhood exposure to intergroup violence widen long-run inequality between majority and minority groups? We link the Longitudinal Ageing Study in India to historical records of Hindu–Muslim riot fatalities to investigate this question. Identification comes from whether Muslim–Hindu gaps differ across childhood district–birth cohorts with different riot exposure, after absorbing district-cohort conditions and allowing religious gaps to vary across districts and cohorts. More than three decades later, greater exposure is associated with worse psychological well-being among Muslims relative to Hindus: depressive symptoms rise, while life satisfaction, perceived social position, and sleep deteriorate. Exposure is also associated with lower household consumption, durable asset ownership, and property ownership. These disparities are accompanied by lower educational progression and a labour-market pattern of higher participation but lower earnings and lower salaried employment, consistent with human-capital and labour-market pathways through which childhood violence may shape later-life inequality. Intergroup violence can therefore leave persistent minority-specific costs in both mental health and material well-being.

JEL classification

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Keywords

communal violence, childhood exposure, mental health, minorities, religious inequality, India

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1 Introduction

Persistent inequality across social groups is a salient feature of economic development. Large differences in economic outcomes and intergenerational mobility remain across ethnic and racial groups even within the same countries (Alesina et al., 2016; Chetty et al., 2020). A related literature shows that historical institutions and shocks can have effects that persist long after the original events have passed (Nunn, 2008; Voigtländer and Voth, 2012). Intergroup violence may be one such shock. Beyond its immediate costs in lives, property, and local economic activity, violence between social groups may have unequal consequences for the groups involved. If these differences persist, episodes of conflict can widen group inequalities long after the violence itself has ended. This raises a broader question: can exposure to intergroup violence generate persistent differences in economic and social outcomes between groups that lived through the same local environment?

We study this question in India, where Hindu–Muslim riots have been a recurrent form of intergroup violence since Independence (Varshney, 2002; Wilkinson, 2004; Mitra and Ray, 2014). Muslims are the country’s largest religious minority, making India a particularly relevant setting in which to study whether communal violence has asymmetric long-run consequences for majority and minority communities. This question is especially important given evidence of substantial differences in economic mobility across social groups in India. For instance, Asher et al. (2024) show that intergenerational mobility among Muslim sons has declined across birth cohorts, even as it has risen among Scheduled Castes. Against this background, we ask whether childhood exposure to Hindu–Muslim communal violence is associated with a persistent widening of Muslim–Hindu gaps in later-life outcomes.

Communal violence may shape long-run outcomes through two distinct channels. First, riots may impose general place-based costs by disrupting local economic activity, credit, and public institutions. Recent evidence from India shows that communal riots can have persistent effects on local economic activity, firm performance, access to credit, and trust in state institutions (Iyer et al., 2026). Second, the consequences of the same episode of violence may differ across the groups involved. For a religious minority, exposure to communal conflict may generate additional costs through insecurity, segregation, or changes in intergroup economic relations. Fisman et al. (2020), for example, show that exposure to Hindu–Muslim riots affects subsequent lending across religious groups, while Kalra (2021) shows that communal violence can displace Muslims into more segregated neighborhoods and affect educational outcomes. When exposure

occurs during childhood, these group-specific costs can also affect schooling and psychological development, with consequences that may carry into adult economic trajectories. The distinction between place-based and group-specific consequences therefore makes the Muslim–Hindu differential, rather than the average local effect of riot exposure, the central empirical query.

We combine the Longitudinal Ageing Study in India (LASI), which records respondents’ childhood locations alongside rich measures of later-life psychological and economic well-being, with historical data on Hindu–Muslim riots to study the long-run consequences of childhood exposure to communal violence. For each respondent, we measure exposure using cumulative riot fatalities in the district where they grew up during ages 0–14. Because exposure varies across both childhood districts and birth cohorts, individuals born in the same district can experience different levels of communal violence depending on when they were born. Our preferred specification asks whether the Muslim–Hindu gap in later-life outcomes varies systematically with childhood exposure to communal violence. Childhood-district-by-birth-cohort fixed effects absorb conditions shared by Hindus and Muslims growing up in the same district and cohort, while district-by-religion and cohort-by-religion fixed effects account for persistent local religious differences and changes in religious gaps across cohorts. The identifying variation therefore comes from whether Muslim–Hindu differences are systematically larger in district-cohort cells that experienced greater childhood exposure to communal violence.¹

Our first set of results shows a persistent relative disadvantage in psychological well-being. Muslims who experienced greater communal violence during childhood are more likely than similarly exposed Hindus to report depressive symptoms, and they also fare worse on life satisfaction, perceived social position, and measures of sleep. These outcomes capture different dimensions of psychological well-being, yet they move consistently in the same direction. The magnitudes are economically meaningful: a one-standard-deviation increase in childhood riot fatalities is associated with a 2.5 percentage point increase in depressive symptoms among Muslims relative to Hindus, against a sample mean of 27.7 percent. The fact that these differences are visible several decades after the exposure period points to a persistent psychological dimension of the relative disadvantage associated with childhood communal violence.

The same pattern extends to material well-being. Greater childhood exposure is associated with lower consumption among Muslims relative to Hindus, but the differences are particularly pronounced for assets accumulated over the life cycle. Exposed Mus-

¹The results are also robust to allowing the Muslim–Hindu gap to follow district-specific linear cohort trends and to vary flexibly across birth cohorts within states.

lims have lower durable asset holdings and are less likely to own their residence or land. For example, a one-standard-deviation increase in childhood riot fatalities is associated with a 0.084 standard deviation decline in the durable asset index for Muslims relative to Hindus. Taken together, these results suggest that the long-run association is not confined to current economic circumstances, but is also reflected in forms of wealth and property that embody cumulative economic advantage.

The education and labour-market results help illuminate how these later-life disparities are accompanied by differences in life-cycle trajectories. Greater childhood exposure is associated with lower probabilities of completing secondary, higher-secondary, and tertiary education among Muslims relative to Hindus. In adulthood, the pattern is not one of withdrawal from the labour market. Exposed Muslims are more likely to be working, but those who work earn less and are less likely to hold salaried jobs, with employment shifting toward family-farm activity. Thus, greater labour-market participation coexists with lower educational progression, lower earnings among workers, and a shift away from salaried employment. These patterns suggest that differences in educational attainment and the types of jobs people enter are important parts of the longer-run economic consequences of childhood communal violence.

These findings contribute primarily to two related strands of literature. Our first contribution is to the literature on the long-run consequences of conflict exposure during childhood. Existing work documents lasting effects on health and human capital (Akresh et al., 2012; Leon, 2012), adult labour-market outcomes (Galdo, 2013), educational attainment (Bharati, 2022), and mental health (Singhal, 2019; Benjet et al., 2020). These studies examine exposure to civil war, political violence, bombing, or war-related stress, typically exploiting differences in the location or timing of exposure. Our setting shifts attention from these war and armed-conflict settings to recurrent inter-group violence experienced over childhood and, more importantly, to how its long-run consequences are distributed across the groups involved. The breadth of the LASI data also allows us to examine psychological and material well-being within a common empirical framework more than three decades after childhood exposure. We find that the relative minority penalty is not confined to a single domain: it appears in psychological well-being and household economic security, alongside differences in educational progression and labour-market trajectories over the life cycle. Rather than estimating only the average scarring from conflict, we ask whether greater childhood exposure widens the later-life gap between a religious minority and the majority sharing the same local environment. In this sense, the paper connects the literature on early-life conflict

exposure to the broader question of how historical shocks can contribute to persistent inequality between social groups.

Our second contribution is to the literature on Hindu–Muslim communal violence in India. A large body of work studies the causes and political economy of riots (Varshney, 2002; Wilkinson, 2004; Mitra and Ray, 2014), while a growing literature examines their downstream consequences. Existing studies document effects on electoral outcomes (Iyer and Shrivastava, 2018), intergroup lending (Fisman et al., 2020), residential segregation and education (Kalra, 2021), women’s marital outcomes (Ghose and Pandey, 2025), and local economic activity (Iyer et al., 2026). Particularly close to our setting, Bharti and Roy (2023) show that exposure to Hindu–Muslim riots in early childhood affects judicial behaviour decades later. We add to this literature by making the long-run Muslim–Hindu differential itself the object of interest. Linking childhood riot exposure at the district-cohort level to individual outcomes decades later, we show that communal violence widens religious-group gaps in psychological well-being and material security, with corresponding differences in educational and labour-market trajectories. In this way, the paper broadens the downstream literature from particular consequences of communal violence to its implications for long-run religious inequality.

Taken together, our findings point to an important distributional dimension of the long-run costs of intergroup violence. Communal violence does not only impose costs on the places in which it occurs; it can also alter the relative position of the groups that share those places, leaving persistent differences in well-being and economic security decades later. This distinction matters for understanding persistent group inequality: focusing on average effects of conflict can obscure how its burden is distributed between majority and minority communities.

The remainder of the paper proceeds as follows. Section 2 presents the conceptual framework. Section 3 describes the data and measurement of childhood conflict exposure. Section 4 presents the empirical strategy. Section 5 reports the results and additional analyses. Section 6 concludes.

2 Conceptual Framework

Childhood exposure to Hindu–Muslim communal violence may shape later-life well-being through two broad, non-mutually-exclusive pathways, as illustrated in Figure 1.

The first is a *general, place-based channel*. Riots can damage local infrastructure, disrupt schools and markets, weaken public institutions, and depress local investment

and economic activity for years afterwards. Recent evidence from India shows that districts experiencing communal riots see measurable declines in nightlight-based economic activity, firm performance, credit access, and trust in local institutions (Iyer et al., 2026). Because this channel operates through the local economy and institutional environment as a whole, it may affect exposed communities broadly, including both Hindu and Muslim residents of an affected district.

The second is a *minority-specific channel*. The consequences of Hindu–Muslim riots may differ across the groups involved, with existing evidence suggesting important asymmetries in exposure to violence across religious groups (Mitra and Ray, 2014). For Muslims, India’s largest religious minority, exposure to communal violence may generate additional costs through insecurity, segregation, discrimination, and changes in intergroup economic relations (Kalra, 2021; Fisman et al., 2020). We therefore hypothesise that childhood exposure to such violence can widen later-life Hindu–Muslim gaps.

The minority-specific channel may operate through several hypothesised pathways and adjustment margins. A broader microeconomic literature shows that exposure to violent conflict can alter household investment and coping decisions, human-capital accumulation, mobility, labour-market outcomes, and long-run well-being (Verwimp et al., 2019). First, violence may disrupt educational attainment by affecting school access, household resources, parental investment, or children’s psychological capacity to learn during formative years. Second, exposure may affect later labour-market outcomes by lowering access to stable or remunerative employment or shifting workers across employment types. Third, communal violence may alter post-childhood mobility, including later-life relocation in response to household constraints, marriage, economic opportunity, or changes in local networks. Fourth, exposure may create psychological trauma and insecurity through stress, fear, and social marginalisation. Fifth, it may weaken asset accumulation and economic security over the life cycle through direct losses, lower earnings, weaker credit access, or reduced security of property rights.

These pathways and adjustment margins can generate two broad sets of later-life outcomes. The first is *economic well-being*: household consumption, durable asset ownership, current residence ownership, and land ownership. The second is *psychological well-being*: depressive symptoms, life satisfaction, perceived social position, sleep problems, and sleep medication use. We view psychological well-being as both a possible direct consequence of childhood violence and a cumulative reflection of later-life disadvantage. Conflict experienced during childhood may leave a lasting imprint through trauma, insecurity, and social marginalisation; the same exposure may also reduce ed-

ucation, employment quality, earnings, and asset accumulation, which can compound psychological distress over the life cycle.

3 Data and Measurement

This section describes the data sources, construction of childhood conflict exposure, sample selection, and measurement of the main outcomes.

3.1 Data Sources

We combine three data sources to link individuals' childhood exposure to communal violence with their later-life outcomes.

Longitudinal Ageing Study in India (LASI), 2017–18. LASI is a nationally representative survey of adults aged 45 and above and their spouses, covering health, mental well-being, economic status, migration history, social participation, and perceived safety. The survey also records respondents' migration histories, including where they lived for most of childhood. This allows us to link later-life outcomes to the conflict environment of the district in which the respondent grew up, rather than only to the district in which they currently reside. We use LASI district identifiers harmonised to Census 2011 geography to link respondents to historical conflict exposure.

Varshney–Wilkinson data on Hindu–Muslim violence. We measure communal violence using the Varshney–Wilkinson event-level dataset on Hindu–Muslim riots (Varshney and Wilkinson, 2006), compiled from reports in *The Times of India* (Bombay edition). The data cover 1950–1995 and record, for each riot episode, the date, location, fatalities, injuries, duration, and reported precipitating cause. The original file contains 1,192 conflict records; after removing 12 observations outside India, 1,180 Indian source records remain. We harmonise these historical riot locations to Census 2011 district boundaries before constructing district-level exposure measures.²

Census of India, 2011. We use Census 2011 district boundaries as the common geographic frame for both LASI locations and historical conflict events. Our objective

²We assign each conflict record to a Census 2011 district using the most geographically precise information available, prioritising village, town, or city names and newspaper-note information over historical district labels. Historical district identifiers are used only when finer location information is unavailable. When a historical district-level record maps to multiple successor districts under Census 2011 boundaries, we propagate the record to the relevant successor districts; when the available information does not support an unambiguous assignment, we leave the district unidentified. The final harmonised conflict file contains 1,176 event-location observations, of which 1,155 are assigned to a Census 2011 district.

is to locate respondents’ childhood residences and historical riot locations within a common geographic unit, rather than to reconstruct the administrative district boundaries prevailing at the time of exposure. This is necessary because Indian district boundaries changed substantially between 1950 and the LASI survey period. Expressing both respondents’ childhood districts and riot locations in a common Census 2011 geography ensures that exposure is measured consistently across birth cohorts and across historical changes in district boundaries.

3.2 Construction of Childhood Conflict Exposure

We define each respondent’s childhood exposure window as ages 0–14. This window corresponds directly to the LASI migration question, which identifies where the respondent lived for most of childhood up to age 14. For each respondent, we identify their assigned childhood district³ and birth year, and sum Hindu–Muslim riot fatalities occurring in that district from the respondent’s birth year through birth year +14. What matters for exposure is whether a riot occurred in the geographic area where the respondent lived during childhood, rather than the administrative district boundary in force at the time; we therefore express both childhood locations and riot locations in Census 2011 district geography to provide a consistent definition of geographic locality across cohorts. Riot locations are first harmonised to Census 2011 district boundaries. Fatalities are then aggregated to district-year cells and summed over each respondent’s childhood exposure window. Figure A.1 illustrates the resulting variation in exposure using a stylised example: holding district fixed, exposure varies across birth cohorts because different cohorts’ childhood windows span different riot years; holding birth year fixed, exposure varies across districts because some districts experienced substantially more communal violence than others.

Our main exposure measure, which we refer to as *Fatalities*, is cumulative childhood riot fatalities divided by the standard deviation of cumulative fatalities in the main Hindu–Muslim conflict-district analysis sample. We use fatalities rather than a simple indicator for riot incidence because fatalities provide a measure of conflict intensity,

³Childhood district is defined as the district where the respondent lived for most of childhood, ages 0–14. We construct a resolved childhood-district code from LASI’s migration module by first using the directly reported childhood district. If the respondent reports that the childhood place was the same as the place of birth, last residence, or current residence, we instead use the corresponding district from the birth-place record, last-residence record, or Census 2011 current-residence crosswalk. If no valid resolved childhood district is available but the respondent reports living in the same village or town since birth, we assign the current district as the childhood district. In all other cases, including childhood abroad or missing childhood-district information, childhood district is left unassigned (809 respondents, or 1.6 per cent of the age-eligible sample).

whereas an incidence measure would treat relatively minor and severe episodes alike. We use the same scaling factor in all specifications and robustness checks, including the full-sample analysis that adds districts with no recorded conflict. We do not subtract the mean before scaling, so *Fatalities* retains its natural zero: a respondent whose childhood district recorded no Hindu–Muslim riot fatalities during the childhood exposure window has *Fatalities* = 0. A one-unit increase in *Fatalities* therefore corresponds to a one-standard-deviation increase in raw cumulative childhood riot fatalities in the main analysis sample.

3.3 Sample Selection

Our sample construction proceeds in four steps, illustrated in Figure A.2. Starting from the full LASI sample ($N = 73,397$), we first restrict to respondents aged 45 or above who were born in 1950 or later, so that their entire childhood exposure window (ages 0–14) falls within the period covered by the Varshney–Wilkinson data ($N = 52,134$). We then retain respondents whose childhood district, assigned using the procedure described above, can be identified and matched to a Census 2011 district code ($N = 51,325$). Third, for the main analysis, we restrict to respondents whose assigned childhood district recorded at least one Hindu–Muslim riot during 1950–1995 ($N = 22,789$). We use this as our preferred sample to improve comparability in the identifying variation. Districts that experience no Hindu–Muslim riot over several decades may differ systematically from conflict-experienced districts in persistent features of intergroup relations, local institutions, and the underlying propensity for communal violence. Restricting attention to conflict-experienced districts therefore defines a more comparable risk set of places in which communal violence was a relevant local possibility, while identification continues to rely on variation across district-cohort cells in childhood conflict exposure.⁴ Finally, we restrict the analysis sample to self-identified Hindus and Muslims, dropping respondents of other religions, for whom Hindu–Muslim riot fatalities are not the relevant group-specific exposure ($N = 21,552$). The resulting analytic sample is 84.2 per cent Hindu and 15.8 per cent Muslim. As a robustness check, Section 5.4.2 returns to the broader Hindu–Muslim sample that includes respondents from districts with no recorded Hindu–

⁴Bharti and Roy (2023) adopt a closely related restriction in their analysis of early-life exposure to Hindu–Muslim riots, noting that districts that never experienced a riot may be systematically different from riot-experienced districts. More broadly, evidence that the recurrence of Hindu–Muslim riots reflects persistent underlying local conditions (van Noort and Goyal, 2026), together with evidence that quasi-random variation in Hindu-festival timing affects the realization of riots (Iyer and Shrivastava, 2018), motivates distinguishing underlying susceptibility to communal violence from the timing and intensity of realized violence.

Muslim riots and shows that the main findings are not driven by the conflict-district restriction.

3.4 Outcome Variables

We measure later-life outcomes across six domains: psychological well-being, economic well-being, educational attainment, employment and earnings, type of employment, and migration.

Psychological well-being. We examine five measures: depressive symptoms, life satisfaction, perceived social position, sleep problems, and sleep-medication use. Depressive symptoms are measured using the 10-item Centre for Epidemiologic Studies Depression Scale (CES-D 10), coded as one if the respondent reports four or more depressive symptoms. Life satisfaction is measured as the mean of five Satisfaction with Life Scale questions, each recorded on a 1–7 scale, with higher values indicating greater satisfaction. Perceived social position is measured using the Cantril ladder, coded as one if the respondent places themselves on rung eight or higher of a ten-rung ladder. Sleep problems are measured using a standardised weighted index, constructed using `swindex`, from four indicators for frequent or occasional sleep difficulties: trouble falling asleep, waking during the night, waking too early, and feeling unrested. We also examine whether the respondent reports taking medication or other treatment for sleep problems.

Economic well-being. Economic well-being is captured using household consumption, durable asset ownership, current residence ownership, and land ownership. Household consumption is measured as the log of one plus annual household food and non-food consumption, excluding health expenditure. Durable asset ownership is measured using a standardised index constructed as the first principal component of 23 durable ownership indicators, following the asset-index approach of [Filmer and Pritchett \(2001\)](#), with higher values indicating greater durable asset ownership. We also consider whether the household owns its current residence and whether it owns land.

Educational attainment. We measure education using completed years of schooling and indicators for completing secondary schooling or higher, higher secondary schooling or higher, and tertiary education.

Employment and earnings. Employment outcomes include current employment status, weekly hours worked, and annual individual income from main and side work. Hours worked is the sum of reported average weekly hours in the respondent’s main job and side job; observations with totals above 168 hours per week are excluded from regressions using this outcome. Annual income is winsorised at the 99th percentile. Among cur-

rently employed respondents, we additionally examine annual income and its logarithm, with log income defined as the log of one plus annual individual income.

Type of employment. Among currently employed respondents, we examine indicators for family farm activities, agricultural labour, self-employment, salaried employment, paid family work, and unpaid work. Family farm activities refer to own or family-operated work in farming, fishery, or forestry.

Migration. We measure whether the respondent has ever migrated, whether they moved to the current residence during ages 1–14, and whether they moved to the current residence at age 15 or older. These measures are based on the respondent’s reported duration of continuous residence in the current location.

3.5 Descriptive Statistics

We use weighted descriptive statistics to characterise childhood conflict exposure and the main outcomes in Table 1. *Fatalities* is summarised for the full analytic sample, while outcome statistics are reported for the samples used in the corresponding main analyses. Observation counts therefore vary across outcomes.

4 Empirical Framework

We estimate the relationship between childhood conflict exposure and later-life outcomes using two related specifications. The first is a baseline district-and-cohort fixed-effects model:

$$Y_{idc} = \alpha + \beta_1 F_{dc} + \beta_2 \text{Muslim}_i + \beta_3 (F_{dc} \times \text{Muslim}_i) + \mathbf{X}_i' \gamma + \mu_d + \lambda_c + \varepsilon_{idc}. \quad (1)$$

Here Y_{idc} is the later-life outcome for individual i , from childhood district d and birth cohort c . F_{dc} is cumulative Hindu–Muslim riot fatalities in district d during ages 0–14 for cohort c , scaled by its standard deviation. Muslim_i is an indicator for Muslim respondents; Hindus form the reference group. $\mathbf{X}_i$ includes caste and sex controls. μ_d are childhood-district fixed effects and λ_c are birth-cohort fixed effects. Standard errors are clustered at the childhood-district level.

In this baseline specification, β_1 captures the association between childhood conflict fatalities and the outcome for Hindus, while β_2 captures the Hindu–Muslim difference in the outcome when measured conflict exposure is zero. The main coefficient of interest is β_3 , which captures whether the association between childhood conflict exposure and

later-life outcomes differs for Muslims relative to Hindus. Equivalently, it estimates whether the Hindu–Muslim gap in the outcome changes systematically with the intensity of childhood exposure to communal violence.

Our preferred specification is a more saturated fixed-effects model:

$$Y_{idc} = \alpha + \beta (F_{dc} \times \text{Muslim}_i) + \mathbf{X}_i' \theta + \mu_{dc} + \phi_d \times \text{Muslim}_i + \psi_c \times \text{Muslim}_i + \varepsilon_{idc}. \quad (2)$$

Here μ_{dc} denotes childhood-district-by-birth-cohort fixed effects, $\phi_d \times \text{Muslim}_i$ denotes childhood-district-specific Muslim effects, and $\psi_c \times \text{Muslim}_i$ denotes birth-cohort-specific Muslim effects. This specification absorbs the main effect of conflict exposure through the district-by-cohort fixed effects, since F_{dc} varies at the childhood-district-by-cohort level. It also absorbs the average Muslim difference within each childhood district and the average Muslim difference within each birth cohort. The coefficient β therefore captures how the within-district-cohort Muslim–Hindu outcome gap varies with childhood conflict exposure, after allowing for persistent district-specific religious gaps and cohort-specific changes in religious inequality.

We report both specifications for the main outcomes. The baseline model is useful because it decomposes the relationship into the association between fatalities and outcomes for Hindus, the Hindu–Muslim difference at zero measured fatalities, and the additional association between fatalities and outcomes for Muslims relative to Hindus. This helps show separately how childhood conflict exposure is associated with outcomes for Hindus and whether that association differs for Muslims. The saturated specification is more demanding and is our preferred model for interpretation. We discuss the identifying role of these fixed effects and the remaining threats to identification in more detail below. For the mechanism and adjustment-margin outcomes, including educational attainment, employment and earnings, type of employment, and migration, we report only the saturated specification.

As a complement to conventional inference, we also report individual randomisation-inference p -values for the coefficient on $Fatalities \times \text{Muslim}$ in the saturated specification, computed using `randcmd` (Young, 2019). These p -values are based on repeated reassignments of *Fatalities* at the childhood district-by-birth-cohort level and are reported in square brackets directly below the conventional clustered standard errors. For the related outcomes within psychological well-being, economic well-being, educational attainment, employment and earnings, and type of employment, we additionally report Westfall–Young randomisation- t p -values that account for multiple hypothesis testing within each family.

Discussion on Identification

We now discuss in more detail the identifying role of the saturated fixed-effects specification and the assumptions underlying our interpretation. The childhood-district-by-birth-cohort fixed effects absorb shocks and conditions shared by Hindus and Muslims born in the same district and cohort, including local economic conditions, public goods, schooling infrastructure, health environments, and cohort-specific local shocks. The childhood-district-by-religion fixed effects absorb persistent Hindu–Muslim differences within each childhood district, such as long-standing differences in socioeconomic status, residential segregation, occupational structure, or access to local institutions. The birth-cohort-by-religion fixed effects absorb changes in Hindu–Muslim gaps across birth cohorts that are common across districts, including national political changes, aggregate educational expansion, or cohort-specific changes in minority status.

The conflict-district restriction complements this fixed-effects structure by narrowing the identifying comparison to locations with demonstrated susceptibility to communal violence. Districts that never experience a recorded Hindu–Muslim riot over several decades may differ systematically from conflict-experienced districts in persistent features of intergroup relations and the underlying propensity for communal violence. Restricting attention to this more comparable risk set therefore reduces reliance on comparisons between places with and without any history of recorded communal conflict. Within this sample, identification asks whether the within-district-cohort Muslim–Hindu outcome gap varies systematically with childhood conflict exposure across district-cohort cells.

The identifying assumption is that, conditional on these fixed effects and individual controls, there are no unobserved district-cohort-specific shocks that differentially affect Muslims relative to Hindus and are also correlated with childhood riot fatalities.

The main threat to this assumption is that conflict occurrence and intensity may reflect unobserved local political or social processes that also change Hindu–Muslim gaps over time within districts. Such differential district-cohort shocks are not absorbed by the fixed effects.⁵ Selective migration may also affect the composition of adults observed in LASI: families who moved in response to violence may differ systematically from those who remained, and LASI observes respondents’ later-life locations and ret-

⁵One concrete version of this concern is suggested by [Mitra and Ray \(2014\)](#), who show that changes in the relative economic position of Muslims can themselves predict Hindu–Muslim violence. Our fixed effects absorb persistent district-specific religious gaps and changes in religious gaps across cohorts that are common across districts, but they do not absorb district-specific changes in relative Muslim economic conditions over time. If such changes both predict childhood riot exposure and persistently affect later-life Hindu–Muslim gaps, they could therefore confound the estimated differential.

rospective childhood residence rather than a complete annual residence history. Finally, measurement error in childhood district assignment and conflict exposure may attenuate the estimates, especially for respondents who moved during childhood or whose childhood district is reported retrospectively.

The exposure measure should also be interpreted as capturing the broader consequences of fatal communal conflict rather than the effect of fatalities alone. Riot fatalities may be accompanied by displacement, school closures, neighbourhood destruction, disruptions to local labour markets, and other forms of local disruption. To the extent that these accompany or result from communal violence, they are part of the broader exposure captured by our reduced-form estimates rather than separate confounders.

We also take several steps to assess the remaining empirical concerns. We assign childhood exposure using the respondent’s reported childhood district where available and leave childhood district unassigned when the available information is insufficient. In the robustness analysis, we show that the main results are not driven by the restriction to conflict-experienced districts, remain strong in a younger-cohort sample that is less exposed to cumulative old-age mortality selection, and are robust to allowing the Muslim–Hindu gap to follow district-specific linear cohort trends and to vary flexibly across birth cohorts within states. Together, these exercises address important sources of measurement and sample-selection concern and allow for substantially greater spatial heterogeneity in the evolution of Muslim–Hindu gaps across cohorts, although the identifying assumption regarding unobserved shocks that differentially affect Muslims and Hindus remains central.

5 Results

We begin with psychological and economic well-being, for which we report both the baseline and saturated fixed-effects specifications. The saturated specification is our preferred model for interpretation, while the baseline estimates help show the underlying components of the relationship. Throughout, we focus on the coefficient on *Fatalities* $\times$ Muslim.

5.1 Psychological Well-Being

We examine whether childhood conflict exposure affects later-life psychological well-being using the estimates in Table 2. Panel A reports the baseline specification, while Panel B reports our preferred saturated fixed-effects specification.

The baseline estimates in Panel A are useful for decomposing the relationship. For Hindus, greater childhood conflict exposure is associated with lower life satisfaction and perceived social position, but shows little relationship with depressive symptoms or sleep problems; sleep-medication use declines slightly. For Muslims, combining the main and interaction coefficients, the association with childhood fatalities is adverse across all five measures and statistically significant at conventional levels. The group contrast is particularly clear for depressive symptoms, where the association for Hindus is essentially zero but becomes clearly positive for Muslims. For life satisfaction and perceived social position, exposure is associated with worse outcomes for both groups, with an additional deterioration among Muslims.⁶

Panel B strengthens the central result. In our preferred specification, greater childhood conflict exposure is associated with a statistically significant worsening of the Muslim–Hindu differential across all five dimensions of psychological well-being. A one standard deviation increase in childhood fatalities raises the probability of depressive symptoms among Muslims relative to Hindus by 2.5 percentage points, about 9 percent of the sample mean. It lowers life satisfaction by 0.044 points on the 1–7 scale and reduces the probability of reporting high perceived social position by 2.0 percentage points, about 26 percent of the sample mean. The same increase in exposure raises the sleep problems index by 0.046 points and increases the use of medication or other treatment for sleep problems by 0.5 percentage points, about 24 percent of the sample mean. Taken together, the results show that the adverse differential extends across depressive symptoms, subjective well-being, perceived social position, and sleep rather than being concentrated in a single outcome.⁷

The comparison across specifications is also informative. The Muslim–Hindu differentials for depressive symptoms and life satisfaction are very similar in the baseline and saturated specifications, while the differentials for perceived social position, sleep problems, and sleep-medication use become more adverse in the saturated model, particularly for the first two outcomes. Thus, the adverse differential is not attenuated when we move to the more demanding specification that absorbs childhood-district-by-birth-cohort conditions and allows religious gaps to vary flexibly across childhood districts

⁶Because *Fatalities* retains its natural zero, the Muslim coefficient in Panel A captures the conditional Muslim–Hindu difference when measured childhood fatalities are zero. This difference generally indicates worse outcomes for Muslims compared to Hindus, although it is statistically significant only for the Cantril ladder, where Muslims are 6.6 percentage points less likely to report high perceived social position.

⁷The individual randomisation-inference p -values are below 0.10 for all five outcomes. The Westfall–Young randomisation- t p -value accounting for multiple hypothesis testing across the five psychological well-being outcomes is 0.005.

and birth cohorts. Taken together, the results show a broad and persistent widening of Muslim–Hindu differences in psychological well-being with greater childhood exposure to communal violence.

The magnitude and persistence of these effects can be viewed in the context of the literature on childhood exposure to violent conflict. Existing studies of major wars find sizeable long-run mental-health consequences of childhood exposure (Singhal, 2019; Akbulut-Yuksel et al., 2025).⁸ Our depressive-symptom effect is more modest than these estimates from severe wartime settings, but it captures a different object: a persistent Muslim–Hindu differential arising from recurrent communal violence rather than the average scarring effect of large-scale warfare. The persistence is notable given that our outcomes are measured more than three decades after childhood exposure. Evidence from Nepal also points to childhood as a particularly consequential exposure period: Benjet et al. (2020) find that nearby conflict violence increased the subsequent incidence of major depressive disorder among those who were younger than 11 at the onset of the conflict, with no comparable effect among older individuals.

The sleep results are also noteworthy, given recent work in development economics linking sleep to psychological well-being, cognition, productivity, and other economic outcomes in low-income settings (Bessone et al., 2021; Rao et al., 2021). In our context, greater childhood conflict exposure is associated with poorer sleep decades later, both through greater sleep difficulties and greater use of medication or other treatment for sleep problems among Muslims relative to Hindus. These results broaden the evidence on the long-run psychological consequences of childhood violence beyond conventional measures of mental health and subjective well-being.

5.2 Economic Well-Being

We next turn to later-life economic well-being using the estimates in Table 3. The outcomes capture both current household resources, through food and non-food consumption, and longer-run economic security, through durable assets, current residence ownership, and land ownership.⁹ As before, Panel A provides the baseline decomposi-

⁸Singhal (2019) reports that a one percent increase in bombing intensity during the Vietnam War raises the probability of severe mental distress in adulthood by 16 percentage points, approximately 50 percent of the mean. Akbulut-Yuksel et al. (2025) report that a one standard deviation increase in World War II bombing intensity is associated with about a 10 percent decline in long-run standardized mental health and a 16.2 percent increase in the likelihood of clinical depression. These magnitudes are not directly comparable to ours because both the exposure scales and mental-health measures differ.

⁹These four economic measures are defined at the household level and therefore take the same value for respondents living in the same household; Section 5.4.5 examines robustness to the resulting repeated-household structure.

tion, while Panel B presents our preferred saturated fixed-effects estimates.

Panel A shows that the relationship with childhood conflict exposure differs across the two groups. Among Hindus, fatalities are only weakly related to consumption, durable assets, and current residence ownership, while land ownership increases with exposure. Among Muslims, the implied total association is negative for consumption and current residence ownership: a one standard deviation increase in childhood fatalities is associated with about 3.7 percent lower household consumption and a 1.8 percentage point lower probability of owning the current residence. The corresponding associations with durable assets and land ownership are small and statistically insignificant. Thus, in the baseline specification, the clearest adverse economic relationships for Muslims appear in current consumption and residence ownership.¹⁰

The preferred estimates in Panel B point to a broader adverse economic differential. A one standard deviation increase in childhood fatalities lowers household consumption among Muslims relative to Hindus by about 2.9 percent. It also reduces the durable asset index by 0.084 standard deviations, lowers the probability of owning the current residence by 2.7 percentage points, and lowers the probability of owning land by 2.2 percentage points. All four estimates are statistically significant. The pattern therefore extends beyond contemporaneous consumption to multiple dimensions of household asset ownership and economic security.¹¹

The shift from Panel A to Panel B is particularly important for asset ownership. The baseline specification provides little evidence of an adverse relationship between fatalities and either durable assets or land ownership among Muslims, whereas both effects become negative and statistically significant in the saturated specification. More generally, once childhood district conditions and religious gaps are allowed to vary flexibly across districts and cohorts, the adverse Muslim–Hindu differential appears consistently across the full set of economic outcomes. Overall, the estimates point to a persistent widening of Muslim–Hindu differences in both current resources and longer-run asset accumulation.

These findings are consistent with a broader literature showing that violent conflict can have lasting effects on household welfare and asset accumulation through disruptions to markets, institutions, social networks, and the security of physical assets (Justino,

¹⁰In Panel A, the Muslim coefficient is evaluated at zero measured childhood fatalities. At this point, Muslims have significantly lower durable asset ownership and land ownership, while the corresponding differences in consumption and current residence ownership are imprecisely estimated.

¹¹The individual randomisation-inference p -values are below 0.01 for all four outcomes. The Westfall–Young randomisation- t p -value accounting for multiple hypothesis testing across the four economic well-being outcomes is 0.007.

2011; Bundervoet, 2010). In the Indian context, Gupte et al. (2014) highlight the relationship between economic vulnerability, community support, and exposure to riot violence. Our results add a long-run dimension to this evidence: childhood exposure to communal violence is associated with lower economic well-being decades later, with particularly adverse consequences for Muslims.

5.3 Mechanisms and Adjustment Margins

We next examine pathways and adjustment margins that may help interpret the psychological and economic disparities documented above. We organise the evidence in life-cycle order: educational attainment, adult employment and earnings, type of employment among currently working respondents, and migration. These outcomes provide evidence on plausible mechanisms through which childhood conflict exposure may shape later-life well-being, although they do not isolate the contribution of each mechanism separately. All tables in this subsection report our preferred saturated fixed-effects specification.

We begin with educational attainment and present the results in Table 4. The estimates reveal a clear contrast between average years of schooling and progression to higher levels of education. Childhood conflict exposure has little detectable effect on total years of schooling, but is associated with significantly lower probabilities of completing secondary, higher secondary, and tertiary education for Muslims relative to Hindus. Moreover, the percentage-point effects are broadly similar across these levels even as baseline completion rates fall sharply, implying increasingly large proportional losses at higher levels of education.

This pattern suggests that the educational consequences of childhood conflict are more visible in progression through the education system than in average years completed. It is consistent with evidence that conflict exposure can disrupt human-capital accumulation in childhood and generate persistent educational losses (Ichino and Winter-Ebmer, 2004; Leon, 2012; Akresh et al., 2012, 2023). In our setting, reduced educational progression provides a plausible mechanism through which childhood communal violence may contribute to later-life economic disadvantage among Muslims.

We then turn to adult employment and earnings and present the results in Table 5. The estimates reveal a striking contrast between employment and earnings. A one standard deviation increase in childhood fatalities raises current employment among Muslims relative to Hindus by 2.3 percentage points, against a sample mean of 59.3 percent, while having little detectable effect on hours worked. Yet this higher employment

is accompanied by substantially lower earnings: among those who are employed, annual income is about 14 percent lower for exposed Muslims relative to Hindus.

This pattern does not look like simple withdrawal from the labour market. Exposed Muslims are, if anything, more likely to be working. The disadvantage instead appears to operate through lower returns to work: even conditional on employment, exposed Muslims earn substantially less. This is consistent with worse access to remunerative jobs, lower-quality employment, or weaker bargaining power in adulthood.

Table 6 clarifies the composition of this labour-market adjustment. Among currently employed respondents, childhood fatalities increase Muslims' probability of working in family farm activities by 2.7 percentage points, while reducing their probability of salaried employment by 3.9 percentage points. The estimates for agricultural labour, self-employment, paid family work, and unpaid work are not statistically significant. The clearest change in employment composition is therefore away from salaried work and toward own or family-operated agricultural activity.

This composition helps reconcile the employment and earnings results. Salaried employment represents an important form of movement away from family-based agricultural activity toward market-based employment. A decline in salaried employment, combined with lower income conditional on working, therefore suggests that exposed Muslims remain more reliant on family-based agricultural work even while remaining economically active. The positive family-farm coefficient should not be read as movement into more productive or asset-rich agriculture, especially given the negative effect on land ownership in Table 3. Rather, taken together, the results point to weaker movement into salaried employment and greater reliance on own or family-operated agricultural work. This interpretation is in line with evidence on discrimination and intergroup frictions in Indian economic institutions, including communal-conflict exposure in lending decisions and discrimination against Muslim applicants in urban labour markets (Fisman et al., 2020; Thorat and Attewell, 2007).

We finally examine residential mobility, with the results reported in Table 7.¹² The estimates show little evidence that childhood conflict exposure increased overall migration for Muslims relative to Hindus: the coefficient on ever migrated is positive but small and statistically insignificant. Exposure is also not associated with greater migration during ages 1–14. The only evidence of a mobility response appears after childhood: a

¹²The migration measures are constructed from the respondent's reported duration of continuous residence in the current location. They therefore capture whether and when the respondent moved to the current location, rather than the full sequence of moves over the life course, and should not be interpreted as direct measures of riot-induced displacement.

one standard deviation increase in fatalities raises the probability of migrating at age 15 or older by 1.2 percentage points.

The timing of the migration response is informative. Childhood conflict exposure is not associated with greater migration during ages 1–14, the period in which exposure itself is measured. The positive response appears instead after age 14, suggesting that migration is more likely to be a later adjustment to the consequences of childhood exposure than an immediate response during childhood. This is consistent with broader evidence that violence can reshape location choices, household risk-coping strategies, and access to local economic networks (Ibáñez and Moya, 2010; Kondylis, 2010). In India, communal violence has also been linked to residential segregation, property rights, and neighbourhood-level incentives to displace minorities (Field et al., 2008).

Overall, the evidence suggests that educational progression and labour-market position are plausible pathways linking childhood conflict exposure to later-life economic disadvantage, with post-childhood migration providing an additional adjustment margin.

5.4 Robustness Analysis

We assess robustness along several dimensions: heterogeneity by gender, inclusion of respondents from childhood districts with no recorded Hindu–Muslim riot during 1950–1995, restriction to respondents born in 1960 or later, allowing the evolution of Hindu–Muslim gaps across birth cohorts to vary more flexibly across locations, and the treatment of household-level economic outcomes when a household contributes multiple respondents. For the first four exercises, because the psychological and economic results span multiple outcomes, we also construct summary indices for these two domains to provide a common measure across the robustness exercises.¹³ Panel A of Figure 2 provides the corresponding index-based summary of the preferred main-sample results, before we turn to the robustness analyses.

¹³We construct standardized inverse-covariance-weighted indices following Anderson (2008), implemented in Stata using the `swindex` command of Schwab et al. (2020). The psychological well-being index combines depressive symptoms (CESD-10), life satisfaction, the Cantril Ladder, sleep problems, and sleep medication. The economic well-being index combines log household consumption, the durable-assets index, current residence ownership, and land ownership. Component outcomes are oriented so that higher values indicate better well-being. Both indices are normalized using the preferred main sample, so that their scale is held fixed across the robustness exercises.

5.4.1 Gender Heterogeneity

We first examine whether the Muslim–Hindu differential associated with childhood conflict exposure varies by respondent gender. Panel B of Figure 2 shows lower psychological and economic well-being among Muslims relative to Hindus for both women and men. The psychological well-being index estimates are similar across genders, suggesting that the widening of the Muslim–Hindu gap in psychological well-being is not concentrated in either gender. The household economic well-being estimate is more negative among female respondents, although the components of this index measure household consumption and asset ownership rather than resources individually owned by women.¹⁴ The main psychological and economic patterns are therefore present for both women and men.

Gender differences are more pronounced in educational attainment. Table A.1 shows that the reductions in years of schooling and secondary completion are larger among women, while the tertiary-completion effect is also somewhat larger; the higher-secondary effects are similar across genders. This pattern is consistent with gendered household responses to conflict and insecurity, including differences in educational investment, mobility constraints, and gender norms that may make Muslim girls’ human-capital accumulation particularly vulnerable to childhood conflict exposure. Related evidence from India shows that communal violence can affect women’s marriage and educational outcomes (Ghose and Pandey, 2025). Our estimates do not identify these particular channels, but they indicate that the educational consequences of childhood exposure are more pronounced for women on several margins. By contrast, the pattern of higher employment but lower earnings and salaried employment is present for both women and men. This strengthens the interpretation that the estimated long-run minority penalty is a broad phenomenon rather than one driven by the gender composition of the sample, while also revealing meaningful gender heterogeneity in educational responses.

5.4.2 Conflict-District Restriction

Our preferred sample restricts attention to respondents whose childhood districts recorded at least one Hindu–Muslim riot during 1950–1995. This focuses the analysis on districts in which communal conflict was a relevant local phenomenon, but it also conditions the sample on having experienced conflict at some point during this period. We therefore

¹⁴Given the prevalence of patrilocal residence, one possible interpretation is that Muslim women exposed to communal conflict during childhood subsequently marry into poorer households, relative to similarly exposed Hindu women.

re-estimate the saturated specification using the full available Hindu–Muslim sample, adding respondents from districts with no recorded conflict.¹⁵ For these respondents, cumulative childhood fatalities are zero by construction.

The results change very little. Panel C of Figure 2 shows that the estimates for the psychological and economic well-being indices are very similar to those in the preferred sample. The outcome-specific estimates in column (2) of Table A.2 show the same pattern. All five psychological well-being estimates remain in the same direction as in the preferred sample and statistically significant. Childhood exposure also continues to be associated with lower household consumption, a lower durable-assets index, and lower probabilities of owning the current residence and land for Muslims relative to Hindus.

The broader life-cycle patterns are similarly preserved. Secondary, higher-secondary, and tertiary completion remain lower; current employment is higher, but earnings are substantially lower; and employment shifts toward family-farm activity and away from salaried work. The positive association with migration after age 14 also persists. The similarity of the estimates across the preferred and full samples therefore shows that the paper’s main findings are not driven by conditioning the analysis on conflict-experienced districts.

5.4.3 Younger Cohorts

A further concern is that LASI observes respondents only if they survive to the time of the survey. If childhood exposure to communal violence differentially affected the survival of Muslims and Hindus, the older cohorts observed in LASI could be increasingly selected. We therefore restrict the preferred sample to respondents born in 1960 or later, who are substantially younger at the time of the survey and hence less exposed to cumulative mortality selection. Column (3) of Table A.2 reports the estimates.

The main results remain strong. The psychological and economic results persist, as do the reductions in higher-secondary and tertiary completion and the labour-market pattern of higher current employment but lower earnings and salaried employment. Panel C of Figure 2 shows corresponding declines in both summary indices. The post-childhood migration effect does not persist in this younger subsample.

¹⁵This substantially expands both the geographic coverage and the number of respondents. The saturated specifications in the preferred sample draw on 222 childhood districts, compared with 500 in the full sample; depending on the outcome, the estimation sample increases from roughly 20,500–21,000 respondents to about 40,000–41,300 respondents.

5.4.4 Additional Cohort Controls

Our preferred specification includes childhood-district-by-birth-cohort fixed effects, which absorb conditions and shocks shared by Hindus and Muslims born in the same district and cohort; childhood-district-by-religion fixed effects, which absorb persistent differences in the Muslim–Hindu gap across districts; and birth-cohort-by-religion fixed effects, which absorb changes in the Muslim–Hindu gap across birth cohorts that are common across districts. A remaining concern is that the evolution of the Muslim–Hindu gap across birth cohorts may itself differ systematically across locations in ways that are correlated with childhood conflict exposure. We examine this possibility using two sets of additional cohort controls. First, we add district-by-religion-specific linear cohort trends, allowing the Muslim–Hindu gap to follow a different linear trajectory in each childhood district. Second, we include state-by-religion-by-cohort fixed effects, allowing the Muslim–Hindu gap to vary flexibly across birth cohorts within each state.

Panel D of Figure 2 shows that greater childhood conflict exposure remains associated with lower psychological and economic well-being among Muslims relative to Hindus under both specifications. The estimates are similar to the preferred specification when state-by-religion-by-cohort fixed effects are included, while the economic well-being estimate becomes larger in magnitude when district-by-religion-specific linear cohort trends are added. Overall, the main results are robust to allowing substantially greater spatial heterogeneity in the evolution of Muslim–Hindu gaps across birth cohorts.

5.4.5 Household-Level Economic Outcomes

The four economic outcomes in Table 3 are measured at the household level, whereas childhood conflict exposure is respondent-specific. Because LASI interviews age-eligible adults and their spouses, some households contribute multiple respondents to the analysis, so that the same household outcome may be associated with more than one respondent-specific childhood exposure. In the preferred sample, 35.4 percent of households contribute more than one eligible respondent. This raises two related concerns: observations from the same household share the same economic outcome and may therefore have correlated residuals, and households with multiple respondents receive greater total influence in the respondent-level specification.

We first address the inference concern by retaining the original specification and individual survey weights but two-way clustering standard errors by childhood district and current household. These clustering dimensions are not nested because childhood district is respondent-specific, and members of the same current household can have

grown up in different districts. The resulting standard errors are virtually unchanged.

We next address the greater influence of multi-respondent households by distributing each household’s survey weight equally across its usable respondents in the relevant estimation sample. This preserves each respondent’s own childhood exposure and the original fixed-effects structure, while preventing households from receiving additional total weight simply because they contribute multiple respondents. The coefficients remain negative across all four household economic outcomes (Table A.3). The consumption estimate becomes smaller but remains statistically significant at the 10 percent level, while the estimates for durable assets, current residence ownership, and land ownership remain precisely estimated. The broader pattern of poorer later-life household economic circumstances among more exposed Muslims relative to Hindus is therefore preserved after accounting for the repeated-household structure.¹⁶

6 Conclusion

This paper asks whether childhood exposure to intergroup violence can widen long-run inequality between the groups that share the same local environment. Linking respondents’ childhood locations to Hindu–Muslim riot fatalities, we find that greater exposure is associated with a persistent worsening of Muslim–Hindu gaps in psychological well-being and material security decades later. The same exposure is also associated with lower educational progression, lower earnings and salaried employment, and greater reliance on family-based agricultural work among Muslims relative to Hindus. These patterns are present across genders and remain when we broaden the sample to include districts with no recorded riots, restrict attention to younger cohorts, and allow substantially greater spatial heterogeneity in the evolution of Muslim–Hindu gaps across birth cohorts.

The central implication is distributional. Communal violence need not leave only a place-based legacy; it can also reshape the relative position of the groups that lived through it. In settings where violence is organised along group lines, average local effects can therefore conceal persistent minority-specific costs. Our findings point to one way in which episodic violence may become embedded in longer-run group inequality: through differences in psychological well-being, human-capital accumulation, and economic trajectories that remain visible long after the original episode has ended.

The findings also point to a longer horizon for post-conflict recovery. Rebuilding

¹⁶Results are also similar if the individual survey weight, rather than the household survey weight, is divided by the number of usable respondents within each household.

infrastructure and restoring local economic activity address immediate losses, but they may not be sufficient when exposure occurs during childhood and its consequences persist through education, psychological well-being, and later economic opportunity. Attention to these longer-run dimensions is therefore important for understanding, and potentially mitigating, the enduring inequality associated with communal violence.

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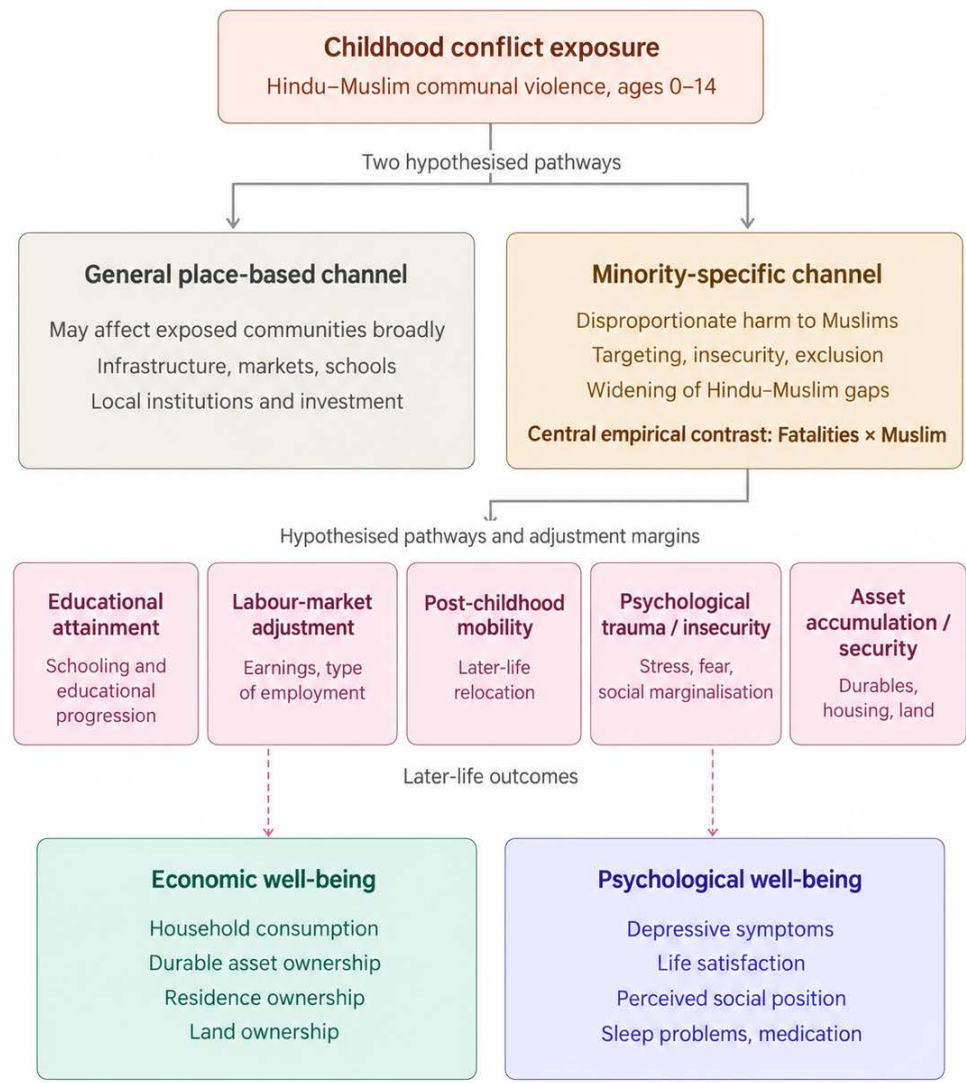
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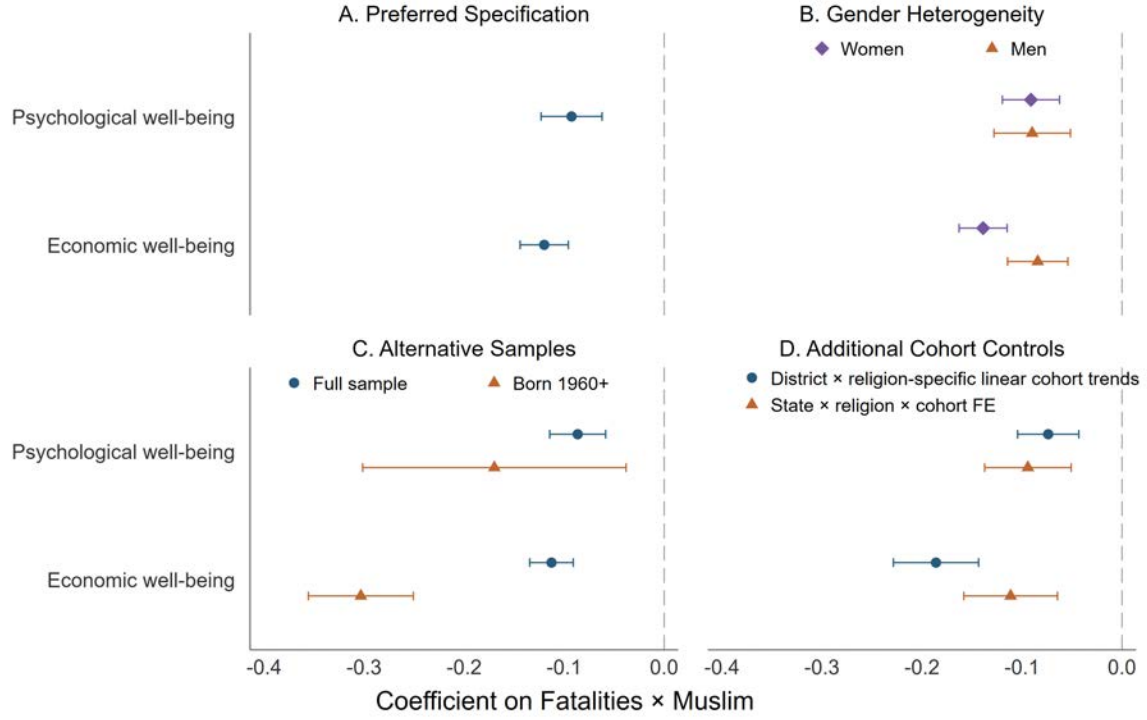
Figures and Tables

Figure 1: Conceptual Framework



Source: Authors’ compilation.

Figure 2: Robustness of Effects on Psychological and Economic Well-Being



Notes: The figure reports coefficients on $Fatalities \times Muslim$ for summary indices of psychological and economic well-being; negative coefficients indicate a worsening of Muslim well-being relative to Hindu well-being. Panel A reports estimates from the preferred specification in Equation (2). Panel B reports gender-specific estimates from a pooled specification allowing the $Fatalities \times Muslim$ association to vary by respondent sex. Panel C reports two alternative-sample estimates: the full available Hindu–Muslim sample, including respondents from childhood districts with no recorded Hindu–Muslim riot during 1950–1995, and the preferred sample restricted to respondents born in 1960 or later. Panel D reports estimates with two sets of additional cohort controls: the first specification augments Equation (2) with childhood-district-by-religion-specific linear birth-cohort trends; the second replaces the birth-cohort-by-religion fixed effects in Equation (2) with childhood-state-by-religion-by-birth-cohort fixed effects. The indices are constructed so that higher values indicate better well-being and are normalized using the preferred main sample. $Fatalities$ is scaled using its standard deviation in the preferred main sample, so coefficients are comparable across panels. All regressions use LASI individual weights and cluster standard errors at the childhood-district level. Horizontal bars show 95% confidence intervals.

Table 1: Descriptive Statistics

	Observations (1)	Mean (2)	Standard Deviation (3)	Minimum (4)	Maximum (5)
<i>Treatment (childhood conflict exposure)</i>					
Fatalities, SD-scaled	21,552	0.160	1.000	0.000	12.058
<i>Psychological well-being</i>					
CES-D 10	20,446	0.277	0.447	0.000	1.000
Life satisfaction	20,446	4.667	1.614	0.000	7.000
Cantril ladder	20,446	0.077	0.267	0.000	1.000
Sleep problems index	20,556	0.037	1.021	-0.603	2.764
Sleep medication	20,556	0.021	0.142	0.000	1.000
<i>Economic well-being</i>					
Log consumption	20,076	11.890	0.759	0.000	16.672
Durable asset ownership	20,556	-0.012	0.994	-1.440	4.922
Current residence ownership	20,556	0.870	0.336	0.000	1.000
Land ownership	20,556	0.447	0.497	0.000	1.000
<i>Education</i>					
Years of schooling	20,556	4.582	5.162	0.000	23.000
Secondary completion	20,556	0.219	0.414	0.000	1.000
Higher secondary completion	20,556	0.131	0.338	0.000	1.000
Tertiary completion	20,556	0.074	0.261	0.000	1.000
<i>Employment</i>					
Currently employed	20,556	0.593	0.491	0.000	1.000
Hours worked	20,554	24.885	26.923	0.000	168.000
Annual income	20,554	52119.812	88039.891	0.000	5.40e+05
Conditional annual income	10,995	87193.094	1.00e+05	0.000	5.40e+05
Conditional log income	10,995	10.663	2.042	0.000	14.691
<i>Type of employment</i>					
Family farm activities	10,997	0.322	0.467	0.000	1.000
Agricultural labour	10,997	0.149	0.356	0.000	1.000
Self-employed	10,997	0.157	0.364	0.000	1.000
Salaried	10,997	0.269	0.443	0.000	1.000
Paid family work	10,997	0.004	0.066	0.000	1.000
Unpaid work	10,997	0.019	0.136	0.000	1.000
<i>Migration</i>					
Ever migrated	20,542	0.516	0.500	0.000	1.000
Migrated ages 1–14	20,542	0.086	0.280	0.000	1.000
Migrated ages 15+	20,542	0.430	0.495	0.000	1.000

Notes: Statistics are weighted using LASI individual sampling weights. Variable definitions are provided in Section 3. *Fatalities* is reported for the full analytic sample, while outcome statistics correspond to the samples used in the respective main analyses; observation counts therefore vary across rows. *Fatalities* is cumulative childhood riot fatalities divided by the standard deviation of cumulative fatalities in the main Hindu–Muslim conflict-district analysis sample, without demeaning.

Table 2: Effect of Childhood Conflict Exposure on Psychological Well-Being

	CESD-10 (1)	Life Satisfaction (2)	Cantril Ladder (3)	Sleep Problems Index (4)	Sleep Medication (5)
<i>Panel A: Baseline Specification</i>					
Fatalities	0.002 (0.003)	-0.057*** (0.012)	-0.011*** (0.003)	-0.001 (0.005)	-0.002** (0.001)
Muslim	0.019 (0.017)	-0.067 (0.083)	-0.066*** (0.025)	0.020 (0.066)	0.006 (0.005)
Fatalities $\times$ Muslim	0.023*** (0.003)	-0.040** (0.017)	-0.004** (0.002)	0.012 (0.007)	0.003*** (0.001)
Effect of Fatalities for Muslims	0.025*** (0.003)	-0.097*** (0.020)	-0.016*** (0.002)	0.011* (0.006)	0.002* (0.001)
Mean dependent variable	0.277	4.672	0.077	0.039	0.021
Observations	21437	21437	21437	21550	21550
R-squared	0.097	0.148	0.098	0.079	0.034
District FE	Yes	Yes	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes	Yes	Yes
<i>Panel B: Saturated Fixed-Effects Specification</i>					
Fatalities $\times$ Muslim	0.025*** (0.007) [0.011]	-0.044** (0.018) [0.059]	-0.020*** (0.003) [0.001]	0.046*** (0.013) [0.014]	0.005** (0.003) [0.084]
Mean dependent variable	0.277	4.667	0.077	0.037	0.021
Observations	20446	20446	20446	20556	20556
R-squared	0.328	0.361	0.336	0.302	0.253
District $\times$ Birth Cohort FE	Yes	Yes	Yes	Yes	Yes
District $\times$ Religion FE	Yes	Yes	Yes	Yes	Yes
Birth Cohort $\times$ Religion FE	Yes	Yes	Yes	Yes	Yes

Notes: Panel A reports the baseline specification, while Panel B reports the saturated fixed-effects specification. In Panel A, “Effect of Fatalities for Muslims” reports the estimated association between *Fatalities* and the outcome for Muslim respondents. The fixed effects included in each specification are shown in the table. In Panel B, the main effect of *Fatalities* is absorbed by District $\times$ Birth Cohort fixed effects, while the main effect of Muslim is absorbed by District $\times$ Religion and Birth Cohort $\times$ Religion fixed effects. CESD-10 is an indicator for depressive symptoms based on the 10-item CESD scale, equal to one if the respondent reports four or more depressive symptoms. Life Satisfaction is the mean of five Satisfaction with Life Scale questions measured on a 1–7 scale. The Cantril Ladder is an indicator equal to one if the respondent places themselves on rung eight or higher of a ten-rung ladder representing their perceived position in society. The Sleep Problems Index is a standardised weighted index, constructed using *swindex*, of four indicators for frequent or occasional sleep difficulties: trouble falling asleep, waking during the night, waking too early, and feeling unrested. Sleep Medication is an indicator equal to one if the respondent reports taking medication or other treatment for sleep problems. Means and observation counts correspond to the estimation sample in each panel. All regressions include caste and sex controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. Randomisation-inference *p*-values for the coefficient on *Fatalities* $\times$ *Muslim* are reported in square brackets directly below the conventional standard errors (Young, 2019). The corresponding Westfall–Young randomisation-*t* *p*-value, accounting for multiple hypothesis testing across the five psychological well-being outcomes, is 0.005. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Effect of Childhood Conflict Exposure on Household Consumption and Asset Ownership

	Log Consumption (1)	Durable Asset Ownership (2)	Current Residence Ownership (3)	Land Ownership (4)
<i>Panel A: Baseline Specification</i>				
Fatalities	-0.006 (0.006)	0.008 (0.007)	-0.007 (0.005)	0.009*** (0.003)
Muslim	0.039 (0.036)	-0.185*** (0.044)	-0.078 (0.060)	-0.166*** (0.031)
Fatalities $\times$ Muslim	-0.031*** (0.006)	0.005 (0.013)	-0.011** (0.006)	-0.015** (0.006)
Effect of Fatalities for Muslims	-0.037*** (0.008)	0.013 (0.009)	-0.018*** (0.006)	-0.006 (0.005)
Mean dependent variable	11.894	0.000	0.870	0.446
Observations	21073	21550	21550	21550
R-squared	0.181	0.299	0.190	0.256
District FE	Yes	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes	Yes
<i>Panel B: Saturated Fixed-Effects Specification</i>				
Fatalities $\times$ Muslim	-0.029*** (0.007) [0.003]	-0.084*** (0.019) [0.006]	-0.027*** (0.005) [0.001]	-0.022*** (0.005) [0.005]
Mean dependent variable	11.890	-0.012	0.870	0.447
Observations	20076	20556	20556	20556
R-squared	0.396	0.511	0.414	0.433
District $\times$ Birth Cohort FE	Yes	Yes	Yes	Yes
District $\times$ Religion FE	Yes	Yes	Yes	Yes
Birth Cohort $\times$ Religion FE	Yes	Yes	Yes	Yes

Notes: Panel A reports the baseline specification, while Panel B reports the saturated fixed-effects specification. In Panel A, “Effect of Fatalities for Muslims” reports the estimated association between *Fatalities* and the outcome for Muslim respondents. The fixed effects included in each specification are shown in the table. In Panel B, the main effect of *Fatalities* is absorbed by District $\times$ Birth Cohort fixed effects, while the main effect of Muslim is absorbed by District $\times$ Religion and Birth Cohort $\times$ Religion fixed effects. Log Consumption is the log of one plus annual household food and non-food consumption. Durable Asset Ownership is the standardised first principal component of 23 household durable ownership indicators, following the asset-index approach of [Filmer and Pritchett \(2001\)](#), with higher values indicating greater durable asset ownership. Current Residence Ownership is an indicator equal to one if the household owns its current residence. Land Ownership is an indicator equal to one if the household owns land. Means and observation counts correspond to the estimation sample in each panel. All regressions include caste and sex controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. Randomisation-inference p -values for the coefficient on *Fatalities* $\times$ *Muslim* are reported in square brackets directly below the conventional standard errors ([Young, 2019](#)). The corresponding Westfall–Young randomisation- t p -value, accounting for multiple hypothesis testing across the four economic well-being outcomes, is 0.007. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 4: Effect of Childhood Conflict Exposure on Educational Attainment

	Years of Schooling (1)	Secondary Completion (2)	Higher Secondary Completion (3)	Tertiary Completion (4)
Fatalities $\times$ Muslim	-0.073 (0.066) [0.374]	-0.023*** (0.005) [0.004]	-0.031*** (0.004) [0.000]	-0.026*** (0.003) [0.002]
Mean dependent variable	4.582	0.219	0.131	0.074
Observations	20556	20556	20556	20556
R-squared	0.531	0.433	0.428	0.359
District $\times$ Birth Cohort FE	Yes	Yes	Yes	Yes
District $\times$ Religion FE	Yes	Yes	Yes	Yes
Birth Cohort $\times$ Religion FE	Yes	Yes	Yes	Yes

Notes: This table reports estimates from the saturated fixed-effects specification. The fixed effects included in the specification are shown in the table. The main effect of *Fatalities* is absorbed by District $\times$ Birth Cohort fixed effects, while the main effect of Muslim is absorbed by District $\times$ Religion and Birth Cohort $\times$ Religion fixed effects. Years of Schooling is the total number of completed years of education. Secondary Completion is an indicator equal to one if the respondent completed secondary schooling or higher. Higher Secondary Completion is an indicator equal to one if the respondent completed higher secondary schooling or higher. Tertiary Completion is an indicator equal to one if the respondent completed tertiary education. Means and observation counts correspond to the estimation sample. All regressions include caste and sex controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. Randomisation-inference p -values for the coefficient on *Fatalities $\times$ Muslim* are reported in square brackets directly below the conventional standard errors (Young, 2019). The corresponding Westfall–Young randomisation- t p -value, accounting for multiple hypothesis testing across the four educational attainment outcomes, is 0.001. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5: Effect of Childhood Conflict Exposure on Employment and Earnings

	Currently Employed (1)	Hours Worked (2)	Annual Income (3)	Conditional Annual Income (4)	Conditional Log Income (5)
Fatalities $\times$ Muslim	0.023*** (0.005) [0.004]	0.248 (0.265) [0.434]	-3811.972*** (809.043) [0.002]	-12475.793*** (1294.682) [0.001]	-0.152*** (0.026) [0.001]
Mean dependent variable	0.593	24.885	52119.811	87193.095	10.663
Observations	20556	20554	20554	10995	10995
R-squared	0.480	0.446	0.428	0.482	0.559
District $\times$ Birth Cohort FE	Yes	Yes	Yes	Yes	Yes
District $\times$ Religion FE	Yes	Yes	Yes	Yes	Yes
Birth Cohort $\times$ Religion FE	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from the saturated fixed-effects specification. The fixed effects included in the specification are shown in the table. The main effect of *Fatalities* is absorbed by District $\times$ Birth Cohort fixed effects, while the main effect of Muslim is absorbed by District $\times$ Religion and Birth Cohort $\times$ Religion fixed effects. Currently Employed is an indicator equal to one if the respondent is currently working. Hours Worked is the sum of reported average weekly hours in the respondent's main job and side job; observations with totals above 168 hours per week are excluded. Annual Income is total annual individual income from main and side work, winsorised at the 99th percentile. Conditional Annual Income is the same winsorised annual income measure, restricted to currently employed respondents. Conditional Log Income is the log of one plus annual individual income, restricted to currently employed respondents. Means and observation counts correspond to each outcome's estimation sample. All regressions include caste and sex controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. Randomisation-inference p -values for the coefficient on *Fatalities* $\times$ *Muslim* are reported in square brackets directly below the conventional standard errors (Young, 2019). The corresponding Westfall–Young randomisation- t p -value, accounting for multiple hypothesis testing across the five employment and earnings outcomes, is less than 0.001. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6: Effect of Childhood Conflict Exposure on Type of Employment

	Family Farm Activities (1)	Agricultural Labour (2)	Self Employed (3)	Salaried (4)	Paid Family Work (5)	Unpaid Work (6)
Fatalities $\times$ Muslim	0.027*** (0.006) [0.006]	0.004 (0.006) [0.588]	-0.002 (0.010) [0.890]	-0.039*** (0.008) [0.009]	0.001 (0.002) [0.598]	0.001 (0.002) [0.713]
Mean dependent variable	0.322	0.149	0.157	0.269	0.004	0.019
Observations	10997	10997	10997	10997	10997	10997
R-squared	0.427	0.389	0.406	0.465	0.439	0.367
District $\times$ Birth Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
District $\times$ Religion FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort $\times$ Religion FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports estimates from the saturated fixed-effects specification among currently employed respondents. The fixed effects included in the specification are shown in the table. The main effect of *Fatalities* is absorbed by District $\times$ Birth Cohort fixed effects, while the main effect of Muslim is absorbed by District $\times$ Religion and Birth Cohort $\times$ Religion fixed effects. Family Farm Activities is an indicator for work in own or family-operated farming, fishery, or forestry activities. Agricultural Labour is an indicator for wage work in agriculture. Self-Employed, Salaried, Paid Family Work, and Unpaid Work are indicators for the corresponding employment types. Means and observation counts correspond to the estimation sample. All regressions include caste and sex controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. Randomisation-inference p -values for the coefficient on *Fatalities* $\times$ *Muslim* are reported in square brackets directly below the conventional standard errors (Young, 2019). The corresponding Westfall–Young randomisation- t p -value, accounting for multiple hypothesis testing across the six type-of-employment outcomes, is 0.033. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

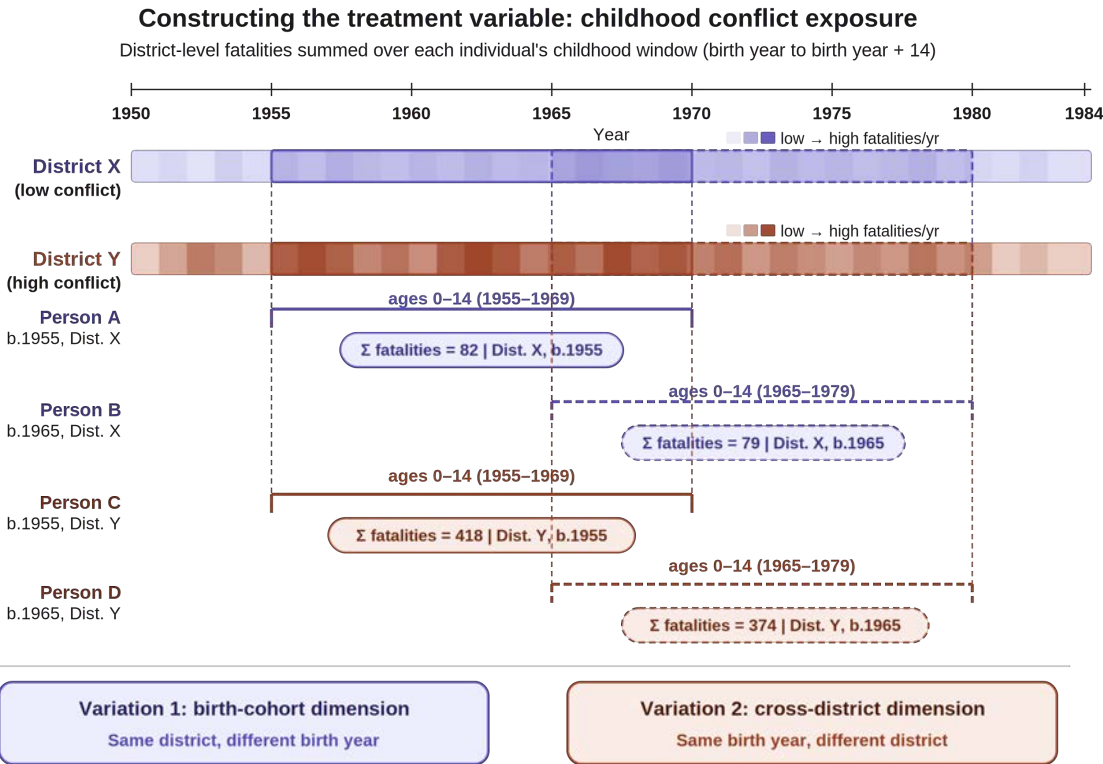
Table 7: Effect of Childhood Conflict Exposure on Migration

	Ever Migrated (1)	Migrated Ages 1–14 (2)	Migrated Ages 15+ (3)
Fatalities $\times$ Muslim	0.006 (0.007) [0.446]	-0.005 (0.004) [0.324]	0.012** (0.006) [0.110]
Mean dependent variable	0.516	0.086	0.430
Observations	20542	20542	20542
R-squared	0.541	0.288	0.468
District $\times$ Birth Cohort FE	Yes	Yes	Yes
District $\times$ Religion FE	Yes	Yes	Yes
Birth Cohort $\times$ Religion FE	Yes	Yes	Yes

Notes: This table reports estimates from the saturated fixed-effects specification. The fixed effects included in the specification are shown in the table. The main effect of *Fatalities* is absorbed by District $\times$ Birth Cohort fixed effects, while the main effect of Muslim is absorbed by District $\times$ Religion and Birth Cohort $\times$ Religion fixed effects. Ever Migrated is an indicator equal to one if the respondent reports not living in the current location since birth. Migrated Ages 1–14 is an indicator equal to one if the respondent moved to the current location between ages 1 and 14. Migrated Ages 15+ is an indicator equal to one if the respondent moved to the current location at age 15 or older. Means and observation counts correspond to the estimation sample. All regressions include caste and sex controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. Randomisation-inference p -values for the coefficient on *Fatalities* $\times$ *Muslim* are reported in square brackets directly below the conventional standard errors (Young, 2019). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

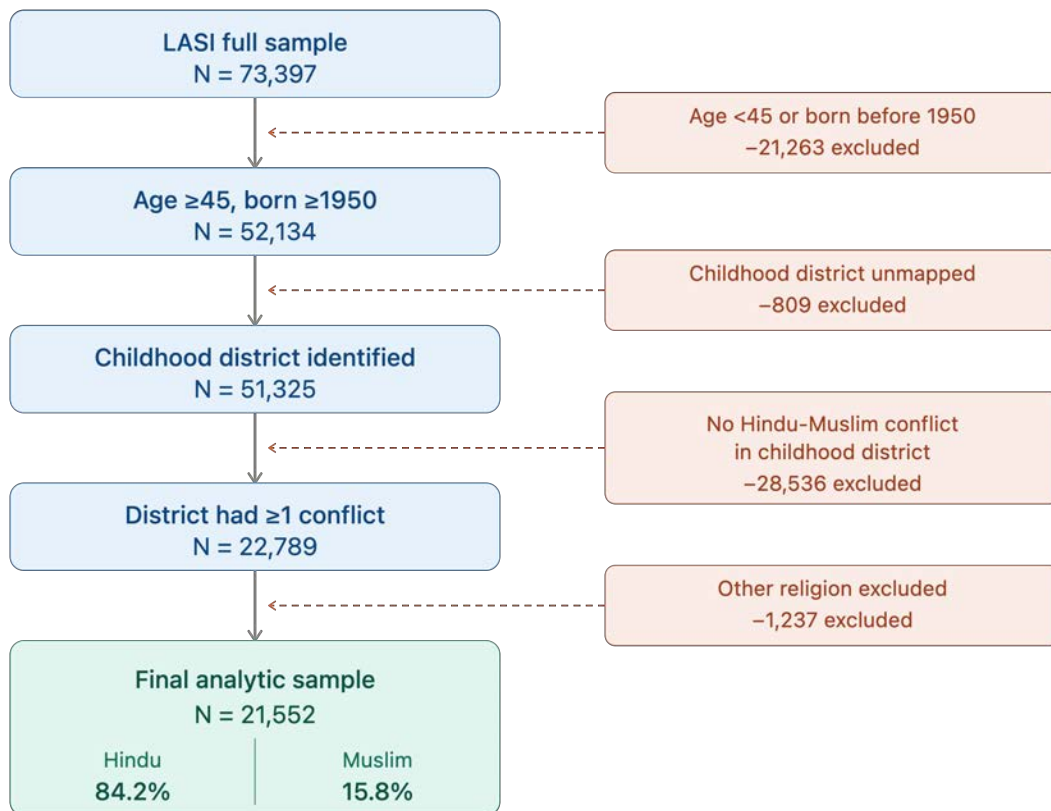
Supplementary Appendix

Figure A.1: Constructing the Treatment Variable: Childhood Conflict Exposure



Source: Authors' compilation, based on Varshney–Wilkinson riot data and the 2017–18 LASI survey.

Figure A.2: Sample Selection



Sources: LASI, 2017–18; Varshney–Wilkinson Dataset on Hindu–Muslim Violence in India, 1950–1995; Census of India 2011.

Table A.1: Gender Heterogeneity in the Effects of Childhood Conflict Exposure

	Female (1)	Male (2)	Female – Male (3)	Observations (4)
<i>Psychological well-being</i>				
CESD-10	0.028*** (0.007)	0.021*** (0.008)	0.007 (0.005)	20,446
Life Satisfaction	-0.107*** (0.017)	0.047** (0.021)	-0.154*** (0.013)	20,446
Cantril Ladder	-0.018*** (0.003)	-0.022*** (0.005)	0.003 (0.004)	20,446
Sleep Problems Index	0.053*** (0.012)	0.029 (0.018)	0.024* (0.013)	20,556
Sleep Medication	-0.001 (0.002)	0.014*** (0.004)	-0.015*** (0.003)	20,556
<i>Economic well-being</i>				
Log Consumption	-0.041*** (0.007)	-0.006 (0.011)	-0.035*** (0.010)	20,076
Durable Asset Ownership	-0.090*** (0.017)	-0.080*** (0.023)	-0.010 (0.011)	20,556
Current Residence Ownership	-0.033*** (0.004)	-0.019*** (0.006)	-0.014*** (0.004)	20,556
Land Ownership	-0.021*** (0.006)	-0.025*** (0.005)	0.004 (0.004)	20,556
<i>Education</i>				
Years of Schooling	-0.172*** (0.060)	0.016 (0.096)	-0.188*** (0.069)	20,556
Secondary Completion	-0.032*** (0.005)	-0.014** (0.006)	-0.017*** (0.004)	20,556
Higher Secondary Completion	-0.034*** (0.003)	-0.031*** (0.004)	-0.003 (0.003)	20,556
Tertiary Completion	-0.030*** (0.003)	-0.024*** (0.003)	-0.006** (0.003)	20,556
<i>Employment</i>				
Currently Employed	0.031*** (0.006)	0.019*** (0.005)	0.012** (0.005)	20,556
Hours Worked	0.284 (0.318)	0.604* (0.343)	-0.320 (0.257)	20,554
Annual Income	-2807.454*** (731.649)	-5144.234*** (1239.906)	2336.780** (1066.140)	20,554
Conditional Annual Income	-1.40e+04*** (2927.277)	-1.24e+04*** (2109.342)	-1600.325 (4242.984)	10,995
Conditional Log Income	-0.150*** (0.031)	-0.168*** (0.029)	0.018 (0.027)	10,995
<i>Type of employment</i>				
Family Farm Activities	0.007 (0.008)	0.035*** (0.007)	-0.029*** (0.008)	10,997
Agricultural Labour	0.016*** (0.006)	-0.006 (0.008)	0.022*** (0.008)	10,997
Self-Employed	-0.008 (0.011)	-0.000 (0.011)	-0.007 (0.006)	10,997
Salaried	-0.031*** (0.009)	-0.038*** (0.008)	0.007 (0.006)	10,997
Paid Family Work	0.001 (0.002)	0.001 (0.002)	-0.000 (0.001)	10,997
Unpaid Work	-0.001 (0.002)	0.003 (0.002)	-0.004* (0.002)	10,997
<i>Migration</i>				
Ever Migrated	0.014** (0.006)	-0.002 (0.012)	0.015 (0.011)	20,542
Migrated Ages 1–14	-0.006 (0.004)	-0.002 (0.004)	-0.004 (0.003)	20,542
Migrated Ages 15+	0.019*** (0.007)	-0.000 (0.010)	0.019* (0.012)	20,542

Notes: This table reports gender-specific estimates of the coefficient on *Fatalities* $\times$ *Muslim* from the saturated fixed-effects specification. Female and Male report the estimated effect separately by gender. Female–Male reports the difference between the two estimates. The fixed effects included in the specification are shown in the table. All regressions include caste controls, use LASI individual weights, and cluster standard errors at the childhood-district level. Standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.2: Robustness Across Alternative Samples

Outcome	Main Sample (1)	Full Sample (2)	Born 1960+ (3)
<i>Psychological well-being</i>			
CESD-10	0.025*** (0.007)	0.024*** (0.006)	0.034** (0.014)
Life Satisfaction	-0.044** (0.018)	-0.035** (0.018)	-0.188*** (0.064)
Cantril Ladder	-0.020*** (0.003)	-0.019*** (0.003)	-0.058*** (0.014)
Sleep Problems Index	0.046*** (0.013)	0.044*** (0.014)	0.021 (0.077)
Sleep Medication	0.005** (0.003)	0.005** (0.002)	-0.001 (0.008)
<i>Economic well-being</i>			
Log Consumption	-0.029*** (0.007)	-0.024*** (0.006)	-0.074*** (0.026)
Durable Asset Ownership	-0.084*** (0.019)	-0.092*** (0.021)	-0.166*** (0.035)
Current Residence Ownership	-0.027*** (0.005)	-0.025*** (0.005)	-0.095*** (0.018)
Land Ownership	-0.022*** (0.005)	-0.017*** (0.005)	-0.033** (0.016)
<i>Education</i>			
Years of Schooling	-0.073 (0.066)	-0.080 (0.062)	-0.253 (0.347)
Secondary Completion	-0.023*** (0.005)	-0.022*** (0.005)	-0.018 (0.013)
Higher Secondary Completion	-0.031*** (0.004)	-0.032*** (0.004)	-0.024** (0.010)
Tertiary Completion	-0.026*** (0.003)	-0.027*** (0.002)	-0.017** (0.006)
<i>Employment and earnings</i>			
Currently Employed	0.023*** (0.005)	0.024*** (0.005)	0.044** (0.019)
Hours Worked	0.248 (0.265)	0.212 (0.216)	0.791 (1.251)
Annual Income	-3811.972*** (809.043)	-4013.889*** (714.331)	-7471.395*** (2868.229)
Conditional Annual Income	-12475.793*** (1294.682)	-12922.740*** (1240.134)	-14491.870*** (3237.082)
Conditional Log Income	-0.152*** (0.026)	-0.156*** (0.023)	-0.182*** (0.040)
<i>Type of employment</i>			
Family Farm Activities	0.027*** (0.006)	0.027*** (0.005)	0.020** (0.008)
Agricultural Labour	0.004 (0.006)	0.007 (0.007)	0.056** (0.022)
Self-Employed	-0.002 (0.010)	-0.006 (0.011)	-0.033 (0.030)
Salaried	-0.039*** (0.008)	-0.034*** (0.008)	-0.058*** (0.017)
Paid Family Work	0.001 (0.002)	0.001 (0.001)	-0.000 (0.001)
Unpaid Work	0.001 (0.002)	0.000 (0.002)	0.002 (0.007)
<i>Migration</i>			
Ever Migrated	0.006 (0.007)	0.005 (0.006)	-0.006 (0.009)
Migrated Ages 1–14	-0.005 (0.004)	-0.006 (0.004)	0.001 (0.007)
Migrated Ages 15+	0.012** (0.006)	0.011** (0.005)	-0.006 (0.010)

Notes: Each cell reports the coefficient on *Fatalities* $\times$ Muslim from the saturated fixed-effects specification, with standard errors in parentheses. Column (1) reports the preferred main-sample estimates, restricting attention to respondents whose childhood districts recorded at least one Hindu–Muslim riot during 1950–1995. Column (2) uses the full available sample, including respondents from childhood districts with no recorded conflict; *Fatalities* is zero throughout the childhood exposure window for these respondents. Column (3) restricts the preferred main sample to respondents born in 1960 or later. In all three columns, *Fatalities* is scaled using the standard deviation calculated in the preferred main Hindu–Muslim analysis sample, so the coefficients are directly comparable across columns. All specifications include childhood-district $\times$ birth-cohort fixed effects, childhood-district $\times$ religion fixed effects, birth-cohort $\times$ religion fixed effects, caste controls, and sex controls; use LASI individual weights; and cluster standard errors at the childhood-district level. Hours Worked excludes observations reporting more than 168 hours per week. Conditional Annual Income, Conditional Log Income, and the type-of-employment outcomes are estimated among currently employed respondents. Outcome definitions follow Tables 2–7. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.3: Household Economic Outcomes: Robustness to Multiple Respondents per Household

	Log Household Consumption (1)	Durable Asset Ownership (2)	Current Residence Ownership (3)	Land Ownership (4)
<i>Panel A: Main specification</i>				
Fatalities $\times$ Muslim	-0.029*** (0.007)	-0.084*** (0.019)	-0.027*** (0.005)	-0.022*** (0.005)
Observations	20,076	20,556	20,556	20,556
<i>Panel B: Equal household influence</i>				
Fatalities $\times$ Muslim	-0.016* (0.009)	-0.060*** (0.018)	-0.034*** (0.006)	-0.023*** (0.006)
Observations	20,076	20,261	20,261	20,261

Notes: Entries report the coefficient on *Fatalities $\times$ Muslim*, with standard errors in parentheses. Panel A reproduces the preferred saturated estimates from Panel B of Table 3. Panel A uses LASI individual weights and clusters standard errors at the childhood-district level. In Panel B, each household's survey weight is divided equally across its usable respondents in the relevant estimation sample, so that households contributing multiple respondents do not receive mechanically greater total weight. Each respondent retains their own childhood conflict exposure and fixed-effect identifiers. Standard errors in Panel B are two-way clustered by childhood district and current household. Where the number of observations differs between Panels A and B, this reflects missing household survey weights. Both panels include childhood-district $\times$ birth-cohort fixed effects, childhood-district $\times$ religion fixed effects, birth-cohort $\times$ religion fixed effects, caste controls, and sex controls. *Fatalities* is scaled using the standard deviation calculated in the preferred main Hindu–Muslim analysis sample. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.