Forging Social Cohesion Through Mass Education: Evidence from a Nationwide Policy Reform in India

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Abstract

We examine the effects of mass schooling on intergroup relations by studying an education reform in India that significantly expanded access to primary education in the 1990s. Using a regression discontinuity design, we find that the reform reduced reports of caste- and religious-based conflict, with effects persisting up to 18 years after launch. Reductions are concentrated in religiously mixed localities with a history of Hindu-Muslim violence. To account for these patterns, we develop a simple decision-theoretic framework in which schooling curbs conflict by lowering bias (through intergroup contact and inclusive curricula), or by raising the opportunity costs to engaging in conflict (through higher wages and employment prospects). The empirical evidence is most consistent with the curriculum-based mechanism. Overall, our findings indicate that mass education can not only empower individuals but also serve as a scalable strategy for improving social cohesion.

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The 1990s marked a turning point in education policy in the developing world. At the 1990 World Conference on Education for All in Jomtien, Thailand, donors, bureaucrats, and teachers came together to champion an "expanded vision" of education that would push beyond "present resource levels, institutional structures, [and] curricula."[1] In the conference's wake, low- and middle-income countries undertook ambitious reform agendas. The governments of eight African nations eliminated school fees between 1994 and 2003, leading to "an instantaneous large increase" in school attendance (Grogan, 2009, p. 184). In Nepal, the Basic and Primary Education Programme (1991) resulted in the construction of 14,000 new classrooms in less than eight years (World Bank, 2000*b*). Ethiopia's Education Sector Development Program (1997) raised primary school enrollment from 35% to 62% during its five-year implementation period (World Bank, 2000*a*). Throughout the decade, school completion rates climbed steadily across the Global South (Hillman and Jenkner, 2004).

What were the social consequences of this expansion in mass education? In particular, did investments in schooling reduce intergroup conflict in deeply divided contexts? Social scientists have long theorized that governments harness public education systems not only to produce a skilled labor force, but also to instill loyalty and a sense of national belonging in their populations (Gellner, 1983; Paglayan, 2022; Paulsen, Scheve and Stasavage, 2023; Weber, 1976). Writing over a century ago, Durkheim (2012, pp. 237–9) observed that in order to unify France, the state had to "combat all forms of particularism," with public education offering "an unexcelled opportunity to exert a kind of influence on the child which nothing else can replace." Building on these insights, we develop a simple framework that highlights two pathways through which mass education could undercut the production of ethnic conflict. The first is a bias-reduction channel, in which schooling lowers animus toward outgroups—either relationally, by fostering sustained intergroup contact in classrooms (Rao, 2019; Alan et al., 2021), or normatively, through inclusive curricula that promote shared national identities and discourage outgroup stigmatization (Nussbaum, 2006; Cantoni et al., 2017; Haas and Lindstam, 2024). The second is an opportunity-cost channel, in which schooling improves wages and employment prospects, reducing the appeal of disruptive activities and easing competition over scarce resources (Fearon, 1999; Blattman and Annan, 2016).

While plausible, these claims merit scrutiny. At a theoretical level, group-wise hostilities—transmitted over generations—may not be amenable to short-term change (Wimmer, 2018); pupils may self-segregate in classrooms rather than forming bonds with outgroup peers; and low *quality* of instruction might undermine curricular impacts. At an empirical level, estimating the effects of mass education poses a host of challenges. Pre-existing factors such as economic conditions, political stability, or cultural norms may concomitantly shape both access to schooling and levels of social cohesion—for example, wealthier regions may be better positioned to invest in education and to

1 *World Declaration on Education for All, Jomtien, Thailand.*

police ethnic unrest. Conversely, areas affected by intergroup animosity may struggle to generate the revenue necessary for education spending, indicating reverse causality (Alesina and La Ferrara, 2005). The bundling of education reforms with other nation-building initiatives, such as language standardization and institutional engineering, further complicates efforts to isolate education's standalone effects (Miguel, 2004). More subtly, existing studies of education typically center on individuals, whereas intergroup conflict is a collective phenomenon, requiring the analysis of interventions at relevant geographic or group levels.

We overcome these hurdles by investigating a nationwide policy reform in India, which provides an "at scale" natural experiment for assessing the effects of mass education on intergroup conflict. India's entrenched caste and religious rifts—the focus of our study—make it a critical setting for probing the causes of ethnic-group tensions. Inspired by the Education for All movement, the Government of India, with the backing of bilateral donors and the World Bank, launched the District Primary Education Programme (DPEP) in 1994. DPEP provided a large set of the country's administrative districts each with up to INR 400 million (≈USD 12.5 million) in funding to be spent on school construction, teacher salaries, community-led enrollment drives, and various other investments.[2] This vast initiative ultimately encompassed "40% of India's territory, making it the largest primary education program in the country" up to that point (Mangla, 2017, p. 12). Districts were assigned to DPEP according to several institutional criteria, the main one being to prioritize areas with female literacy rates below the national average as per the 1991 census. The discrete change in treatment assignment probability at this cutoff allows for a regression discontinuity design. We estimate the impact of schooling expansion on intergroup conflict in districts just above versus below the assignment threshold.

To measure ethnic conflict holistically, we rely on survey data from a two-wave, nationwide survey of about 40,000 households, conducted between 10 and 18 years after DPEP began. These surveys registered conflict perceptions by asking respondents to document the incidence of everyday ethnic conflict in their community, paralleling how victimization surveys record experiences of crime. Crucially for internal validity, most respondents were household heads who, given their age, were themselves unaffected by the education expansion of interest, obviating concerns about correlated measurement error. By employing this older cohort as "impartial reporters" of ethnic conflict, therefore, our approach marks a significant improvement on prior studies of ethnic violence in India, which have relied on newspaper and police reports (Varshney, 2003; Jha, 2013; Nellis, Weaver and Rosenzweig, 2016). Such sources tend to capture only large-scale conflict episodes, and suffer from well-known reporting biases inherent in both administrative data (Knox, Lowe and Mummolo, 2020) and the press (Shaver, 2022).

Our central finding is that households in districts quasi-randomly assigned to DPEP are 21 percentage points

2 https://educationforallinindia.com/dpep-financial-parameters/

less likely to report that they experience "some" or "a lot" of intergroup conflict locally, compared to those not assigned to the program. (For context, 36 percent of respondents in the full sample report such conflict.) The results are robust to myriad alternative RD specifications, including the use of different bandwidths and control functions. A suite of RD validity and falsification checks—including tests of threshold sorting and balance on pre-treatment covariates—further attest to the design's soundness. Bolstering the credibility of our finding that expanded access to education drives a decline in outgroup hostility, we observe that the effect is strongest in religiously mixed localities and in districts with a history of Hindu-Muslim violence. We take these subgroup effects to suggest that mass education primarily mitigates communal, inter-religious conflict rather than caste-based conflict. Correspondingly, we find no statistically significant effects of DPEP on discriminatory behaviors or attitudes toward lower-caste groups, reinforcing the conclusion that education mostly operates on Hindu-Muslim relations in this setting.

To unpack mechanisms, we analyze treatment effect heterogeneity and the policy's impact on additional outcomes, marshaling supplementary data sources. Several conclusions emerge. One is that there is little evidence that the estimated effects flow from economic improvements: DPEP did not measurably affect household-level income or employment, nor did it change districts' average night-time luminosity, a common indicator of overall economic activity (Martinez, 2022). It thus seems unlikely that DPEP diminished intergroup conflict by neutralizing economic or employment grievances, or by raising the opportunity costs to engaging in group-related disputes.[3] The other null result is that DPEP neither influenced the diversity of survey respondents' social networks (in terms of religion and caste) nor students' perceptions of classroom climate, captured by their enjoyment of school and by whether their teachers are seen as ethnically biased. We take this as evidence against a bias-reduction channel working primarily through intergroup contact.

Instead, two empirical findings support the idea that DPEP moved norms in a progressive direction in exposed districts. First, we show that DPEP broadened children's access to—and internalization of—inclusive education programming that may have helped combat the formation of intergroup hatreds. In a descriptive analysis, we show that textbooks from the DPEP era featured "unity in diversity" narratives much more prominently than textbooks from previous eras. Assembling data from national standardized testing, we also show that schools in DPEP districts fared significantly better on exams that measure competence in social studies curricula, a subject that includes civics, equality, and respect for human rights. This result resonates with an explicit aim of the Indian government, which, when formulating the education policy that led to DPEP in the late 1980s, noted "an upsurge in favour of national integration ... through introduction of a national core curriculum; an insistence on observance of secular, scientific

3 These null impacts appear to run contra to the findings of Khanna (2023), who looks at the effects of DPEP on employment outcomes. However, it is worth emphasizing that Khanna focuses on a subset of individuals who were most likely to see wage and employment effects, whereas we look at population-representative data on household-level income, consumption, and employment.

and moral values [and] inculcation of an understanding of our composite culture" (Government of India, 1986, p. 6). These findings, then, point toward the importance of curriculum content in stemming intergroup conflict.

A second normative shift brought on by DPEP was female empowerment. The program reduced reports of female harassment, and increased participation in *mahila mandals*, women's civil society organizations that have acted "as vehicles of resistance that enable women to enter the public domain" (Das, 2000, p. 4391). We find tentative evidence that men's attitudes also became more pro-feminist in response to the program. Efforts to "protect" women and girls, and to regulate their behavior, are common catalysts of intergroup discord in India and beyond (Robinson, 2010). As such, DPEP may have disrupted the patriarchal structures that frequently serve to instigate ethnic conflict.

We make several contributions. Most notably, our study adds to the literature on education's social impacts. Existing research has tended to focus on attitudinal change. Educational attainment can reduce individuals' hostility toward immigrants (Cavaille and Marshall, 2019), diminish anti-Semitism (Nyhan, Yamaya and Zeitzoff, 2024), and usher in more cosmopolitan worldviews (Solodoch, 2024). Clots-Figueras and Masella (2013) demonstrate that schooling in Catalan-language instruction heightens individuals' national identification as Catalan. In a paired comparison of districts along the Kenya-Tanzania border, Miguel (2004) argues that Tanzania's inclusive nation-building policies weakened the negative association between ethnic diversity and public goods provision, though does not parse the role of education per se (see also Robinson (2014)).

We also speak to research on education and nation-building by probing the mechanisms through which schooling fosters unity in divided societies. Some scholars emphasize its use as a tool of elite control and state-building (Paglayan, 2022; Cantoni et al., 2017), while others stress its capacity to cultivate civic identities that resist authoritarianism (Darden and Grzymala-Busse, 2006). Building on this line of inquiry, we document that education can also operate from the "bottom up," improving quotidian intergroup relations in contexts marred by violence. These findings thus underscore education as a policy lever for long-term conflict prevention, complementing more conventional economic and security-based approaches (e.g., Blattman and Annan, 2016; Berman et al., 2013). A further implication of our study is that gender-sensitive reforms—such as promoting girls' enrollment and inclusive curricula—might amplify education's peace-building effects.

Why might mass schooling reduce intergroup conflict?

We present a simple decision-theoretic framework to capture two prominent pathways through which schooling can reduce intergroup conflict, synthesizing key arguments from the literature into conceptually distinct channels. The first is a *bias-reduction* channel, in which schooling lowers antipathy toward outgroups, reducing individuals' willingness to participate in conflict; this, in turn, may occur through cooperative intergroup contact, or through the value-shaping effects of inclusive curricula. The second is an *opportunity-cost* (human capital) channel, whereby schooling raises wages and improves employment prospects, increasing the economic costs of risky or disruptive activities.

Decision environment To fix ideas, we consider a population consisting of two identity groups, A and B. Individual $i \in \{A, B\}$ chooses whether to participate in intergroup conflict or remain at peace. The respective utilities for the individual are given by:

- Utility from **peace**: $u_p = w_i - \delta b_i$,

  where:

  - w_i is the individual's wage, reflecting the returns to education;
  - $b_i \in [0, 1]$ is the individual's bias or animus toward the outgroup;
  - $\delta > 0$ captures the psychic cost of cooperating with disliked groups, i.e., the "pain" per unit of prejudice when cooperating across groups.

- Utility from **conflict**: $u_c = \theta + \gamma b_i - c$,

  where:

  - θ is the expressive benefit from participating in conflict;
  - $\gamma > 0$ captures the "social reward" per unit of prejudice for acting on bias—so higher b_i yields more utility from conflict and larger γ reflects settings where hateful action is socially permitted or even valorized;
  - c is the cost of participation in conflict (e.g., the risk of arrest or injury).

Comparing payoffs, individual i chooses conflict iff $u_c > u_p$, which, after rearranging, gives the conflict threshold,

$$b_i \; > \; \frac{w_i - \theta + c}{\gamma + \delta} \; \equiv \; T_i.$$

Here, we interpret T_i as the bar for conflict: higher wages and higher costs of violence (w_i, c) raise T_i (harder to exceed), while greater expressive payoff or "identity heat" $(\theta, \ \gamma + \delta)$ lower T_i (easier to exceed).[4] Conflict is therefore more likely when b_i is high or w_i is low. As we now elaborate, education can matter insofar as it lowers b_i (bias channel) or raises w_i (opportunity-cost channel).

Channel 1: Bias reduction

Two features of schooling—one relational, the other curriculum-based—might plausibly attenuate conflict by reducing bias towards outgroups.

4 By "identity heat" we mean $\gamma + \delta$, where γ is the marginal utility from acting on prejudice ($\partial u_c / \partial b_i = \gamma$) and δ is the marginal disutility from peaceful cross-group interaction ($\partial u_p / \partial b_i = -\delta$). For a given numerator $(w_i - \theta + c)$, larger $\gamma + \delta$ lowers $T_i = (w_i - \theta + c)/(\gamma + \delta)$, making conflict more likely for a given b_i.

Relational explanations A leading behavioral account of group conflict reduction is the contact hypothesis: the claim that routine interactions across group lines can reduce fear and foster mutual understanding (Lowe, 2025; Tropp et al., 2022). Allport (1954) posited several scope conditions needed for contact to be prejudice-reducing; schools, by design, tend to embody these, involving equal status among students, cooperative educational tasks, and institutional sanctioning of intergroup engagement (Green and Hyman-Metzger, 2024). Research demonstrates that student interactions and cross-group friendships developed throughout schooling can be potent in affecting attitudes (Rutland, Nesdale and Brown, 2017; Corno, La Ferrara and Burns, 2022), and that contact with a "broad" segment of outgroup members shapes attitudes about the wider outgroup through Bayesian belief updating (Chakraborty et al., 2024). Similarly, seeing teachers from diverse backgrounds showcase their expertise, kindness, and leadership might humanize the outgroup in the eyes of students (Weiss, 2021), akin to findings on the societal effects of mandated political representation for marginalized groups (Beaman et al., 2009; Chauchard, 2014).

Still, the efficacy of school-based intergroup contact cannot be taken for granted (Spiel and Strohmeier, 2012; Vervoort, Scholte and Scheepers, 2011). Class sizes may be so large as to allow children from different ethnic groups to self-segregate—resulting in *exposure* to outgroup peers without serious interaction (Ramiah et al., 2015). For instance, a recent study of Hindu and Muslim teenagers in India finds that outgroup members constitute roughly one-third of the average student's classmates but a mere one in twenty of students' close friends (Ghosh et al., 2025). While teachers from outgroups can serve as role models, those who harbor prejudices may instead reinforce negative stereotypes and, through biased pedagogical practices, strain interethnic relations in the classroom (Alan et al., 2023). The overall impact of school-based contact on bias hinges on institutional design and local context.[5]

Curriculum-based explanations A second route by which schooling may reduce bias is through the intentional shaping of norms and values.[6] Governments have long sought to influence young people's attitudes and behaviors by manipulating curricular content. Such attempts at indoctrination can be effective: Chinese students exposed to

5 We can formalize the contact mechanism by defining an individual's *effective contact* as

$$\chi_i = f(\sigma_i)(1-\rho), \quad f' > 0,\ f'' < 0,$$

where σ_i is the share of outgroup peers in i's school, $f(\sigma_i)$ maps diversity into potential contact opportunities (with diminishing returns), and $\rho \in [0,1]$ measures the extent of self-segregation. Higher χ_i thus reflects more impactful cross-group interaction. Bias is then updated as:

$$b_i \leftarrow b_0 - \lambda \chi_i,$$

with $\lambda > 0$ denoting the bias-reduction effect per unit of contact. This formulation highlights why institutional conditions matter: when schools structure interaction to minimize segregation (low ρ) and facilitate engagement (large $f(\sigma_i)$), contact reduces bias substantially; when segregation or shallow exposure dominates, χ_i—and therefore the effect on bias—approaches zero.

6 We may straightforwardly represent this curriculum-induced bias shift as:

$$b_i \leftarrow b_i - \phi(s_i, \kappa),$$

where $s_i \in \{0,1\}$ denotes individual i's schooling status, $\kappa \in \{\text{inclusive}, \text{majoritarian}\}$ indexes curriculum type, and ϕ is the induced value change, with $\phi > 0$ for inclusive content (bias-reducing) and $\phi < 0$ for majoritarian/exclusionary content (bias-reinforcing).

pro-Communist textbooks show greater support for the government and skepticism toward free markets in adulthood (Cantoni et al., 2017), while states emerging from civil war have used primary school curricula to instill compliance and discipline (Paglayan, 2022). By contrast, education systems espousing a pluralist ethos may dampen conflict. Research highlights the role of schooling in advancing equal citizenship (Nussbaum, 2006) and in spurring political participation (Paulsen, Scheve and Stasavage, 2023). Strategic programming—even outside formal classroom arenas—has also been demonstrated to improve students' attitudes toward outgroups (Alan et al., 2021; Weiss, Ran and Halperin, 2023). Yet evidence remains limited on whether inclusive curricula ultimately translate into reduced group conflict.

A particularly salient domain of value formation concerns gender norms. Appeals to the "protection" of ingroup women frequently serve as a proximate justification for interethnic violence. Exogamous romantic partnerships, treated as violations of group boundaries, can spark retaliation and collective punishment (Frydenlund and Leidig, 2022), while accusations of sexual violence can ignite cycles of revenge among men from rival communities (Horowitz, 2001, p. 74–80). Patriarchal norms linking women's autonomy to group honor thus constitute a potent trigger for conflict. Curricula that explicitly promote female agency and equality may help weaken this connection (Dhar, Jain and Jayachandran, 2022). By fostering critical reflection on gender and power, schools may play a direct role in disarming one of the cultural logics most often invoked as a rationale for intergroup conflict.

Channel 2: Opportunity costs

Endogenous growth theory holds that schooling can enhance individuals' earnings potential and contribute to macroeconomic growth (e.g., Romer, 1986; Angrist and Krueger, 1991; Card, 1999; Krueger and Lindahl, 2001). If education facilitates gainful employment, educated individuals face higher opportunity costs for participating in risky activities such as intergroup conflict (Dal Bó and Dal Bó, 2011; Blattman and Annan, 2016; Blair, Christensen and Rudkin, 2021). Furthermore, when economic inequality dovetails with group identity—intensifying competition over scarce resources—the risk of intergroup conflict and violence increases (Safarzynska and Sylwestrzak, 2021; Justino, 2025). Consequently, expanded access to education could reduce intergroup conflict by improving incomes and lowering the stakes of economic competition.[7]

7 Formally, we model wages as:

$$w_i = w_0 + \psi(s_i), \quad \psi' > 0,$$

where w_0 is baseline income and $\psi(s_i)$ captures the wage return to schooling. Higher w_i raises the opportunity cost of engaging in conflict, effectively shifting the participation threshold: individuals require a higher level of bias before choosing conflict.

Implications

Our framework predicts that educational expansion should reduce participation in intergroup conflict most strongly where prejudice is initially high but also responsive to intergroup contact or curricular reform, and where labor markets are sufficiently elastic to reward additional schooling with higher wages. In these settings, the joint effect of lower bias and higher opportunity costs shifts individuals away from conflict.

The framework also has less intuitive implications. First, because both channels operate through the same threshold, their effects may be substitutes as well as complements: in contexts where schooling delivers large wage gains, even modest reductions in bias can have outsized effects, while in stagnant labor markets, bias reduction alone may not be enough. Second, the role of curriculum content highlights the possibility of perverse effects: if education reproduces exclusionary or majoritarian narratives, bias may rise even as wages increase, yielding ambiguous overall effects. Third, the framework underscores a potential inequality trap: if economic returns to schooling are unequal across groups, one community may experience an increase in wages while another does not, potentially increasing intergroup competition.

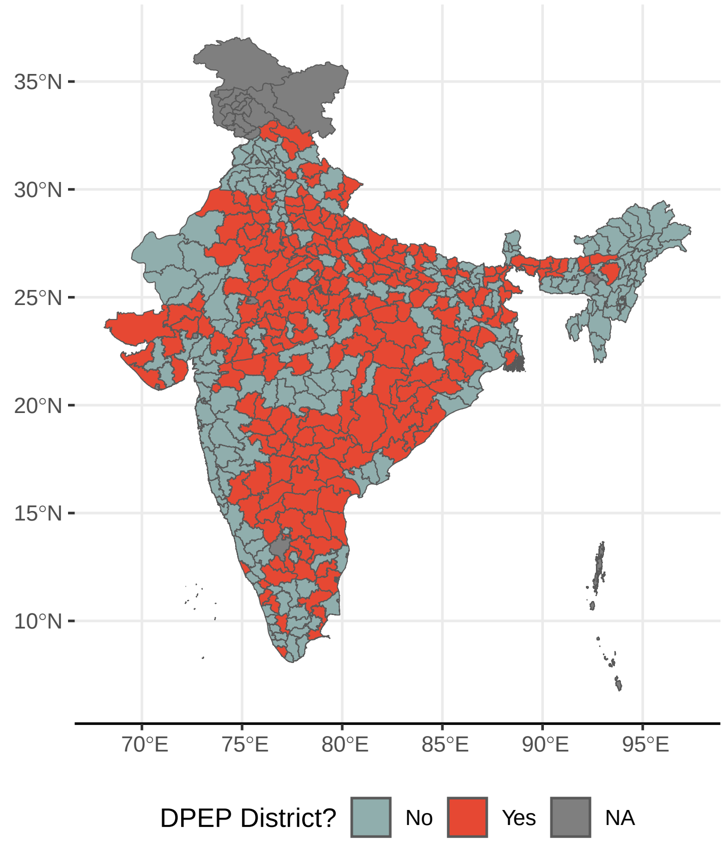
Context

India's District Primary Education Programme

DPEP began in 1994 as an initiative by the Government of India—in conjunction with state governments and external donors—to universalize primary education and improve its quality. DPEP targeted districts according to two criteria: (a) those with female literacy rates below the national average of 39.29% in 1991 (Government of India, 1995, p. 4); and (b) those where a total literacy campaign (TLC) had been successfully carried out (Azam and Saing, 2017, p. 1131, fn. 8). The policy sought to elevate both access and quality in primary education, particularly for groups with historically low levels of educational attainment, such as girls, Scheduled Castes, and Scheduled Tribes (Government of India, 1995, pp. 1–5).

The policy's mandate comprised specific policy initiatives. DPEP supported teacher training, enrollment initiatives, community-led mobilization campaigns, school construction, curricular reforms, and monitoring efforts (Pandey, 2000; Sipahimalani-Eao and Clarke, 2003, p. 9). Funding for DPEP was extensive, with the World Bank alone contributing \$1.2 billion by 2002 (World Bank, 2002). Other international donors backed the project, including UNICEF and the governments of the UK and the Netherlands (World Bank, 2003*a*, p. 2). The policy stipulated that 85 percent of DPEP funding for districts would come through the Government of India, while state governments would contribute the remaining 15 percent (Government of India, 1995, p. 6). The policy required state governments

Figure 1: DPEP's geographic coverage.



Notes: This figure shows the districts assigned to receive DPEP funding (red) or not (gray) according to India's 1991 district boundaries. The dark gray districts in Jammu and Kashmir are excluded from the sample as they were not covered by the 1991 Census of India due to regional instability.

to maintain their 1991–92 education spending levels (in real terms), to prevent states from using central government DPEP funds as a substitute (Government of India, 1995, pp. 6–7).

In total, DPEP reached approximately half (271) of the country's districts, across 18 states (Jalan and Glinskaya, 2015, p. 4). Figure 1 illustrates DPEP's wide geographical spread.[8] The phased implementation of the program began with an initial rollout in 42 districts before expanding to its final footprint in 1996 (World Bank, 2003*b*, p. 11). Throughout both phases, the Government of India maintained the eligibility criterion for DPEP based on districts' female literacy rates in 1991.

Monitoring data highlight DPEP's impact across multiple domains of primary education. It is estimated that DPEP reached 51.3 million children and 1.1 million primary school teachers, and resulted in the construction of over 40,000 primary school buildings (Jalan and Glinskaya, 2015, p. 5). A detailed report on the program's implementation in Rajasthan found that over 80 percent of primary school teachers in the state's DPEP districts underwent

8 Because its chief determinant was a key socioeconomic indicator (female literacy) there are also patterns of spatial clustering evident in Figure 1. It is important to note that the ability of our empirical design to estimate causal effects is unaffected by this clustering.

annual training funded by the program, and that 1,051 fully functional training centers had been set up across the state (World Bank, 2006, pp. 7–16). DPEP trainings for instructors explicitly featured modules on gender sensitivity within the classroom (Government of India, 1995; Viswanathappa et al., 1999). DPEP offered financial assistance to states for textbook revision and distribution, with specific directives to address social biases and to improve the overall quality of instructional materials (World Bank, 2003*b*, pp. 31–3).

Analysis of primary school textbooks demonstrates that texts from the DPEP period indeed integrated themes surrounding inclusivity and gender equality to a greater degree than materials in circulation before the reform. To gauge such thematic changes, we collected a diverse array of primary school textbooks used in social studies and civics courses before and after the reform from two public databases (Azim Premji University, 2025; National Digital Library of India, 2025).[9] We systematically compare the content of the pre- versus post-reform texts with a keyword-assisted topic model (Eshima, Imai and Sasaki, 2024), focusing specifically on gender inclusivity, religious diversity, and caste discrimination.[10] The results—presented in Online Appendix Figure A1—reflect a near-doubling in the extent to which post-reform textbooks discussed those three themes, indicating that DPEP's mandate for textbook reform achieved its stated aims.

Intergroup conflict in India

The main axis of intergroup conflict in India falls along religious lines. Most of India's population identify as Hindu (80 percent), while a minority identify as Muslim (14 percent). Animosity between these communities has longstanding roots, but intensified under British colonial rule (Bayly, 1985). Today, religious-group conflict is a recurring phenomenon, varying in intensity and type. On one end of the spectrum, local disputes in villages or towns can flare up due to contests over property boundaries, inheritance claims, or access to shared resources like grazing land or water. On the other, disagreements periodically snowball into full-fledged pogroms. Hindu nationalist groups have harnessed institutionalized village networks to abet and prolong such violence (Singh, 2016, pp. 98–9).

The second major axis of intergroup conflict involves the caste system, which exists primarily in Hinduism and stratifies society into rigid endogamous categories based on occupation and hereditary status. Such divisions have involved the subjugation of lower-caste groups, particularly Dalits (formerly "untouchables") and tribal communities. Despite India's Constitution prohibiting caste-based discrimination and expansive affirmative action policies geared toward mitigating inequality, anti-Dalit prejudice remains pervasive. Caste-based conflict manifests in vio-

9 The full list of sampled textbooks—including grade levels, date of publication, and the state in India in which the text was used—can be found in Online Appendix Table A2.

10 In this approach, the analyst provides a small number of keywords associated with a particular topic (or topics) of interest which the topic model estimates along with topics which were not prespecified. Keyword-assisted topic modeling provides more interpretable and coherent topics and superior document classification performance compared with unsupervised approaches. Online Appendix Table A3 lists the keywords provided for the three prespecified topics.

lent clashes, including atrocities that occasionally make national headlines. For example, in 2006, in the village of Kherlanji, Maharashtra, four members of a Dalit family were paraded naked through the village and then hanged, following a land dispute.[11] In 2012 in Dharmapuri district in Tamil Nadu, a mob of Vanniyar caste members—in response to a young woman from the Vanniyar community marrying a Dalit man—demolished and set ablaze over 200 houses, leaving 1,500 Dalits homeless.[12]

Caste and religious conflicts regularly intersect. Dalits and other marginalized groups often face discrimination within their own religions. Conversion movements, whereby individuals seeking to escape caste oppression adopt other religions like Buddhism, Islam, Christianity, or Sikhism, have provoked backlash from dominant caste and religious groups (Jaffrelot, 2010, pp. 144-69). Communal violence, such as riots or lynchings, disproportionately affects lower-caste individuals (either as perpetrators or victims) due to their socio-economic vulnerability (Brass, 2003, pp. 122, 184–5; Lobo, 2002; Berenschot, 2011). Given this entanglement, examining caste and religious conflict in tandem offers valuable analytical insight.

Empirical strategy

Data Our main independent variable is district-level DPEP eligibility. A district is classified as more likely to be eligible if its female literacy rate in 1991 fell below the national average, based on data we take from the 1991 Census of India—the same dataset the government used to determine program eligibility. For first-stage tests, we construct an indicator for whether a district received any DPEP funding, drawing on data from Roodman (2023). Importantly, the intensity of DPEP "treatment" varied across districts. Because district-level funding data are not directly available, we impute district-level allocations in DPEP districts by distributing state-level funding totals from 1998–2003 proportionally according to district populations (see Online Appendix Section I for further details). Along with these first-stage indicators, we list the variables used for the main empirical tests and mechanism analyses in greater detail in Table A1.

Our primary outcome measure comes from the Indian Human Development Survey (IHDS), a two-wave, nationally representative longitudinal survey capturing the social, economic, and health conditions of individuals, households, and localities India-wide (Desai, Vanneman et al., 2008). IHDS's first wave was fielded in 2004–05 and covered 41,554 households in 1,504 villages and 970 urban neighborhoods across 33 states and union territories. The second wave, conducted in 2011–12, surveyed 42,152 households, most of which had also been included in the first round. For our primary analysis, we pool data from both waves. Of the 612 districts in India as of the

11 Lyla Bavadam, "Dalit blood on village square," *Frontline, The Hindu*, December 2006.

12 R. Arivanantham, "Attack on Dalits was planned: National Commission for SCs," *The Hindu*, November 2012.

2001 Census, 373 were sampled by IHDS. We verify that DPEP eligibility neither predicts a district's inclusion in the IHDS sample, nor correlates with the demographic composition of surveyed respondents (see Online Appendix Figure A3). The outcome variable is taken from the social capital module of IHDS, where household heads were asked: "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" In standard Indian usage, jati refers to caste, while community (and the term communal) denotes religious groups. Response options to the question were "a lot of conflict," "some conflict," and "not much conflict." We recode the responses into both an ordinal three-point scale and a binary indicator.

Advantages of perception-based measures of ethnic conflict Reliably measuring the incidence of ethnic conflict is difficult. Common approaches use news reports (e.g., Varshney and Wilkinson, 2006; Barron, Jaffrey and Varshney, 2016; Nellis and Siddiqui, 2018) or official police data (e.g., Jassal, 2020).[13] Our paper takes a different tack, employing citizen reports of local intergroup conflict from a nationally representative survey. This method has two key advantages. First, it captures lower-intensity forms of conflict, like assaults and minor brawls, which typically fly under the radar of mainstream media reporting. Second, it circumvents biases inherent in police data, which are known to suffer from underreporting of taboo crimes, selective enforcement, and political manipulation (Knox, Lowe and Mummolo, 2020).

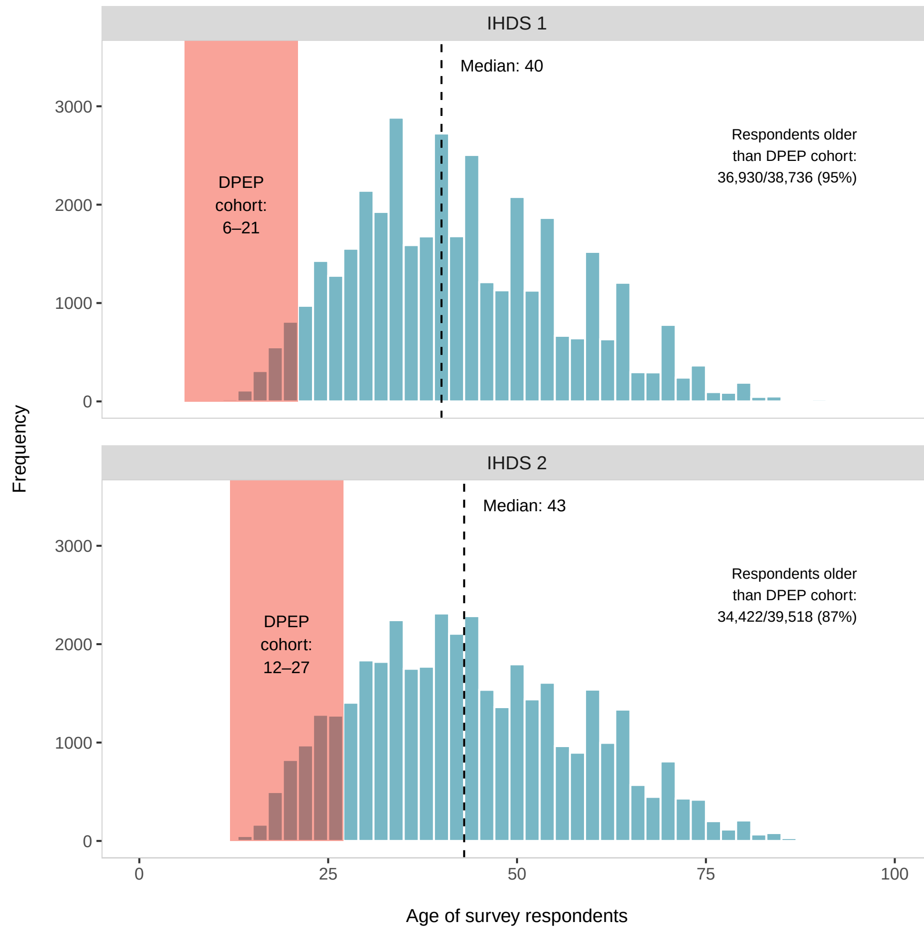
At the same time, perception-based measures face possible limitations. A principal concern for our study is that respondents' education levels might shape how they interpret and answer survey questions, generating measurement error correlated with treatment assignment. For example, individuals educated under DPEP's inclusive programming may be more attuned to the ethnic undertones of local disputes, and so report them at higher rates as adults. Two vital features of our design counter these concerns.

1. **DPEP-ineligibility of respondent cohort** The IHDS survey was administered to household heads: respondents who, by virtue of their older age, were rarely beneficiaries of DPEP themselves. Figure 2 shows that in the IHDS 1 and IHDS 2 surveys, fewer than 5% and 13% of respondents, respectively, *could have been* exposed to DPEP as students, since the program targeted younger cohorts. Importantly, we also later show our results' robustness to dropping those small numbers of potentially-exposed respondents altogether.

2. **Obfuscation** The survey was neither connected to DPEP nor was it primarily focused on intergroup conflict, militating against researcher demand effects: the tendency for study participants to adjust their survey responses to align with perceived study goals.

13 Note that systematic newspaper-based data on Hindu–Muslim riots are unavailable for India after 2000. Moreover, with the exception of the Gujarat riots in 2002, all evidence suggests that such large-scale communal violence has become considerably less common over the past two decades.

Figure 2: Age distribution of IHDS respondents and overlap with DPEP cohort.



Notes: This figure shows the distribution of survey respondents in IHDS-1 and IHDS-2 by age. IHDS-1 was carried out 2004-05 and IHDS-2 in 2011-12. The highlighted area shaded in red indicates the age ranges of respondents, in each wave, that could have potentially been exposed to the DPEP program, based on assumptions laid out in Online Appendix Table A20. The vertical dashed lines represent the median age of respondents in each survey wave.

We help validate our measure of self-reported conflict with data from the Uppsala Conflict Data Program (UCDP), which compiles violent events from media sources. Although UCDP event counts carry the drawbacks we previously noted, the known incidents documented in UCDP should be captured by survey-based conflict perceptions. As shown in Online Appendix Table A22, respondents in districts that experienced at least one violent event in the two years prior to the IHDS waves were 13.5 percentage points more likely to report "some" or "a lot" of ethnic conflict in their community, a nearly 40% increase relative to districts without such events. While this is not a definitive benchmark—among other things, the UCDP data cannot be disaggregated into ethnic versus non-ethnic conflict events—the correlation provides reassurance that our self-reported measure captures meaningful variation in local conflict.

Regression discontinuity design We implement an RD design that compares administrative districts just below and just above the DPEP eligibility threshold. The running variable is district-level female literacy in 1991, with the threshold set at the national average of female literary in that year ($c = 0.3929$). Recall that, in addition to district-level female literacy rates, DPEP assignment was also a function of whether a given district had successfully carried out a total literacy campaign (TLC). By the start of DPEP, however, virtually all districts had successfully implemented TLCs, meaning "below national average female literacy remained the main criterion" (Azam and Saing, 2017, p. 1131). Thus, our analysis solely leverages the uptick in DPEP assignment as a function of district-level female literacy rates.

Our design allows us to estimate the effect of a treatment in which the probability of assignment changes discontinuously as a function of a running variable R. Formally, treatment assignment is represented by the indicator function $D_i = \mathbf{1}\{R_i \leq c\}$, where R_i is the running variable and c is the cutoff that separates treated from untreated units. Districts with female literacy below the national average in 1991 were assigned to treatment, while those above were assigned to control. For ease of interpretation, we reverse-code the female literacy running variable in most analyses so that higher values correspond to a greater likelihood of DPEP assignment.

Each district i reveals its treated potential outcome $Y_i(1)$ when $R_i \leq c$ and its control outcome $Y_i(0)$ when $R_i > c$. Assuming that units' potential outcomes are continuous at the cut point, the RD design identifies the effect of treatment at the cut point c. We express this estimand formally: $\tau_{rd} = E[Y_i(1) - Y_i(0) | R_i = c]$. We apply the following estimating equation:

$$Y_{k,i} = \alpha + \tau_{rd} D_i + f(R_i - c) + \varepsilon_i \tag{1}$$

where τ_{rd} represents the treatment effect at the cut point, $Y_{k,i}$ is the level of interethnic conflict reported by individual

k in district i, D_i indicates district i's treatment, and $f()$ represents a flexible function of units' distance from the cut point. We implement our analysis using the method proposed by Calonico, Cattaneo and Titiunik (2015). For simplicity and transparency, we estimate and present intent-to-treat effects throughout. Standard errors are clustered by 1991 district, the level at which treatment is assigned. We validate the assumptions on which our RD framework rests in Online Appendix Section C.

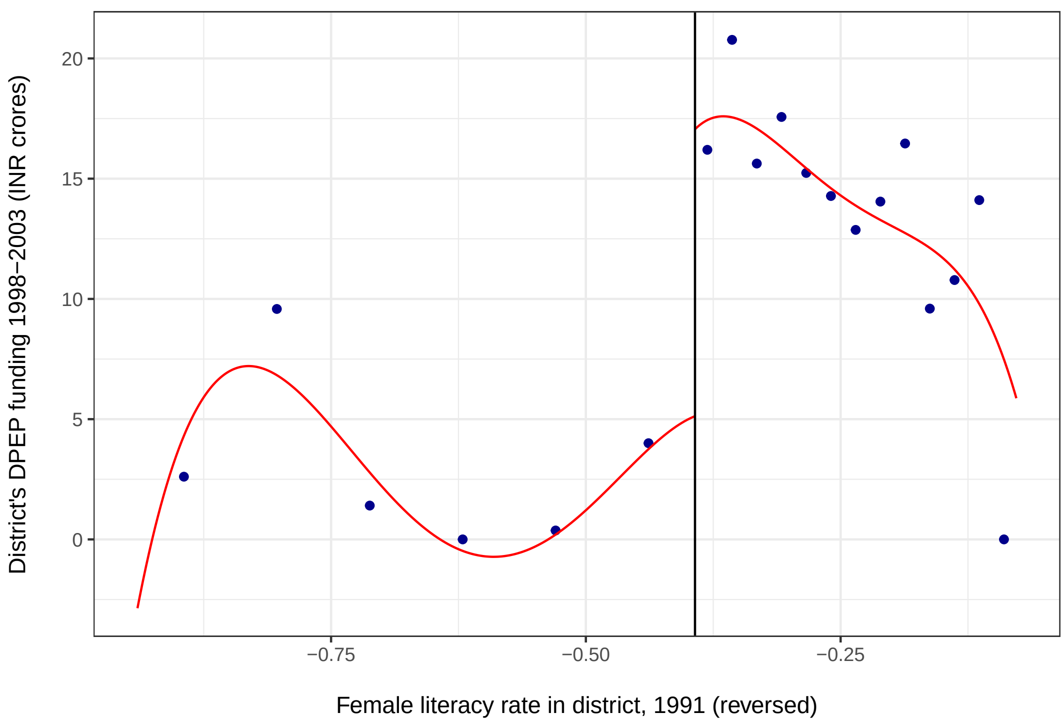
Empirical results

First-stage effects Was DPEP assigned as intended, and did it affect the quantity and quality of education received by Indian children? Several tests strongly support the notion that DPEP allocation changed discontinuously around the assignment cutoff. Figure 3 shows a sizable jump in the amount of DPEP funding received by districts around the average female literacy rate in 1991. Using the IHDS district sample, we estimate that districts just above the threshold received INR 11.8 crores more in DPEP funding compared to districts just below the threshold, a highly statistically significant difference. On the extensive margin, narrowly eligible districts were 44 percentage points more likely to receive *any* DPEP funding than those that were just ineligible (see Online Appendix Table A6).

Next, in Table 1, we consider whether DPEP increased school enrollment and educational attainment using IHDS survey data. Panel A, Column 1 shows that DPEP significantly increased school enrollment among individuals aged six or younger in 1994—the cohort most likely to be affected by the program, given that six is the normal age for starting primary school and 1994 marked the program's launch. The estimated effect is 9.2 percentage points ($p < 0.01$). As expected, the effect on the older cohort (Column 2) is smaller and not statistically significant, consistent with DPEP primarily mattering for younger children.

To see whether DPEP influenced learning outcomes, we analyze test scores from three basic assessments of core skills administered by IHDS enumerators to school-age children (ages 8 to 11) in sampled households. These doorstep tests ensure a relatively controlled environment where all children take the same standardized test without school-related distractions or variation in test administration. Table 1, Panel A, shows that DPEP had a marginally statistically significant, positive effect on reading skills, with an RD estimate of 0.249 ($p < 0.1$, Column 3). Effects on math and writing scores are smaller and not statistically significantly different from zero (Columns 4 and 5). Table 1, Panels B and C examine gender differences. Among boys (Panel B), DPEP significantly increased school enrollment within the younger cohort of individuals, to the tune of 10 percentage points, but test score impacts are modest and imprecisely estimated. Among girls (Panel C), the enrollment effect is similarly positive—8 percentage points—and marginally statistically significant, while the estimated impact on reading scores is larger (0.35 points) than that for boys, suggesting somewhat stronger literacy gains among girls.

Figure 3: DPEP funding by 1991 female literacy rate for the IHDS estimation sample.



Notes: This figure displays DPEP funding in a district as a function of 1991 female literacy rate, estimated using `rdplot` (estimation bins and polynomials are chosen automatically using `rdplot` defaults). District-level DPEP funding is imputed using the method described in Online Appendix Section I. The unit of analysis is the 2001 district. The sample includes 373 districts where the IHDS survey was conducted. The vertical line marks the national average of district-level female literacy in 1991, which served as a cutoff for DPEP funding allocation. One INR crore is equivalent to INR 10 million.

Table 1: Estimated effects of DPEP on school enrollment and academic attainment.

	Ever attended school (0/1)		Standardized test score		
	Younger cohort	Older cohort	Reading (0-4)	Math (0-3)	Writing (0/1)
	(1)	(2)	(3)	(4)	(5)
Panel A: Both genders					
RD estimate	0.092***	0.042	0.249*	0.121	0.053
	(0.033)	(0.061)	(0.142)	(0.160)	(0.055)
Effective N	20,980	46,880	3,629	3,891	2,778
Overall N	76,258	124,111	11,804	11,762	11,708
$\hat{h}$	0.083	0.105	0.090	0.097	0.072
K clusters	280	280	280	280	280
Outcome mean	0.651	0.614	2.589	1.574	0.77
Panel B: Male					
RD estimate	0.099***	0.031	0.167	0.179	0.092
	(0.029)	(0.050)	(0.148)	(0.158)	(0.059)
Effective N	10,565	29,705	2,234	1,904	1,591
Overall N	39,669	62,340	6,268	6,243	6,200
$\hat{h}$	0.078	0.122	0.101	0.090	0.075
K clusters	280	280	278	278	278
Outcome mean	0.657	0.733	2.629	1.548	0.743
Panel C: Female					
RD estimate	0.080*	0.063	0.349*	0.081	0.014
	(0.045)	(0.076)	(0.192)	(0.184)	(0.062)
Effective N	11,565	20,765	1,490	1,879	1,274
Overall N	36,589	61,771	5,536	5,519	5,508
$\hat{h}$	0.094	0.097	0.083	0.100	0.070
K clusters	280	280	278	278	278
Outcome mean	0.648	0.49	2.56	1.578	0.793

Notes: This table presents RD estimates of the effect of DPEP on school enrollment and academic attainment. Outcomes come from the household roster of the first wave of the IHDS. The unit of analysis is the individual. The outcome in columns 1 and 2 indicates whether an individual in a sampled household ever attended school. The younger cohort consists of individuals who were 6 years old or younger in 1994, while the older cohort consists of individuals who were older than 6 in 1994. Outcomes in columns 3–5 come from standardized tests. The IHDS assessed reading, math, and writing skills of children between 8 and 11 years old in sampled households. *Reading* skills are recorded from 0 (=cannot read) to 4 (=can read a story). *Math* skills are recorded from 0 (=cannot do math) to 3 (=can do division). *Writing* skills are recorded from 0 (=cannot write) to 1 (=writes with 2 or fewer mistakes). The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Finally, we estimate the effect of DPEP assignment on primary school construction using census data from Asher et al. (2021), which reports the district-wise number of primary schools in 1991 (pre-treatment), 2001 (shortly after treatment began), and 2011 (well after treatment began). We test whether DPEP led to an increase in the number of primary schools per capita between 1991 and 2001, and between 1991 and 2011. The coefficient plot in Online Appendix Figure A6 summarizes the results. DPEP had a positive and statistically significant effect on school construction—modest by 2001, but substantially larger by 2011, at which point treated districts had an estimated 25 additional primary schools per 100,000 residents.

These results collectively substantiate that DPEP was implemented as intended, significantly expanded access to education, modestly improved reading skills, and increased investments in school infrastructure. Combined with the suite of RD validation checks in Online Appendix Section C, these findings thus support the credibility of the design for evaluating DPEP's downstream effects—our next step.

Conflict incidence The paper's main results are presented in Table 2. We estimate the effect of DPEP on two different codings of the intergroup conflict outcome: the original three-point scale in Panel A, and a dichotomized version of that measure in Panel B. Both codings range from zero to one such that coefficients are interpretable as percentage point effects. In the benchmark specifications in Column 1, we find that the expansion of primary education through DPEP caused a 13 percentage point drop in reports of intergroup conflict according to the 3-point outcome measure, and a 21 percentage point drop according to the binary outcome measure. These effects are statistically significant at the 95% confidence level. They are substantively large given that the means of the outcomes for the effective samples (those within the RD bandwidth) are 0.22 in Panel A and 0.36 in Panel B.

The remaining columns in Table 2 show alternative specifications of the RD estimator. The results are robust when doubling the bandwidth used (Column 2), employing the Imbens and Kalyanaraman (2012) algorithm for computing the optimal bandwidth (Column 3), and using quadratic (Column 4), cubic (Column 5), and quartic (Column 6) control functions. Online Appendix Figure A5 presents a placebo test assessing the robustness of the estimated effect of DPEP on intergroup conflict using alternative, falsified cutoff points for treatment assignment. The figure shows that *only* the estimate at the true cutoff yields a statistically significant negative effect on intergroup conflict, while the placebo estimates are small and not statistically distinguishable from zero. Online Appendix Table A18 demonstrates robustness to using all other optimal bandwidth selection procedures available in the `rdrobust` package. In Online Appendix Table A19, we confirm the results' robustness to employing the fuzzy RD estimator—where DPEP funding is treated as the endogenous regressor and instrumented using the RD cutoff—and to the inclusion of additional control variables. Online Appendix Table A17 shows the results hold up to dropping multi-parent districts (of which there are few in the sample). Online Appendix Table A21 finds that the results are unaffected by dropping younger survey respondents who may themselves have been partial DPEP beneficiaries. Together, the results provide consistent evidence that our estimates are not sensitive to modeling or sample choices.

Table 2: Estimated effect of DPEP on intergroup conflict.

Control function:	Linear			Quadratic	Cubic	Quartic
Bandwidth:	$\hat{h}$	$2\hat{h}$		$\hat{h}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 3-point outcome measure						
RD estimate	-0.129**	-0.111**	-0.153**	-0.138**	-0.190***	-0.182***
	(0.050)	(0.046)	(0.070)	(0.056)	(0.069)	(0.070)
Overall *N*	80,563	80,563	80,563	80,563	80,563	80,563
Effective *N*	21,613	45,798	18,092	37,873	39,558	54,920
Overall *N* districts	295	295	295	295	295	295
Effective *N* districts	83	172	70	142	148	202
$\hat{h}$	0.078	0.157	0.066	0.123	0.126	0.183
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.216	0.219	0.220	0.224	0.224	0.222
Panel B: Binary outcome measure						
RD estimate	-0.214***	-0.177**	-0.273**	-0.263***	-0.290***	-0.330***
	(0.083)	(0.075)	(0.111)	(0.098)	(0.106)	(0.125)
Overall *N*	80,563	80,563	80,563	80,563	80,563	80,563
Effective *N*	20,585	43,645	18,092	28,845	43,124	48,342
Overall *N* districts	295	295	295	295	295	295
Effective *N* districts	78	163	70	109	161	180
$\hat{h}$	0.074	0.149	0.066	0.102	0.143	0.162
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.360	0.365	0.371	0.366	0.366	0.366

Notes: This table presents RD estimates of the effect of DPEP on intergroup conflict. The unit of analysis is the household. Data come from both IHDS waves. Outcome variables are based on the survey question (asked to the heads of sampled households): "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" In Panel A, responses are recoded as: "a lot of conflict" (=1), "some conflict" (=0.5), "not much conflict (=0). In Panel B, the binary outcome measure takes 1 if the respondent reported "some" or "a lot of conflict," and zero if they reported "not much conflict." The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using a triangular kernel. We employ either the Calonico, Cattaneo, and Titiunik (CCT) or Imbens and Kalyanaraman (IK) optimal bandwidth selection method. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Does DPEP mainly reduce caste or religious conflict? Our outcome measure does not differentiate between communal (religious-based) and caste-based conflicts, instead combining both into a single survey item. However, supplementary analyses help us to disentangle which type of conflict was primarily swayed by DPEP.

We start by examining DPEP's impact on outcomes that capture caste-based discrimination—on the assumption that if DPEP reduces caste conflict, these outcomes should shift, too. Further outcomes include the presence of public toilet facilities requiring manual scavenging (from the 2011 Census of India) and the number of households dependent on scavenging as their primary source of income (from the 2012 Socio-Economic and Caste Census). Manual scavenging—the practice of removing human waste from dry toilets or sewers, often without protective equipment—is a form of labor historically assigned to Dalit communities, particularly sub-castes such as the Valmiki. We also analyze IHDS survey responses on the continued practice of untouchability. RD estimates in Online Appendix Tables A8, A9, and A10 reveal no evidence that DPEP impacted these outcomes. This suggests that caste-based norms and forms of exclusion remained resistant to change.

By contrast, heterogeneous treatment effect analyses indicate that DPEP reduced Hindu-Muslim conflict above all. We estimate subgroup effects based on two pre-treatment characteristics. First, we compare respondents in localities (i.e., villages or neighborhoods) where there are resident Muslims to those in localities without any Muslim residents. Second, we examine districts with a history of Hindu-Muslim violence—defined as districts that experienced at least one riot between 1950 and 1991—against districts that did not witness such violence in the post-independence period.[14] Finally, we assess whether DPEP's effects differ based on the combination of these two dimensions: whether respondents lived in a settlement with Muslim residents and in a district with a history of communal rioting.

Estimated subgroup effects visualized in Figure 4 suggest that DPEP reduced Hindu-Muslim conflict. The RD estimates demonstrate that DPEP's conflict-mitigating effects are concentrated among units where Muslims reside, and among districts with a history of Hindu-Muslim violence. Indeed, the point estimates are larger than the RD estimates presented for the entire sample in Table 2, Panel A. We detect null results when estimating the effect of DPEP among settlements in which no Muslims reside as well as districts with no prior Hindu-Muslim conflict. These patterns suggest that DPEP's conflict-reducing effects were driven by its impact on religious rather than caste-based conflict.

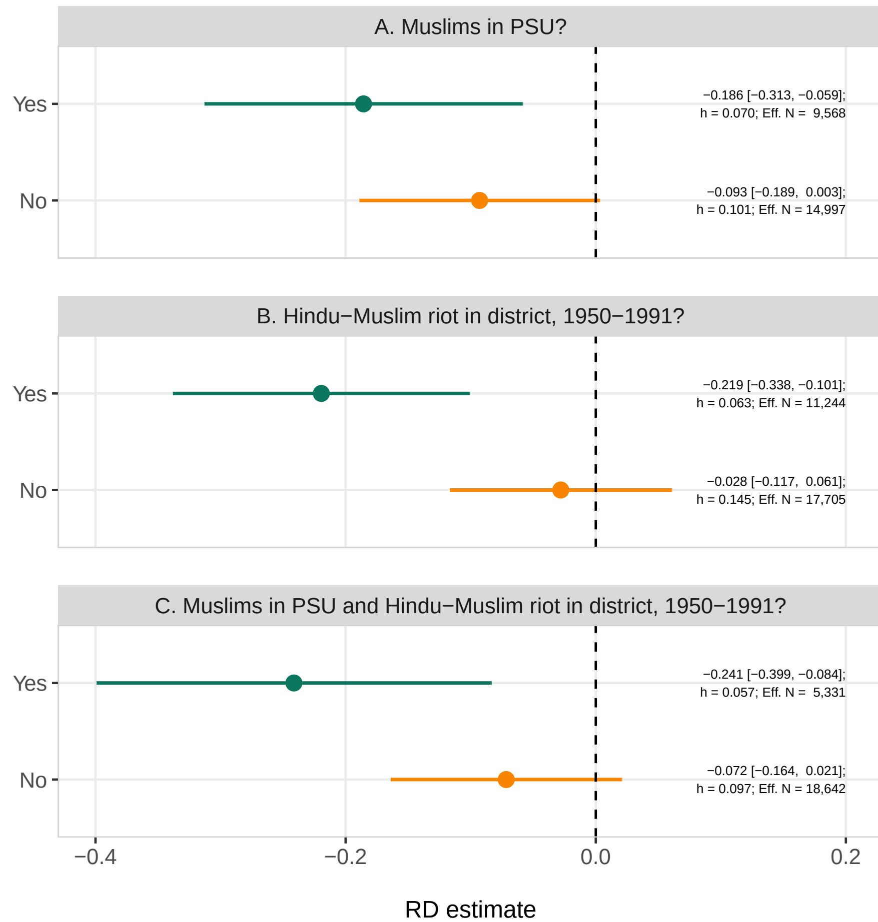
Mechanisms

Testing bias-reduction explanations Did DPEP advance civic norms that emphasized inclusive citizenship and human rights for India's diverse population? We initially look at whether DPEP improved students' scores on standardized social studies tests, particularly on items relevant to intergroup peace and cooperation. For this analysis, we use data gathered by NCERT, an autonomous body under India's Ministry of Education, which conducted nationwide exams in 2017 among a representative sample of primary school students (NCERT, 2017).[15] Outcomes of interest are (a) the share of students in each district who passed the overall social studies exam, and (b) the share passing the two modules most relevant to intergroup relations—democracy and equality, and human rights. We apply the same RD approach used in the main analysis using the district as the unit of analysis—i.e., the level at which the exam data are reported.

14 Riot data are drawn from Varshney and Wilkinson (2006).

15 While the exam was administered in 2017—over a decade after DPEP concluded—it nonetheless offers valuable insights into the program's longer-term influence on salient civic norms.

Figure 4: Geographic heterogeneity in the estimated effect of DPEP on intergroup conflict.



Notes: This figure presents RD estimates of the effect of DPEP on intergroup conflict, stratified by district riot history and locality demographics. The unit of analysis is the household. Data come from both IHDS waves. We code primary sampling units (PSUs) "with Muslims" as those that are villages with a non-zero percentage population of Muslims (as recorded in the IHDS village-survey module), or PSUs in urban areas. PSUs without Muslims are villages with zero Muslims. In all regressions, the running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel. 95% confidence intervals are based on robust standard errors obtained from `rdrobust`'s bias-corrected and cluster-adjusted variance estimator (clustered by 1991 districts).

Table 3 presents the results. Among all districts, Column 1 shows a positive estimate of DPEP on social studies scores, significant at the 10% level. The coefficient suggests that DPEP increased the proportion of students receiving a passing grade by 4.7 percentage points. The effect on scores for diversity-relevant subtopics (Column 2) is positive but not statistically significant. Columns 3–6 stratify the analysis by districts' histories of Hindu-Muslim riots, recalling that earlier results showed DPEP's strongest conflict-reducing effects in riot-prone areas. The contrast is pronounced: in districts with prior Hindu-Muslim riots, DPEP increased the share of students passing the overall exam by 11.9 percentage points (Column 3), and increased the share passing the relevant exam modules by 12.8 percentage point (Column 4). We find no effects in districts without a history of Hindu-Muslim riots (Columns 5–6). We interpret these findings as evidence that DPEP delivered inclusive content to students, and that this content was influential in regions with severe ethnic polarization.

Table 3: Estimated effects of DPEP on social studies attainment.

	All districts		Hindu-Muslim riot in district, 1950-1991?			
			Yes		No	
	All topics	Civics topics	All topics	Civics topics	All topics	Civics topics
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	4.738*	4.649	11.919**	12.761**	1.077	-0.031
	(2.782)	(3.055)	(5.590)	(5.950)	(2.764)	(2.740)
Effective *N*	157	151	73	69	160	147
Overall *N*	614	614	310	310	304	304
$\hat{h}$	0.068	0.066	0.063	0.061	0.130	0.121
K clusters	450	450	220	220	230	230
Outcome mean	43.809	40.003	44.583	41.202	41.666	37.221

Notes: This table presents RD estimates of the effect of DPEP on the proportion of students passing a standardized social studies exam. Odd numbered columns look at students' overall performance on all social studies topics; even numbered columns focus on the topics of the assessment that measure students' performance on modules relating to democracy, equality, and human rights. The unit of analysis is the 2011 district. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Our next analyses investigate whether DPEP shaped norms surrounding gender. Perceived violations of established gender expectations are often flashpoints for group conflict. DPEP may have curtailed conflict by eroding the patriarchal ideologies that help fuel it. To explore this possibility, we return to the IHDS survey data, which tracks women's participation in civil society organizations, female harassment, and women's reports of intimate partner violence in the community.

Table 4 presents results from our main RD estimator, providing evidence in support of this mechanism for three indicators of female empowerment. First, in districts assigned to DPEP, we observe a 9.7 percentage point increase in the likelihood that women are members of *mahila mandals*: civil society organizations focused on empowering women and mothers, particularly in rural areas (Column 1). This represents nearly a doubling of participation relative to non-DPEP districts. Second, we find an 11.2 percentage point decline in reported instances of gender-based harassment of girls, an effect that is marginally statistically significant (Column 2). We also detect a marginally significant reduction in the share of women who report that it is typical for husbands to beat their wives for leaving the house (Column 3).

Table 4: Estimated effects of DPEP on women's empowerment.

Respondent:	Household head		Woman		
			Usual for husbands to beat wives if wives ...?		
	Mahila mandal member in HH	Unmarried girls harrassed locally	Leave house	Neglect house	Cook badly
	(1)	(2)	(3)	(4)	(5)
RD estimate	0.097**	-0.112*	-0.150*	-0.140	-0.075
	(0.044)	(0.065)	(0.080)	(0.115)	(0.077)
Effective *N*	22,533	23,625	31,598	21,389	30,604
Overall *N*	80,762	80,522	68,228	68,240	68,205
$\hat{h}$	0.083	0.086	0.120	0.090	0.116
K clusters	295	295	279	279	279
Outcome mean	0.052	1.23	0.542	0.449	0.333

Notes: Outcomes are based on the following survey questions. Column 1—Does anybody in the household belong to a mahila mandal (Yes = 1, No = 0)? Column 2—How often are unmarried girls harassed in your village / neighborhood (Rarely/never = 1, Sometimes = 2, Often = 3)? Columns 3–5—In your community, is it usual for husbands to beat their wives in each of the following situations: If she goes out without telling him; If she neglects the house or the children; If she doesn't cook food properly (Yes = 1, No = 0)? The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

A final normative explanation is that DPEP altered the composition of school types within treated districts. In particular, DPEP could have crowded out religious schools that promote socially divisive ideas. To assess this possibility, we examine DPEP's impact on the construction of Vidya Bharati (VB) schools—a nationwide network affiliated with the Rashtriya Swayamsevak Sangh (RSS) through its educational wing, the Vidya Bharati Akhil Bharatiya Shiksha Sansthan. Founded in 1952, VB claims to offer an education rooted in Indian culture and tradition, operating thousands of schools across rural and urban India. Critics and supporters have noted VB's role in advancing Hindu nationalism (Iwanek, 2022). By improving the quality and accessibility of secular state schooling, DPEP may have reduced the prevalence of such divisive institutions.

We find no evidence that DPEP reduced the supply of VB schools. On the contrary, our estimates suggest that DPEP increased VB school openings in treated districts. RD estimates in Online Appendix Table A11 indicate that DPEP led to an 11 percentage point rise in the share of years between 1994 and 2016 in which a new VB school was established in a given district. Leveraging the district-year panel structure of the VB school openings data, we also conduct an event study across all districts. The results in Online Appendix Figure A7 underscore that new VB school construction rose sharply in DPEP-treated districts.

It remains unclear whether the RSS established VB schools in response to DPEP investments into secular education, or whether DPEP funds—channeled through local decision-making bodies—might have been diverted to support the construction of VB schools. Regardless, the results suggest that DPEP did not reduce conflict by displacing Hindu nationalist schools. If anything, the presence of VB schools would have made DPEP's goals even more challenging in the districts where it operated, making our core findings all the more striking.

We also test the relational explanations posited by our theoretical framework. DPEP may have reduced conflict by increasing intergroup contact across communities. To consider this mechanism, we deploy several outcome measures. First, we use data from the second wave of the IHDS (2011–12), which asked households whether they had acquaintances across a broad range of occupations who belonged to a caste or community different from their own.[16] If DPEP created interethnic social networks through classroom integration, we might expect an increase in outgroup acquaintances once treated students had completed schooling and became adults.[17] However, we find null results for acquaintances with outgroup individuals working in any of the aforementioned occupations as well as for acquaintances with outgroup teachers and school staff in particular (see results in Online Appendix Table A12).

We also evaluate whether DPEP encouraged positive intergroup contact in the classroom. We rely on the first round of IHDS conducted in 2005-06—years in which DPEP was still in force and its effects on the classroom environment are detectable. The survey asked respondents to indicate whether their children's school teacher is biased against members of other communities and whether their children enjoy school. We find null results for these outcomes as well (see Online Appendix Table A12).[18]

Testing opportunity-cost explanations Finally, we test whether the reduction in conflict can be partially explained by improved economic conditions. Such improvements may have increased the opportunity costs to participating in intergroup conflict: economically-productive activities become more "profitable" than utility gained when engaging in conflict. We estimate the effects of DPEP on household-level measures of income and consumption as well as individual-level measures of permanent employment—as recorded in the second round of the IHDS survey. Across those measures, the RD estimates are substantively small and not statistically significant (Online Appendix Table

16 These occupations include doctors, health workers, government officers, other government employees, elected politicians, political party officials, inspectors, police personnel, military service members, teachers, and other school workers.

17 It is for this reason that we focus on the second round of IHDS such that sufficient time had passed for DPEP-treated students to complete school.

18 In Online Appendix Table A13, we observe null effects for three of the four outcomes, both in districts with and without a history of Hindu-Muslim riots. We do find that DPEP significantly increased students' reported enjoyment of school in riot-prone districts; however, given its isolation, we do not interpret this result as compelling evidence that DPEP transformed intergroup interactions in classrooms.

A14).

We also ask whether DPEP stimulated local macroeconomic growth, potentially increasing economic abundance and reducing intergroup competition over scarce resources. We test this mechanism by estimating DPEP's effect on district-level night-time luminosity. Satellite-based night light intensity is a widely used proxy for economic activity, particularly in contexts where reliable subnational GDP data are unavailable. We find no evidence that DPEP influenced average night-time luminosity across the 1997–2013 period (Online Appendix Table A14). All null results concerning socioeconomic mechanisms hold when disaggregating by districts by their histories of Hindu-Muslim riots (Online Appendix Table A15). As such, there is little support for the hypothesis that DPEP reduced conflict by improving individual or community-level material conditions.

Discussion

Education may be among the most powerful tools states have at their disposal to shape society. Our findings reveal that a large-scale primary education reform in India—the world's largest democracy—contributed to a durable reduction in intergroup conflict. This result offers a hopeful counterpoint to recent scholarship emphasizing how mass education is used to instill authoritarian values. Greater access to education, together with content that challenges patriarchal norms and promotes an inclusive vision of the nation, has the potential to forge social cohesion. Our results thus underscore the simple but powerful observation that the effects of education are conditional on the content of the curriculum. These findings have broad implications for political science, and in particular for understanding contexts where religious polarization and group-based conflict continue to hinder economic development and threaten fundamental freedoms.

India is an important case for studying whether education reforms can stem conflict. As Amartya Sen (2012, p. 347) observed, "it is ... the pull toward internal separation of communities that has presented the strongest challenge in recent years to the integrity of the national identity." Since 1947, the secularist vision that animated the country's independence movement has been under pressure from political forces targeting minority communities. Identifying policies that can counter such exclusion is essential for managing social tensions and protecting democratic institutions. Our findings suggest that mass education may serve as one such policy tool—not only in India but also in other divided societies. In Northern Ireland, for example, the government invested £25 million in 2007 to reform school curricula aimed at improving Catholic-Protestant relations (Council for the Curriculum, Examinations and Assessment, 2007). In Nigeria, expanded access to primary education in the 1970s has been linked to greater civic and political engagement (Larreguy and Marshall, 2017).

There is broad scope for future research. Our study's main outcome measure relies on self-reported perceptions of intergroup conflict. While this measure captures critical variation in respondents' lived experiences at the most local level, it does not permit disaggregation by conflict type or intensity. Future work would benefit from more fine-grained measures that distinguish among different modalities of conflict. Researchers might also examine whether expanded access to secondary or post-secondary education produces similar effects, building on credible research designs such as Duflo, Dupas and Kremer (2021). Studies going forward could exploit random or quasi-random variation in the adoption of specific curricular reforms—similar to the approach in Haas and Lindstam (2024) and Cantoni et al. (2017)—to identify which aspects of curriculum design are most effective in suppressing conflict. Last, our null results regarding caste-based conflict and discrimination suggest a greater resistance to change, echoing prior research highlighting the particularly entrenched nature of caste norms in India (Anderson, 2011; Jodhka and Naudet, 2023). As such, the findings point to the need for further research to discover policy instruments capable of addressing this seemingly more intractable dimension of social exclusion.

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A Descriptive statistics

Table A1: Summary statistics.

Measure	N	Mean	St. Dev.	Min.	Median	Max.
IHDS individual-level survey round 1						
Ever attended school, younger cohort (0/1)	78740	0.68	0.46	0	1	1
Ever attended school, older cohort (0/1)	128681	0.65	0.48	0	1	1
Math standardized test score (0-3)	12381	1.58	1.03	0	2	3
Reading standardized test score (0-4)	12430	2.62	1.33	0	3	4
Writing standardized test score (0/1)	12323	0.69	0.46	0	1	1
IHDS household-level survey round 1						
Any outgroup acquaintances (0/1)	41935	0.68	0.47	0	1	1
Any outgroup school staff acquaintances (0/1)	41975	0.50	0.50	0	0	1
Children enjoy school (0/1)	11490	0.93	0.25	0	1	1
School teacher is biased against outgroup (0/1)	11495	0.10	0.29	0	0	1
IHDS household-level survey round 2						
Consumption (Rs. 1000)	42129	118.06	120.92	0.18	87.25	4080.76
Income (Rs. 1000)	42152	127.76	216.67	-1037.04	73.50	11360
Permanent employment (0/1)	64278	0.17	0.38	0	0	1
IHDS household-level survey rounds 1 and 2						
Unmarried girls harrassed locally (0/1)	80522	1.18	0.42	1	1	3
Group conflict (3-point scale)	80563	0.21	0.31	0	0	1
Group conflict (0/1)	80563	0.35	0.48	0	0	1
Mahila mandal member in HH (0/1)	80762	0.08	0.28	0	0	1
IHDS women's individual-level survey rounds 1 and 2						
Usual for husbands to beat wives... cook badly (0/1)	72806	0.29	0.46	0	0	1
Usual for husbands to beat wives... leave house (0/1)	72834	0.44	0.50	0	0	1
Usual for husbands to beat wives... neglect house (0/1)	72845	0.38	0.48	0	0	1
Economic Census of India (1990), district-level						
Prop. firms employing women	423	0.19	0.12	0.05	0.16	0.63
Prop. government firms	423	0.08	0.06	0.01	0.07	0.36
Prop. employment in manufacturing	423	0.30	0.11	0.02	0.29	0.72
Population Census of India (1991), district-level						
Female lab. force participation	450	0.25	0.13	0.02	0.26	0.60
Male lab. force participation	450	0.51	0.04	0.39	0.52	0.69
Avg. rainfall in towns	430	1299.22	721.58	144.60	1087.32	3973.20
Banks per 100k residents in towns	439	22.28	14.86	0	19.02	147.06
Maximum temperature in towns	412	39.35	11.99	0	39.85	180.20
Medical facilities per 100k residents in towns	439	5.87	8.79	0	4.08	147.06
Middle schools per 100k residents in towns	439	22.91	13.88	0	21.16	110.63
Primary schools per 100k residents in towns	439	50.21	24.39	0	45.31	234.39
Railroad facilities in towns	436	16.94	76.37	0	0	1190
Secondary schools per 100k residents in towns	439	15.59	8.93	0	13.31	56.25
Town distance from state headquarters	437	243.55	186.59	0	214	1025
Middle schools per 100k residents in villages	247	22.26	11.62	5.08	20.25	87.91
Primary schools per 100k residents in villages	247	90.95	39.51	28.45	84.46	257.25
Railroad facilities in villages	435	0.83	0.37	0	1	1
Secondary schools per 100k residents in villages	247	7.35	6.13	0	5.60	33.16
Tank water access per 100k residents in villages	218	15.48	30.94	0	1.97	181.81
Tubewells per 100k residents in villages	218	17.29	30.86	0	4.64	216.79
Well water per 100k residents in villages	247	76.73	56.59	0	74.86	354.52
Female residents as proportion of population	450	0.48	0.02	0.43	0.48	0.55
SC residents as proportion of population	450	0.15	0.08	0	0.16	0.52
ST residents as proportion of population	450	0.15	0.25	0	0.03	0.98
Population Census of India (2011), district-level						
Any service latrines (0/1)	544	0.47	0.50	0	0	1
Service latrines per 100k residents in towns	544	98.33	389.04	0	0	5657.52

Socioeconomic and Caste Census of India (2012), district-level						
Any households reliant on scavenging (0/1)	591	0.76	0.43	0	1	1
Share of households reliant on scavenging	591	0.00	0.01	0	0.00	0.22
Varshney and Wilkinson (2006), district-level						
Any riots between 1950-1991 (0/1)	467	0.50	0.50	0	0	1
National Achievement Survey (2017), district-level						
Proportion of students passing civics assessment	637	38.05	9.84	9.82	37.03	76.27
Proportion of students passing social studies assessment	637	42.68	9.67	10.63	42.16	78.22
SHRUG Night Lights, district-level						
Average annual night time luminosity	640	5.42	9.19	0	2.90	62.98
Vidya Bharati schools, district-level						
Share of years when any VB schools founded (1995-2015)	450	0.21	0.21	0	0.17	0.96
Number of VB schools founded (1995-2015)	450	16.65	28.40	0	7	301

Notes: This table presents summary statistics for the variables used in our main analyses, RD validation exercises, and mechanism analyses.

B Primary school textbooks content analysis

Table A2: Grade school civics textbooks used in topic model estimation.

Year	State	Grade level	Text title	Source
1970	Mysore (Karnataka)	5	*Social Studies*	Azim Premji
1972	Mysore (Karnataka)	5	*Social Studies*	Azim Premji
1977	India-wide	6	*History and Civics*	Azim Premji
1977	Karnataka	3	*Social Studies*	Azim Premji
1978	Karnataka	8	*Social Studies: History and Civics*	Azim Premji
2006	Maharashtra	6	*Our Local Government Bodies (Civics and Administration)*	NDLI
2010	Tamil Nadu	6	*Social Science*	NDLI
2011	Tamil Nadu	8	*Social Science*	NDLI
2012	Andhra Pradesh	7	*Social Studies*	Azim Premji
2013	Andhra Pradesh	8	*Social Studies*	Azim Premji
2014	Maharashtra	4	*Environmental Studies (Part One)*	NDLI

Notes: This table presents the list of primary school social studies texts used in the topic model estimation. The sources are drawn from Azim Premji University (2025) and National Digital Library of India (2025). For each text, we exclude topics unrelated to civics and social studies such as environmental conservation and geology. The Maharashtra grade 4 text on environmental studies included units on civics and social studies, which we use for our analysis.

Table A3: Keywords used in keyword-assisted topic model estimation.

Topic	Keywords

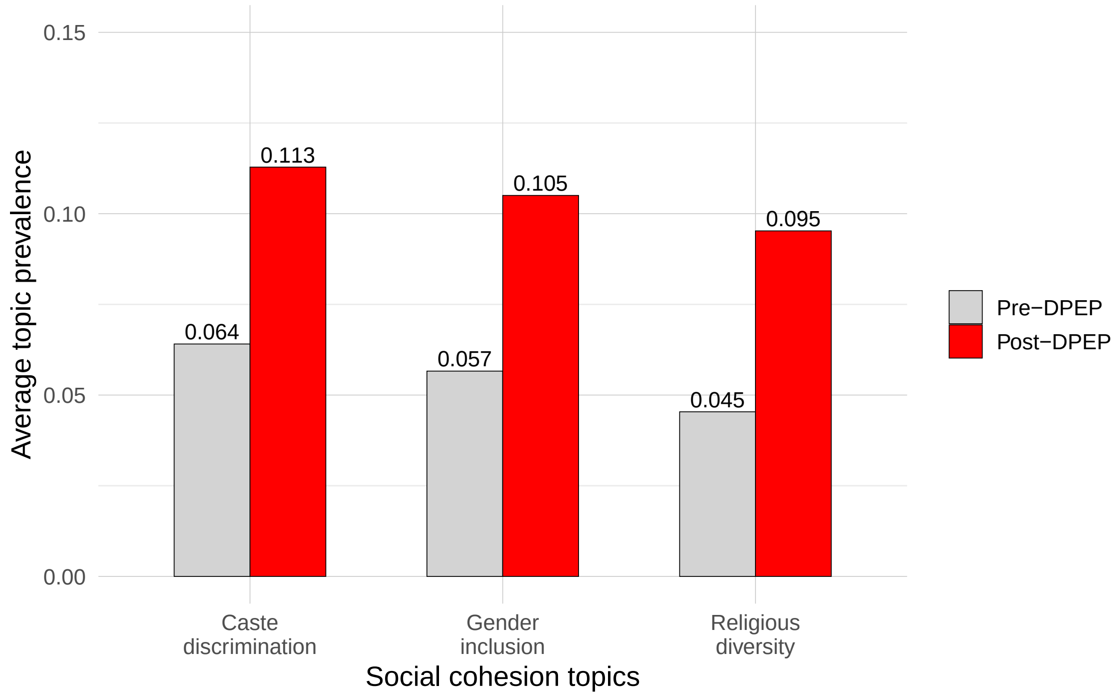
Gender inclusivity	`women`, `woman`, `female`
	`girls`, `girl`, `gender`
	`equality`, `empowerment`, `rights`

Religious diversity	`religion`, `communal`, `muslim`
	`religious`, `islam`, `hindu`
	`tolerance`, `harmony`, `faith`
	`inclusive`, `secular`, `diversity`

Caste discrimination	`caste`, `dalit`, `untouchability`
	`untouchable`, `harijan`, `scheduled caste`
	`bahujan`, `sc`, `atrocity`, `atrocities`

Notes: This table presents the keywords associated with inclusivity that we specified when estimating the keyword assisted topic model according to the procedure outlined by Eshima, Imai and Sasaki (2024).

Figure A1: Prevalence of inclusivity content in primary school social studies textbooks.



Notes: This figure compares the prevalence of topics associated with greater social inclusivity in textbooks prior to DPEP (pre-1994) and following the program's implementation (post-1994). Each estimate indicates the proportion of documents' text relating to each of the topics, as measured through the keyword assisted topic model procedure developed in Eshima, Imai and Sasaki (2024). The keywords we specified for each topic can be found in Table A3 . Red bars refer to estimates using social studies textbooks published after DPEP's implementation, whereas gray bars indicate topic proportions among textbooks published prior to the reform. The complete list of social studies textbooks used in this analysis can be found in Table A3.

C RD design validation

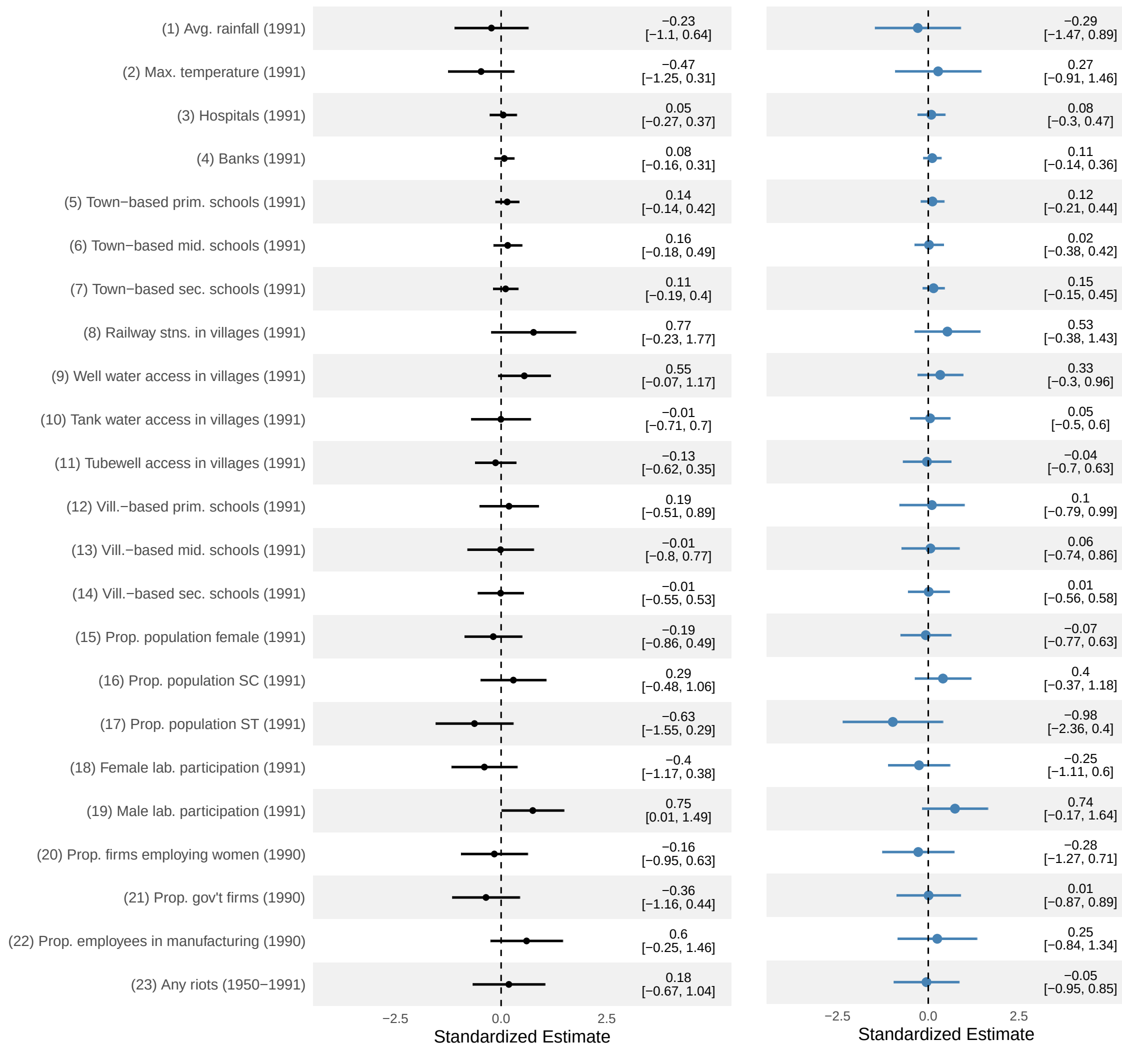
Validating RD assumptions Our RD framework rests on several assumptions necessary for a causal interpretation of our estimates. Chief among them is that districts near the DPEP eligibility threshold should be similar in their underlying characteristics. We validate this with balance tests, comparing the distribution of a set of pre-treatment covariates between districts just above and just below the threshold. As shown in Online Appendix Figure A2, we find no meaningful differences, on average, between eligible and ineligible districts. We also search for evidence of "manipulation" around the threshold, which could threaten our identification strategy if, say, districts close to the threshold could influence their DPEP treatment assignment. We find no evidence of such sorting, either visually (see Online Appendix Figure A4) or using a formal statistical density test (see Online Appendix Table A4).[1]

1 We further validate our design by conducting power analyses (Stommes, Aronow and Sävje, 2023) in Online Appendix Table A5, and placebo tests in Online Appendix Table A7.

C.1 Balance tests

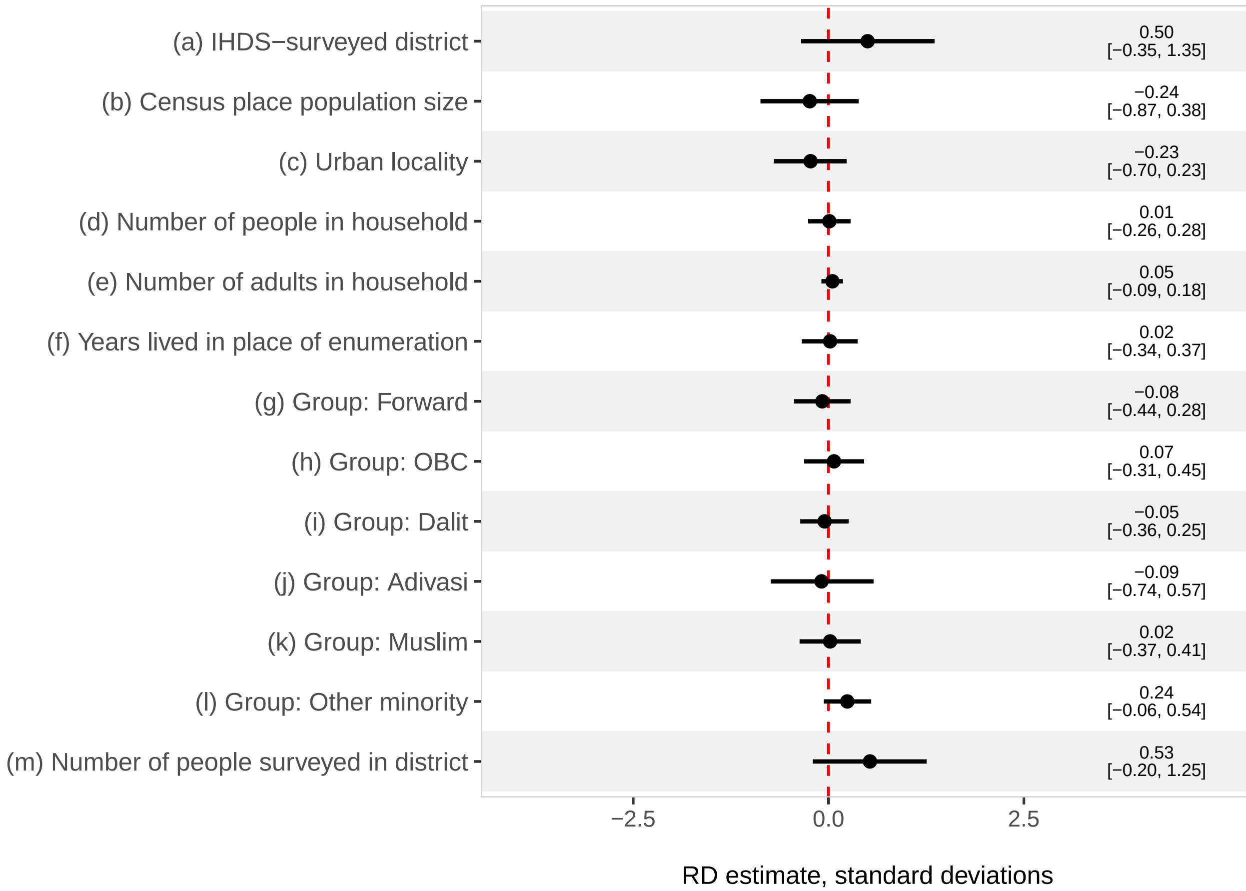
Figure A2: Pre-treatment covariate balance tests.

(a) All districts. (b) Districts included in IHDS.



Notes: This figure includes results from RD analyses testing whether districts eligible versus ineligible for DPEP are balanced on pre-treatment covariates. The estimates in the left panel use all districts, and estimates in the right hand panel estimate among districts included in the IHDS panel. District-level outcomes 1-19 are drawn from the 1991 Population Census of India, outcomes 20-22 were measured in the 1990 Economic Census of India, and outcome 23 is a binary indicator of riot occurrence between 1950 and 1991 (Varshney and Wilkinson, 2006). The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that districts above the cutoff are those eligible for DPEP. The coefficient represents the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. 95% confidence intervals are based on robust standard errors obtained from `rdrobust`'s bias-corrected and cluster-adjusted variance estimator. All coefficients and confidence interval values are standardized.

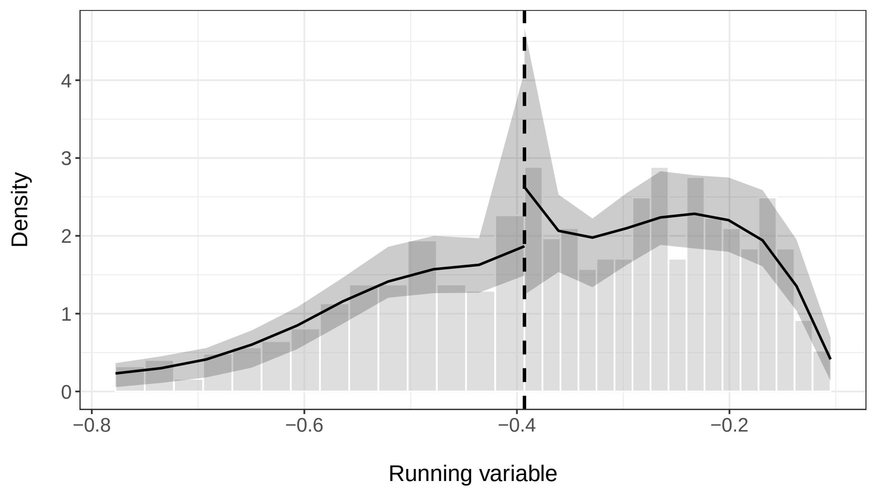
Figure A3: IHDS survey demographics balance tests.



Notes: This figure tests whether IHDS district- and household-level characteristics vary discontinuously at the DPEP eligibility cutoff. The unit of analysis in rows (a) and (m) is the 2001 district; for all other rows, the unit of analysis is the household. The sample in row (a) includes all 2001 districts for which a measure of female literacy in the 1991 Census is available. The remaining rows use data from IHDS-surveyed districts only. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that districts above the cutoff are those eligible for DPEP. Coefficients represent the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. 95% confidence intervals are based on robust standard errors obtained from `rdrobust`'s bias-corrected and cluster-adjusted variance estimator. All coefficients and confidence intervals are expressed in standardized units.

C.2 Density tests

Figure A4: Density of the running variable to the left and right of the DPEP eligibility cutoff.



Notes: This figure plots the distribution of the running variable to the left and right of the treatment assignment threshold. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the units above the RD cutoff are eligible for treatment. Bins and confidence intervals are constructed based on the local polynomial density estimator proposed in Cattaneo, Jansson and Ma (2020).

Table A4: Density test estimates (`rddensity`).

	Estimates
	(1)
Density (left of cut point)	2.799
Density (right of cut point)	2.956
Density difference	0.157
t-statistic (density difference)	0.138
p-value (density difference)	0.89

Notes: This table displays the results of a density test (Cattaneo, Jansson and Ma, 2020) that looks for evidence of manipulation around the regression discontinuity cut-off—average female literacy rate based on the 1991 Census of India. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access. The robust t-statistic and p-value estimates suggest no reason to believe there was manipulation in the running variable around the cutoff.

C.3 RD power results

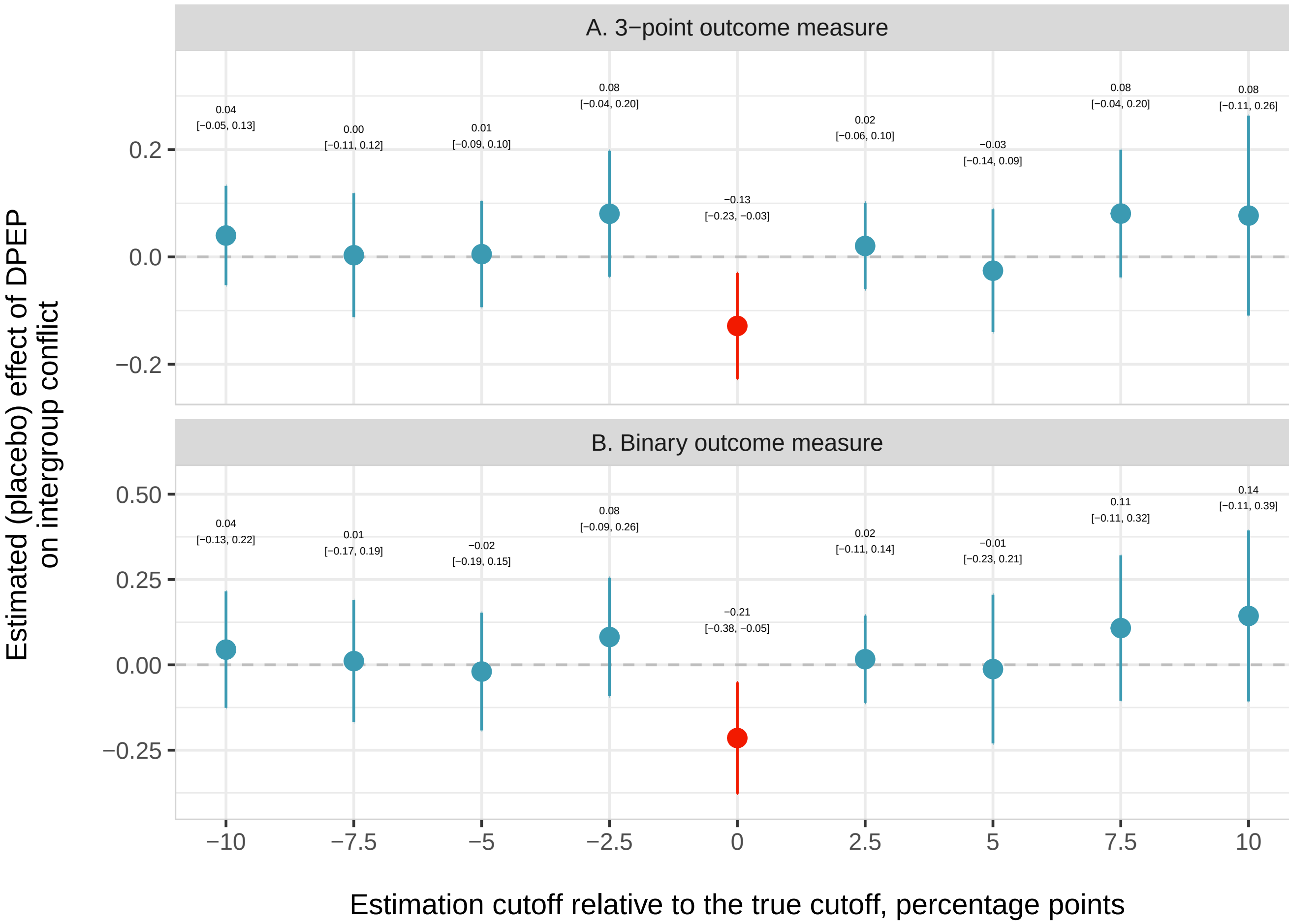
Table A5: Power analysis results (rdpower).

	Group conflict (3-point scale)	Group conflict (0/1)
	(1)	(2)
Effect size	-0.129	-0.210
Power	0.728	0.714

Notes: This table presents power calculations implemented by the R package `rdpower` by Cattaneo, Titiunik and Vazquez-Bare (2019). It estimates the power of a two-tailed test at the 5% significance level based on a central limit approximation for the sampling distribution. The power analyses presume that p-values will be constructed with the bias adjustment and robust standard errors, clustered by 1991 districts, as implemented by `rdrobust`. The unit of analysis is the 2001 district. The outcomes are the group conflict outcomes from IHDS measured on a three-point scale (Column 1) and a binary scale (Column 2). The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access.

C.4 Placebo cutpoints

Figure A5: Placebo estimates of the effect of DPEP on intergroup conflict using different cutpoints.



Notes: This figure presents the estimated effect of DPEP on intergroup conflict across different placebo cutpoints, using two methods of coding the outcome variable. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Estimates at the true DPEP cutoff are highlighted in red, while other placebo estimates are shown in blue. 95% confidence intervals are based on robust standard errors obtained from `rdrobust`'s bias-corrected and cluster-adjusted variance estimator.

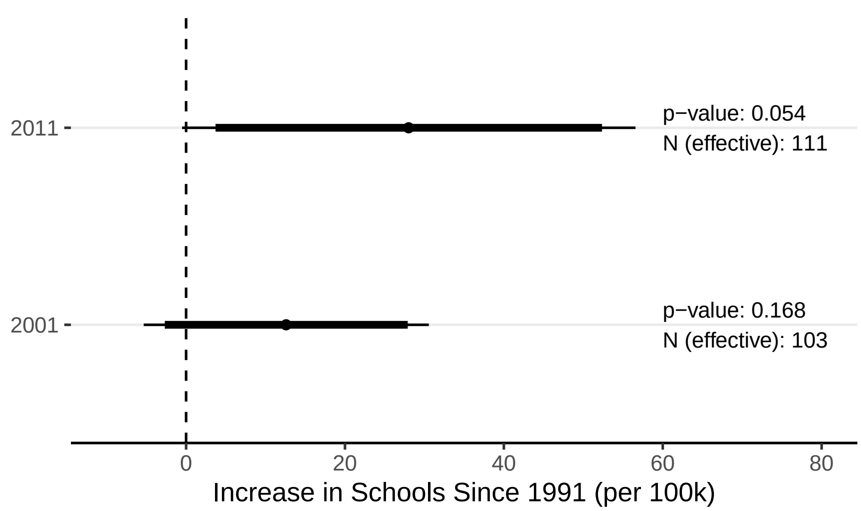
C.5 Supplementary first stage tests

Table A6: Estimated first-stage effects of 1991 female literacy rate on DPEP assignment and funding.

	Any DPEP funding (0/1) (1)	IHDS funding (INR crores) (2)
RD estimate	0.441**	11.830***
	(0.205)	(3.973)
Effective N	130	204
Overall N	373	373
$\hat{h}$	0.099	0.147
K clusters	295	295
Outcome mean	0.354	5.52

Notes: This table presents RD estimates of the effect of DPEP on DPEP assignment uptake. The sample is composed of districts where the IHDS survey was conducted. The outcome in Column 1 is a binary variable taking a value of one (and zero otherwise) if the district was included in the DPEP program. The outcome in Column 2 is the DPEP funding received by a district, imputed using the method described in Section I. The unit of analysis is the 2001 district. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access. The robust standard error obtained from `rdrobust`, clustered by 1991 districts, is in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Figure A6: Estimated effect of DPEP on school construction.



Notes: This figure presents RD estimates of the effect of DPEP on a district's increase in the number of schools per 100,000 residents from 1991 to 2011 (top estimate) and 1991 to 2001 (bottom estimate). Outcomes are drawn from the 1991, 2001, and 2011 Census of India data collated by Asher et al. (2021). The units of analysis are at the 2001 (bottom estimate) and 2011 (top estimate) district levels. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access. 95% confidence intervals are based on robust standard errors obtained from `rdrobust`'s bias-corrected and cluster-adjusted variance estimator (clustered at the 1991 district level).

C.6 Evidence against stacked discontinuities and policy confounding

Table A7: Placebo tests of DPEP's effect on public facilities.

	2001 Census	2011 Census					
	Secondary schools	Secondary schools	Community health centers	Primary health centers	Maternity and child welfare centers	Family welfare centers	Community centers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD estimate	-1.285	0.664	-0.174	1.784	1.999	-15.453	-0.109
	(3.091)	(6.516)	(0.521)	(1.324)	(1.996)	(19.545)	(0.159)
Effective N	300	307	153	100	150	260	147
Overall N	578	617	607	607	607	607	607
$\hat{h}$	0.135	0.126	0.067	0.041	0.064	0.112	0.061
K clusters	450	450	444	444	444	444	444
Outcome mean	16.87	24.674	0.738	2.99	3.978	20.275	0.199

Notes: This table presents RD estimates of the effect of DPEP on a district's school, community, and welfare facilities per 100,000 residents in 2001 and 2011. These tests aim to assess whether the discontinuity used for DPEP eligibility was also used to determine the allocation of public goods and services unrelated to the program. Outcomes are drawn from 2001 and 2011 Census of India data collated by Asher et al. (2021). The units of analysis are at the 2001 district levels. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access. We include robust standard errors obtained from `rdrobust`'s bias-corrected and cluster-adjusted variance estimator (clustered at the 1991 district level) *p<0.1; **p<0.05; ***p<0.01.

D Caste conflict and discrimination analyses

Table A8: Estimated effects of DPEP on scavenging as main source of income.

	Share of households			Any households (0/1)		
	All	Riots	No riots	All	Riots	No riots
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	-0.001	0.001	-0.002	0.000	-0.025	0.016
	(0.001)	(0.001)	(0.002)	(0.133)	(0.197)	(0.200)
Effective N	212	82	77	250	78	131
Overall N	591	293	298	591	293	298
$\hat{h}$	0.097	0.078	0.063	0.110	0.072	0.110
K clusters	428	205	223	428	205	223
Outcome mean	0.002	0.002	0.002	0.694	0.813	0.557

Notes: This table presents RD estimates of the effect of DPEP on the prevalence of scavenging as households' main source of income. District-level measures of scavenging are constructed using the 2012 Socioeconomic and Caste Census of India curated by Asher et al. (2021). The outcomes analyzed include the share of households engaged in scavenging in a district (Columns 1–3) and a binary indicator of whether any households in the district rely on scavenging (Columns 4–6). Columns 2–3 and 5–6 refer to subgroup analyses based on districts' riot histories. The unit of analysis is the 2011 district. The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A9: Estimated effects of DPEP on prevalence of service latrines.

	Service latrines per 100k residents			Any service latrines (0/1)		
	All	Riots	No riots	All	Riots	No riots
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	44.199	50.325	54.258	0.222	-0.079	0.581***
	(58.802)	(70.471)	(87.851)	(0.180)	(0.236)	(0.201)
Effective N	154	75	122	157	143	77
Overall N	544	290	254	544	290	254
$\hat{h}$	0.078	0.079	0.112	0.081	0.134	0.075
K clusters	400	205	195	400	205	195
Outcome mean	38.078	25.872	26.908	0.327	0.443	0.147

Notes: This table presents RD estimates of the effect of DPEP on the prevalence of scavenging as households' main source of income. District-level measures of scavenging are constructed using the 2011 Census of India curated by Asher et al. (2021). The outcomes analyzed include the number of service latrines per 100,000 residents (Columns 1–3) and a binary indicator of whether there are any service latrines present in the district (Columns 4–6). Columns 2–3 and 5–6 refer to subgroup analyses based on districts' riot histories. The unit of analysis is the 2011 district. The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A10: Estimated effects of DPEP on untouchability practices.

	Practices untouchability (0/1)	Problem with SC in kitchen (0/1)
	(1)	(2)
RD estimate	-0.027	0.005
	(0.082)	(0.066)
Effective N	14,548	11,185
Overall N	40,566	31,129
$\hat{h}$	0.102	0.101
K clusters	294	294
Outcome mean	0.214	0.141

Notes: This table presents RD estimates of the effect of DPEP on households' practice of untouchability. Household measures of untouchability practices are drawn from IHDS waves 1 and 2. The outcomes analyzed include a binary indicator of whether the household self reports practicing untouchability (Column 1) and a binary indicator of whether the household reports opposing the presence of an SC member in their kitchen (Columns 2). The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

E Vidya Bharati school construction analyses

Table A11: Estimated effects of DPEP on Vidhya Bharati school establishment: regression discontinuity design.

	Share years any new Vidhya Bharati school	Total number new Vidhya Bharati schools
	(1)	(2)
RD estimate	0.116*	5.988
	(0.070)	(9.523)
Effective *N*	122	121
Overall *N*	450	450
$\hat{h}$	0.071	0.070
Outcome mean	0.117	12.543

Notes: This table presents RD estimates of the effect of DPEP on the construction of new Vidya Bharati schools. We estimate the effect on the share of years between 1996 and 2015 with any new Vidya Bharati schools (Column 1) and the total number of new Vidya Bharati schools constructed between 1996 and 2015 (Column 2). The unit of analysis is the 1991 district. The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that being above the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered at the 1991 district level, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Figure A7: Estimated effects of DPEP on Vidya Bharati school establishment: event study.

Notes: This figure presents event study estimates of the effect of DPEP on the construction of new Vidya Bharati schools. The unit of analysis is the 1991 district. We estimate an event study model of the form:

$$Y_{it} = \alpha_i + \delta_t + \sum_{k \neq -1} \beta_k \cdot \mathbb{1}\{t - T_i = k\} + \varepsilon_{it},$$

where Y_{it} is the outcome for unit i in time period t; α_i and δ_t are unit and time fixed effects, respectively; T_i is the period in which unit i first receives treatment (the time period 1991–1995 for all units in this application); and $\mathbb{1}\{t - T_i = k\}$ is an indicator for being k periods relative to treatment. The omitted period $k = -1$ serves as the reference category (here, 1986–1990). The coefficients β_k trace the dynamic effects of treatment over time, and ε_{it} is the error term. We estimate effect of DPEP on the share of years—within five-year increments—that involved construction of any new Vidya Bharati schools (Panel A), as well as the total number of new Vidya Bharati schools constructed within five-year increments (Panel B). 95% confidence intervals are estimated using standard errors clustered at the 1991 district-level. Note, we detect violations of the parallel trends assumption in the analysis presented in Panel B (though not in Panel A), suggesting that less credence should be placed in that analysis.

F Mechanism analyses

F.1 Relational mechanisms

Table A12: Estimated effects of DPEP on outgroup contact.

	Outgroup acquaintances		School environment	
	Any outgroup acquaintance (0/1)	Any school staff outgroup acquaintance (0/1)	Teacher biased against outgroups (0/1)	Children enjoy school (0/1)
	(1)	(2)	(3)	(4)
RD estimate	0.048	-0.010	0.066	0.021
	(0.083)	(0.111)	(0.084)	(0.024)
Effective N	20,114	13,025	4,145	2,727
Overall N	39,456	39,493	10,882	10,874
$\hat{h}$	0.130	0.096	0.105	0.075
K clusters	280	280	280	280
Outcome mean	0.631	0.521	0.105	0.931

Notes: This table presents RD estimates of the effect of DPEP on outgroup contact. The outcome in Column 1 is a binary variable that takes 1 (and zero otherwise) if the respondent has an acquaintance with people from outside their caste/community holding any of the following roles: doctor, health worker, government officer, other government employee, elected politician, political party official, inspector, other police, military, teacher, or other school worker. The outcome in Column 2 is a binary indicator for a respondent's acquaintance with a teacher or other school worker from outside their caste/community. The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A13: Estimated effects of DPEP on outgroup contact (heterogeneity by riot history).

	Outgroup acquaintances				School environment			
	Any outgroup acquaintance (0/1)		Any school staff outgroup acquaintance (0/1)		Teacher biased against outgroups (0/1)		Children enjoy school (0/1)	
	Riots	No riots	Riots	No riots	Riots	No riots	Riots	No riots
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD estimate	0.049	0.065	0.183	0.105	-0.010	0.103	0.074***	-0.031
	(0.144)	(0.149)	(0.183)	(0.152)	(0.068)	(0.192)	(0.028)	(0.029)
Effective N	6,648	5,042	5,425	6,389	1,498	1,729	1,495	1,327
Overall N	23,099	16,357	23,123	16,370	6,277	4,605	6,275	4,599
$\hat{h}$	0.078	0.094	0.060	0.108	0.064	0.109	0.064	0.093
K clusters	157	123	157	123	157	123	157	123
Outcome mean	0.71	0.574	0.566	0.389	0.08	0.124	0.928	0.938

Notes: This table presents RD estimates of the effect of DPEP on outgroup contact by districts' riot history between 1950 and 1991. The outcome in Columns 1 and 2 is a binary variable equal to one (and zero otherwise) if the respondent has an acquaintance with people from outside their caste/community holding any of the following roles: doctor, health worker, government officer, other government employee, elected politician, political party official, inspector, other police, military, teacher, or other school worker. The outcome in Columns 3 and 4 is a binary indicator for a respondent's acquaintance with a teacher or other school worker from outside their caste/community. The unit of analysis is the 2001 district. The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

F.2 Opportunity-cost mechanisms

Table A14: Estimated effects of DPEP on socioeconomic outcomes.

	Household-level		Individual-level	District-level
	Income (Rs. 1000)	Consumption (Rs. 1000)	Perm. Employ. (0/1)	Night-time Luminosity
	(1)	(2)	(3)	(4)
RD estimate	21.015	20.585	0.027	0.957
	(21.465)	(17.323)	(0.055)	(1.348)
Effective N	17,564	11,359	24,163	182
Overall N	39,661	39,640	60,840	617
$\hat{h}$	0.116	0.085	0.105	0.083
K clusters	280	280	280	450
Outcome mean	113.775	101.8	0.135	4.188

Notes: This table presents RD estimates of the effect of DPEP on economic outcomes. Outcomes in Columns 1, 2, and 3 come from the second wave of IHDS. The outcome used in Column 4 is districts' average night time luminosity from 1997 through 2013 (Asher et al., 2021). The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A15: Estimated effects of DPEP on socioeconomic outcomes (heterogeneity by riot history).

	Household-level				Individual-level		District-level	
	Income (Rs. 1000)		Consumption (Rs. 1000)		Perm. Employ. (0/1)		Night-time Luminosity	
	Riots	No riots	Riots	No riots	Riots	No riots	Riots	No riots
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD estimate	-28.401	42.455	14.441	25.468	-0.009	0.002	-0.708	2.149
	(21.345)	(35.514)	(18.164)	(31.354)	(0.085)	(0.088)	(2.295)	(1.575)
Effective N	6,229	8,666	6,666	6,646	10,432	8,208	82	115
Overall N	23,223	16,438	23,208	16,432	34,393	26,447	311	306
$\hat{h}$	0.070	0.147	0.076	0.111	0.080	0.093	0.080	0.097
K clusters	157	123	157	123	157	123	220	230
Outcome mean	124.513	111.901	105.284	102.656	0.133	0.167	6.522	2.239

Notes: This table presents RD estimates of the effect of DPEP on socioeconomic outcomes by districts' riot history between 1950 and 1991. Outcomes in Columns 1 through 6 come from the second wave of IHDS. The outcome used in Columns 7 and 8 is districts' average night time luminosity from 1997 through 2013 (Asher et al., 2021). The unit of analysis is the 2001 district (IHDS outcomes) and 1991 district (night-time luminosity outcomes). The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. The coefficient presents the estimate of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel and an MSE-optimal bandwidth. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

G Matching 2001 districts to 1991 districts

District reorganization in India presents a methodological challenge for studies that seek to link data for districts across different time periods. This linking is something our study demands: our RD design uses female literacy rates measured at the 1991 district level as the forcing variable, but our key outcome measures come from surveys geo-tagged to 2001 district boundaries.

The complication arises because some 1991 districts were subdivided into new administrative units by 2001. Many of these splits are straightforward: they yield "single-parent" districts—child districts that are clean offshoots from one 1991 predecessor. For these cases, treatment assignment (i.e., the 1991 female literacy rate) can be cleanly inherited from the parent district. More problematic are "multi-parent" districts, which are formed by merging fragments from multiple predecessor districts—each potentially having different characteristics and program eligibility (e.g., different female literacy rates relevant for DPEP treatment assignment). Multi-parent districts can thus create ambiguity in treatment classification and running variable assignment.

We address this issue in two ways. First, we diagnose how many districts in the IHDS analysis sample were multi-parent districts. Second, we test the robustness of our main findings to excluding these districts. The results are reassuring. Online Appendix Table A16 shows that fewer than 4 percent of districts in the IHDS sample are multi-parent districts where the largest contributing parent accounts for less than 95 percent of the territory. Online Appendix Table A17 demonstrates that excluding those districts does not alter the main RD results, either substantively or statistically.

Table A16: Diagnosis of multi-parent districts.

	All districts (N = 578)		IHDS districts (N = 373)	
Largest parent threshold:	Number (1)	Percent (2)	Number (3)	Percent (4)
< 0.95	16	2.77	15	4.02
< 0.9	14	2.42	14	3.75
< 0.85	13	2.25	13	3.49
< 0.8	12	2.08	12	3.22
< 0.75	9	1.56	9	2.41
< 0.7	7	1.21	7	1.88
< 0.65	3	0.52	3	0.80
< 0.6	2	0.35	2	0.54
< 0.55	2	0.35	2	0.54
< 0.5	2	0.35	2	0.54
< 0.45	2	0.35	2	0.54
< 0.4	0	0.00	0	0.00

Notes: This table summarizes the extent to which newly created districts in 2001 can be cleanly matched to 1991 districts. This table reports the number and percentage of districts for which the share of the district population derived from a single 1991 parent district falls below various thresholds. The analysis is presented separately for all 578 districts included in the DPEP dataset and the 373 districts surveyed by IHDS. A lower value on the "largest parent threshold" measure indicates a higher degree of fragmentation across 1991 district boundaries. For instance, a value below 0.85 implies that no single 1991 district accounts for more than 85% of the 2001 district's population.

Table A17: Estimated effects of DPEP on intergroup conflict: Robustness to dropping multi-parent districts.

		Keep only districts for which the largest 1991 parent district makes up ... of 2001 child district					
	Full sample (1)	> 0.4 (2)	> 0.5 (3)	> 0.6 (4)	> 0.7 (5)	> 0.8 (6)	> 0.9 (7)
RD estimate	-0.129**	-0.129**	-0.126**	-0.126**	-0.124**	-0.123**	-0.124**
	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)
Effective N	21,613	21,613	21,517	21,517	21,517	21,721	21,721
Overall N	80,563	80,563	80,160	80,160	79,169	78,111	77,980
$\hat{h}$	0.078	0.078	0.079	0.079	0.081	0.082	0.082
K clusters	295	295	295	295	291	287	286
Outcome mean	0.317	0.317	0.316	0.316	0.315	0.315	0.316

Notes: This table presents the robustness of RD estimates of the effect of DPEP on intergroup conflict, progressively restricting the sample to districts that can be more cleanly matched to a single 1991 parent district. Each column imposes a different threshold for how dominant the parent district must be—i.e., what share of the 2001 district's landmass comes from its largest contributing 1991 district. Column 1 shows the main RD estimate using the full sample. Columns 2–7 sequentially exclude districts with more ambiguous lineage, requiring that the largest 1991 district comprise at least 40%, 50%, ... up to 90% of the child district's population. The unit of analysis is the household. Data come from both IHDS waves. Outcome variables are based on the survey question (asked to the heads of sampled households): "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" Responses are recoded as: "a lot of conflict" (=1), "some conflict" (=0.5), "not much conflict (=0). The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using local linear regression with a triangular kernel. Standard errors, clustered by district, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

H Robustness

Table A18: Robustness of DPEP's estimated effects on intergroup conflict to different optimal bandwidth selection procedures.

Bandwidth selection procedure:	RD estimate (1)	SE (2)	$\hat{h}$ (3)	Overall N (4)	Effective N (5)
mserd	-0.129**	(0.050)	0.078	80,563	21,613
msetwo	-0.086**	(0.042)	0.145	80,563	31,040
msesum	-0.146***	(0.051)	0.067	80,563	18,364
msecomb1	-0.146***	(0.051)	0.067	80,563	18,364
msecomb2	-0.133***	(0.050)	0.078	80,563	20,370
cerrd	-0.148***	(0.054)	0.059	80,563	16,769
certwo	-0.092**	(0.045)	0.109	80,563	24,003
cersum	-0.155***	(0.058)	0.050	80,563	14,077
cercomb1	-0.155***	(0.058)	0.050	80,563	14,077
cercomb2	-0.144***	(0.054)	0.059	80,563	16,201

Notes: This table reports DPEP's estimated RD effects of DPEP on intergroup conflict using alternative optimal bandwidth selection procedures. Each row corresponds to a different bandwidth selector implemented via the `rdrobust` package. Robust standard errors clustered at the 1991 district level are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A19: Robustness of DPEP's estimated effects on intergroup conflict to covariate adjustment and fuzzy RD estimation.

Estimation:	Reduced form				Fuzzy RD / 2SLS							
					First stage				Second stage			
Outcome:	Intergroup conflict				DPEP funding (crores)				Intergroup conflict			
Bandwidth:	$\hat{h}$		$2\hat{h}$		$\hat{h}$		$2\hat{h}$		$\hat{h}$		$2\hat{h}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment	-0.129**	-0.135***	-0.111**	-0.113**	9.027**	10.837***	9.403**	9.910***				
	(0.050)	(0.050)	(0.046)	(0.047)	(4.196)	(4.154)	(3.809)	(3.829)				
DPEP funding (crores)									-0.011**	-0.009**	-0.008***	-0.009***
									(0.005)	(0.005)	(0.003)	(0.003)
Controls	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Effective *N*	21,613	20,582	45,798	43,541	48,342	40,813	75,387	72,391	48,342	40,813	75,387	72,391
Overall *N*	80,563	80,542	80,563	80,542	80,563	80,542	80,563	80,542	80,563	80,542	80,563	80,542
$\hat{h}$	0.078	0.073	0.157	0.147	0.163	0.134	0.326	0.267	0.163	0.134	0.326	0.267

Notes: This table presents results from reduced-form and fuzzy RD estimates of the effects of DPEP on intergroup conflict. The unit of analysis is the household. Data come from both IHDS waves. The main outcome variable is based on the survey question (asked to the heads of sampled households): "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" Responses are recoded as: "a lot of conflict" (=1), "some conflict" (=0.5), "not much conflict (=0). The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff. Each pair of columns alternates between using the optimal bandwidth $\hat{h}$ and twice the optimal bandwidth $2\hat{h}$, as selected by the MSE-optimal bandwidth procedure implemented in `rdrobust`. Controls include the following covariates: an indicator for any riot between 1950–1991 in the district, household-level caste and religion fixed effects (Forward, OBC, Dalit, Adivasi, Muslim), and an indicator for urban residence. Robust standard errors clustered at the 1991 district level are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A20: Cohorts exposed to DPEP.

		Age of:			
	Year	...first full DPEP cohort	...first partial DPEP cohort	...final DPEP cohort	
DPEP	1994	6	10		
	1995	7	11		
	1996	8	12		
	1997	9	13		
	1998	10	14		
	1999	11	15		
	2000	12	16		
	2001	13	17		
	2002	14	18		
	2003	15	19		
	2004	16	20		
	2005	17	21	6	IHDS 1
	2006	18	22	7	
	2007	19	23	8	
	2008	20	24	9	
	2009	21	25	10	
	2010	22	26	11	
	2011	23	27	12	IHDS 2

Notes: The table reports the ages (by year) of cohorts contemporaneous with the launch of the District Primary Education Programme (DPEP) in 1994. The "first full DPEP cohort" refers to children who were age 6 (typical primary school starting age) at the program's commencement, and thus experienced the entirety of primary school under DPEP, while the "first partial cohort" were age 10 at launch and received only the final part part of their primary schooling under the program. The "final cohort" captures the youngest children who completed primary school before DPEP ended. Shaded rows indicate the ages of these cohorts at the time of the first (main fieldwork 2005) and second (main fieldwork 2011) rounds of the India Human Development Survey (IHDS).

Table A21: Estimated effect of DPEP on intergroup conflict among IHDS respondents in older cohort not directly exposed to DPEP.

Control function:	Linear			Quadratic	Cubic	Quartic
Bandwidth:	$\hat{h}$	$2\hat{h}$	$\hat{h}$			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 3-point outcome measure						
RD estimate	-0.148***	-0.124***	-0.201***	-0.169***	-0.226***	-0.229***
	(0.050)	(0.047)	(0.066)	(0.056)	(0.071)	(0.070)
Overall *N*	71,097	71,097	71,097	71,097	71,097	71,097
Effective *N*	18,878	40,034	17,681	31,229	31,467	46,840
Overall *N* districts	280	280	280	280	280	280
Effective *N* districts	78	164	73	128	129	188
$\hat{h}$	0.077	0.153	0.073	0.116	0.118	0.175
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.214	0.218	0.215	0.229	0.229	0.220
Panel B: Binary outcome measure						
RD estimate	-0.242***	-0.203***	-0.277***	-0.310***	-0.364***	-0.406***
	(0.087)	(0.079)	(0.092)	(0.103)	(0.112)	(0.137)
Overall *N*	71,097	71,097	71,097	71,097	71,097	71,097
Effective *N*	18,368	38,838	25,521	25,143	36,935	40,034
Overall *N* districts	280	280	280	280	280	280
Effective *N* districts	75	158	105	103	150	164
$\hat{h}$	0.074	0.149	0.104	0.099	0.138	0.154
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.358	0.363	0.365	0.365	0.366	0.363

Notes: This table reproduces the analyses from Table 2, restricting the sample only to IHDS respondents who were not themselves (because of their older age) exposed to DPEP. It presents RD estimates of the effect of DPEP on intergroup conflict. The unit of analysis is the household. Data come from both IHDS waves. Outcome variables are based on the survey question (asked to the heads of sampled households): "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" In Panel A, responses are recoded as: "a lot of conflict" (=1), "some conflict" (=0.5), "not much conflict (=0). In Panel B, the binary outcome measure takes 1 if the respondent reported "some" or "a lot of conflict," and zero if they reported "not much conflict." The running variable is a district's female literacy rate in the 1991 Census of India; the inverse of the rate is used such that the RD cutoff entails greater educational access. Coefficients are estimates of the average treatment effect at the cutoff, calculated using a triangular kernel. We employ either the Calonico, Cattaneo, and Titiunik (CCT) or Imbens and Kalyanaraman (IK) optimal bandwidth selection method. Standard errors, clustered by 1991 districts, are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A22: Estimated association between UCDP conflict and IHDS intergroup conflict.

	IHDS: Any Conflict (0/1)			
	(1)	(2)	(3)	(4)
Events: Any	0.135***			
	(0.035)			
Events: Total		0.009**		
		(0.004)		
Fatalities: Any			0.134***	
			(0.036)	
Fatalities: Total				0.002
				(0.002)
Constant	0.339***	0.349***	0.340***	0.351***
	(0.011)	(0.011)	(0.011)	(0.011)
N	80,563	80,563	80,563	80,563

Notes: This table presents results from an analysis examining the relationship between UCDP's conflict data and the IHDS measure of reported conflict in respondents' local communities. The main outcome variable is based on the survey question (asked to the heads of sampled households): "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" Responses are recoded as: "a lot of conflict;; (=1), "some conflict" (=1), "not much conflict" (=0). The explanatory variables are both count and dichotomized measures of conflict, reflecting the incidence of events in a given district or whether there were any civilians fatalities from the event. The UCDP data are subset to "one-sided" violence, in which at least one of the parties was an organization. UCDP data are further subset to those events that took place within two years of the IHDS survey wave. Robust standard errors clustered at the 1991 district level and are reported in the parentheses. *p<0.1; **p<0.05; ***p<0.01.

I Imputing district-level DPEP funding data

Administrative data on district-level DPEP funding allocations are not available. To construct district-level funding estimates capturing the intensity of the DPEP program at the district level, we implement a population-based imputation procedure using state-level DPEP funding data. Data on state-wise DPEP allocations are available from the statistical website *IndiaStat* in comparable format for the years 1998–2003. These data, in turn, are drawn from official Government of India answers to parliamentary questions. The data sources are:

- **1998–2000**: `rb.gy/7cs17v`.
- **2000–2003**: `rb.gy/d1tz9p`.

We first sum the total DPEP allocations by state for 1998–2003. We then merge these state-level DPEP funding totals with our district-level dataset. For each district, we calculate a variable `pop_if_dpep`, defined as the district's 2001 population if and only if it was a DPEP-covered district. Next, for each state, we compute the total DPEP-covered population in 2001, `state_tot_pop_dpep_dists`, by summing `pop_if_dpep` across all DPEP districts within that state. Each DPEP district's share of the received state-level DPEP funding is then estimated proportionally based on its population share:

$$\text{District share} = \frac{\texttt{pop\_if\_dpep}}{\texttt{state\_tot\_pop\_dpep\_dists}}$$

Finally, this population share is multiplied by the total DPEP funding received by the state over 1998–2003, yielding an imputed value for district-level DPEP funding. This simple imputation approach ensures that funding is distributed across districts in a manner consistent with available administrative data and demographic size.