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—effects that persist up to 18 years after the program's launch. Reductions are concentrated in religiously mixed localities with a history of Hindu-Muslim violence. To interpret these results, we develop a framework in which schooling curbs conflict through two primary mechanisms: bias reduction (via intergroup contact and inclusive curricula) and higher opportunity costs (via improved wages and employment prospects). Follow-up evidence favors the bias-reduction channel. 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 ushered in a new era for 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 highlighting 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 fund education and to police

1 Article 2, *World Declaration on Education for All*, shorturl.at/4VicR.

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 global 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 with up to INR 400 million ($\approx$USD 12.5 million) each 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 this probabilistic assignment threshold.

To measure ethnic conflict locally and 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). A suite of other placebo and validation tests suggests that social desirability bias in our survey outcomes is highly unlikely to explain the estimated treatment effects.

Our central finding is that households in districts quasi-randomly assigned to DPEP are 22 percentage points less

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

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 alternative RD specifications, including the use of different bandwidths and control functions. 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 find no statistically significant effects of DPEP on discriminatory behaviors or attitudes toward lower-caste groups. Jointly, these results may suggest that mass education mitigates communal, inter-religious conflict more than caste-based conflict, although caution is warranted since we cannot definitively identify a direct mapping between caste-based discrimination and conflict: education may impact one without necessarily impacting the other.

To unpack mechanisms, we analyze treatment effect heterogeneity and the policy's impact on additional outcomes, marshaling supplementary data sources. Several insights 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, it did not widen group-wise inequality in those indicators or with respect to educational attainment, 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.

Several empirical findings support the idea that DPEP, and its inclusive curriculum especially, reduced intergroup bias in exposed districts. First, we show that DPEP broadened children's access to education programming that may have helped combat the formation of intergroup hatreds. In a descriptive analysis, we find that textbooks from the DPEP period featured "unity in diversity" narratives much more prominently than in previous periods. We also show that schools in DPEP districts fared significantly better on national exams that measure competence in social studies, a subject that includes civics, equality, and respect for human rights. A second normative shift brought on by DPEP was female empowerment—another theme emphasized in the program's instructional content. 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). 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 serve to instigate ethnic conflict. Finally, our evaluation of relational explanations yields more mixed results. We find 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. However, DPEP did increase the share of lower-caste teachers and raised religious fractionalization among teaching staff, indicating changes in the composition of authority figures in schools.

There are 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 solidarity in divided societies. The imperative to overcome entrenched ethnic cleavages is core to recent theories of ethnic growth traps, in which social cleavages, underinvestment in public goods, and violent conflict reinforce one another (Jha, 2025). Some scholars emphasize schooling's 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? A Bias-Wage Threshold Model

We present a simple decision-theoretic framework to capture two major pathways through which schooling can reduce intergroup conflict. 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. We later draw out the model's testable implications.

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 (Wood, 2003);
  - $\gamma > 0$ 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).[3] 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.

3 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). Allport (1954) posited several scope conditions needed for contact to be prejudice-reducing. Schools, by design, tend to embody these, as they involve 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).

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 fortify 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.

Curriculum-based explanations A second route by which schooling may reduce bias is through the intentional shaping of norms and values. 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 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 in the wild.

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 evoking critical reflection on gender and power, schools may play a direct part 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). 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.

Implications

Our model 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. Fourth, the net effect of secular public schooling on bias depends not only on what students learn in state schools, but on what they would have learned instead: expanding public provision may reduce demand for exclusionary private or religious schools, pulling students away from institutions that might otherwise have reinforced outgroup hostility.

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 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). OA Figure A1 illustrates DPEP's wide geographical spread. 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 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 enhance 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 far 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).[4] 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.[5] The results—presented in OA Figure A10—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

Arguably, religion constitutes the central axis of intergroup conflict in India. 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 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

4 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 OA Table A23.

5 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. Online Appendix Table A24 lists the keywords provided for the three prespecified topics.

geared toward mitigating inequality, anti-Dalit prejudice remains pervasive. Caste-based conflict manifests in violent 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.[6] 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.[7]

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). 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 (a) a binary district indicator for DPEP receipt, using data from Roodman (2023), and (b) an imputed measure of district-level funding intensity, distributing state-level totals from 1998–2003 proportionally by population (see OA Section A.4). Summary statistics are given in Online Appendix 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 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 OA Figure A3).

6 Lyla Bavadam, "Dalit blood on village square," *Frontline, The Hindu*, December 2006.
7 R. Arivanantham, "Attack on Dalits was planned: National Commission for SCs," *The Hindu*, November 2012.

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.

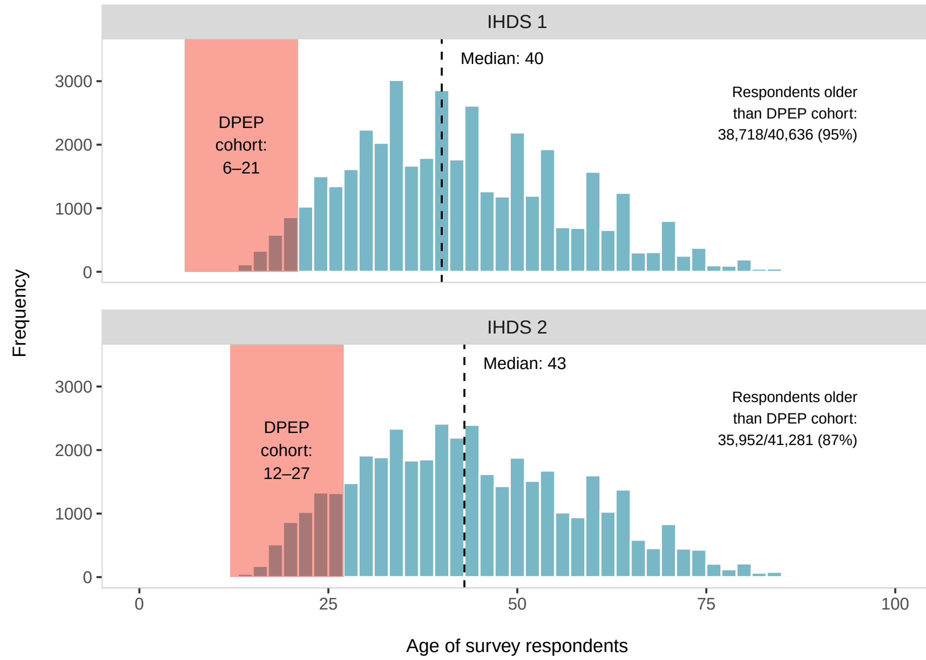
Advantages of perception-based measures of ethnic conflict Reliably measuring the incidence of ethnic conflict is difficult. Common approaches use news reports (e.g., Varshney and Wilkinson, 2006; Barron, Jaffrey and Varshney, 2016; Nellis and Siddiqui, 2018) or police or courts data (e.g., Jassal, 2020).[8] 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 official 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 relating to social desirability bias. We highlight two features of our data the mitigate this concern. First, one 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. However, the IHDS survey was administered to household heads: respondents who, by virtue of their older age, were rarely beneficiaries of DPEP themselves. Figure 1 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, and to dropping respondents from households with any school-aged children during the duration of DPEP's program (OA Table A14). Second, the survey was neither connected to DPEP nor was it primarily focused on intergroup conflict, thus minimizing concerns over researcher demand effects (i.e., the tendency for study participants to adjust their survey responses to align with perceived study goals).

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 per-

8 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 1: 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 in 2004-05 and IHDS-2 in 2011-12. The red shaded areas indicate the age ranges of respondents in each wave who would have been of primary school age during the rollout of the DPEP program, and thus were potentially exposed to it, based on the cohort definitions described in OA Table A19. The vertical dashed lines represent the median age of respondents in each survey wave.

ceptions. As shown in OA Table A3, respondents in districts that experienced at least one violent event in the two years prior to the IHDS waves were 14 percentage points more likely to report "some" or "a lot" of ethnic conflict in their community, a 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 meaningfully captures 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 robust, bias-corrected local linear estimator method proposed by Calonico, Cattaneo and Titiunik (2015), which has been established as a best-practice in applying the RD design (Cattaneo, Idrobo and Titiunik, 2019). 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 OA Section B.1.

Mapping theory to empirical tests Before proceeding to the results, we clarify the link between our theoretical framework and empirical approach. Our theoretical model generates empirically distinct predictions regarding mechanisms, which we evaluate using a range of supplementary data sources—including textbook archives, national exam records, and additional survey modules—described in detail as each test is introduced. To centralize our expectations regarding each component analysis, Table 1 ties each prediction from the model to its corresponding empirical tests and hypothesized effects.

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 analyses strongly support the notion that DPEP allocation changed discontinuously

Table 1: Mapping theory to empirical tests.

Mechanism	Theoretical claim	Empirical tests
Net effect	[1] Conflict occurs when $b_i > T_i \equiv \frac{w_i - \theta + c}{\gamma + \delta}$; schooling reduces conflict by lowering bias b_i or raising opportunity costs w_i.	• RD: DPEP $\rightarrow$ lower intergroup conflict
Relational channel	[2] Intergroup contact in school lowers b_i, reducing the likelihood that $b_i > T_i$.	• RD: DPEP $\rightarrow$ more adult outgroup acquaintances • RD: DPEP $\rightarrow$ improved school climate for intergroup interaction • RD: DPEP $\rightarrow$ greater exposure to outgroup teachers
Curriculum channel	[3a] Inclusive curriculum lowers b_i, reducing the likelihood that $b_i > T_i$. [3b] Patriarchal norms link women's autonomy to group honor, making perceived violations flashpoints for conflict; schooling weakens this link.	• Text analysis: post-DPEP textbooks emphasize cross-group inclusion • RD: DPEP $\rightarrow$ higher social-studies pass rates, esp. on diversity modules • RD: DPEP $\rightarrow$ improved gender norms, increased women's civic participation, reduced harassment
Opportunity-cost channel	[4] Schooling raises w_i, increasing the cost of conflict and raising T_i.	• RD: DPEP $\rightarrow$ higher household income, consumption, and employment • RD: DPEP $\rightarrow$ higher economic activity
Crowding-out effect	[5] Mandated expansion of secular public schooling may reduce demand for religious/exclusionary private alternatives.	• RD and event study: DPEP $\rightarrow$ fewer Vidya Bharati schools constructed locally
Differential-returns effect	[6] Returns to schooling shift T_i unevenly across groups, sustaining conflict among those with weaker gains.	• RD: DPEP $\rightarrow$ widening group-wise gap in educational attainment

around the assignment cutoff. OA Figure A4 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 OA Table A9).

Next, in OA Table A4, 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 by 7.8 percentage points—the cohort most likely to be affected by the program. OA Table A4 also shows that DPEP had a marginally statistically significant positive effect on reading skills, with an RD estimate of 0.233 ($p < 0.1$, Column 3). The estimated impact on reading scores is larger for girls (0.365 points) than for boys (0.142 points), suggesting possibly literacy gains among girls. Finally, we estimate the effect of DPEP assignment on primary school construction using census data from Asher et al. (2021). In OA Figure A7, we show that 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.

Net effect on conflict incidence The paper's main results are presented in Table 2. We estimate the net 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. (We offer this as a test of Claim 1 in Table 1.) 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 22 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. Figure 2 visualizes the same relationship, lending graphical support to the estimated discontinuity.[9]

Our outcome measure combines communal (religious) and caste-based conflict into a single survey item, so we cannot directly distinguish between them. To shed suggestive light on which axis drives the main result, we examine supplementary outcomes and heterogeneity analyses. On the caste side, we find no detectable effects of DPEP on the prevalence of activities thought to constitute caste-based discrimination, such as manual scavenging (from the Census of India and Socio-Economic and Caste Census) or the self-reported practice of untouchability (from IHDS), though we interpret these null effects cautiously: such measures may not fully capture caste-based conflict, and discriminatory practices may persist even as overt conflict declines (OA Table A25). On the religious side, heterogeneity tests comparing areas with and without Muslim residents, and districts with and without a history of Hindu-Muslim violence (Varshney and Wilkinson, 2006), reveal that DPEP's conflict-reducing effects are concentrated in settings where communal tensions are more likely to arise. Together, the evidence tentatively suggests that the main result is driven at least partly by reductions in religious rather than caste-based conflict.[10]

9 See also OA Figure A12 for visualizations using the global sample.

10 In OA Table A27, we show that the effects of DPEP on conflict reduction are larger in areas with lower, rather than higher Hindu nationalist vote share historically, although the difference in estimates is itself not statistically significant.

Table 2: Estimated effect of DPEP on intergroup conflict.

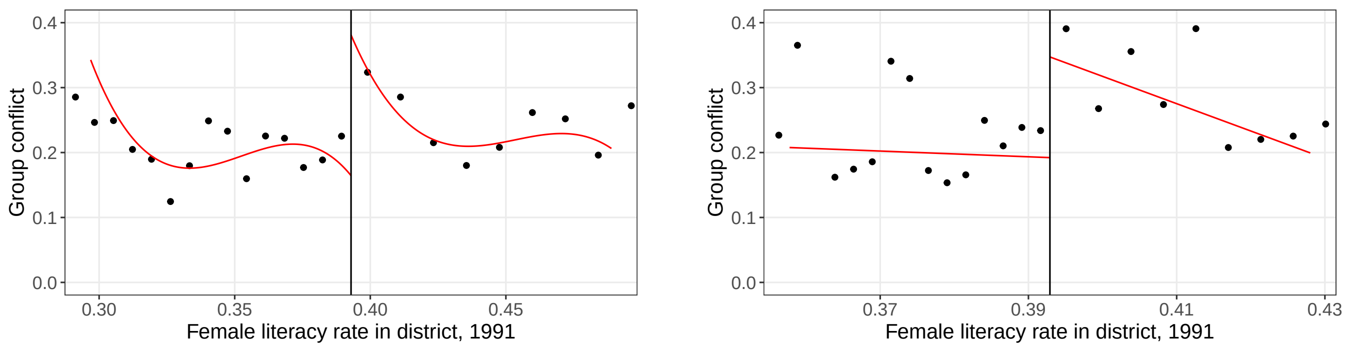
Control function:	Linear			Quadratic	Cubic	Quartic
Bandwidth:	$\hat{h}$	$2\hat{h}$		$\hat{h}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 3-point outcome measure						
RD estimate	-0.129**	-0.111**	-0.142***	-0.138**	-0.190***	-0.181***
	(0.050)	(0.046)	(0.052)	(0.056)	(0.069)	(0.070)
Overall *N*	81,945	81,945	81,945	81,945	81,945	81,945
Effective *N*	21,613	45,873	32,678	37,873	39,558	54,995
Overall *N* districts	297	297	297	297	297	297
Effective *N* districts	83	173	120	142	148	203
$\hat{h}$	0.078	0.156	0.108	0.123	0.126	0.183
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.216	0.219	0.227	0.224	0.224	0.222
Panel B: Binary outcome measure						
RD estimate	-0.215***	-0.178**	-0.231***	-0.262***	-0.287***	-0.327***
	(0.083)	(0.075)	(0.086)	(0.098)	(0.106)	(0.124)
Overall *N*	81,945	81,945	81,945	81,945	81,945	81,945
Effective *N*	20,585	43,720	32,108	28,845	43,624	48,932
Overall *N* districts	297	297	297	297	297	297
Effective *N* districts	78	164	117	109	163	183
$\hat{h}$	0.074	0.148	0.107	0.103	0.144	0.164
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.360	0.365	0.374	0.366	0.365	0.366

Notes: RD estimates of DPEP's effect on intergroup conflict. The unit of analysis is the household; data come from both IHDS waves. Outcomes are based on the survey question (asked of household heads): "In this village/neighborhood, how much conflict would you say there is among the communities/jatis that live here?" Panel A recodes responses as: "a lot of conflict"=1, "some conflict"=0.5, "not much conflict"=0. Panel B's binary outcome takes 1 if the respondent reported "some" or "a lot of conflict," and 0 otherwise. The running variable is the reverse of a district's female literacy rate in the 1991 Census, such that the RD cutoff entails greater educational access. Coefficients are average treatment effects at the cutoff, estimated via local linear regression with a triangular kernel, using either the Calonico, Cattaneo, and Titiunik (CCT) or Imbens and Kalyanaraman (IK) optimal bandwidth. Standard errors clustered by 1991 districts in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Further robustness OA Figure A6 presents a placebo test using falsified cutoff points: only the estimate at the true cutoff yields a statistically significant negative effect, while placebo estimates are small and indistinguishable from zero. Results are robust to all other optimal bandwidth selection procedures available in `rdrobust` (OA Table A11), to a range of manually-selected bandwidths (OA Figure A8), and to employing exactly half the optimal bandwidth (OA Table A12). The results are also robust to the inclusion of control variables (OA Table A17). OA Table A16 confirms the results under a randomization-inference approach in the immediate vicinity of the cut point. The findings are not driven by outlier districts, as shown by the drop-one-out analysis in OA Figure A9, nor by the small number of multi-parent districts in the sample (OA Table A21). OA Table A20 shows the results are unaffected by dropping younger respondents who may themselves have been partial DPEP beneficiaries. Finally, OA Tables

Figure 2: RD plot of the relationship between DPEP eligibility and intergroup conflict.

(a) Cubic specification. (b) Linear specification.



Notes: Reduced form relationship between districts' female literacy rate and the 3-point group conflict measure. The solid vertical bar at 0.39 marks the DPEP eligibility threshold (units to the left were eligible). The gap between fitted functions at this threshold approximates the RD treatment effect. Figures 2a and 2b use cubic and linear forms, respectively. Points are binned averages.

A6 and A7 show no differences in migration patterns between DPEP-exposed and unexposed districts.

We recover the local average treatment effect among compliers via a fuzzy RD framework, instrumenting DPEP funding with the eligibility cutoff. Results are reported in OA Table A17. The estimates indicate a statistically significant reduction in intergroup conflict among compliers: an additional 1 crore (in 1998 INR) of DPEP funding reduces conflict by approximately 1 percentage point, consistent in direction and significance with the intent-to-treat estimates.

We conduct two further exercises to probe social desirability bias. First, we drop all respondents from households containing members of any age cohort potentially exposed to DPEP—addressing not only direct exposure but also the possibility that children's internalization of inclusive norms spilled over to shape parents' conflict perceptions. Results are robust to this more restrictive sample (OA Table A14). Second, we examine heterogeneity by whether a respondent was observed by an outsider during their interview, on the presumption that such presence may activate social desirability concerns; OA Table A13 uncovers no such variation. These tests provide additional corroboration that social desirability bias is unlikely to lie behind our estimates.

Mechanisms

Testing curriculum-based explanations Did DPEP reduce bias by advancing civic norms emphasizing inclusive citizenship and human rights for India's diverse population, in accord with Claim 3a from Table 1? We earlier demonstrated descriptively that primary school textbooks from the DPEP reform period showed nearly double the coverage of gender inclusivity, religious diversity, and caste discrimination compared to pre-reform materials. We now look at whether DPEP causally 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). The 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 as before, with the district as the unit of analysis, which is the level at which the exam data are reported.

Table 3 presents the results. Among all districts, Column 1 shows a positive estimated impact 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 estimated effect on scores for diversity-relevant subtopics is positive but not statistically significant (Column 2). 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)—statistically significantly so. 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 more 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	4.736*	4.658	11.919**	12.761**	1.127	0.002
	(2.787)	(3.064)	(5.590)	(5.950)	(2.773)	(2.750)
Effective *N*	156	151	73	69	158	140
Overall *N*	615	615	310	310	305	305
$\hat{h}$	0.068	0.066	0.063	0.061	0.129	0.120
K clusters	451	451	220	220	231	231
Outcome mean	43.823	40.028	44.583	41.202	41.666	37.212

Notes: RD estimates of DPEP's effect on the proportion of students passing a standardized social studies exam. Odd columns cover overall performance; even columns focus on modules relating to democracy, equality, and human rights. The unit of analysis is the 2011 district. The running variable is the reverse of a district's female literacy rate in the 1991 Census, such that the RD cutoff entails greater educational access. Coefficients are average treatment effects at the cutoff, estimated via local linear regression with a triangular kernel and MSE-optimal bandwidth. Standard errors clustered by 1991 districts 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 (Claim 3b). DPEP's socially conscious curriculum may have curtailed conflict by eroding the patriarchal ideologies that help nourish 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 channel for two 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 0.11-point decline on the 1–3 scale in reported instances of gender-based harassment of girls, an effect that is marginally statistically significant (Column 2). We do not detect reductions in the share of women who report that it is typical for husbands to beat their wives for leaving the house, neglecting the upkeep of the house, or cooking badly (Columns 3, 4, and 5).

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 harassed locally	Leave house	Neglect house	Cook badly
	(1)	(2)	(3)	(4)	(5)
RD estimate	0.097**	-0.112*	-0.120	-0.135	-0.073
	(0.044)	(0.065)	(0.079)	(0.110)	(0.075)
Effective N	22,448	23,625	35,710	22,424	31,132
Overall N	82,146	81,897	71,647	71,659	71,621
$\hat{h}$	0.083	0.086	0.130	0.092	0.119
K clusters	297	297	297	297	297
Outcome mean	0.052	1.23	0.523	0.448	0.334

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

Testing relational explanations We next test the relational explanations posited by our theoretical framework (Claim 2). We summarize a collection of tests in Figure 3 that look across three venues and audiences of intergroup contact: adults' acquaintances, parents' reports of students' experiences in school, and teachers' demographics at government primary schools. To start, 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. 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.[11] 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 (rows i and ii).

We also evaluate whether DPEP encouraged positive intergroup contact in the classroom. We rely on the first round of IHDS conducted in 2004-05, years in which DPEP was still in force and thus any effects on the classroom environment should be apparent. 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 (Figure 3, rows iii and iv).

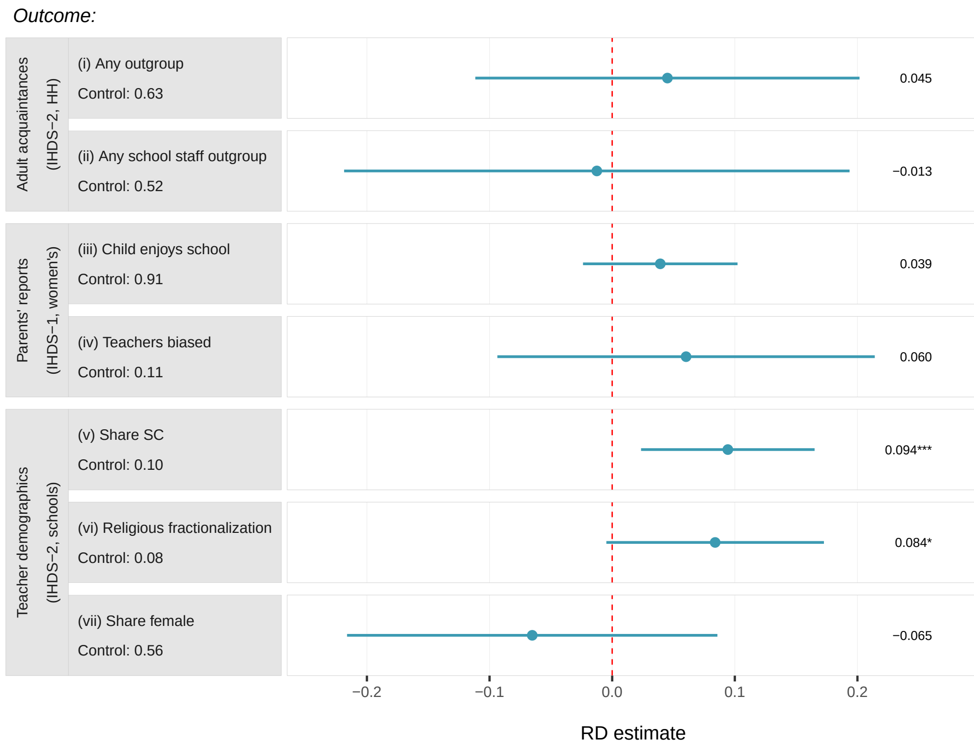
Our third set of contact-relevant outcomes home in on the demographic composition of teachers in government primary schools, harnessing the fact that IHDS-2 published detailed staff rosters for approximately one school per primary sampling unit. We look at three measures: the share of teachers from Scheduled Castes, a religious fractionalization (Herfindahl) index capturing diversity among school staff, and the share of female teachers. Applying our benchmark RD specification, we find that DPEP increased the share of Scheduled Caste teachers by 9.8 percentage points and detect a near-doubling in the religious fractionalization of teachers in DPEP-exposed schools—though the latter effect falls just short of conventional levels of statistical significance. The share of female teachers, however, shows no measurable increase. On balance, we interpret these findings as suggestive of a role-model-based relational mechanism: students in DPEP-exposed schools were substantially more likely to encounter teachers from outgroups, an experience that may have gradually helped undercut the intergroup antipathies that can seed long-run conflict.

Testing opportunity-cost explanations It is important to know whether the reduction in conflict can be partially explained by improved economic conditions, owing to a local boost in human capital caused by DPEP. 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 (Claim 4). 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 (OA Table A22).

We also ask whether DPEP stimulated local macroeconomic growth, potentially increasing economic abundance

11 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.

Figure 3: Estimated effect of DPEP on intergroup contact and school climate.



Notes: Outcomes are as follows: (i) an indicator for having any acquaintance from outside one's caste/community across occupations (doctor, health worker, government officer, other government employee, elected politician, political party official, inspector, other police, military, teacher, or other school worker); (ii) an indicator for such acquaintance with a teacher or other school worker; (iii) parent report that the child enjoys school; (iv) parent report that teachers favor certain communities/jatis; (v)–(vii) are school-level measures, specifically: (v) share of teachers (including head and para-teachers) who are Scheduled Caste; (vi) Herfindahl index of religious fractionalization among teachers; (vii) share of female teachers. The running variable is the reverse of a district's female literacy rate in the 1991 Census, such that the RD cutoff entails greater educational access. The coefficient is the average treatment effect at the cutoff, estimated via local linear regression with a triangular kernel and MSE-optimal bandwidth. Standard errors clustered by 1991 districts in parentheses. *p<0.1; **p<0.05; ***p<0.01.

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 (OA Table A22). As such, there is little support for the hypothesis that DPEP reduced conflict by improving individual or community-level material conditions.

Testing alternative explanations DPEP could have altered the composition of school types within treated districts (Claim 5) by crowding out religious schools that promote socially divisive ideas. To assess this, we examine DPEP's impact on the construction of Vidya Bharati (VB) schools, a nationwide network affiliated with the Rashtriya Swayamsevak Sangh that critics and supporters alike have associated with Hindu nationalism (Iwanek, 2022). We find no evidence of such crowding out. On the contrary, RD estimates 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 (OA Table A26), a pattern repeated in an event study across all districts (OA Figure A13). Parsing whether VB deliberately expanded its school system in response to DPEP's secular investments, or whether DPEP funds were partly diverted toward VB school construction, is beyond the scope of this paper. Either way, the proliferation of Hindu nationalist schools would have made DPEP's goals more challenging to achieve—making our core findings all the more striking.

Lastly, Claim 6 holds that if access to education through DPEP differed across groups, it could have widened intergroup inequality in educational attainment and thereby intensified intergroup competition (Alcorta, Smits and Swedlund, 2018; Dunlop, 2021). We test this possibility in OA Table A15 by examining educational gaps in attendance and years of education across both Hindu-Muslim and upper caste-SC/ST cleavages. The analysis provides no support for this contention: the gaps in school attendance and years of education are small and statistically indistinguishable from zero, suggesting that DPEP neither narrowed nor exacerbated educational inequality across these group lines.

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 marginalized 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).

Much remains to be explored in 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 granular measures that distinguish among different modalities of conflict. Researchers might also examine whether expanded access to secondary or post-secondary education produces analogous 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 consequential for suppressing group conflict. Last, our null results regarding caste-based discrimination suggest a greater resistance to change in this domain, echoing prior research highlighting the entrenched nature of caste norms in India (Anderson, 2011; Jodhka and Naudet, 2023). These findings point to the need for research into policy instruments capable of addressing this seemingly more intractable dimension of social exclusion.

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Online Appendix

A Preliminaries and data description

A.1 Descriptive statistics

Table A1: Summary statistics.

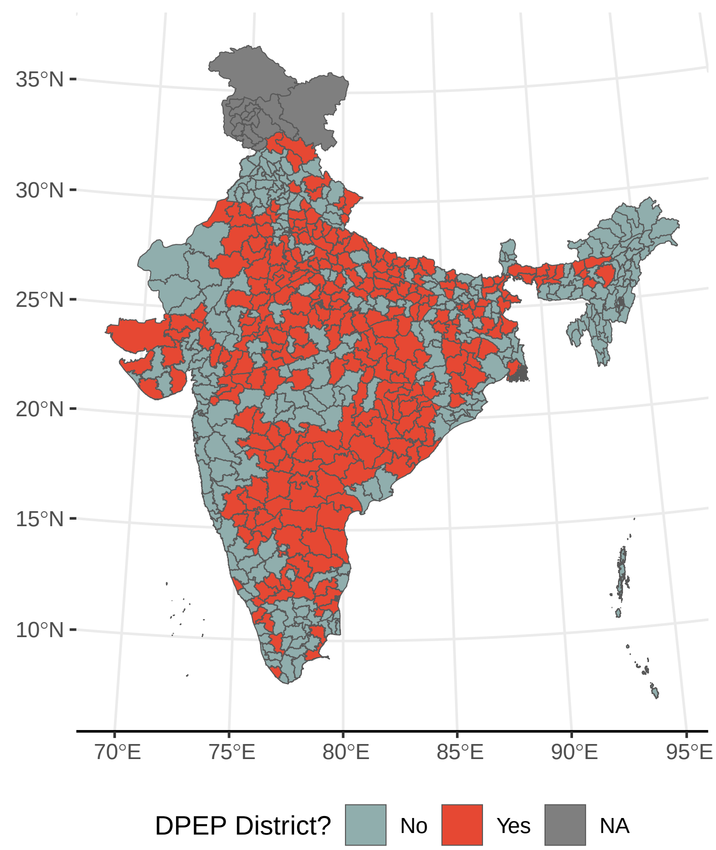
Measure	N	Mean	St. Dev.	Min.	Median	Max.
IHDS individual-level survey round 1						
Ever attended school, younger cohort (0/1)	78740	0.68	0.46	0	1	1
Ever attended school, older cohort (0/1)	128681	0.65	0.48	0	1	1
Math standardized test score (0-3)	12381	1.58	1.03	0	2	3
Reading standardized test score (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.

A.2 Geographic map of DPEP coverage

Figure A1: DPEP's geographic coverage.



Notes: This figure shows the districts assigned to receive DPEP funding (red) or not (gray) according to India's 2001 district boundaries. The dark gray districts in Jammu and Kashmir are excluded from the sample: they were not covered by the 1991 Census of India due to regional instability.

A.3 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 A2 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 A21 demonstrates that excluding those districts does not alter the main RD results, either substantively or statistically.

Table A2: 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.

A.4 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.

A.5 Validating IHDS conflict measure with UCDP data

Table A3: Estimated association between UCDP conflict and IHDS intergroup conflict.

	IHDS: any conflict (0/1)							
	All UCDP violent events				One-sided violent events			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Events: Any	0.100***				0.136***			
	(0.028)				(0.035)			
Events: Total		0.004**				0.009**		
		(0.002)				(0.004)		
Fatalities: Any			0.134***				0.134***	
			(0.036)				(0.036)	
Fatalities: Total				0.002				0.002
				(0.002)				(0.002)
Constant	0.334***	0.348***	0.339***	0.350***	0.339***	0.348***	0.339***	0.350***
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
N	81,945	81,945	81,945	81,945	81,945	81,945	81,945	81,945

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.

B Research design validation and robustness

B.1 Regression discontinuity design validation

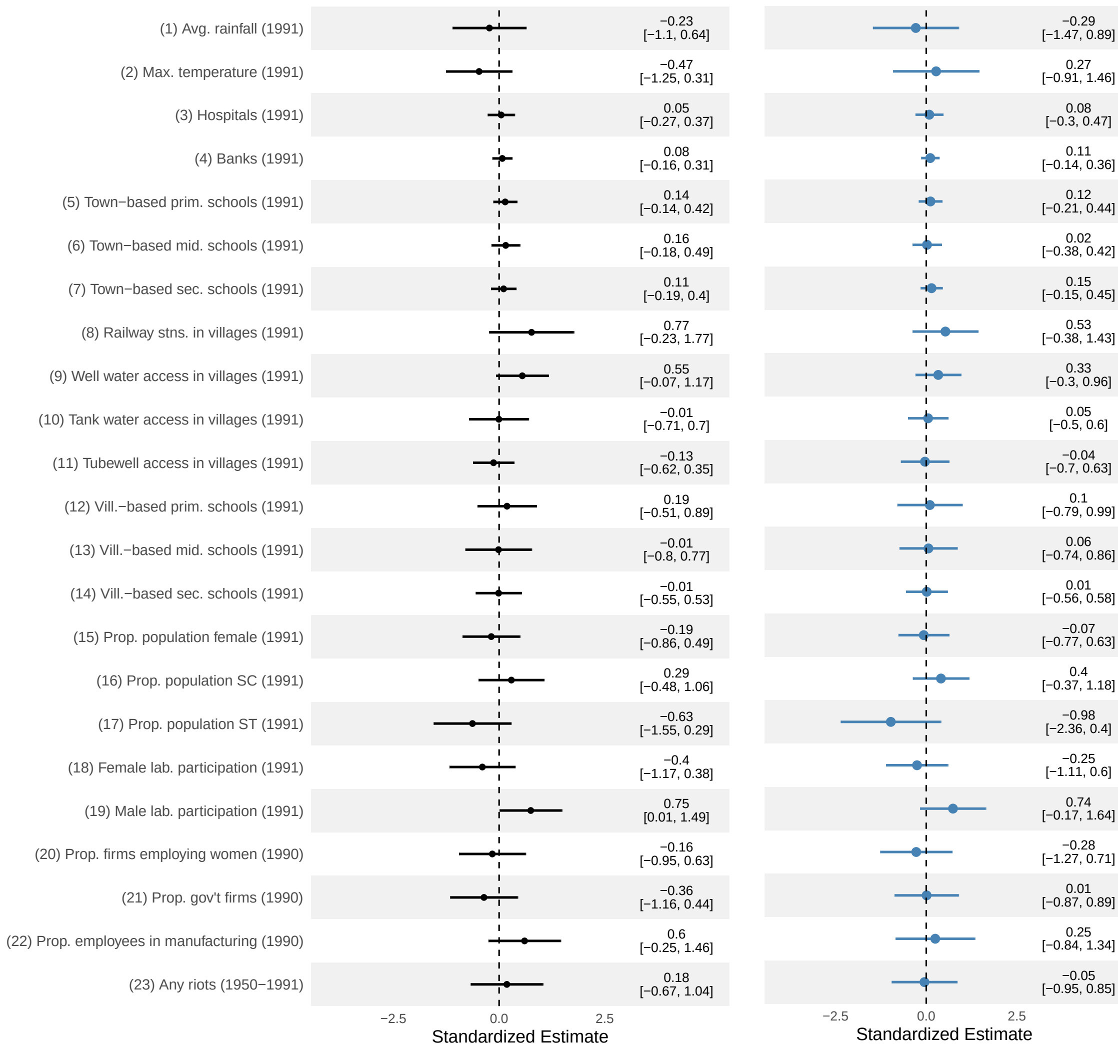
Validating RD assumptions Our RD framework rests on several assumptions necessary for a causal interpretation of our estimates. Chief among them is that districts near the DPEP eligibility threshold should be similar in their underlying characteristics. We validate this with balance tests, comparing 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 A5) or using a formal statistical density test (see Online Appendix Table A5).[1]

1 We further validate our design by conducting power analyses (Stommes, Aronow and Sävje, 2023) in Online Appendix Table A8, and placebo tests in Online Appendix Table A10.

B.1.1 Balance tests

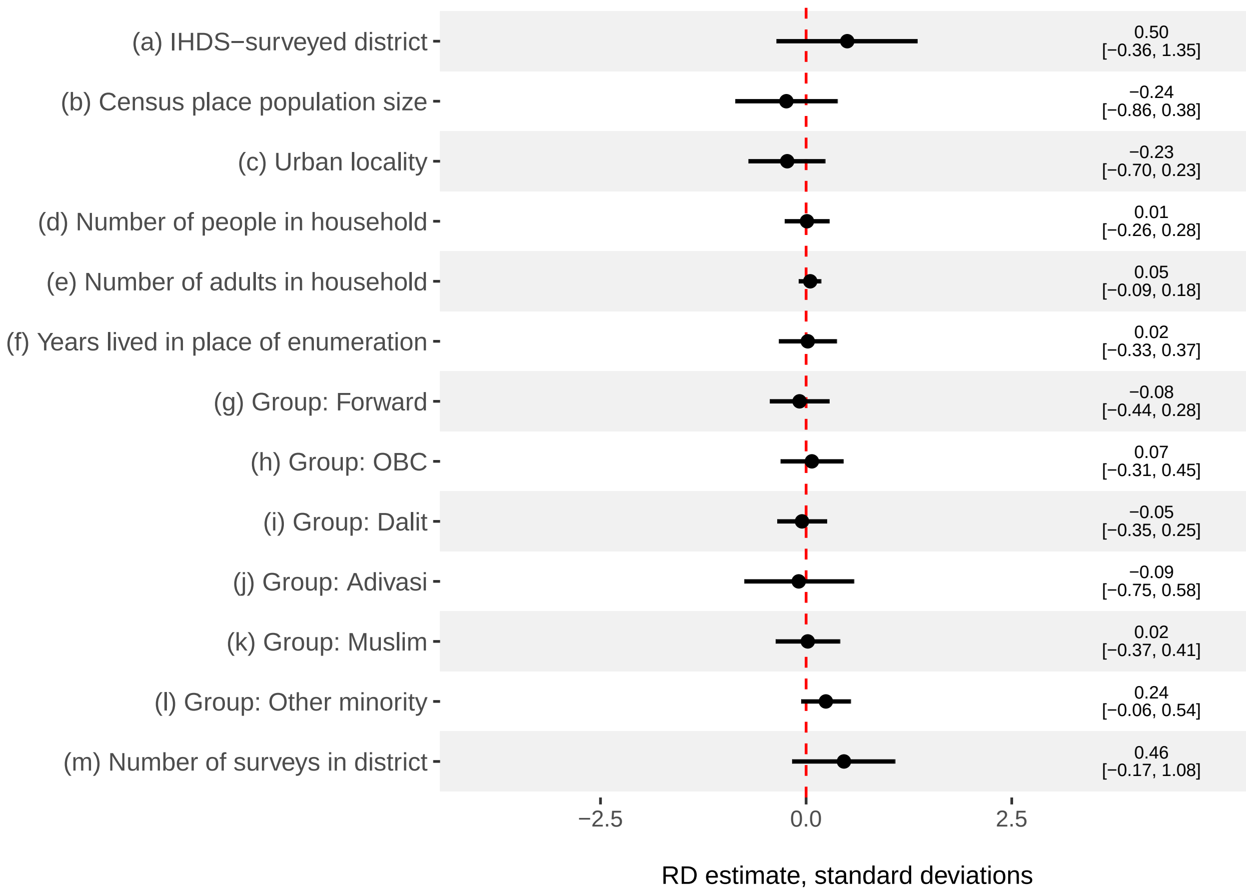
Figure A2: Pre-treatment covariate balance tests.

(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 variable is reversed so that districts above the cutoff are those eligible for DPEP. Coefficients represent the average treatment effect at the cutoff, estimated using local linear regression with a triangular kernel and MSE-optimal bandwidth selection. 95% confidence intervals are based on bias-corrected estimates with cluster-adjusted standard errors, clustered by 1991 districts, as implemented in `rdrobust`. All coefficients and confidence intervals are expressed in standardized units, using the standard deviation of the outcome among observations below the cutoff.

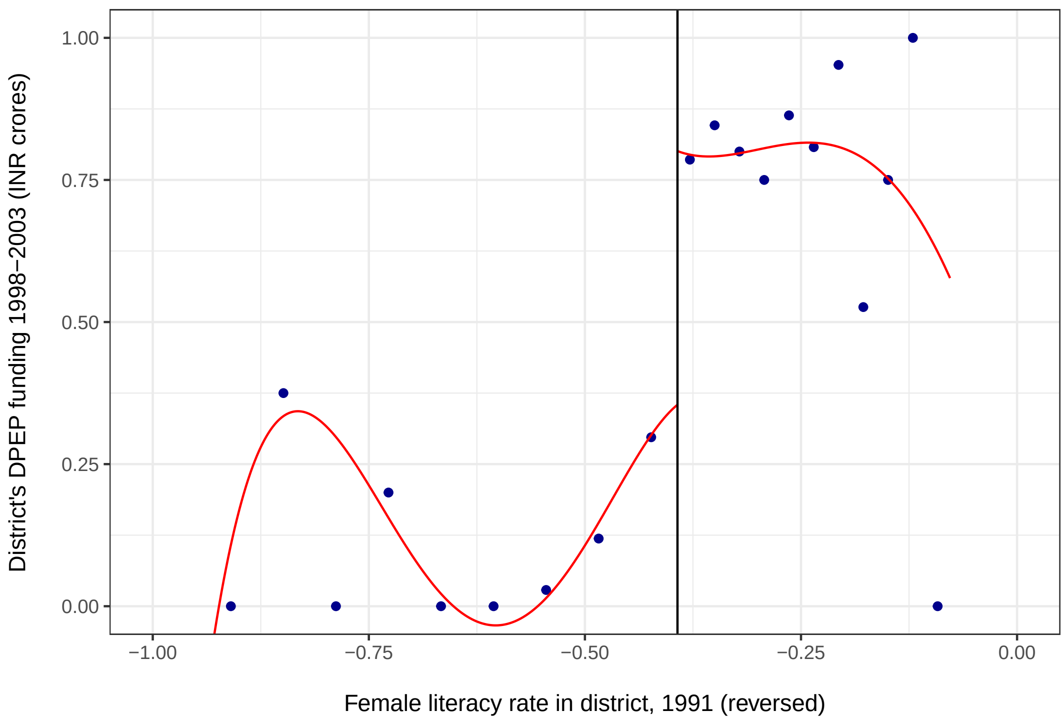
B.1.2 First stage estimation

Table A4: 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	0.078**	0.030	0.233*	0.105	0.040
	(0.032)	(0.060)	(0.138)	(0.157)	(0.054)
Effective *N*	23,618	47,894	3,922	4,035	2,803
Overall *N*	79,483	129,935	12,203	12,155	12,100
$\hat{h}$	0.089	0.105	0.095	0.098	0.072
K clusters	297	297	297	297	297
Outcome mean	0.66	0.623	2.599	1.581	0.781
Panel B: Male					
RD estimate	0.080***	0.014	0.142	0.148	0.086
	(0.029)	(0.050)	(0.146)	(0.159)	(0.058)
Effective *N*	11,377	27,539	2,321	2,092	1,606
Overall *N*	41,365	65,336	6,475	6,447	6,403
$\hat{h}$	0.085	0.114	0.103	0.094	0.075
K clusters	297	297	294	294	294
Outcome mean	0.669	0.742	2.638	1.574	0.749
Panel C: Female					
RD estimate	0.075*	0.050	0.365**	0.073	-0.006
	(0.044)	(0.073)	(0.185)	(0.178)	(0.063)
Effective *N*	12,489	21,470	1,511	1,875	1,285
Overall *N*	38,118	64,599	5,728	5,708	5,697
$\hat{h}$	0.098	0.098	0.084	0.098	0.071
K clusters	297	297	295	295	295
Outcome mean	0.652	0.5	2.55	1.58	0.807

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 reverse 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.

Figure A4: DPEP funding by 1991 female literacy rate for the IHDS estimation sample.

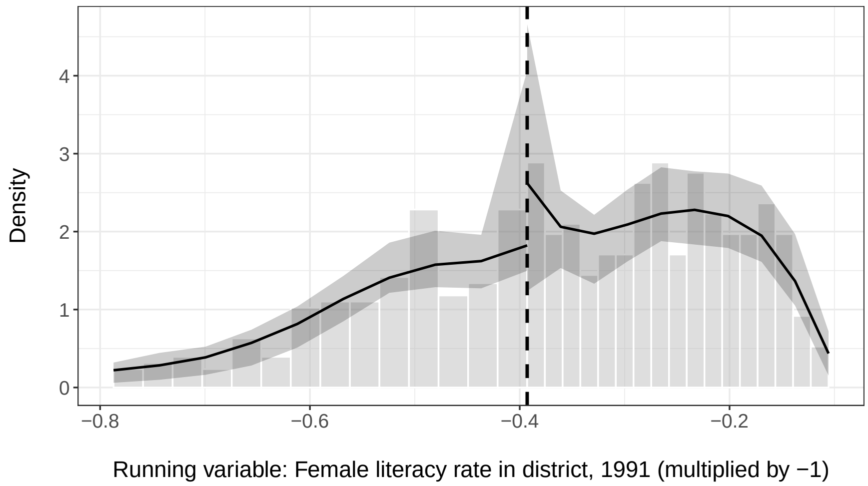


Notes: This figure displays DPEP funding in a district as a function of 1991 female literacy rate, estimated using `rdplot` (estimation bins and polynomials are chosen automatically using `rdplot` defaults). District-level DPEP funding is imputed using the method described in Online Appendix Section A.4. 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.

B.1.3 First stage visualization

B.1.4 Density tests

Figure A5: 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 on either side of the treatment assignment threshold. The unit of analysis is the 1991 district. The running variable is a district's female literacy rate in the 1991 Census of India; the variable is reversed so that units above the RD cutoff are more likely to receive treatment. The figure tests for discontinuities in the density of the running variable at the cutoff using the local polynomial estimator of Cattaneo, Jansson and Ma (2020). Confidence intervals are shown around the estimated densities.

Table A5: Density test estimates (`rddensity`).

	Estimates
	(1)
Density (left of cut point)	2.781
Density (right of cut point)	2.950
Density difference	0.169
t-statistic (density difference)	0.150
p-value (density difference)	0.881

Notes: This table reports the results of a density test (Cattaneo, Jansson and Ma, 2020) for discontinuities in the distribution of the running variable at the regression discontinuity cutoff in district female literacy rates based on the 1991 Census of India. The running variable is a district's female literacy rate in the 1991 Census; the variable is reversed so that observations above the cutoff are more likely to receive treatment. The estimated test statistic and corresponding p-value provide no evidence of a discontinuity in the density of the running variable at the cutoff.

B.1.5 Migration and sorting tests

Table A6: IHDS residence tenure balance.

	Years in place	Recent migrant (0/1)
	(1)	(2)
RD estimate	-1.138	0.030
	(8.564)	(0.029)
Effective N	13,282	16,374
Overall N	38,854	38,854
$\hat{h}$	0.097	0.112
K clusters	280	280
Outcome mean	70.183	0.057

Notes: This table displays the results of balance tests to detect immigration sorting as a function of the DPEP program. In the first column, we look at a question from IHDS in which respondents were asked "How many years ago did your family first come to this village/town/city?" Approximately 71% of respondents report that their families have been in their current village/town/city for 90 years, which is the maximum value allowed (hence the high outcome mean in column (1))). The second column adopts a more sensitive measurement that binarizes whether a respondent has lived in the location for less than ten years. 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.

Table A7: Migration and population sorting balance checks.

	1991–2001 changes				1991–2011 changes				Pre-treatment
	Log pop. growth	Δ Young share	Δ Worker share	Δ Female share	Log pop. growth	Δ Young share	Δ Worker share	Δ Female share	Urban share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
RD estimate	0.014	0.003	-0.003	0.002	-0.035	-0.001	0.001	0.002	0.018
	(0.133)	(0.004)	(0.013)	(0.002)	(0.134)	(0.006)	(0.014)	(0.003)	(0.071)
Effective N	145	173	214	142	190	163	193	142	71
Overall N	450	450	450	450	450	450	450	450	246
$\hat{h}$	0.088	0.105	0.122	0.086	0.111	0.099	0.113	0.086	0.100
K clusters	450	450	450	450	450	450	450	450	246
Outcome mean	0.007	-0.031	0.02	0.001	0.116	-0.049	0.029	0.004	0.194

Notes: This table presents RD estimates of the effect of DPEP on migration and population sorting outcomes. Columns 1–4 report estimates on changes in district-level demographic characteristics between the 1991 and 2001 Population Censuses of India: log population growth (Column 1), change in the share of the population aged 0–6 (Column 2), change in the share of the population classified as workers (Column 3), and change in the female population share (Column 4). Columns 5–8 report the corresponding changes between 1991 and 2011. Column 9 reports the RD estimate on the pre-treatment urban population share in 1991 as a placebo check. Census data are drawn from the Socioeconomic High-resolution Rural-Urban Geographic (SHRUG) platform (Asher et al., 2021). 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 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.

B.1.6 RD power results

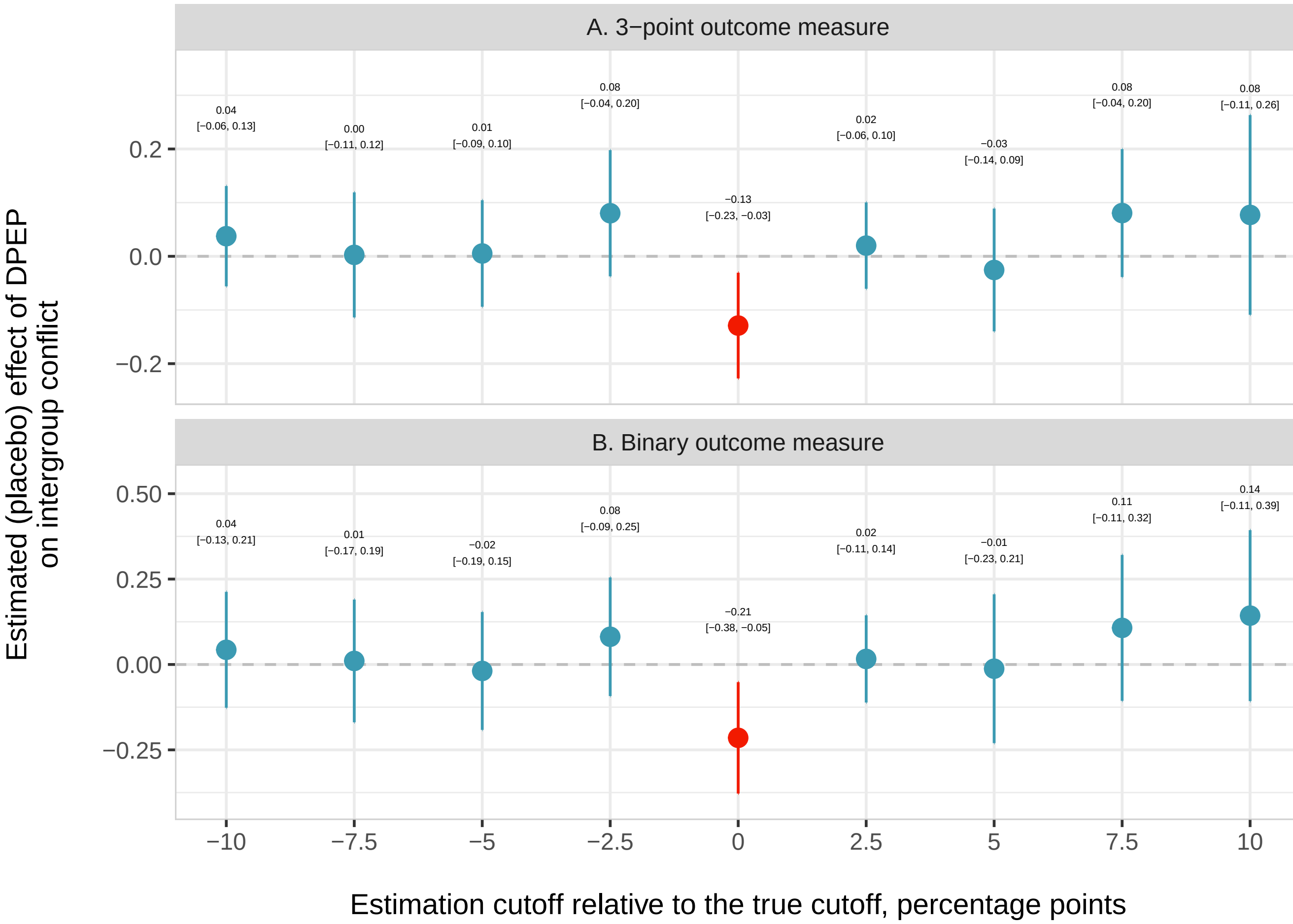
Table A8: Power analysis results (rdpower).

	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.

B.1.7 Placebo cutpoints

Figure A6: 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 (clustered by 1991 districts).

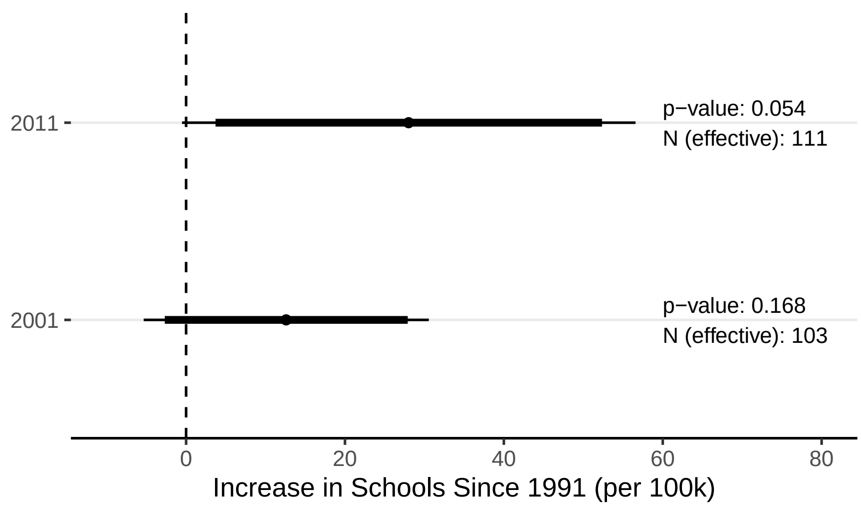
B.1.8 Supplementary first stage tests

Table A9: 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**	11.837***
	(0.205)	(3.966)
Effective N	130	205
Overall N	375	375
$\hat{h}$	0.099	0.147
K clusters	297	297
Outcome mean	0.354	5.522

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 OA Section A.4. 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 reverse 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 A7: 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).

B.1.9 Evidence against stacked discontinuities and policy confounding

Table A10: 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.

B.2 Robustness

B.2.1 Alternative Bandwidths

Table A11: 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	81,945	21,613
msetwo	-0.087**	(0.042)	0.141	81,945	31,115
msesum	-0.146***	(0.051)	0.067	81,945	18,364
msecomb1	-0.146***	(0.051)	0.067	81,945	18,364
msecomb2	-0.133***	(0.051)	0.078	81,945	20,370
cerrd	-0.148***	(0.054)	0.059	81,945	16,769
certwo	-0.095**	(0.046)	0.106	81,945	22,486
cersum	-0.155***	(0.058)	0.050	81,945	14,077
cercomb1	-0.155***	(0.058)	0.050	81,945	14,077
cercomb2	-0.145***	(0.054)	0.059	81,945	16,201

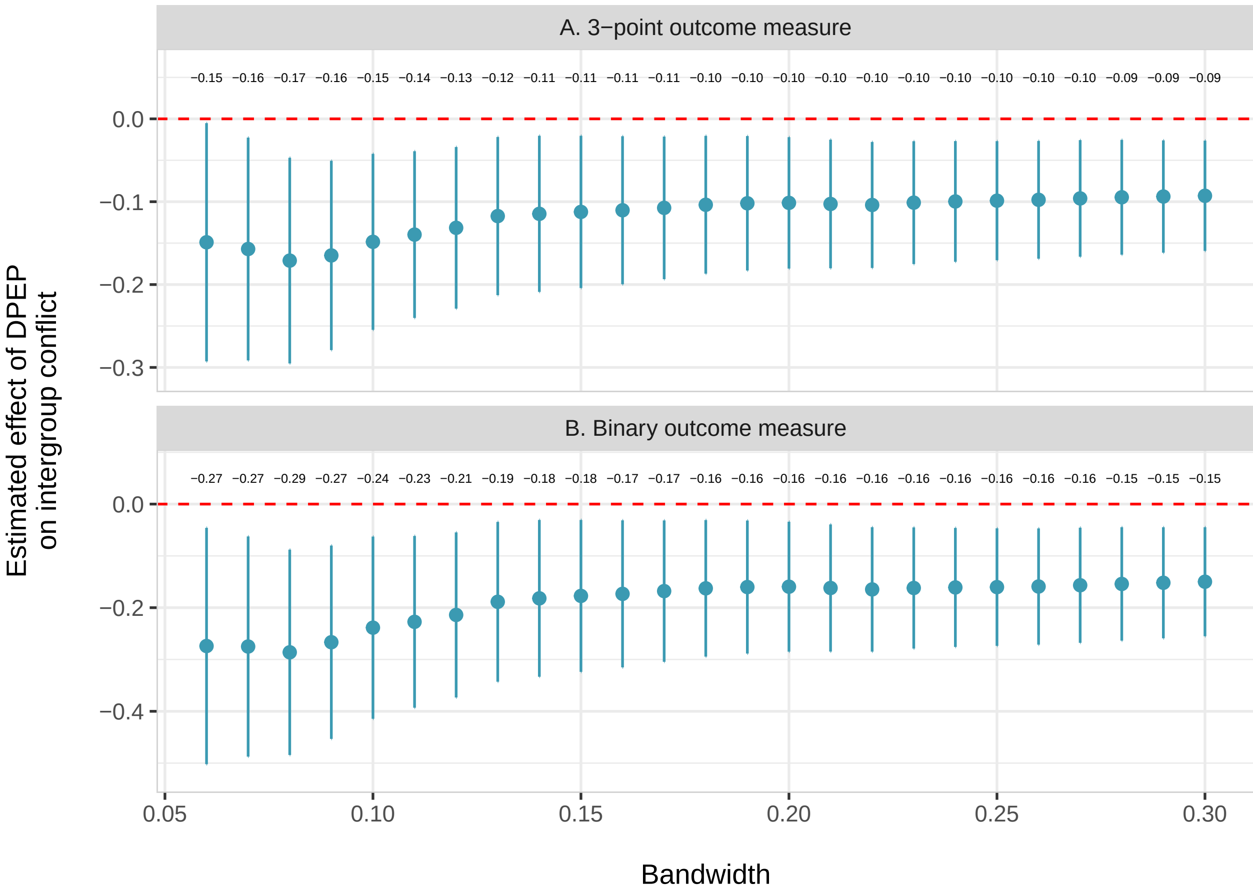
Notes: This table reports DPEP's estimated RD effects of DPEP on intergroup conflict using alternative optimal bandwidth selection procedures. Each row corresponds to a different bandwidth selector implemented via the `rdrobust` package. Robust standard errors clustered at the 1991 district level are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

Table A12: Estimated effects of DPEP on group conflict using narrow RD bandwidths.

	Three-point conflict measure	Binary conflict measure
	(1)	(2)
RD estimate	-0.151**	-0.274**
	(0.073)	(0.117)
Effective *N*	12,337	12,050
Overall *N*	81,945	81,945
$\hat{h}$	0.039	0.037
K clusters	48	47
Outcome mean	0.225	0.373

Notes: This table reports the estimated RD effects of DPEP on intergroup conflict using bandwidths that are one-half the value of the MSE-optimal bandwidths calculated by the `rdrobust` package (i.e., half the value of the bandwidths used in our benchmark analysis, Table 2, Panel A, Column 1). Columns 1 and 2 correspond to estimates drawing upon the three-point and binary conflict outcome measures (as described in Table 2, respectively. Robust standard errors clustered at the 1991 district level are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Figure A8: Robustness of DPEP's estimated effects on intergroup conflict to different manually chosen bandwidths.



Notes: This figure presents the estimated effect of DPEP on intergroup conflict across a range of bandwidth choices, using two alternative codings of 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. Each point reports the bias-corrected (robust) estimate of the discontinuity at the cutoff from a local linear regression using the specified bandwidth. The horizontal axis varies the bandwidth symmetrically around the cutoff. 95% confidence intervals are based on standard errors clustered at the 1991 district level. Coefficient estimates are displayed above each point.

B.2.2 Reporting bias analyses

Table A13: Estimated effect of DPEP on intergroup conflict, by presence of other non-household member during interview.

	3-point conflict measure		Binary conflict measure	
	Outsider present	Outsider absent	Outsider present	Outsider absent
	(1)	(2)	(3)	(4)
RD estimate	-0.144***	-0.124**	-0.243***	-0.200**
	(0.043)	(0.062)	(0.075)	(0.099)
Effective *N*	6,232	14,198	6,777	13,078
Overall *N*	26,873	52,593	26,873	52,593
$\hat{h}$	0.065	0.085	0.070	0.080
K clusters	297	297	297	297
Outcome mean	0.327	0.313	0.535	0.512

Notes: This table presents RD estimates of the effect of DPEP on intergroup conflict, splitting the sample by whether a non-household member was present during the IHDS interview. Data come from both IHDS waves. The unit of analysis is the household. 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?" 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. Robust standard errors clustered at the 1991 district level are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

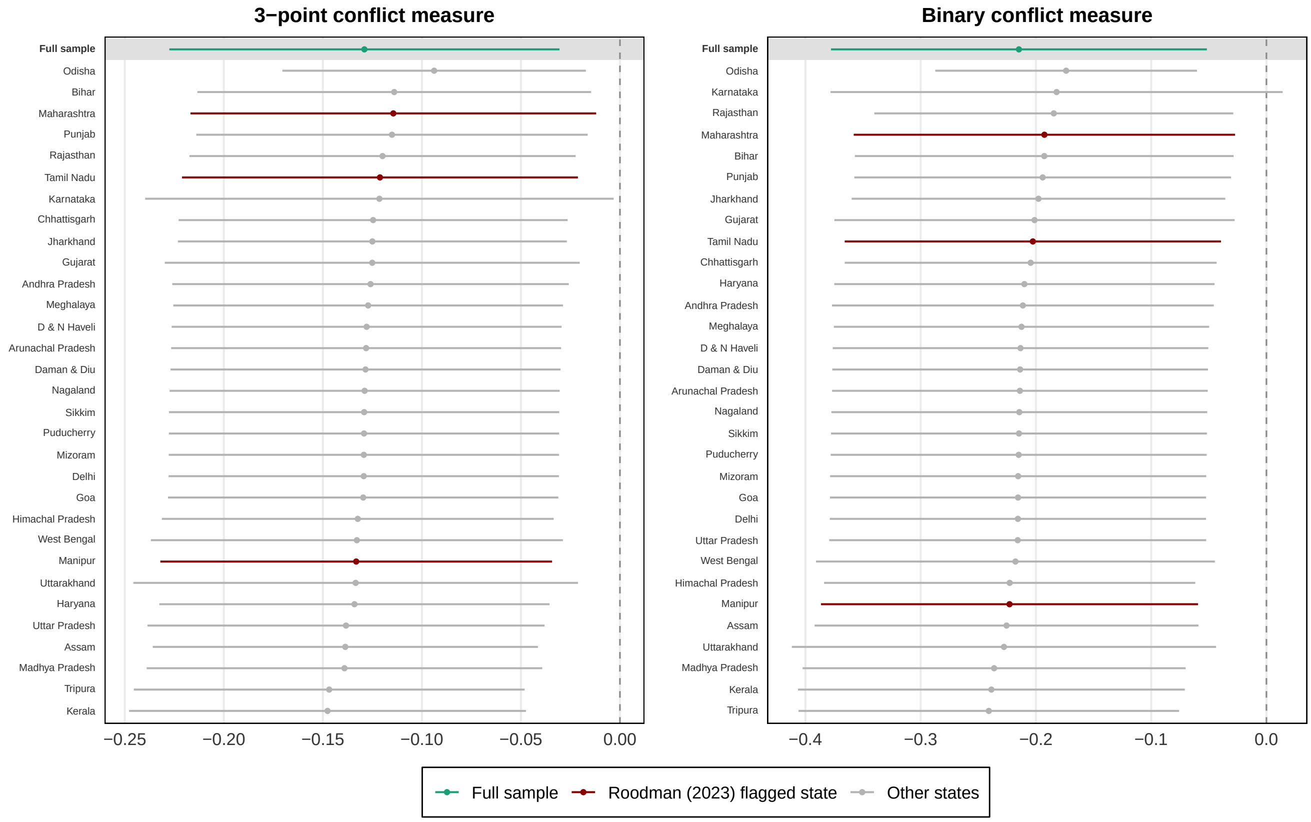
Table A14: Estimated effect of DPEP on intergroup conflict among households with no DPEP-exposed members

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	-0.133**	-0.111**	-0.147***	-0.138**	-0.159**	-0.163**
	(0.052)	(0.048)	(0.056)	(0.058)	(0.073)	(0.080)
Overall *N*	20,286	20,286	20,286	20,286	20,286	20,286
Effective *N*	5,175	10,964	6,325	9,182	8,452	10,888
Overall *N* districts	295	295	295	295	295	295
Effective *N* districts	83	170	102	142	132	169
$\hat{h}$	0.078	0.155	0.095	0.123	0.116	0.152
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.217	0.217	0.225	0.222	0.230	0.217
Panel B: Binary outcome measure						
RD estimate	-0.218***	-0.172**	-0.175**	-0.239**	-0.291**	-0.293**
	(0.084)	(0.077)	(0.078)	(0.098)	(0.115)	(0.118)
Overall *N*	20,286	20,286	20,286	20,286	20,286	20,286
Effective *N*	4,783	10,547	10,163	7,961	9,598	13,104
Overall *N* districts	295	295	295	295	295	295
Effective *N* districts	76	162	158	121	148	198
$\hat{h}$	0.072	0.144	0.140	0.110	0.125	0.177
$\hat{h}$ selection	CCT	CCT	IK	CCT	CCT	CCT
Outcome mean	0.358	0.360	0.364	0.371	0.366	0.360

Notes: This table presents RD estimates of the effect of DPEP on intergroup conflict, restricting the sample to households with no member aged 6–21 at IHDS-1 or 12–27 at IHDS-2. Because these households contain no school-age children, they are unlikely to have been directly treated by DPEP's educational expansion. Persistent effects among this subsample suggest that DPEP's conflict-reducing impact operates through community-wide spillovers rather than solely through direct exposure to schooling. 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.

B.2.3 Leave-one-out analysis

Figure A9: Effect of DPEP on intergroup conflict, dropping one state out.



Notes: This analysis re-estimates the main regression discontinuity (RD) specification while sequentially excluding one state at a time. The goal is to assess the robustness of the estimated effects to the influence of any single state, and in particular to address concerns that the results may be driven by a small number of influential observations. This exercise is motivated by the critique of Khanna (2023) by Roodman (2023), who argues that the original findings may be disproportionately influenced by four districts located very close to the eligibility threshold: Aurangabad in Maharashtra (39.64%), Tamenglong in Manipur (39.68%), Cuddalore in Tamil Nadu (39.70%), and Latur in Maharashtra (39.74%). All but Tamenglong received DPEP funding, implying that three of the four districts are non-compliers under an intent-to-treat rule based on the 39.29% cutoff. To address this concern, the figure reports RD estimates obtained after dropping all observations from one state at a time. For each leave-one-state-out sample, we re-estimate the local linear RD model using the same specification as in the main analysis, with bias-corrected inference and heteroskedasticity-robust standard errors clustered at the 1991 district level. The optimal bandwidth is re-selected in each iteration using the standard `rdrobust` procedure. Each point in the figure corresponds to the estimated treatment effect after excluding a given state, with horizontal line segments indicating 95% confidence intervals. The full-sample estimate is shown separately and highlighted for reference. States containing the districts flagged by Roodman (2023)—Maharashtra, Tamil Nadu, and Manipur—are distinguished to facilitate comparison.

B.2.4 Educational divergence analyses

Table A15: Estimated effect of DPEP on intergroup educational inequality.

	Hindu − Muslim gap		Upper caste − SC/ST gap	
	Attendance	Years of ed.	Attendance	Years of ed.
	(1)	(2)	(3)	(4)
RD estimate	-0.009	-0.379	0.050	0.497
	(0.078)	(1.152)	(0.041)	(0.502)
Effective N (districts)	61	59	115	164
Overall N (districts)	193	193	316	316
$\hat{h}$	0.090	0.088	0.100	0.124
K clusters	177	177	260	260
Outcome mean	0.036	0.713	0.056	0.893

Notes: This table presents RD estimates of the effect of DPEP on intergroup educational inequality. The outcomes are district-level gaps in school attendance and years of education between social groups, constructed from individual-level data in the first wave of the IHDS. Columns 1 and 2 report estimates on the Hindu–Muslim gap (Hindu mean minus Muslim mean) in school attendance and years of education, respectively. Columns 3 and 4 report estimates on the upper caste–SC/ST gap (Forward caste and OBC mean minus Dalit and Adivasi mean) in the same outcomes. A negative coefficient indicates that DPEP narrowed the intergroup gap, while a positive coefficient indicates that DPEP widened it. The sample is restricted to individuals aged 5–25, and districts are included only if they contain at least 10 individuals from each group. 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 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. Robust standard errors clustered at the 1991 district level are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

B.2.5 Local randomization-based RD analyses

Table A16: Local randomization-based RD estimation of DPEP's effect on intergroup conflict.

Bandwidth $\hat{h}$:	0.015	0.025	0.035	0.045	0.055
	(1)	(2)	(3)	(4)	(5)
Panel A: 3-point outcome measure					
RD estimate	-0.124	-0.088	-0.069	-0.056	-0.053
RI cluster-robust p-value	0.004	0.006	0.024	0.033	0.038
Clusters K	21	35	46	53	62
N obs.	6,016	9,452	11,964	13,593	15,820
Control mean	0.311	0.286	0.267	0.256	0.256
Panel B: Binary outcome measure					
RD estimate	-0.210	-0.123	-0.106	-0.083	-0.086
RI cluster-robust p-value	0.002	0.019	0.017	0.047	0.026
Clusters K	21	35	46	53	62
N obs.	6,016	9,452	11,964	13,593	15,820
Control mean	0.519	0.458	0.438	0.417	0.427

Notes: This table presents estimates of DPEP's effect on group conflict using the local randomization-based approach to RD estimation. We compute cluster-robust p-values with randomization inference. 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 using difference-in-means within bandwidths of varying sizes.

B.2.6 Fuzzy regression discontinuity analyses

Table A17: Robustness of DPEP's estimated effects on intergroup conflict to covariate adjustment and fuzzy RD estimation.

	$\hat{h}$		$2\hat{h}$		Global	
Bandwidth:	0.166	0.135	0.332	0.271	1.000	1.000
Controls:	N	Y	N	Y	N	Y
	(1)	(2)	(3)	(4)	(5)	(6)
A. Fuzzy RD estimates						
- Outcome: Conflict (3-point, 0–1)						
- Endogenous regressor: DPEP funding (in INR crores)						
RD estimate	-0.010**	-0.009**	-0.008***	-0.009***	-0.005**	-0.005**
	(0.005)	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)
Effective N	49,783	40,888	77,181	72,435	81,945	81,924
Overall N	81,945	81,924	81,945	81,924	81,945	81,924
B. First-stage relationship RD estimates						
- Outcome: DPEP funding (in INR crores)						
RD estimate	11.925***	13.991***	12.006***	12.702***	12.793***	12.931***
	(3.700)	(3.365)	(3.378)	(3.258)	(2.876)	(2.733)
Effective N districts	234	195	358	341	375	375
Overall N districts	375	375	375	375	375	375
F-statistic (conventional)	15.2	24.3	34.2	35.5	63.5	71.1
F-statistic (robust)	10.4	17.3	12.6	15.2	19.8	22.4
C. Reduced-form (sharp) RD estimates using bandwidths from corresponding fuzzy RD specifications						
- Outcome: Conflict (3-point, 0–1)						
RD estimate	-0.097***	-0.102**	-0.091***	-0.097***	-0.063**	-0.062**
	(0.037)	(0.040)	(0.033)	(0.036)	(0.028)	(0.028)
Effective N	49,783	40,888	77,181	72,435	81,945	81,924
Overall N	81,945	81,924	81,945	81,924	81,945	81,924

Notes: This table presents fuzzy RD (Panel A), first-stage RD (Panel B), and reduced-form (sharp RD) estimates (Panel C) of the effects of DPEP. The unit of analysis is the household in Panels A and C, and the district in Panel B. Data are drawn from both waves of the Indian Human Development Survey (IHDS). The outcome variable in Panels A and C is based on the survey question: "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), and "not much conflict" (=0). In Panel B, the outcome is district-level DPEP funding (in INR crores). The running variable in all specifications is district female literacy from the 1991 Census of India, reversed so that higher values correspond to lower literacy; the RD cutoff therefore captures increased program eligibility. The $\hat{h}$ bandwidths in Panel A, Columns 1 and 2 are selected automatically using the MSE-optimal procedure implemented in `rdrobust`. To achieve common bandwidths across panels, both the estimation bandwidth h and bias bandwidth b are held fixed to match the corresponding fuzzy RD specification. Controls are: an indicator for any riot in the district between 1950–1991, indicators for household caste and religion (Forward, OBC, Dalit, Adivasi, Muslim), and an indicator for urban residence (Panels A and C). In Panel B, these covariates are included as district-level averages. Robust standard errors clustered at the 1991 district level are reported in parentheses. Panel B also reports conventional and robust first-stage F-statistics. *p<0.1; **p<0.05; ***p<0.01.

Table A18: Fuzzy RD estimates of DPEP's effect on intergroup conflict using optimal bandwidth from the reduced-form RD model.

	$\hat{h}$ = 0.078	
Controls:	N	Y
	(1)	(2)
RD estimate	-0.026	-0.022**
	(0.017)	(0.010)
Effective *N*	21,613	21,610
Overall *N*	81,945	81,924

Notes: This table presents fuzzy RD estimates of the effects of DPEP, using the same bandwidth as that calculate for the benchmark intent-to-treat analysis (Table 2, Panel A, Column 1). The unit of analysis is the household and the outcome is the same as that used in Table 2, Panel A. The endogenous regressor is district-level DPEP funding (in INR crores). The running variable in all specifications is district female literacy from the 1991 Census of India, reversed so that higher values correspond to lower literacy; the RD cutoff therefore captures increased program eligibility. Controls are: an indicator for any riot in the district between 1950–1991, indicators for household caste and religion (Forward, OBC, Dalit, Adivasi, Muslim), and an indicator for urban residence. Robust standard errors clustered at the 1991 district level are reported in parentheses. We caution that the fuzzy RD analysis in OA Table A17, Panel A, Column 1, should be considered the primary fuzzy RD specification, since that uses the appropriate optimal bandwidth computed from `rdrobust`. *p<0.1; **p<0.05; ***p<0.01.

B.2.7 Cohort depiction and analysis

Table A19: 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 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 A20: 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.

B.2.8 Robustness to dropping multi-parent districts

Table A21: Estimated effects of DPEP on intergroup conflict: Robustness to dropping multi-parent districts.

	Full sample	Keep only districts for which the largest 1991 parent district makes up ... of 2001 child district					
		> 0.4	> 0.5	> 0.6	> 0.7	> 0.8	> 0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD estimate	-0.129**	-0.129**	-0.127**	-0.127**	-0.124**	-0.123**	-0.123**
	(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*	81,945	81,945	81,542	81,542	80,626	79,568	79,437
$\hat{h}$	0.078	0.078	0.079	0.079	0.081	0.082	0.082
K clusters	297	297	297	297	294	290	289
Outcome mean	0.318	0.318	0.316	0.316	0.315	0.315	0.315

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.

C Extensions and mechanisms

C.1 Mechanism tests

C.1.1 Opportunity-cost mechanisms

Table A22: 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.

C.1.2 Textbooks content analysis

Table A23: 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 A24: Keywords used in keyword-assisted topic model estimation.

Topic	Keywords

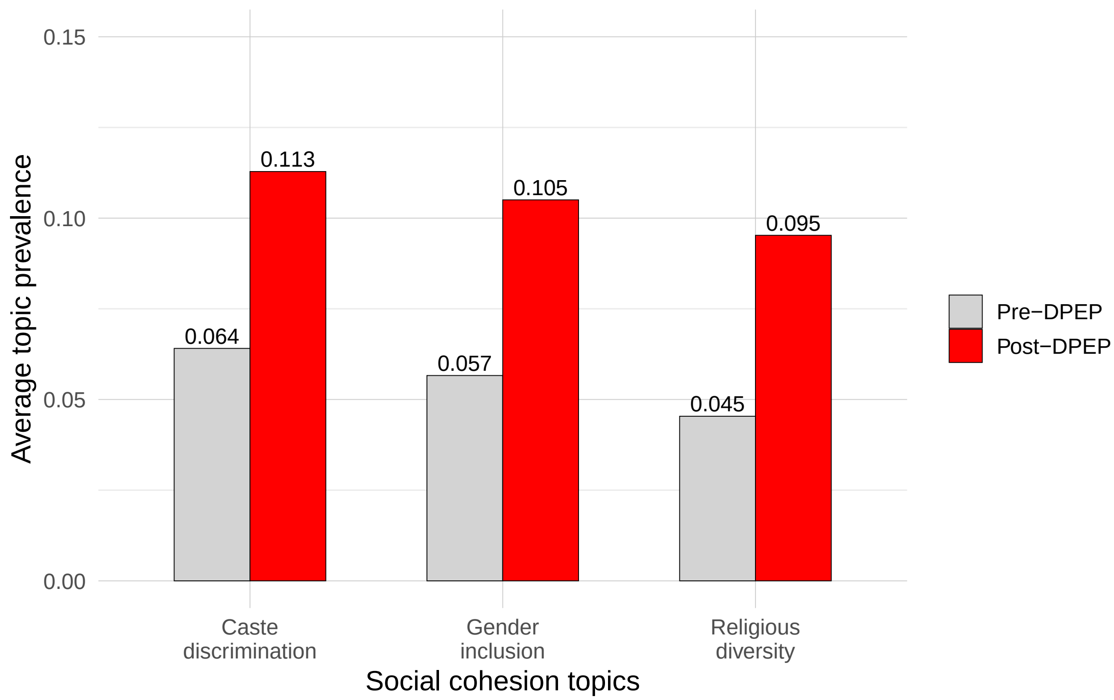
Gender inclusivity	`women`, `woman`, `female`
	`girls`, `girl`, `gender`
	`equality`, `empowerment`, `rights`

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`

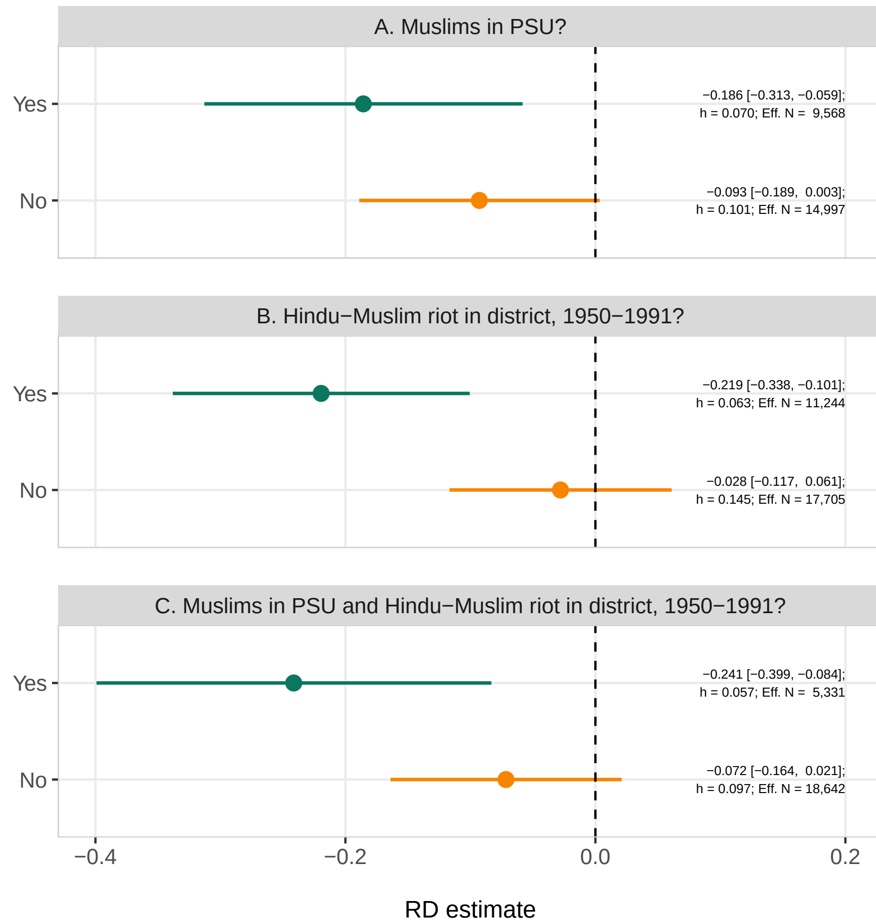
Notes: This table presents the keywords associated with inclusivity that we specified when estimating the keyword assisted topic model according to the procedure outlined by Eshima, Imai and Sasaki (2024).

Figure A10: Prevalence of inclusivity content in primary school social studies textbooks.



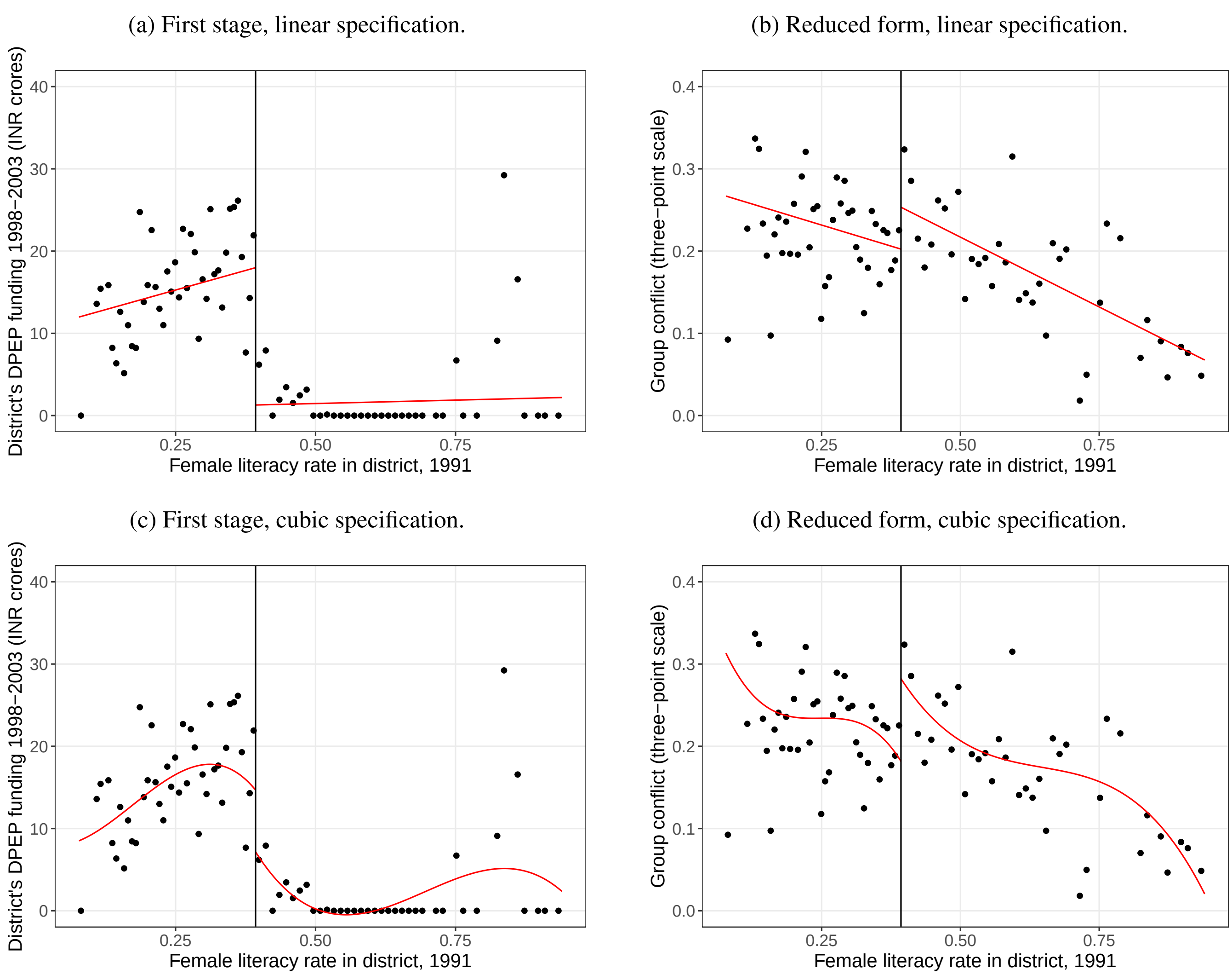
Notes: This figure compares the prevalence of topics associated with greater social inclusivity in textbooks prior to DPEP (pre-1994) and following the program's implementation (post-1994). Each estimate indicates the proportion of documents' text relating to each of the topics, as measured through the keyword assisted topic model procedure developed in Eshima, Imai and Sasaki (2024). The keywords we specified for each topic can be found in Table A24 . 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 A24.

Figure A11: 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).

Figure A12: RD plots of the first stage and reduced form relationships around the DPEP eligibility threshold.



(a) First stage, linear specification.

(b) Reduced form, linear specification.

(c) First stage, cubic specification.

(d) Reduced form, cubic specification.

Notes: The left column presents first-stage RD plots relating DPEP eligibility to treatment take-up, and the right column presents reduced-form RD plots relating eligibility to the 3-point outcome measure of group conflict. The top row uses linear specifications and the bottom row uses cubic specifications. The vertical solid bar represents the DPEP eligibility threshold. The vertical gap between the red fitted functions at the threshold visually approximates the corresponding RD effect. The points represent binned averages as per standard practice in plotting RD data.

C.1.3 Caste conflict and discrimination analyses

In OA Table A25, we use several measures, both behavioral and reported, to capture the prevalence of caste-based discrimination. Scavenging is the manual removal of human waste and has historically been performed almost exclusively by members of Dalit (Scheduled Caste) communities, making its prevalence a direct measure of caste-based occupational segregation. Service latrines, which depend on manual rather than water-based waste disposal, are closely tied to scavenging, thus their presence can similarly capture whether caste-ordered sanitation arrangements persist at the local level. The more direct untouchability measures gauge discriminatory attitudes rooted in the traditional caste practice of treating lower-caste individuals (and particularly Dalits) as ritually "impure" and thus subject to social and physical exclusion. Under untouchability norms, sharing food, water, and domestic space with Scheduled Caste members is forbidden. One measure records whether a household self-reports practicing untouchability; the other records opposition to the presence of a Scheduled Caste member in the household kitchen, the most symbolically charged site of these purity-based restrictions, given its role in food preparation and consumption.

Note, the scavenging and service latrine measures are derived from census records where direct questions about discrimination were not posed; thus, these measures are unlikely to be subject to social desirability bias. Arguably, the self-reported untouchability measures may be more susceptible to such bias. However, even if responses reflect some degree of social desirability, this is informative in its own right: a willingness to openly avow discriminatory attitudes signals that exclusionary norms remain socially acceptable, which may itself license discriminatory behaviors.

Table A25: Estimated effects of DPEP on caste-based discrimination: scavenging, service latrines, and untouchability practices.

	Scavenging		Service latrines		Untouchability	
	Share of HHs	Any HHs (0/1)	Per 100k residents	Any (0/1)	Practices (0/1)	SC kitchen prob. (0/1)
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	-0.001	0.000	44.633	0.224	-0.028	0.005
	(0.001)	(0.133)	(58.862)	(0.180)	(0.082)	(0.065)
Effective N	164	246	154	157	14,548	11,310
Overall N	592	592	545	545	41,239	31,743
$\hat{h}$	0.072	0.109	0.078	0.081	0.102	0.102
K clusters	429	429	401	401	296	296
Outcome mean	0.002	0.693	38.065	0.327	0.215	0.141

Notes: This table presents RD estimates of the effect of DPEP on caste-based discrimination. Columns 1–2 present estimates for scavenging outcomes: district-level measures are constructed using the 2012 Socioeconomic and Caste Census of India curated by Asher et al. (2021), and outcomes include the share of households engaged in scavenging (Column 1) and a binary indicator of whether any households rely on scavenging (Column 2). Columns 3–4 present estimates for service latrine outcomes: district-level measures are constructed using the 2011 Census of India curated by Asher et al. (2021), and outcomes include the number of service latrines per 100,000 residents (Column 3) and a binary indicator of whether any service latrines are present in the district (Column 4). Columns 5–6 present estimates for untouchability outcomes drawn from IHDS waves 1 and 2, including a binary indicator of whether the household self-reports practicing untouchability (Column 5) and a binary indicator of whether the household opposes the presence of an SC member in their kitchen (Column 6). The unit of analysis for Columns 1–4 is the 2011 district; for Columns 5–6 it is the household. The running variable in all regressions is a district's female literacy rate in the 1991 Census of India; the reverse 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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

C.2 Vidya Bharati school construction analyses

Table A26: 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	0.117*	6.190
	(0.070)	(9.580)
Effective N	122	119
Overall N	451	451
$\hat{h}$	0.070	0.069
Outcome mean	0.117	12.489

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. Robust standard errors are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

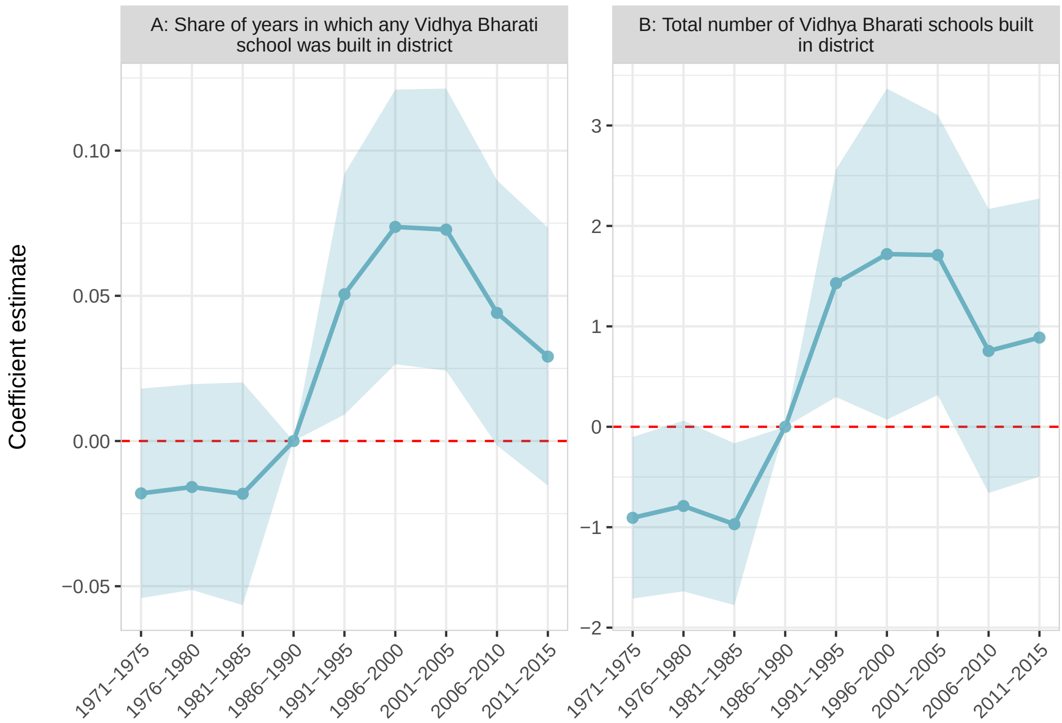
C.3 Heterogeneity analyses by Hindu nationalist vote share

Table A27: Heterogeneity in the estimated effects of DPEP on intergroup conflict, by prior Hindu nationalist vote share.

	3-point outcome measure		Binary outcome measure	
	Hindu nationalist vote share, 1951–1991:			
	Below median	Above median	Below median	Above median
	(1)	(2)	(3)	(4)
RD estimate	-0.119*	-0.074	-0.174*	-0.105
	(0.064)	(0.057)	(0.100)	(0.074)
Effective N	15,719	9,438	18,404	10,027
Overall N	41,984	37,183	41,984	37,183
$\hat{h}$	0.088	0.093	0.104	0.101
K clusters	152.000	137.000	152.000	137.000
Outcome mean	0.310	0.303	0.482	0.491

Notes: This table presents RD estimates of the effect of DPEP on intergroup conflict, by baseline Hindu nationalist vote strength. The unit of analysis is the household. Data come from both IHDS waves. The sample is split by whether a district's average Hindu nationalist vote share in Lok Sabha elections between 1951 and 1991 (inclusive) is above or below the median in the full set of 2001 districts. Elections data are taken from: Weaver, Michael; Nellis, Gareth; Baral, Siddhartha, 2025, "Replication Data for: The Electoral Consequences of Mass Religious Events: India's Kumbh Mela," `https://doi.org/10.7910/DVN/J8BBJB`, Harvard Dataverse, V1. The running variable is the inverse of female literacy in the 1991 Census, thus being above the cutoff implies greater educational access. Estimates are local linear RD coefficients using a triangular kernel and MSE-optimal bandwidths. Standard errors, clustered at the 1991 district level, are in parentheses. Outcome means correspond to the control side of the cutoff. *p<0.1; **p<0.05; ***p<0.01.

Figure A13: 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.

D Results summary

Table A28: Summary of results in relation to theoretical claims.

Mechanism	Theoretical claim	Results
Net effect	[1] Conflict occurs when $b_i > T_i \equiv \frac{w_i - \theta + c}{\gamma + \delta}$; schooling reduces conflict by lowering bias b_i or raising opportunity costs w_i.	• **Supported**: DPEP reduced reported intergroup conflict by 13 pp (3-point scale) and 22 pp (binary scale), both significant at $p < 0.05$.
Relational channel	[2] Intergroup contact in school lowers b_i, reducing the likelihood that $b_i > T_i$.	• **Mixed**: No significant effect on adult outgroup acquaintances or school climate (teacher bias, student enjoyment). • DPEP increased the share of SC teachers (+9.4 pp, $p < 0.01$) and near-doubled religious fractionalization among teachers ($p < 0.10$), consistent with a role-model mechanism.
Curriculum channel	[3a] Inclusive curriculum lowers b_i, reducing the likelihood that $b_i > T_i$. [3b] Patriarchal norms link women's autonomy to group honor, making perceived violations flashpoints for conflict; schooling weakens this link.	• **Supported [3a]**: Post-DPEP textbooks nearly doubled coverage of gender inclusivity, religious diversity, and caste discrimination. DPEP increased social studies pass rates by 4.7 pp overall and 11.9–12.8 pp in riot-prone districts ($p < 0.05$). • **Supported [3b]**: DPEP increased mahila mandal membership by 9.7 pp ($p < 0.05$) and reduced reported harassment of unmarried girls by 11.2 pp ($p < 0.10$). No significant effect on intimate partner violence norms.
Opportunity-cost channel	[4] Schooling raises w_i, increasing the cost of conflict and raising T_i.	• **Not supported**: No significant effects on household income, consumption, permanent employment, or district-level night-time luminosity.
Crowding-out effect	[5] Mandated expansion of secular public schooling may reduce demand for religious/exclusionary private alternatives.	• **Not supported**: DPEP increased (rather than decreased) the share of years with new Vidya Bharati school construction by 11 pp ($p < 0.10$).
Differential-returns effect	[6] Returns to schooling shift T_i unevenly across groups, sustaining conflict among those with weaker gains.	• **Not supported**: No significant effects on Hindu–Muslim or upper caste–SC/ST gaps in school attendance or years of education.