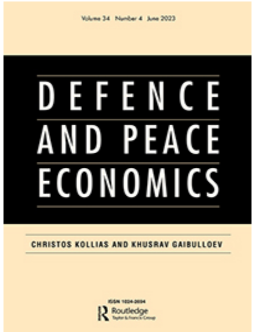

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Conflict Trajectories and Education: Gender-Disaggregated Evidence from India

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ABSTRACT
This paper investigates the relationship between conflict trajectories and years of schooling in India for girls and boys. It adopts propensity score matching methods on panel data from the India Human Development Survey (2004/05-2011/12) merged with conflict data from the South Asia Terrorism Portal. Conflict is measured according to the dynamic trajectory of Naxal violence-related fatalities at the district level, distinguishing areas of chronic conflict with those experiencing dynamism in conflict intensity over time. ATT estimates indicate that conflict is associated with a reduction in years of schooling for both genders, though relatively high for girls (by a quarter of a year for girls and by 0.16 of a year for boys), driven by large reductions in school accumulation for girls living in areas of chronic conflict. Results are consistent when adopting different methods, alternative measures of conflict fatalities, and accounting for other conflicts and selective migration. Examining transmission mechanisms suggest that household spending on girls' education may be de-prioritised amidst conflict, while conflict may also weaken or destroy school infrastructure. Results suggest that policy responses should prioritise girls' education in areas of chronic conflict, not only in 'fragile states' but in countries where conflict remains a subnational concern.

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Introduction

Armed conflict has devastating consequences on the education of girls and boys. There has been a notable increase in the empirical literature on this relationship over the last decade (e.g. see Justino 2016, for an overview), a limited subset of which examines conflict dynamically using panel data (e.g. Pivovarova and Swee 2015; Valente 2014; Roy and Singh 2016; Michaelsen and Salardi 2020; Diwakar 2021). However, even where panel data is used, the analyses typically do not distinguish between different conflict trajectories, for example, comparing areas of persistent conflict to areas that have become conflict-affected over time, or those that were never conflict-affected. This contributes to limited understanding of the complexity of conflict and its dynamics of change over time, which may have different human capital consequences. Moreover, research on conflict also tends to focus on countries that are classified by international standards as 'fragile states' or conflict-affected countries. Though India typically does not feature on lists of fragile states, subnational conflict is a real concern within its borders.

This paper exploits large-scale household panel survey and armed conflict microdata in the Indian context to analyse the role of Naxalite violence in influencing years of schooling for girls and boys and investigates potential supply- and demand-side channels through which schooling may be affected. It examines the relationship between conflict trajectories and education outcomes, which is

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important since different trajectories over time may contribute to different outcomes and so merit a more nuanced policy or programming response to effectively promote education in conflict-affected areas. The Naxalite violence itself is important to learn from given its intensity; it was described by former Prime Minister Manmohan Singh in the mid-2000s as the 'single biggest internal security challenge' that India had ever faced ('Naxalism' 2006). Moreover, the Maoist origins of this subnational conflict make this a unique case study for analysis, to examine potentially counter-intuitive relationships that expand our understanding of the gender-disaggregated relationship between conflict and education and assess scope for generalizability across country contexts.

To investigate this relationship, the analysis adopts propensity score matching methods on panel data from the India Human Development Survey (IHDS) 2004/05 and 2011/12 merged with conflict data from the South Asia Terrorism Portal (SATP) over the survey period. The intensity of conflict over the panel survey period and the presence of geo-spatial conflict data disaggregated by fatality type provide ideal grounds for investigating this relationship in India. Conflict is measured in terms of its trajectories over time, comparing districts without any conflict fatalities to those that are newly conflict-affected, and comparing districts that are persistently conflict affected to those that over time have escaped conflict-related fatalities. Sensitivity of results is assessed using different measures of conflict trajectories, distinguishing by type of conflict fatality, interrogating the role of selective migration, considering other sources of conflict, and adopting panel fixed effects estimators in linear regressions as robustness checks, with results consistent across models. The analysis also examines potential demand- and supply-side transmission mechanisms in terms of spending on tuition and presence of school infrastructure.

Estimates of the Average Treatment Effect on the Treated in the main specification indicate that conflict is associated with a reduction in years of schooling that is relatively larger for girls (around 0.25 or a quarter of a year) than boys (0.16 of a year) and driven by particularly large reductions in years of schooling for girls living in areas of chronic conflict in both survey periods. In terms of transmission mechanisms, the results suggest that while households generally spend more on tuition in conflict-affected areas, this relationship disappears in households with only girls and households with girls in areas of chronic conflict. These findings suggest that policy responses to supporting years of schooling should prioritise girls' education in areas of chronic conflict.

Section 2 overviews the gender-disaggregated relationship between conflict and schooling, while Section 3 presents background on this relationship amidst Naxal violence in India. Section 4 then presents the empirical method, data and descriptive statistics, and tests to ensure a proper fit of the model, before Section 5 presents results and discusses study findings. Section 5 reviews contributions of the study and concludes.

Armed Conflict and Education of Girls and Boys

There is generally a negative relationship between conflict and education in the literature (e.g. Akresh and de Walque 2008; Dabalen and Paul 2014; Diwakar 2015; Justino 2012, 2016; Merrouche 2011; Serneels and Verporten 2015; Bertoni et al. 2019). This can operate through a variety of pathways, such as damaged schools and reduced government non-military expenditures (Lai and Thyne 2007; Jones and Naylor 2014), reduced quality of teaching and fewer women volunteers (Bruck, Di Maio, and Miaari 2019; Roy and Singh 2016), depressed labour markets and social networks (Justino 2009), destroyed assets (Justino and Verwimp 2013; Shemyakina 2011), mortality, morbidity and displacement (Buvinic et al. 2013), child soldiering (McKay and Mazurana 2004), and widespread insecurity.

The literature on gendered impacts of conflict is divided, reflecting the heterogeneity of conflict processes and impacts. Some studies demonstrate that boys experience lower education outcomes in situations on armed conflict, often through the channel of child soldiering and a larger number of male deaths in conflict situations (Stewart, Huang, and Wang 2001; Blattman and Annan 2010; Akresh and de Walque 2008; Swee 2009; Kecmanovic 2013; Diwakar 2015). Others find that girls fare worse (Chamarbagwala and Morán 2011; Shemyakina 2011; Menon and van der Meulen Rodgers 2010; Singh and Shemyakina 2016). Moreover, it is not always the case that armed conflict yields negative results on years of schooling of girls compared to boys. For example, primary education completion and enrolment rates were found to be higher for girls in districts of Nepal experiencing more casualties from the Maoist conflict, attributed to the insurgency's policing of teacher absenteeism and public opposition of gender inequality in access to schooling (Valente 2014; Hart 2001). In India, primary school children's acquisition of basic skills typically is lower amidst Naxal violence, though in some cases enrolled girls' learning outcomes are observed to improve amidst Naxal policing of school quality (Diwakar 2021).

Research on education amidst Naxal violence in Nepal and India reflect conditions that may be applicable more widely in South Asia. In much of South Asia, armed conflict is asymmetric and subnational, between the central government and a group of armed actors representing a particular identity group (Parks, Colletta, and Oppenheim 2013). A comparison between armed conflict in Nepal and India might be relevant given the Maoist origins of the Naxal conflict in India. However, even in this setting, prevalent insecurity from conflict fatalities may offset any potential education gains. Moreover, comparisons between the conflict in India and Nepal point to ideological differences that a unitary approach to analysis would hide (Nayak 2008; Bownas 2003). The overall ambiguity in conflict effects renders gender-disaggregated investigations into education access particularly salient in the case of India.

To date, there are only a handful of quantitative studies linking armed conflict and years of schooling in India. Singh and Shemyakina (2016) observe a negative effect of the 1981-83 Punjab insurgency on education attainment operating through reduced education expenditures in households with a higher share of girls to boys of school-going age. Roy and Singh (2016), using panel data between 2005 and 2014 from Assam, find that gender differential responses to the ethnic Bodo conflict are more negative for rural schools and also for children in poorer districts. Diwakar (2021) examines Naxal violence and learning quality of primary school-age children but does not investigate the ways in which access to education itself may be constrained over time or distinguish the conflict trajectories that may affect education.

This paper contributes to the existing literature in three ways. First, it investigates the relationship between Naxalite violence and years of education using panel data to capture the dynamic nature of conflict. The Maoist dimensions of Naxal violence may offer unique outcomes by gender based on its left-wing ideology. Second, the paper provides new evidence of the heterogenous relationship between conflict and education depending on trajectories of violence and the gender and education level of the child. Finally, the paper examines potential supply- and demand-side transmission mechanisms underpinning these relationships.

Background: Naxalite Violence

The Naxalite conflict has its origins in India's failure to enforce equitable land distribution for the poor outlined in its constitution. Its initial trigger traces to March 1967, when a tribal farmer in Naxalbari, rural West Bengal, was attacked by landlords in a land dispute. This led to a farmer uprising in several Indian states, where revolutionaries over time split and merged into parties that ultimately led to the creation of the Communist Party of India – Maoist in 2004 (Singhal and Nilakantan 2016). The movement increasingly adopted violent means, though with variations pointing to increased violent intensity in some states and reducing in others. In recent years, Naxal violence has been concentrated in Odisha, Jharkhand and Chhattisgarh, referred to as a 'three state Red Corridor' along a contiguous narrow strip that comprises rebel support bases and acts as a pathway for easier mobility of Naxal forces (Tikku 2013).

Figure 1 shows the number of fatalities in districts that reported fatalities over at least 1 year between 2004 and 2012, disaggregated by type of fatality. The figure points to a peak in Naxal violence between the two IHDS survey years from 2004/05 to 2011/12. This makes the analysis

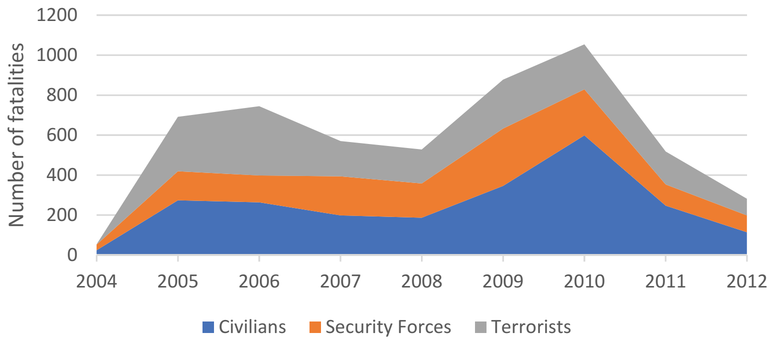

Figure 1. Number of fatalities per year between 2004 and 2012. Source: author's visualisation of SATP data.

particularly relevant given that it corresponds to a period of Naxal intensity. According to the SATP data, there was almost a thirteen-fold increase in conflict fatalities between 2004 and 2005, with the period marked by the creation of the Communist Party of India – Maoist (Singhal and Nilakantan 2016). The height of violent casualties occurred in 2010, when 1054 people were killed, predominantly in West Bengal, Chhattisgarh, Jharkhand and Odisha. Of these, over half (57%) were civilian casualties, caught in the crossfire of violent conflict.

Figure 2 next provides a heat map of Naxalite activity in India by conflict intensity (left) and conflict trajectory (trajectory), again highlighting that most of the violence occurred in the 'Red Corridor'. The map of conflict intensity draws attention to the relative share of conflict as a size of the district population. For example, Dantewada in Chhattisgarh also saw between 164 and 280 fatalities in four of the 5 years between 2006 and 2010, representing up to 52 deaths per 100,000 population of the district according to 2011 population values. In Odisha, Malkangiri experienced 71 Naxal-related fatalities in 2008, equivalent to 12 deaths per 100,000 population of the district. Overall, in terms of conflict-affected districts included in the IHDS survey, fatalities ranged from 0.01 to 12.23 deaths per 100,000 population between 2004 and 2012.

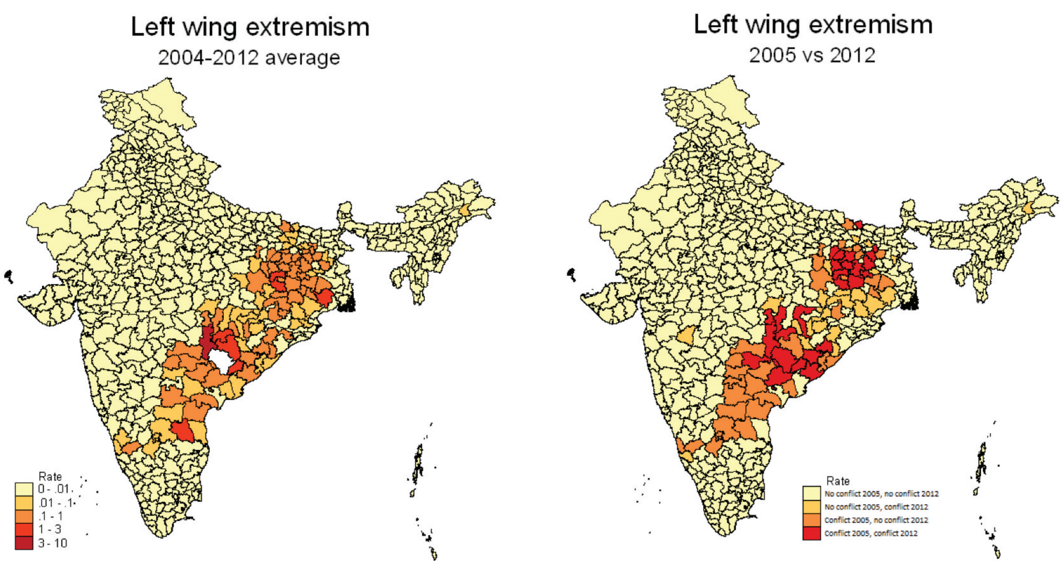

Figure 2. Heat map of Naxalite activity in India 2004/05-2011/12 average (left) and trajectory (right). Note: Rate per 10 thousand people. Source: author's visualisation of SATP data.

Naxal violence has affected education. Naxalites have recruited children in military operations, by pressuring parents or visiting schools and asking children to join (HRW 2008). State governments have also played a part, where for example, Chhattisgarh police have been known to recruit people including children from camps to take part in anti-Naxal operations as special police officers (HRW 2008; Asian Centre for Human Rights 2013). Household responses to conflict may also deter investments in and demand for education. Lower expenditures by parents in times of distress may contribute to spending being prioritised on sons (Singh and Shemyakina 2016), or children and households may self-exclude on account of safety concerns (EFA 2011).

The conflict has also affected supply-side attributes, with reports pointing to Naxals targeting public infrastructure including roads and schools. A study using district-level data in 151 districts from six states under Maoist influence found that districts with high intensity of conflict in 2001 had lower access to public goods, including schools, health centres, drinking water sources, paved roads, and communication infrastructure (Vadlamannati and Khan 2017). Other studies, though, also suggest that Naxalites capitalise on underdevelopment in tribal areas, by providing a form of parallel government of service provision including in education (Sahoo 2015; Das 2017), so helping drives their anti-state rhetoric. Thus, both supply- and demand-side channels could be hypothesised to affect education accumulation variably depending on the presence and dynamics of violent conflict.

Data and Methods

Empirical Method

The study relies on a natural experiment: subnational armed conflict in India, and how it affects years of schooling. With this natural experiment, a concern is that we can only observe the outcome of interest – years of schooling – for children in conflict areas, but not what their years of schooling might have been in the absence of conflict. To establish this counterfactual and assess the difference between the observed and unobserved outcomes, propensity score matching methods have been employed (Rubin 1997). This strategy helps minimise selection bias and confounding in the estimated relationship between conflict and education (Dabalen and Paul 2014), and thus is important in helping address key sources of measurement error and misspecification that may arise through other regression-based modes of analysis.

To estimate propensity scores, probit regressions are conducted on observed characteristics, which calculate the probability of individuals receiving the treatment (Rosenbaum and Rubin 1983). Covariates included are those that affect both participation in the treatment and the outcome. The presence of any conflict fatalities at the district level is the 'treatment' in this study. We wish to assess the average impact of conflict on children who reside in conflict-affected districts of the country, which is the average effect of treatment on the treated (ATT). The focus on ATT recognises the non-random assignment of conflict, by restricting the sample to conflict-affected communities (Wharton and Oyelere 2011). Understanding the impact of conflict in conflict-affected areas is more realistic since it is restricted to the population who experience conflict. With difference-in-difference modelling using panel data with baseline information (Nguyen 2011), the ATT is calculated as:

$$ATT_M = \frac{1}{n_p}\left\{\sum\nolimits_{i=1}^{n_p}\left(Y^i_{12} - \sum\nolimits_{j=1}^{n_{ic}}\psi(i,j)Y^j_{02}\right) - \sum\nolimits_{i=1}^{n_p}\left(Y^i_{01} - \sum\nolimits_{j=1}^{n_{ic}}\psi(i,j)Y^j_{01}\right)\right\} \quad (1)$$

where individual i is in the treatment group and can be matched with nonparticipant(s) j, Y^i_{12} is the observed outcome of treatment individual i and nonparticipants j in the second period, ψ is the weight, and n captures the number of participants. In the context of this study, the ATT would represent the average effect of conflict on individuals residing in conflict-affected areas.

There are certain challenges to this modelling for this investigation. It cannot control for heterogeneity of unobserved attributes between treatment and control group. To help mitigate this

Table 1. Treatment and control groups for matching using two-period panel data.

Compare	Treatment	Control
A:	Conflict in 2004/05; Conflict in 2011/12	Conflict in 2004/05; No conflict in 2011/12
B:	No conflict in 2004/05; Conflict in 2011/12	No conflict in 2004/05; No conflict in 2011/12

concern, we guide variable selection based on the literature on conflict and education, undertake a balance test on the observables, and tested for sensitivity to omitted variables (Mantel and Haenszel 1959). Another challenge is that the treatment takes place continuously, insofar as some districts become conflict-affected over time, while others saw an end to conflict. Cross-section estimates of ATT that do not consider the panel structure of the data will not adequately capture the effect of treatment.

Nguyen (2011) proposes a method to calculate the ATT in the presence of panel data without baselines. The intervention impact with panel data can be written as (Nguyen 2011):

$$\begin{aligned} ATT_X^2 = {} & \Pr(D_1 = 1 \mid X, D_2 = 1)[E(Y_{12}|X, D_1 = 1, D_2 = 1) - E(Y_{02}\,|X, D_1 = 1, D_2 = 1)] + \\ & \Pr(D_1 = 0 \mid X, D_2 = 1)[E(Y_{12}\,|X, D_1 = 0, D_2 = 1) - E(Y_{02}\,|X, D_1 = 0, D_2 = 1)] \end{aligned} \quad (2)$$

where Y_{12} and Y_{02} is the potential outcomes with and without conflict in the second period, and treatment is defined through D. Effectively, this model constructs two treatment groups and two control groups (Table 1). The first treatment group comprises children residing in conflict-affected areas in 2004/05 and 2011/12 and is matched to a control group of people in areas that were only conflict-affected in 2004/05. The second treatment group comprises children who were residing in conflict-affected areas only in 2011/12, matched with a control group of children who did not reside in a conflict-affected area in either survey year. The ATT is then the difference in outcome between the treatment and control groups over time.

There are two identification assumptions when relying on panel data without baselines (Nguyen 2011). The first requires that the difference in the no-conflict outcome, conditional on X, between people who are not living in conflict-affected areas in both periods and those who are living in conflict-affected areas only in 2012 is unchanged over time. The second assumption requires that difference between the second period outcome in non-conflict areas and the first period outcome of conflict is same for people in conflict-affected areas in both periods and those who were only in conflict-affected areas in 2005 (Nguyen 2011). Finally, common support assumptions to find control groups with similar X variables require that for a given intervention status in the first period, there are non-treated individuals with observed X variables similar to treated individuals in the second period. Under these assumptions, the ATT is measured as a weighted average of treatment impacts on groups with different treatment statuses in the two periods (Nguyen 2011).

We estimate ATT using the five nearest neighbours matching, which matches the treated individual i to the control individuals with the smallest distance from individual i. Matched individuals are assigned a weight of 1, and all other observations are assigned weights of zero (Rubin 1973). Even so, there are inherent trade-offs between bias and efficiency in matching methods (Caliendo and Kopeinig 2008). Therefore, as sensitivity checks, kernel and radius matching algorithms within PSM are also employed in the Annex, and double-robust estimation techniques. The double-robust method ideally would select only observations which are on common support and then run a standard OLS regression on remaining observations. The double-robust estimator provides unbiased estimates of the treatment effect if either the propensity score or the OLS models are correctly specified (Bang and Robins 2005; Dabalen and Paul 2014). Finally, we also rely on fixed effects estimators in OLS models to assess the sensitivity of results, further exploit the panel structure of the data, and examine potential transmission mechanisms.

Dataset and Key Variables

This analysis involves newly merging two datasets: 1) the India Human Development Survey (IHDS), a panel dataset from 2004/05 to 2011/12 (Desai and Vanneman 2017), and 2) a dataset on armed conflict casualties from the South Asia Terrorism Portal (SATP 2017). These datasets are described below:

- *India Human Development Survey*: The IHDS offers modules with individual-level information on education, employment, health, and demographics, as well as information on household-level expenditures and the presence and quality of wider community facilities such as health and education. It is a nationally representative survey of 41,554 households and 150,995 individuals across the country (Desai and Vanneman 2017). The first survey round was undertaken in 2004/05 and the second in 2011/12, hereafter referred to as 2005 and 2012 for convenience, respectively, when the majority of survey interviews were conducted.[1]
- *South Asia Terrorism Portal*: SATP is the 'largest website on terrorism and low intensity warfare in South Asia', created to counter the international community's neglect of terrorism within South Asia (SATP 2017). The portal includes information on conflict-related civilian, special forces, and terrorist fatalities per year, state, and district, drawn from local and national newspapers and official figures. As such, it is also a panel dataset, focused on conflict fatalities.

The IHDS is merged with district-level microdata from the SATP, to investigate the relationship between subnational armed conflict and education outcomes. The time interval, with the IHDS collected in 2005 and 2012, and relying on conflict data covering the entirety of this period, is critical to investigating the conflict. It makes the dataset hugely relevant to answer the research question, given that Naxal violence reached its highest intensity during this period, before subsiding into more recent years. Having subnational conflict and human development data covering this period thus offers strong motivation for analysing this relationship in our study.

Outcome of Interest: Years of Schooling by Gender

The dependent variable is years of schooling at the time of the survey. For this, the data makes use of all individuals aged 6 to 14 years in 2005 (and thus who have aged around 12 to 23 years by 2012, depending on survey data collection dates), thus including children of primary, secondary, and by the latter wave also of tertiary school-going age. The inclusion of children of primary school age in 2005 who have aged into older young people by 2012 allows us capture the effect of conflict across the school-age distribution for the cohort.

Several studies of conflict and education similarly rely on children of school-going age (Pivovarova and Swee 2015; Dabalen and Paul 2014; Wharton and Oyelere 2011). Given that the outcome is a dynamic measure, many children are unlikely to have completed their education within the age group. Some models tend to overcome this right-censoring by focusing only on completed years of schooling, but these can fail to account for rising levels of education attainment of youth (Orazem and King 2008; Holmes 2003). Several studies instead attempt to address the challenges of unobserved completed schooling for the sample of school-age children, for example, by including age as a covariate in an OLS regression within the schooling attainment model (Anderson, King, and Wang 2003), or controlling for enrolment status through probit models (Dabalen and Paul 2014; Holmes 2003; Lillard and King 1984). Our analysis includes both age and enrolment status controls, and adopts a probit model when generating propensity scores that helps address some of the biases from OLS estimates.

A concern is around endogeneity, where conflict could affect education accumulation, but education accumulation could also affect conflict. There is also a risk of omitted variable bias in these relationships. Socioeconomic controls, as discussed below, are used to help mitigate concerns around endogeneity and assess sensitivity of results using fixed effects estimators. In addition, PSM comparisons by subgroup by restricting comparisons to areas with similar starting points offer another means of limiting omitted variable bias. This is comparable to other research on conflict and education that, for example, restricts the sample to conflict-affected communities (Wharton and Oyelere 2011).

Conflict Trajectories and Control Variables

The independent variable of interest is the conflict trajectory corresponding to the district in which the household resides. To identify this trajectory, the paper relies on SATP annual conflict fatality data from India covering 2005 to 2012 to derive district-level conflict trajectories. We develop a trajectory measure that captures the four conflict outcomes in Table 1: any conflict fatality in both 2005 and 2012, conflict in 2005 but not 2012, conflict in 2012 but not 2005, no conflict in either year. Though this does not explicitly cover intermediate years, there is strong overlap, where, for example, 96% of households that are affected by chronic conflict using this measure are also affected by conflict in over two-thirds of the years based on annual conflict data between 2004 and 2012. Finally, to further assess sensitivity, in addition to different matching methods, we also distinguish by type of conflict fatality to nuance the discussion of conflict, consider the potential for selective migration, consider the presence of other types of conflict within India's borders, and examine conflict fatality rates through panel fixed effects OLS regressions.

Our PSM model also accounts for other factors related to the treatment and outcome variables, by including these (baseline) covariates into the model used to estimate the propensity scores. The choice of model controls is similar to a demand for schooling model, with modified regressors to explore the role of conflict on years of schooling as identified in previous literature (e.g. Rosenzweig and Schultz 1982; Strauss and Thomas 1995; Shemyakina 2011). Covariates from a demand for schooling model typically include a range of demographic factors related to the child and household that influence schooling of by girls and boys that we consider in the specification: gender of child, education of parents, household size, gender of household head, and log of per capita expenditures. Limited access to education in India has been observed to correlate with poverty, parents' education, and gender in favour of boys (Borooah 2012; Woodhead et al. 2013; Asadullah and Chaudhury 2), which provides further justification to include these controls.

In addition to the typical demand for schooling model, we consider some additional variables relevant to our analysis. Given the importance of caste in affecting educational opportunities in India (Islam et al. 2016), we control for whether the child is a Dalit or an Adivasi. We also control for age and enrolment status in the empirical model to help address the issue of incomplete schooling, as discussed above (Dabalen and Paul 2014; Holmes 2003; Lillard and King 1984).

Finally, we include regional variables in our model specific to the context. One such variable related to socio-economic conditions of the area is the district-level economic activity rate. This is a survey-derived measure that captures the share of labour-force population per district who are working at least 240 hours. Areas with less economic activity or work participation might intuitively be related to an increase in conflict through the grievance channel (Gurr 1970; Horowitz 1985) and potentially be associated with lower levels of schooling given resource constraints from limited job opportunities. Another area control is captured by whether the child resides in an urban area. Finally, recognising that areas close to the Red Corridor might be particularly prone to 'treatment', we also include a variable measuring the distance to Dantewada, Chhattisgarh, which is the district with the highest conflict fatality rate over the survey period. This variable is thus a measure of intensity to reflect potential spillover effects that may not be adequately captured by state boundaries.

Descriptive Statistics

As noted earlier, data on conflict is merged with the IHDS. In the dataset, 82 out of the 376 districts in the IHDS were affected by Naxalite violence in either survey year. In terms of coverage of children, out of the 150,995 individuals surveyed in both 2005 and 2012, there are 30,135 children who were

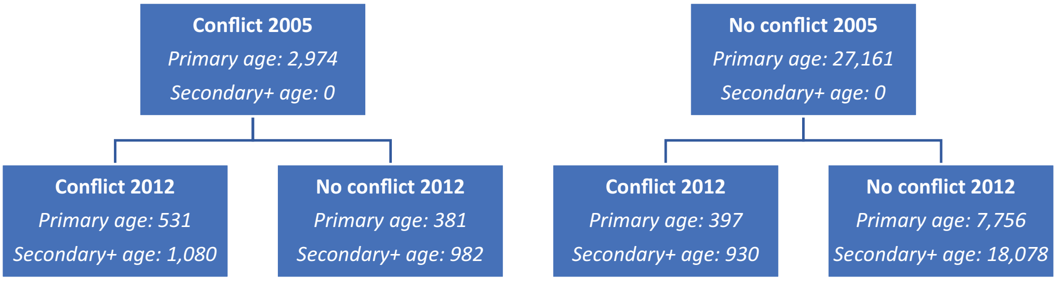

Figure 3. Sample size by conflict dynamics and school-age group.

Table 2. Years of schooling by level of education, gender, and conflict.

Gender		A: 2005	B: 2012	C: 2012	Change
[Obs]	Conflict dynamic	Primary age	Primary age	Secondary age	(C-A)
Boys	No conflict	3.34	6.22	8.95	5.61
[*N*=16,764]	Conflict 2005, no conflict 2012	3.25	6.25	8.49	5.23
	No conflict 2005, conflict 2012	3.60	6.41	9.67	6.06
	Persistent conflict	2.59	5.38	8.69	6.11
Girls	No conflict	3.04	6.22	9.12	6.08
[*N*=13,371]	Conflict 2005, no conflict 2012	2.61	6.03	8.90	6.30
	No conflict 2005, conflict 2012	3.14	6.85	9.78	6.64
	Persistent conflict	2.67	5.92	8.99	6.32

of primary school age in 2005 that were accordingly followed into 2012. Within the age group, 4,301 children (14.3% of the total sample) were residing in districts that had experienced fatalities from Naxal violence in either or both survey years. This disaggregation by primary- and secondary-school age and conflict trajectory is presented in Figure 3.

Table 2 next displays years of schooling for children for this cohort, by gender and conflict dynamic. Again, conflict is measured in terms of whether the district of residence had conflict fatalities. Average values indicate lower average years of schooling for children in persistently conflict-affected areas, compared to areas that never experienced conflict and to areas that newly experienced conflict only after the first survey wave. By secondary school age, moreover, areas with persistent conflict and areas that escaped conflict continued to have children with lower average years of schooling compared to areas that did not experience any conflict. The initial presence of conflict thus appears to bear some relationship with lower years of schooling, even if the change over time may have been large in some cases.

By gender, interestingly, while girls generally begin the period with lower years of schooling, over time they typically fare better over time than boys. However, the difference by gender in the *change over time* for secondary-age children is smallest in areas of persistent conflict. Specifically, girls have increased their years of schooling on average by 0.22 more years than boys over the same period when residing in persistently conflict-affected areas (i.e. 6.32 years for girls and to 6.11 years for boys), compared to a 0.47-year gender differential in no-conflict areas, and a 1.06-year gender differential in areas that were only conflict-affected after 2005. In other words, the change in years of schooling during persistent conflict appears to comparatively penalize progress in girls' schooling, even amidst general increases in their years of schooling over time.

Model Fit

In conducting propensity score matching methods, we first check for the appropriateness of the matching model. One assumption, of conditional independence, is that given a set of observable covariates that are not affected by treatment, the potential outcomes themselves are independent of

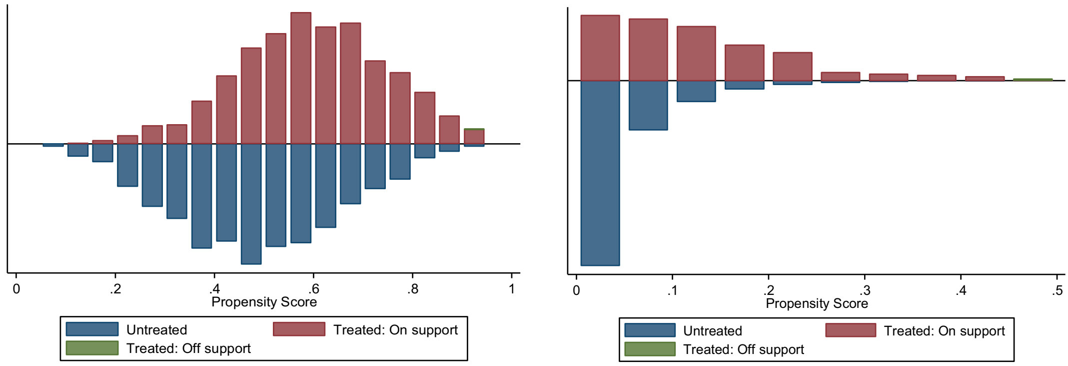

Figure 4. Common support between comparison groups (D1 left; D2 right).

treatment assignment. We use existing theory and literature to inform the variables used to generate the propensity scores, relying on the theoretical and empirical literature on conflict and education, including other analyses adopting PSM methods (e.g. Dabalen and Paul 2014).

The second assumption in propensity score models is of common support, which ensures that there is adequate overlap in characteristics of treated and untreated observations to find enough matches. In other words, observations with the same covariate values should have a positive probability of being both participants and nonparticipants (Heckman, LaLonde, and Smith 1999). Figure 4 visually depicts propensity score distributions between the comparison subsets for children of school-going age, which offers an easy check of the region of common support between the control and treatment groups and thus the quality of the match (Caliendo and Kopeinig 2008). The models do not appear to encounter any common support problems.

Moreover, most of the observed characteristics are balanced between treatment and control groups after the matching is conducted, which also lends credence to the applicability of the PSM model for the data. We can see this through conducting a two-sample t-test between the difference in means of treated and untreated control groups for a range of variables. In these tests, most variables lack statistically significant difference in means and standard deviations between treatment and control groups at the 1% significance level (Table 3). This is an improvement when comparing means of covariates for the treated with the full unmatched group of untreated individuals. Moreover, we derive the average mean bias, and Rubin's B and R statistics for the estimations.[2] These values generally fall within Rubin's (2001) recommendations, providing more evidence that the samples are sufficiently balanced.

Results and Discussion

Violent Conflict Trajectories and Years of Schooling

Results of nearest matching estimates are provided in Table 4. We also run sensitivity tests using alternative matching methods and doubly robust estimation, with results largely similar and presented in Table A1 in the Annex. In all of this, bootstrapping errors are used to estimate standard errors. Significant coefficients all have the same sign regardless of the measure of conflict adopted.

ATT estimates indicate that armed conflict was associated with a reduction in years of schooling by just over a fifth of a year (Table 4, panel A). Heterogeneity analysis by gender is also presented in Table 4. For girls, the reduction was relatively higher, by 0.25 (i.e. a quarter) of a year, and for boys years of schooling was decreased by 0.16 of a year though the result was only statistically significant at marginally higher levels ($p = 0.1150$). Relative to average education accumulation for adults, this represents a decrease of 3.4% in the latest survey year. The estimated ATT_1 and ATT_0 results also

Table 3. Comparison of treated and untreated groups.

Variable	Treated d1 (mean)	Untreated matched d1 (diff)	Treated d2 (mean)	Untreated matched d2 (diff)
Age	9.66	9.76	9.64	9.67
Female child	0.4554	0.4626	0.4446	0.4381
Dalit or Adivasi child	0.3227	0.3247	0.3422	0.3199
Enrolment rate	0.8369	0.8745	0.8592	0.8586
Father's education				
Some years of primary school	0.2571	0.2634	0.3955	0.3913
Completed primary school	0.2308	0.2433	0.2132	0.2300
Completed secondary school	0.0954	0.1019	0.0691	0.0711
Mother's education				
Some years of primary school	0.2674	0.2643	0.3980	0.3983
Completed primary school	0.2149	0.2304	0.2032	0.2248
Completed secondary school	0.1057	0.1084	0.0766	0.0783
Household size	6.76	6.52*	6.67	6.55
Log of per capita expenditures	6.1249	6.2289**	6.0696	6.1201*
District economic activity rate	0.6921	0.6764**	0.6735	0.6610*
Urban residence	0.2937	0.3697**	0.3464	0.3602
Mean bias after matching: d1= 6.3%. d2= 2.3%.				
Rubin's B test statistic (d1): Unmatched= 71.1. Matched= 26.1.				
Rubin's B test statistic (d2): Unmatched= 122.6. Matched= 13.0.				
Rubin's R test statistic (d1): Unmatched= 0.90. Matched=0.60.				
Rubin's R test statistic (d2): Unmatched= 0.59. Matched= 0.94.				

Asterisks indicate statistically significant difference from treatment group: *p<0.05, **p<0.01.

Table 4. Average treatment effect on treated, 5-nearest neighbour matching method.

Model	Gender	$\widehat{ATT}_1$ (SE)	$\widehat{ATT}_0$ (SE)	$\Pr(D_1=1 \mid D_2=1)$ (SE)	$\widehat{ATT}$ (SE)
A: Any fatality	All	−0.1840	−0.2290	0.5471***	−0.2044***
		(0.1130)	(0.0910)	(0.0037)	(0.0688)
	Girls	−0.3091	−0.1776	0.5471***	−0.2495**
		(0.1578)	(0.1282)	(0.0037)	(0.1050)
	Boys	−0.0964	−0.2376	0.5471***	−0.1605
		(0.1744)	(0.1061)	(0.0037)	(0.1018)
B: Any civilian fatality	Girls	0.0710	−0.7045***	0.6650***	−0.1886
		(0.1641)	(0.1814)	(0.0039)	(0.1203)
	Boys	0.2696	−0.7367***	0.6650***	−0.0674
		(0.1750)	(0.1801)	(0.0039)	(0.1333)
C: Any terrorist fatality	Girls	−0.8654***	0.0018	0.4945***	−0.4270***
		(0.2153)	(0.1608)	(0.0046)	(0.1294)
	Boys	−0.7009***	−0.1431	0.4945***	−0.4191***
		(0.2363)	(0.1597)	(0.0046)	(0.1427)

standard errors in parentheses, estimated using nonparametric bootstrap with 100 replications; **p<0.10, **p<0.05, ***p<0.01.*

confirm this negative relationship, which is high for ATT_0 overall and for boys, though generally lacking statistical significance. In contrast, ATT_1 is relatively higher for girls. Recall from Table 4 that ATT_0 involves areas that were conflict affected in the last year compared to areas that were not conflict-affected at all. In contrast, ATT_1 involves the calculation of areas that were conflict-affected in both periods compared to areas that were conflict affected only in the first year.

These results suggest that the chronic presence of conflict has a particularly negative impact on years of schooling for girls. This builds on budding literature pointing to the particularly low welfare of households in areas of chronic conflict (Corral et al. 2020), by providing empirical evidence on this relationship and highlighting the wider multidimensional deprivations that emerge in these areas, especially for girls. It is different to research in Nepal on the Maoist conflict, which pointed to increased school attendance of girls (Valente 2014). The differences would suggest that a unitary view of the Maoist movement in Nepal and India is inadequate (Bownas 2003; Nayak 2008), and instead a country-specific or subnational approach to analysing the consequences of the movement would be better suited.

We additionally explore the ATT using alternative measures of treatment to the above: 1) a binary equal to one for districts that had any civilian fatalities in the year of or preceding the survey and 2) a binary equal to one for districts that had any terrorist fatalities over the same timeframe. Results presented in Table 4 (panels B and C) continue to display a negative ATT. For terrorist fatalities, there is a particularly large decrease in girls' years of schooling measured by ATT_1, suggesting that this mechanism is driving the result discussed above where the chronic presence of conflict has especially negative impacts for girls' education. This is intuitive if the presence of terrorist casualties might reflect increased insecurity associated with an increased presence of both Naxal and police forces. Indeed, the government's early response to Naxal violence has been a policy and security-oriented approach, which was then complemented with attempts to address development issues in historically marginalised areas of left-wing extremism (HRW 2008, 2009).

In contrast, ATT_0 indicates a relatively larger decline in years of schooling amongst boys, which may be the case if new-onset conflict contributes to out-migration of boys to search for work in safer contexts, thus limiting years of schooling. This increase in child labour in conflict-affected areas is reflected in the wider literature. In Columbia, Rodriguez and Sanchez (2012) observe that conflict affects older children to drop out of school and enter the labour market. Similar findings are also observed in Iraq especially for economic labour (Naufal, Malcolm, and Diwakar 2019) and amidst high-intensity conflict between Israel and the West Bank (Di Maio and Nandi 2013). Our results build on this literature by suggesting that this relationship is particularly pronounced for boys in areas of new-onset conflict.

Threats to Identification: Selective Migration and Other Conflict Dynamics

The relationships above may be affected by unobserved variables, despite our attempts to mitigate this as noted earlier. One such variable could be selective migration, whereby wealthier families may respond to an increase in conflict by migrating to safer areas to continue education, thus having older children who are more likely to be enrolled and thus complete more years of education. The per capita expenditure control would be inadequate in capturing the effect of migration. In this case, the ATT would overestimate the effect of conflict on years of schooling in conflict-affected areas.

To explore this hypothesis and its impact on the results presented above, the PSM is modified to include a covariate capturing household heads who have lived in the same area since the onset of violence with the merger of the Communist Party of India-Maoist in 2004, and separately also with household heads who have lived in the same area all their lives. The ATT again provides similar sign and directionality (Table 5, panel A). Though the results still do not account for households where families may have died in the conflict, these additional checks suggest that selective migration does not drive the results.

Table 5. ATT considering migration and other conflict, 5-nearest neighbour matching method.

Model	Status	$\widehat{ATT}_1$ (SE)	$\widehat{ATT}_0$ (SE)	$Pr(D_1=1 \mid D_2=1)$ (SE)	$\widehat{ATT}$ (SE)
A: Migration	Live in same area since 2004	−0.1614	−0.2266***	0.5469*	−0.1910***
		(0.1142)	(0.0867)	(0.0037)	(0.0691)
	Live in same area whole life	−0.1573	−0.2364***	0.5469***	−0.1932***
		(0.1179)	(0.0844)	(0.0037)	(0.0724)
B: Other conflict	Without other conflict states	−0.2206*	−0.2009**	0.5469***	−0.2118***
		(0.1329)	(0.0982)	(0.0037)	(0.0819)
	Other conflict covariate	−0.2206*	−0.1850**	0.5469***	−0.2045*
		(0.1329)	(0.0937)	(0.0037)	(0.0842)

standard errors in parentheses, estimated using nonparametric bootstrap with 100 replications; **p<0.10, **p<0.05, ***p<0.01*.

Another concern is that there are conflicts in other parts of India which may understate the impact of Naxal violence if treated children are compared to untreated children in areas suffering from other violence. This includes terrorism in the hinterland, insecurity in Jammu and Kashmir, and insurgency in the North Eastern States (SATP 2017). As a sensitivity test, we remove affected states in one specification, and in another specification include affected states as a covariate into the matching model.[3] Results, presented in Table 5 (panel B), indicate that the ATT is largely unaffected, in both cases maintaining a similar sign and magnitude to the main specification.

Additional Robustness Checks: Intensity of Violence

In addition to the different measures of the treatment (Table 4), heterogeneity analysis by gender (Table 4), and the use of alternative matching methods (Table A1), we also consider the intensity of violence and how this might shape education accumulation. As a robustness check and to investigate variability within areas affected by conflict, we consider fixed effects estimators. Part of this involves examining changes in education for individuals in conflict-affected areas, rather than for individuals between areas that have experienced conflict and other areas. Entity fixed effects are important in conflict settings in relation to issues of self-selection, insofar as households that are better able to cope may be less likely to move or be displaced. If this is the case, conflict-affectedness might positively correlate to better schooling outcomes of affected cohorts and thus bias estimation results (Swee 2009). The time fixed effects capture common reasons across entities for variables to change over time. In our model:

$$Y_{ijt} = \beta_0 + \beta_1 C_{ijt} + x_{ijt}\rho + \gamma_t + \delta_i + \in_{ijt} \quad (3)$$

where outcome Y refers to years of schooling for individual i living in zone j and year t, C is the conflict fatality rate measured per 100,000 population, x is a vector of individual (whether child is enrolled, age of the child), household (household size, log of per capita expenditures), and district (area and zone of residence, district economic activity) controls, and the latter three terms refer to year fixed effects, time-invariant household-specific effects, and the idiosyncratic error term.

Table 6 presents results for the subset of children of school-going age and points to the negative relationship between an increase in the conflict fatality rate and years of schooling for girls and boys across types of conflict and subsamples. This is similar to other literature, including the few which rely on panel data, which also generally finds a negative relationship between the intensity of violence and education accumulation in India (Singh and Shemyakina 2016; Roy and Singh 2016). Finally, the relationship is again negative in areas of chronic conflict, and also with a large effect size amongst girls in areas experiencing an increasing number of terrorist fatalities, complementing the PSM results presented above.

Table 6. Conflict fatality rates and years of schooling.

	Full sample, overall fatalities		Full sample, terrorist fatalities		Chronic conflict, overall fatalities		Chronic conflict, terrorist fatalities	
VARIABLES	Boys (1)	Girls (2)	Boys (3)	Girls (4)	Boys (5)	Girls (6)	Boys (7)	Girls (8)
Fatality rate	−0.952***	−0.850***	−0.865***	−1.131***	−1.149***	−0.790***	−0.980***	−0.974***
(SE)	(0.138)	(0.123)	(0.233)	(0.204)	(0.185)	(0.165)	(0.268)	(0.236)
Constant	0.0704	−0.582	0.0365	−0.598	−4.898***	−3.792***	−5.183***	−3.925***
(SE)	(0.341)	(0.424)	(0.342)	(0.425)	(1.108)	(1.190)	(1.092)	(1.199)
R-squared	0.801	0.837	0.801	0.837	0.787	0.840	0.780	0.838
Observations	33,488	26,729	33,488	26,729	1,745	1,473	1,745	1,473

OLS regression results with fixed effects estimator; conflict fatality rate is measured per 100,000 people; all models include household and time controls; robust standard errors in parentheses; *p<0.10, **p<0.05, ***p<0.01.

Table 7. Conflict fatality rates and household spending on school fees.

	Full sample, overall fatalities			Chronic conflict, overall fatalities		
VARIABLES	Girls (1)	Boys (2)	Both (3)	Girls (4)	Boys (5)	Both (6)
Fatality rate	−0.379	0.249	0.670***	−0.244	0.535**	0.748**
(SE)	(0.261)	(0.181)	(0.236)	(0.396)	(0.233)	(0.304)
Constant	−1.398**	−1.671***	−1.843***	−3.071	−2.351	−2.022
(SE)	(0.611)	(0.367)	(0.410)	(2.799)	(1.459)	(1.849)
R-squared	0.239	0.334	0.280	0.226	0.228	0.235
Observations	10,002	26,300	24,080	568	1,346	1,313

OLS regression results with fixed effects estimator; conflict fatality rate is measured per 100,000 people; all models include individual, household, area and time controls; robust standard errors in parentheses; *p<0.10, **p<0.05, ***p<0.01.

While the statistically significant estimates from these specifications appear large, only a subset of observations contribute to the estimation of the coefficient of interest in the regressions with fixed effects estimators, given that these rely on within-unit variation (Mummolo and Peterson 2018). In the merged dataset, only 2,689 children (8.9% of the total sample) lived in districts that were conflict affected in only one of the two survey years, while this number increases to 4,298 children (14.3% of the total sample) when exploring districts that had different conflict fatality rates between the survey years. Even so, the directionality supports the negative relationship more generally.

Possible Transmission Mechanisms

As noted in Section 2, there are demand- and supply-side channels that may affect children's schooling amidst Naxal violence. We investigate potential mechanisms in this section. Demand-side channels are examined through education expenditures, given literature that confirms a shifting priority towards boys' education amidst other armed conflict in India. Similar to Singh and Shemyakina (2016), we examine the relationship between conflict and household investments in education depending on the gender composition of children in the household. We take this a step further to then show how these investment decisions are correlated with years of schooling. We rely on OLS regressions with controls similar to equation 3. The dependent variable is presented as the log of household expenditures.

Results of the first step are presented in Table 7, disaggregated by whether the household has only girls, only boys, or both boys and girls of primary school-going age, based on their age in the baseline survey. The findings point to an increase in conflict intensity being associated with an increase in education expenditures in areas of chronic conflict within households with all boys of school-going age (Table 7, column 5), or a mix of boys and girls (Table 7, column 6). However, in both the full sample (Table 7, column 1) and amidst areas of chronic conflict (Table 7, column 4), there is a negative though statistically insignificant relationship between the intensity of violence and changes in expenditures on school fees over time for households with only girls. In spite of this, the relationship between spending on school fees and years of schooling is positive and statistically significant across sub-samples, as observed in Table 8. Together, these results reinforce the penalty in human capital investments and outcomes that girls in particular experience amidst chronic conflict.

Finally, in terms of supply side, we consider the potential of conflict-induced change in school infrastructure, using IHDS data comprising information on up to two schools in selected villages of the survey that we pool across the two survey years. Though the school module data is not nationally representative, it offers insights into the relationship between conflict and infrastructure, which we embed in the evidence from secondary literature. In our model, we regress school infrastructure variables on the locality-level fatality rate, controlling for school, district and time fixed effects:

$$Y_{jt} = \beta_0 + \beta_1 C_{jt} + x_{jt}\rho + \gamma_t + \epsilon_{jt} \tag{4}$$

Table 8. Spending on school fees and years of schooling.

VARIABLES	Girls	Boys	Both
Spending on school fees	0.102***	0.113***	0.114***
(SE)	(0.0101)	(0.00704)	(0.00728)
Constant	−0.224	0.601	−0.759
(SE)	(0.668)	(0.374)	(0.506)
R-squared	0.844	0.796	0.830
Observations	9,987	26,269	24,052

OLS regression results with fixed effects estimator; conflict fatality rate is measured per 100,000 people; all models include individual, household, area and time controls; robust standard errors in parentheses; *p<0.10, **p<0.05, ***p<0.01.

Table 9. Conflict fatality rates and presence of school infrastructure.

VARIABLES	Electricity	Toilet	Toilet, girls	Toilet, boys	Chairs and desks
Fatality rate	−0.204***	−0.0711**	−0.118***	−0.130***	−0.0755**
(SE)	(0.0264)	(0.0314)	(0.0306)	(0.0361)	(0.0370)
Observations	7,955	7,963	6,304	6,287	7,789
Pseudo R-Squared	0.3048	0.1556	0.0697	0.0735	0.2223

Probit regression results, marginal effects presented; conflict fatality rate is measured per 100,000 people; all models include area and time controls; robust standard errors in parentheses; *p<0.10, **p<0.05, ***p<0.01.

where Y refers to: whether the school has electricity, whether the school has toilet facilities (overall and specific to girls or boys), and whether the school has chairs and desks for all students. These reflect common proxies for the presence of school infrastructure in the wider literature. Again, C is the conflict fatality rate measured per 100,000 population, while x is a vector of school (whether the school is co-ed, whether it is a government school), and area (zone and area of residence, security-related expenditures by state, district economic activity and average per capita expenditures) controls, and the latter two terms again refer to year fixed effects and the idiosyncratic error term.

Results are presented in Table 9, and indicate that an increase in conflict fatalities is associated with a lower probability that schools have electricity, toilets, and chairs and desks for all students. This result is in line with the wider reported evidence of Naxal attacks and responding policy violence that in some cases destroyed infrastructure including schools (HRW 2009). For example, in 2009 alone, at least 50 schools were attacked in Jharkhand and Bihar, when at least 20 schools in these states were blown up during the conflict in the first half of the year (UNESCO 2010). In addition, Naxals were also observed to intimidate local authorities and prevent contractors from coming into conflict-affected areas, thus preventing the development of schools and wider infrastructure (Eynde et al. 2015).

Conclusion

This paper makes an important contribution to research on armed conflict and education. It explores the relationship between Naxal violence in India and education through the use of longitudinal data, and relies on multiple measures of armed conflict to capture differences in the type and duration of conflict across areas. The focus is on conflict trajectories, rather than the presence or absence of conflict at a given point in time. The analysis indicated that while there is a decrease in years of schooling in conflict-affected areas for boys and girls, the relative decrease tends to be larger for girls, reinforcing the gender penalty in a ‘country of first boys’ (Sen 2015). The associated reduction in years of schooling for girls is particularly large in areas of chronic conflict. Results are stable and statistically significant when relying on a range of PSM estimators and falsification exercises. The negative directionality moreover persists whether conflict is measured as a binary capturing area that had any presence of conflict fatality, when measured in terms of areas that had any civilian or any terrorist fatality separately, and when employing fixed effects estimators.

Certain transmission mechanisms are explored to explain the findings. First, conflict is associated with higher spending on tuition only for boys or in mixed households in areas of chronic conflict. Instead, there is a negative albeit statistically insignificant relationship between conflict and education expenditures in households with only girls amidst chronic conflict. Second, there is suggestive evidence of a negative relationship between conflict and school infrastructure, which is further reinforced by the wider literature on school attacks amidst Naxal violence. Together, results suggest that Naxal violence has a negative impact on years of schooling by affecting both demand- and supply-side mechanisms of school access.

The analysis offers insights on designing effective policies to limit the negative consequences of conflict on education. It suggests that strategies should consider conflict trajectories. Moreover, these strategies should not be limited to fragile states, but extend to countries like India where conflict remains a subnational concern within its borders. Learning from neighbouring countries and similar conflict ideologies is useful, but results from our analysis in India compared to other research on the Maoist conflict in Nepal suggest that this should not substitute for country- and context-specific understanding of conflict that may lead to differential outcomes. Compared to other research on Naxal violence in India, too, results in this analysis suggest that there may be differences depending on the education outcomes explored, further reflecting the complex pathways of impact due to conflict. Finally, policy responses should account for both supply- and demand-side factors that might affect education accumulation. In these responses, consideration of the heterogeneity in school accumulation amidst conflict, especially by gender and conflict trajectories, is needed to prevent widening the gender differential in schooling.

Notes

1. This study relies on panel design weights suggested by the survey data providers, given that an attrition analysis provided weights that did not result in any significant differences in descriptives and outcomes.
2. Rubin's B statistic is the absolute standardized difference of means of the linear index of the propensity score in treated and matched untreated groups. Rubin's R is the ratio of treated to matched untreated variables of the propensity score index.
3. Other than Jammu and Kashmir, Assam, and Punjab, these are smaller populations with generally under 4 million people (Meghalaya, Tripura, Manipur, Mizoram, Nagaland, and Arunachal Pradesh).

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Annex

Alternate matching methods and estimators We run sensitivity tests using alternative matching methods. In particular, we test the sensitivity of results using an 0.05 bandwidth for the Kernel estimator. We also use two radii (0.005 and 0.01) given that different choice of radii will affect the number of cases adhering to the common support requirement. Results from these sensitivity checks are presented in Table A1 (panels A to C), and largely mirror the earlier results obtained through the five nearest neighbour matches. Results from the doubly robust estimation are also presented in Table A1(panel D) and have the expected directionality, suggesting that the equations are likely to be correctly specified across models. Results indicate a negative relationship between conflict and education that results in a large decline in years of schooling for girls (0.32 of a year) and less of a decline for boys (0.25 of a year) relative to their respective means. The effect sizes again are largest for girls in areas affected by chronic conflict compared to areas that were conflict affected only in the first year.

Table A1. Alternate matching estimators and doubly robust estimation.

Model	Gender	$\widehat{ATT_1}$ (SE)	$\widehat{ATT_0}$ (SE)	$Pr(D_1=1\|D_2=1)$ (SE)	$\widehat{ATT}$ (SE)
A: Kernel (0.05 bandwidth)	Girls	-0.3157**	-0.1780*	0.5471***	-0.2533***
		(0.1322)	(0.1037)	(0.0037)	(0.0861)
	Boys	-0.0929	-0.2614***	0.5471***	-0.1693**
		(0.1450)	(0.0892)	(0.0037)	(0.0846)
B: Radius (0.01 caliper)	Girls	-0.3033*	-0.1871***	0.5471***	-0.2506***
		(0.1567)	(0.1030)	(0.0037)	(0.0921)
	Boys	-0.1164	-0.2464*	0.5471***	-0.1753**
		(0.1505)	(0.0874)	(0.0037)	(0.0868)
(0.0868)C: Radius (0.005 caliper)	Girls	-0.3055*	-0.1851***	0.5471***	-0.2509***
		(0.1642)	(0.1037)	(0.0037)	(0.0965)
	Boys	-0.1229	-0.2437*	0.5471***	-0.1777**
		(0.1545)	(0.0846)	(0.0037)	(0.0874)
D: Doubly robust estimator	Girls	-0.3646***	-0.2583*	0.5471***	-0.3164***
		(0.1238)	(0.1538)	(0.0037)	(0.0984)
	Boys	-0.4486***	-0.0155	0.5471***	-0.2525**

Note: standard errors in parentheses, estimated using nonparametric bootstrap with 100 replications; *p<0.10, **p<0.05, ***p<0.01