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# **Impacts of violent extremism on educational content**

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# Impacts of violent extremism on educational content

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## Abstract

This paper examines whether and how Islamist violence affects educational content in secondary schools in Burkina Faso. We combine geocoded conflict-event data with nationwide school census data containing detailed information on teachers' subject-specific teaching hours and mobility across schools. Using a staggered difference-in-differences design, we estimate the dynamic effects of violence on school availability and instructional time. We find no change in total hours of core subjects following local conflict, but evidence of increased Arabic instruction. We also document effects on teacher retention. Following local episodes of Islamist violence, teachers become more likely to move to schools in other municipalities, with the strongest mobility responses observed among those teaching French and mathematics. These findings suggest that violent extremism can reshape educational content even without formal curriculum reform, through changes in school availability, the allocation of instructional time, and the composition of the teaching workforce.

**Key words:** Conflict; violence; education; schooling; Sub-Saharan Africa; Sahel; Burkina Faso

**JEL codes:** D74; I20; I25; O12; O55

## 1 Introduction

An estimated 103 million children worldwide are out of school due to conflict and insecurity (Save the Children, 2024). Yet violent conflict may affect education not only by preventing children from attending school, but also by affecting the quality and content of education. Previous research has examined how conflict affects enrollment, attendance and learning outcomes (e.g., Akresh and de Walque 2008; Bertoni et al. 2019; Brük et al. 2019 and Burde et al. 2019), as well as changes in curriculum and textbook content following regime change (e.g., Cantoni and Yuchtman 2013; Cantoni et al. 2017). However, less is known about how

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non-state armed actors with distinct ideological agendas influence educational content during ongoing conflict.

This paper investigates whether and how Islamist violence affects educational content in secondary schools in Burkina Faso. We focus on a specific and, to our knowledge, previously unstudied margin: the allocation of instructional time across subjects. In the Burkinabè context, armed Islamist groups have explicitly opposed what they refer to as Western education (Benedikter and Ouedraogo 2017) and, in some cases, demanded greater emphasis on Arabic and Islamic learning (HRW 2020). This makes Burkina Faso a particularly relevant setting in which to study whether violent extremism changes not only the availability and quality of education, but also the educational content.

We examine three channels through which violence may alter instructional content. First, conflict may change the school supply landscape if certain types of schools are more likely to close or if newly opened schools differ in their curricular profile. Second, violence may affect teacher retention and recruitment, thereby changing the subject-specific teaching capacity available to schools. Third, schools that remain open may adapt their instructional plans in response to insecurity, either to accommodate the preferences of Islamist groups or to reduce perceived exposure to attack. Together, these channels imply that conflict may reshape educational content both through changes in which schools operate and through adjustments within surviving schools.

Our analysis combines nationwide secondary school census data for the academic years 2014/15 to 2021/22 with geocoded conflict-event data from ACLED. The school census contains unique detailed information on teachers, including their weekly teaching hours by subject. In addition, we can track the movement of individual teachers across schools over time using their teacher certificate numbers.

We estimate dynamic treatment effects of Islamist violence on the availability of different types of schools and on school level changes in instructional time using a staggered difference in differences design. We account for likely spillovers by excluding municipalities that are especially likely to receive displaced teachers and pupils from conflict-affected areas.

We find no change in total hours of instruction in core subjects in schools that remain open following local episodes of Islamist violence. However, results show an increase in Arabic instruction. While the increase in absolute terms is a modest 0.2 hours per week and class, it represents a 50 percent increase relative to the very low pre-conflict baseline. Schools opening

in conflict-affected municipalities after the onset of violence, though lacking a clear counterfactual, offer even more Arabic instruction, 2.5 times more than what schools in the same municipalities offered before the first Islamist violence event, although total hours of instruction are lower.

Furthermore, we show that Islamist violence affects teacher retention. Teachers become more likely to move to schools in other municipalities, with the strongest mobility responses observed among teachers whose main subjects are French and Mathematics.

Taken together, our results suggest that violent extremism can reshape educational content even in the absence of formal curriculum reform. These effects operate through a combination of impacts on instructional hours across subjects in schools that remain open, school closures, selective school openings, and changes in the composition of the teaching workforce.

This paper contributes to the literature on the educational impacts of violent conflict (e.g. Akresh and de Walque, 2008; Justino, 2012; Di Maio and Nandi, 2013; Buvinic, 2014; Verwimp and Van Bavel, 2014; Burde et al, 2016; Noury and Speciale, 2016; Khan and Seltzer, 2016; Brück et al., 2019; Kazibwe, 2025, Bertoni et al., 2019, Alfani and Görlach, 2023: 2024) by moving beyond school access and quality to examine how insecurity affects instructional content. Moreover, it complements the literature on how governments shape curriculum and education content (Cantoni & Yuchtman, 2013) and on changes in curricula and textbooks after conflict-induced regime change (Cantoni et al, 2017) by studying impacts on effective education content in the absence of regime change or formal educational reforms.<sup>5</sup>

## **2. Background**

### **2.1 Secondary education in Burkina Faso**

Secondary education in Burkina Faso is divided into two levels, Junior High School (enseignement post primaire general), spanning grades 7 to 10, and Senior High School, (enseignement secondaire general), from grade 11 to 13, where students can choose the academic track or a vocational track. Historically, secondary schooling was first introduced by Christian missionary institutions some of which continue to operate today.<sup>6</sup> The education system is now dominated by public schools, with private non-confessional schools constituting

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<sup>5</sup> In 2022 there were two military coups in Burkina Faso, which did lead to regime change, but this is not studied here and not covered by the period for which we have data.

<sup>6</sup> Among the schools still operational, and therefore in the school census data, the oldest one is a Catholic school that opened in 1923.

the second largest category. While there is a long tradition of Muslim primary schools, Muslim secondary schools remain relatively rare reflecting the later development of this school type compared to primary-level Muslim education. Secondary education in Burkina Faso has expanded substantially over the past two decades, although access remains limited and unevenly distributed across regions (UNOCHA 2023).

The official language of instruction is French. The core curriculum includes languages (French and usually English), Mathematics, Sciences, History/Geography, and Physical Education. While all schools are required to follow the national curriculum, there exists variation in exact instructional hours across schools and across school types. For example we see in the school census that Muslim schools tend to allocate more time to Arabic while public schools typically devote little or no time to this subject. This institutional feature in the education system is central to our analysis, as it implies that changes in the composition of school types or within school adjustments may translate into shifts in instructional content.

The expansion of secondary schooling accelerated around 2010 but has been disrupted in recent years by increasing insecurity (HRW 2021). School openings and closures as well as constraints on teacher recruitment and retention, have become key features of the education landscape in Burkina Faso (Congdon Fors et al., 2026) and other conflict-affected areas in the region (Bertoni et al. 2019).

## 2.2 The conflict

Over the past decade, sub-Saharan Africa has experienced a surge in Islamist violence. The Sahel region is the epicentre of this development, with Mali, Niger and Burkina Faso being particularly hard-hit (International Institute for Strategic Studies, 2023). Before the people's social uprising and mobilization in late 2014, which resulted in a revolution ousting an authoritarian leader and prevented a military coup in 2015, Burkina Faso had not experienced widespread violence. In December 2015, the country carried out a democratic election. However, from 2015 and onwards, jihadist groups active in Mali were pushed across the border into Burkina Faso (Benedikter & Ouedraogo, 2017, [HRW, 2021](#)). These groups proceeded to establish a foothold in Burkina Faso, despite the fact that the country had no previous problems with domestic religious extremists and was not a focus for violent extremist groups wishing to recruit (Turse, 2020; HRW, 2021). Since 2016, Burkina Faso has seen a sharp increase in Islamist attacks in the northern, eastern and southwestern parts of the country, but also in the capital Ouagadougou.

A distinctive feature of this conflict is the explicit targeting of the education system. Armed groups have attacked schools, threatened teachers, and discouraged attendance, often invoking opposition to “Western-style education” (HRW, 2020). The first recorded school attacks took place in 2017, after which armed Islamist groups have “burned, looted, and destroyed scores of schools, [...] intimidated students, terrorized parents into keeping their children out of school, and killed, abducted, brutalized, or threatened scores of teachers” (ibid, p. 1). According to HRW, assailants typically justify the attacks by citing their opposition to western education, demanding that children should study only Arabic and the Quran, or not study at all. The attacks, and the fear and insecurity they generate, have resulted in a massive number of school closures across the country.

Beyond these direct effects, the ideological stance of violent extremist groups suggests that conflict may also influence the content of education. Schools operating under threat may adjust instructional practices in response to perceived risks or local pressures. This could involve shifts away from subjects associated with Western education such as French or English, and towards subjects such as Arabic. In addition, differential closure and opening of schools across types may change the composition of education provision (see Congdon Fors et al., 2026, for an overview), further affecting the distribution of instructional time across subjects. These features of the education system and the violent conflict setting in Burkina Faso guide our empirical strategy, which we describe next.

### **3. Data and empirical strategy**

In this section, we describe the data and empirical strategy used to investigate whether and how Islamist violence affects educational content in secondary schools in Burkina Faso.

#### **3.1 Data**

Our primary data source is the annual school census for Burkina Faso, covering academic years 2013/14 to 2021/22. The census includes all registered secondary schools and provides detailed information on school characteristics, staffing, and teaching assignments. In the academic year 2021/22 the school census includes 5,750 schools, of which 5,603 can be geographically matched to a municipality.

An important feature of the school census data is the availability of teacher level information, including weekly teaching hours by subject. For a subset of teachers, the data include a unique certificate identifier, which allows us to track teachers over time and across

schools. This enables the construction of a teacher panel that we use to study retention, mobility, and subject-specific transitions. In addition, the panel predates the 2015-2016 spread of extremist Islamic groups to Burkina Faso from Mali. Regarding the composition of religious and non-confessional schools, the data includes information on the school's religious profile, if any.

To measure conflict exposure, we use event-level data from the Armed Conflict Location and Event Data (ACLED) project (Raleigh et al., 2010). This data builds on reports from government institutions, local media, humanitarian agencies and research publications, and includes information on the date and exact location (longitude and latitude) of the conflict event, the type of conflict event (battles, explosions, violence against civilians, protests etc.), the groups involved, and the number of reported fatalities (ACLED, 2021). We focus on events attributed to Islamist armed groups.

We merge the school census and conflict data at the municipality level. Our analysis focuses on 350 municipalities, of which 194 do not experience Islamist violence during the study period.

We consider three sets of outcomes. The first focuses on changes in the supply of education in terms of availability of schools by school type – public, private non-confessional, Christian<sup>7</sup> or Muslim. We construct municipality level measures of the number and share of open schools by type each academic year as well as the number of schools that open and close each academic year by type.

Second, to measure educational content, we use school-level instructional hours by subject. The census includes hours of instruction by subject for each teacher. In addition to information on core curriculum subjects several schools report teaching in a wide range of practical subjects such as agriculture, broderie and mechanics. It also reports teaching in languages other than French and English, for example German, Arabic or local languages. Religion is not a separate subject. Insofar as there is any religious teaching at schools this is not recorded in the census. To measure a possible influence of Islamist groups on teaching we consider the main “Western” languages, French and English, and Arabic. We also consider, Mathematics, since this is often considered a key subject, and total hours. The census reports teaching hours per teacher and week. We aggregate the teaching hours to the school level. Our preferred measure is teaching hours per week and class, since this most closely corresponds to the number of teacher-led school hours of a student. However, we do not have number of

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<sup>7</sup> We combine Catholic and Protestant schools into a common Christian category.

classes in the school for the first and last year (2013/14 and 2021/22), we therefore complement this measure with teaching hours per 100 students in the school. Since number of students per class may be endogenous to violence this does not necessarily estimate the impact on teacher-led hours of a student, however, since it would also capture possible changes in class size, it may be a better reflection of “quality hours”. It should be noted that reported teaching hours are likely to reflect the number of hours the teacher should teach a subject according to schedule. Thus, we are not capturing possible impacts of violence on deviations from the planned schedule. Table A1 shows teaching hours per week and class by school type in i) municipalities that are not exposed to Islamist violence within our data period, and ii) before and iii) after the first Islamist violence event in municipalities that are exposed. There are sizeable differences in teaching hours between types of schools. In particular Arabic is unusual in other schools than Muslim ones.

Third, using the teacher panel, we construct measures of teacher transitions, including movement between municipalities and exits from the observed data. We analyze teacher transitions by main subject of the teacher. Table A2 shows reporting of teacher certificate by school type for schools outside of Ouagadougou. Reporting is uneven across school types, it is highest in public schools and lowest in religious schools. Importantly, reporting is not lower after violence onset (if anything it is slightly higher). If violence induces teacher migration to Ouagadougou, low reporting for other schools than public ones in the capital could bias the estimated relative effects of violence on starting a position in a different municipality versus leaving the data.

Table A3 shows descriptive statistics on teacher transitions. About 50% of teachers observed at a school in a given year stay in the same school the next year, about 10% transition to another school in the same municipality, 10% to another school in a different municipality, and 30% are not in the data.

We define treatment at the municipality level as the geographical identifiers in the school census data are at the municipality level. In line with Bertoni et al. (2019), we categorized the events depending on the aggressor and target of the attack. Specifically, we run key-word searches in the ACLED conflict event descriptions, which we triangulate against conflict event descriptions in the Global Terrorism Database (National Consortium for the Study of Terrorism and Responses to Terrorism, 2019). In our baseline setup we single out all conflict events caused by Islamic terrorists. Thus, we classify a municipality as treated from the first conflict episode and onwards and estimate dynamic (event-study) treatment effects over time.

Figure A1 shows the spread of conflict events over time across the 350 municipalities of the country. The first events were close to the Malian border and in the capital Ouagadougou. Conflict thereafter spread gradually in particular in the North and in the East. The spread of Islamist conflict to new municipalities was particularly strong in academic years 2018/19, 2019/2020 and 2021/22. As of June 2022. Most municipalities, 194, did not experience any Islamist violence.

Figure A2 displays the number of open schools of each type over time for municipalities that were affected by Islamist violence during the study period and municipalities that were not.<sup>8</sup> Public schools dominate the secondary school system, while there is also a substantial share of private non-confessional schools,<sup>9</sup> and relatively few religious schools, in particular Muslim. The number of open schools has expanded over the period, in line with efforts to increase secondary education in the country. In violence affected municipalities the number of open public schools decrease after 2018/19.<sup>10</sup> Table A4 shows descriptive statistics on open schools with one observation per school and year.

### 3.2 Empirical strategy

To identify causal effects, we exploit the staggered expansion of violent Islamist conflict across municipalities over time. Since violence escalates over the period, our baseline specification estimates dynamic treatment effects using a staggered difference-in-differences model following Callaway and Sant’Anna (2021). We define treatment at the municipality level as the first occurrence of Islamist violence. This event-study framework allows us to trace the evolution of outcomes before and after the onset of violence, while accommodating treatment effect heterogeneity across cohorts.

A central challenge in estimating the effects of conflict is the presence of spillovers. Violence may affect not only directly exposed municipalities but also nearby or connected areas through migration of teachers and students or through perceived insecurity. To address this, we exclude from the control group (i) municipalities sharing a border with conflict-affected areas and (ii) municipalities that are likely destinations of teacher migration from

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<sup>8</sup> From the year 2019 there is recorded data on schools being closed. We also categorize a school as closed if it is either not in the census in a given year despite being in the census in a previous year or if there are no recorded teachers at the school.

<sup>9</sup> Private non-confessional schools are heavily concentrated in the capital, Ouagadougou, where they constitute 78 percent. Outside of the capital, non-confessional private schools make up around 35 percent of secondary schools.

<sup>10</sup> From school year 2017/18 there are municipalities without any open secondary schools: 4 in school year 2017/18, 12 in the next year, 15 in year 2019/20, 31 in year 2020/21 and 50 in 2021/22.

affected areas based on historical migration patterns, using similar estimation strategy as Congdon-Fors et al. (2026). In essence we use a doughnut estimator (Butts, 2021) only that the excluded municipalities in the doughnut are not necessarily only the nearest ones. The panel on teachers with certificates allow us to empirically determine which municipalities are likely destinations of migrants from violence affected municipalities. The predictive power of historical migration patterns for migration flows widely outperforms geography-based measures. Since the number of teachers and therefore observed migrants in the school census is much lower in secondary than in primary schools, we use observed migrant patterns in both secondary and primary schools to classify municipalities as expected destinations. Details on the classification of expected destination municipalities are in Appendix B. The omission of spillover municipalities from the estimation sample will have obvious and intended impacts on the control municipalities used to identify treatment effects. It will also have impacts on the treated municipalities used to identify treatment effects. Since dynamic treatment effects are based on comparison of outcomes to those in the period before treatment, municipalities that are spillover municipalities before the first event will not be used, since their outcomes without any treatment (direct or through spillovers) cannot be observed in the period before the first event. Pre-trends are estimated comparing outcomes in two consecutive periods for municipalities that eventually will be treated using the never treated as a control group. Hence the event time minus one effect may differ from zero and has a standard error.

For school supply and instructional hours in surviving schools (meaning schools that were established before violence onset and remain open), we estimate a series of 2x2 difference-in-difference regressions. For school supply outcomes (number of schools of a type closing, number of schools of a type opening and share of schools being of a type) these are:

$$y_{mgt} = \alpha + \beta_{jg}D_{mg,t-j} + \theta_m + \tau_t + \varepsilon_{mt}$$

where  $y_{mgt}$  is outcome  $y$  for municipality  $m$  belonging to treatment cohorts  $g \in (2015/16, \dots, 2021/22)$  at time  $t$ .  $j \in (\underline{j}, \dots, \bar{j}]$  is time in relation to the first event,  $g$ .  $D$  is a treatment indicator.  $\theta_m$  and  $\tau_t$  are municipality and time fixed effects. Standard errors are clustered at the municipality level. We report the aggregate dynamic effects, i.e. event study type estimates,

$$\beta_j = \sum_g 1\{g + j \leq \bar{t}\}w_{jg}\beta_{jg},$$

where the weight  $w_{jg}$  is proportional to the sample size used to estimate each  $\beta_{jg}$ , that is, larger treatment cohorts receive a higher weight.  $\bar{t}$  is the last academic year in the data. Note that the composition of the treated and control group is more inclusive closer to the event time, while the dynamic effects are based on fewer underlying 2x2 comparisons as we move further away from the event time. We estimate effects up until three years after the first Islamist violence event ( $\bar{j} = 3$ ) and pre-trends from four years before treatment ( $\underline{j} = -4$ ). We do not estimate effects further away from the event time since both the treated and the control groups become small and estimates very imprecise.

The same method is used to estimate dynamic effects on instructional hours in different subjects in schools that remain open after violence onset, only that the unit of observation is at the school level and municipality fixed effects are replaced by school fixed effects.

For newly opened schools that did not exist before violence onset it is hard to establish a credible counterfactual without violence. Their opening may be endogenous to violence and the resulting closure of other schools. Since we cannot use any empirical method that uses before after comparisons of the same schools, we use a simple descriptive approach where teaching hours in these newly opened schools are compared to teaching hours in schools in the same municipalities before violence onset, a mix of schools that will eventually close down or remain open after violence onset. The estimation equation is given by:

$$y_{smt} = \alpha + \beta After_{mt} + \theta_m + \varepsilon_{st}$$

where municipality fixed effects  $\theta_m$  ensures that we compare with instructional hours before violence in the same municipality and standard errors are clustered at the municipality level.

We also estimate teacher retention using teacher-level panel data with teacher and municipality fixed effects, focusing on differences across subject specialization. If teachers specializing in a specific subject are likelier than other teachers to leave schools in violence affected municipalities this may affect both the quantity and quality of teaching in the subject. Our initial hypothesis is that English teachers may have been viewed as representing Western values and therefore targeted by Islamist groups, and that they should therefore leave more than other teachers. Since we do not have many observations on the same teacher over several years we use a teacher – municipality fixed effects model to estimate an average effect of violence exposure on teacher transitions. Four types of transition between year  $t$  and the following year ( $t+1$ ) are possible: 1) remain in the same school; 2) work in a different school in the same municipality, 3) work in a new school in a different municipality; 4) be absent from the data.

We estimate the following equation for each outcome,  $y_{imt+1}$ , against the three alternative outcomes combined.

$$y_{imt+1} = \alpha_0 + \theta_1 Viol_{mt} + \theta_2 Viol\_nb_{mt} + \theta_3 Viol\_nb\_nb_{mt} + \theta_4 Viol_{mt} * subject_i + \theta_5 Viol\_nb_{mt} * subject_i + \beta X_{imt} + \gamma_i + \delta_m + \pi_t + \varepsilon_{imt},$$

where  $Viol_{mt}$ ,  $Viol\_nb_{mt}$  and  $Viol\_nb\_nb_{mt}$  are binary indicators of any violence in municipality  $m$ , in municipalities with a shared border to  $m$  and in municipalities with a common neighbor (i.e. municipalities that have a shared border with a neighbor to municipality  $m$  but not with municipality  $m$  itself).  $subject_i$  is a binary indicator equal to 1 for teachers with a specific main subject.  $X_{imt}$  control for number of schools teacher  $i$  works at in year  $t$ , the type of school (Public, Private non-confessional, Muslim, or Christian), and whether the teacher has a permanent contract in year  $t$ . Teacher fixed effects,  $\gamma_i$ , control for observed and unobserved fixed teacher characteristics, municipality fixed effects,  $\delta_m$ , control for observed and unobserved time-constant differences between municipalities. Standard errors are clustered at the municipality level

## 4. Results

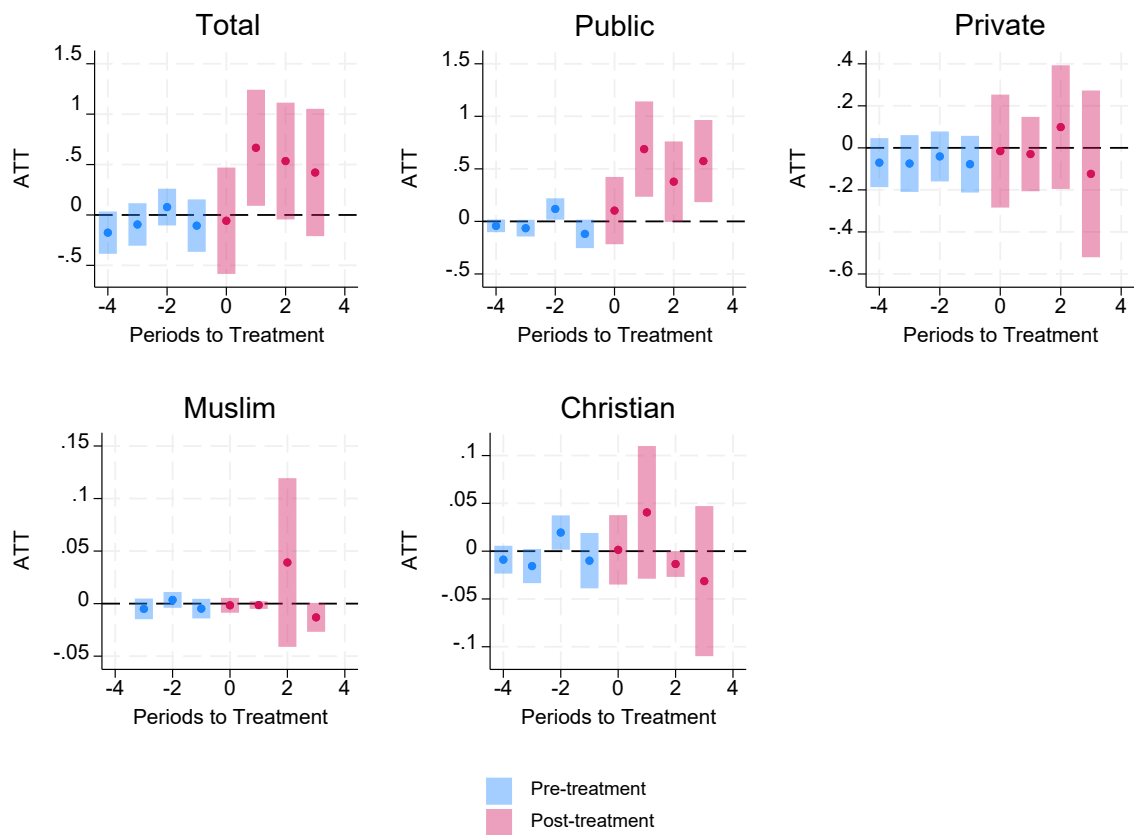
This section presents the main results on how Islamist violence affects the structure and content of secondary education. We begin with impacts on the school supply landscape, before looking at instructional content in existing schools, newly opened schools, and teacher transitions.

### 4.1 Impacts on the school supply landscape

We first examine how violence affects school closures, openings, and the composition of schools across types. Figure 1 shows dynamic effects of Islamist violence on the number of schools of different types that close in an academic year. Table A5 shows precise estimates together with the number of observations and outcome means for comparison. Figure 2 and Table A6 show impacts on the number of new schools opening of different types.

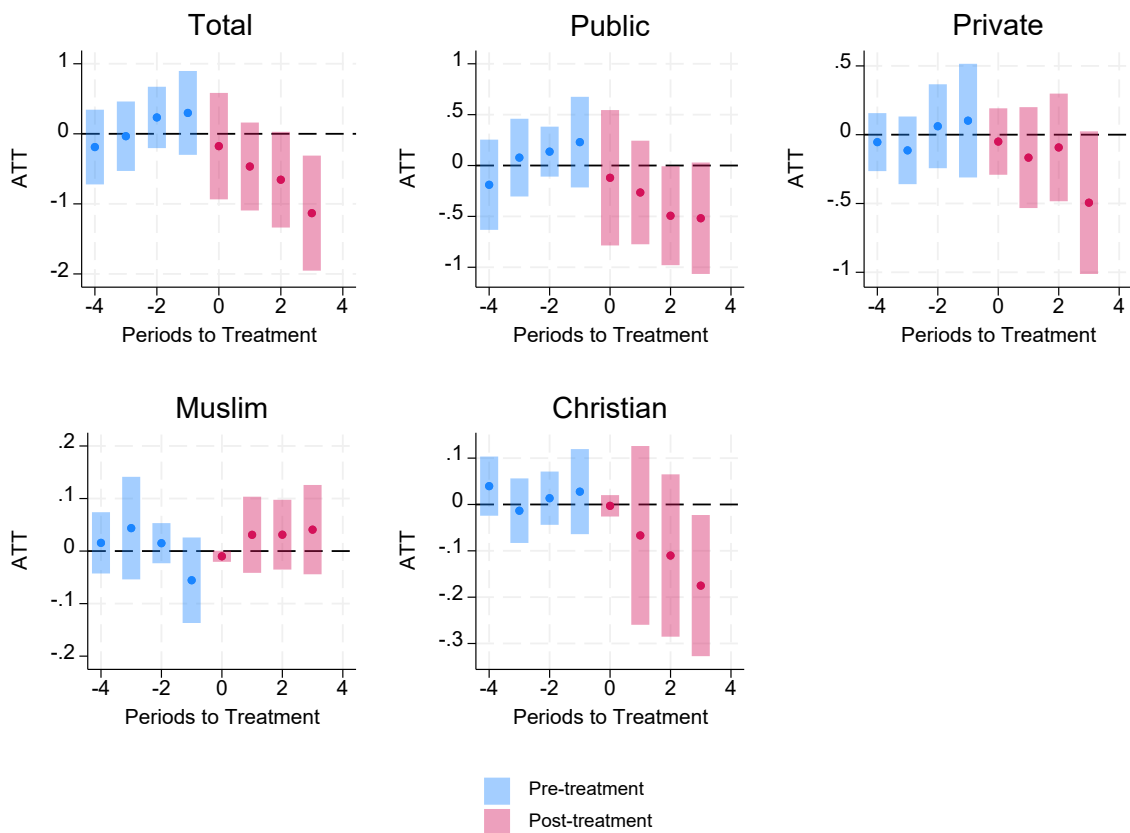
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Figure 1: Dynamic treatment effects of Islamist violence on number of schools closing in a municipality



Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant'Anna (2021) with 95% confidence intervals. Standard errors are clustered at the municipality level. Spillover communities are excluded from the estimation sample. Precise estimates, outcome means and number of observations in table A5

Figure 2: Dynamic treatment effects of Islamist violence on number of new schools in a municipality



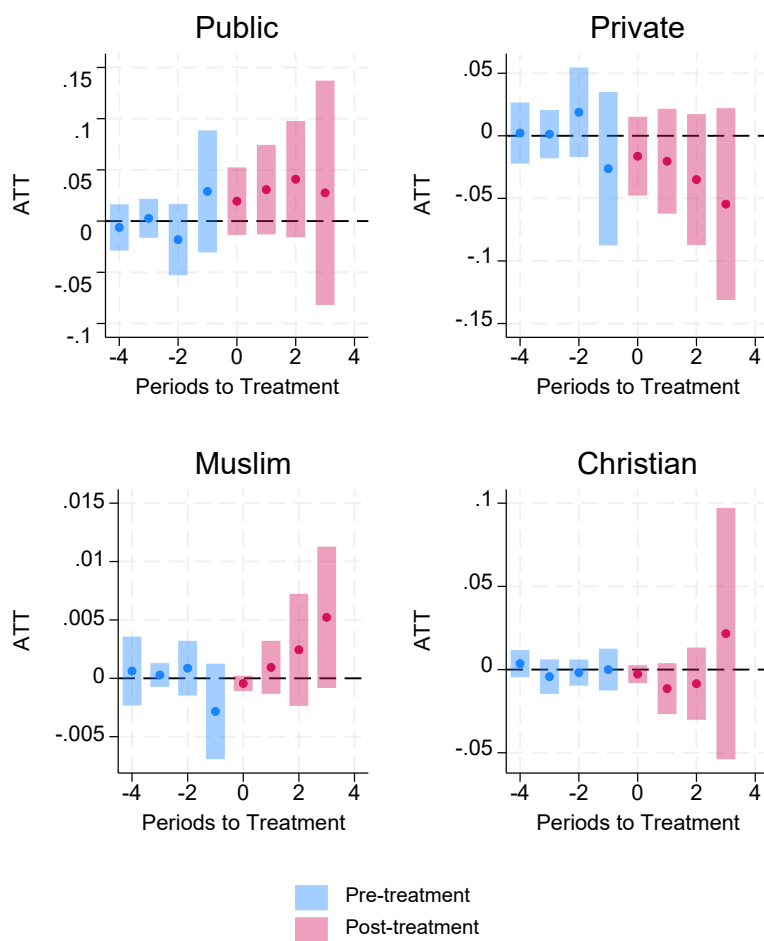
Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant'Anna (2021) with 95% confidence intervals. Standard errors are clustered at the municipality level. Spillover communities are excluded from the estimation sample. Precise estimates, outcome means and number of observations in table A6

Exposure to Islamist violence leads to a statistically significant increase in school closures and a statistically significant decrease in new schools opening. This effect is driven primarily by public schools. The impact on school closures is largest two years after the first violent Islamist conflict event, at 0.66, while the impact on new school openings is largest three years after the first event at -1.13. On average 0.4 schools close each during the period from the first event to three years thereafter, while about 0.6 fewer new schools open. Hence, there appears to be a larger impact on opening of new schools than on closing of old ones. Since Burkina Faso was in a phase of secondary school expansion when hit by Islamist violence, this makes sense. As mentioned, effects are driven primarily by public schools. Since other types of schools are fewer, we have less power to detect possible effects. For school closures, impacts on public schools are of about the same size as the total effects though, but for opening of new schools public school effects are much smaller than the total, and there are weakly statistically significant reductions for private schools and Christian schools three years after the first event.

Muslim schools stand out with positive estimated impacts on school openings, but they are not statistically significant.

Figure 3 and table A7 show dynamic effects of Islamist violence on the shares of different types of open schools in a municipality.

Figure 3: Dynamic treatment effects of Islamist violence on the share of open schools, by type



Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant’Anna (2021) with 95% confidence intervals. Standard errors are clustered at the municipality level. Spillover communities are excluded from the estimation sample. Precise estimates, outcome means and number of observations in table A7

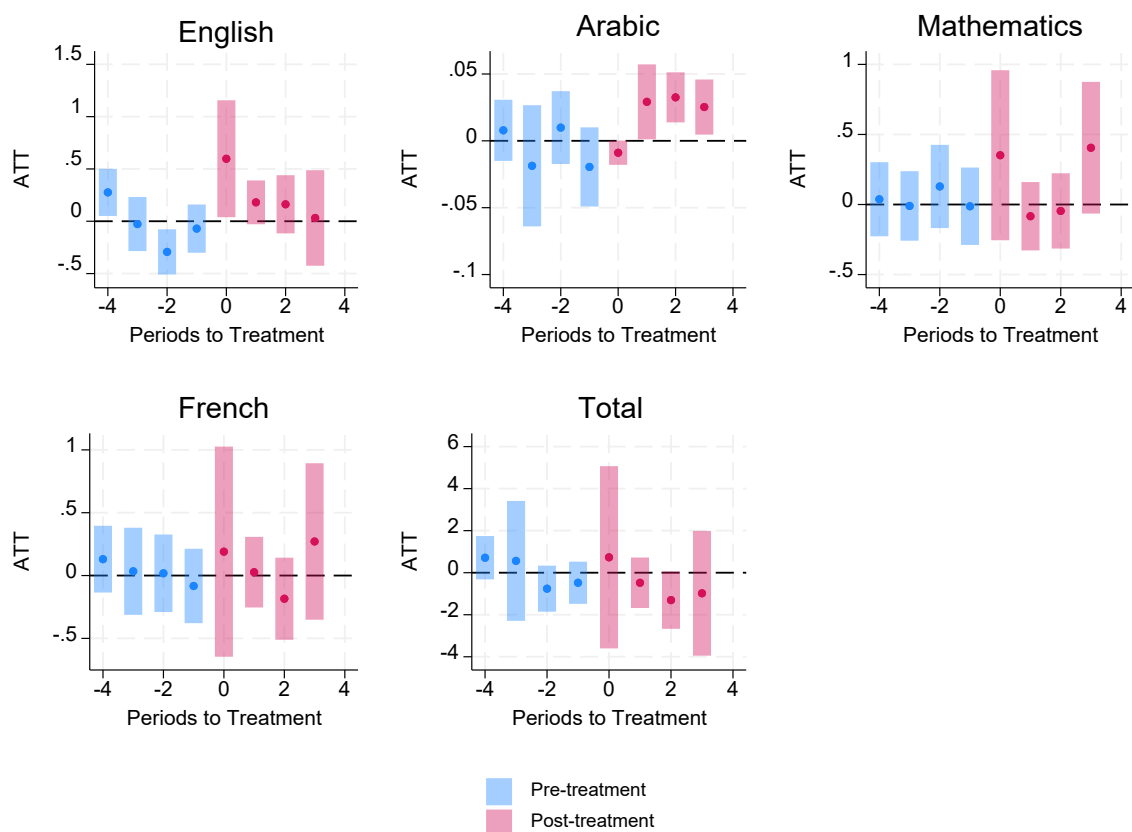
Even if public schools drive the effects on school closures and openings, we find no statistically significant effects on the overall composition of school types, and in particular the share of public schools does not decrease. Indeed, point estimates suggest a statistically insignificant increase. Point estimates are negative for private schools and positive for Muslim schools, but

generally not statistically significant. For private school the average decrease over the post-period is statistically significant at the ten percent level, and for Muslim schools there is a 0.005 increase three years after the first event, also statistically significant at the ten percent level.<sup>11</sup>

#### 4.2 Impacts on hours of instruction in different subjects

We next turn to our main outcome of interest, instructional hours in different subjects. Figure 4 and Table A8 show estimated dynamic treatment effects of Islamist violence on hours per week and class in schools that remain open in spite of the Islamist violence. Figure 5 and Table A9 estimate effects on hours per 100 students, which means that we can include observations from years 2013/14 and 2021/22 for which we do not have the number of classes in the schools.

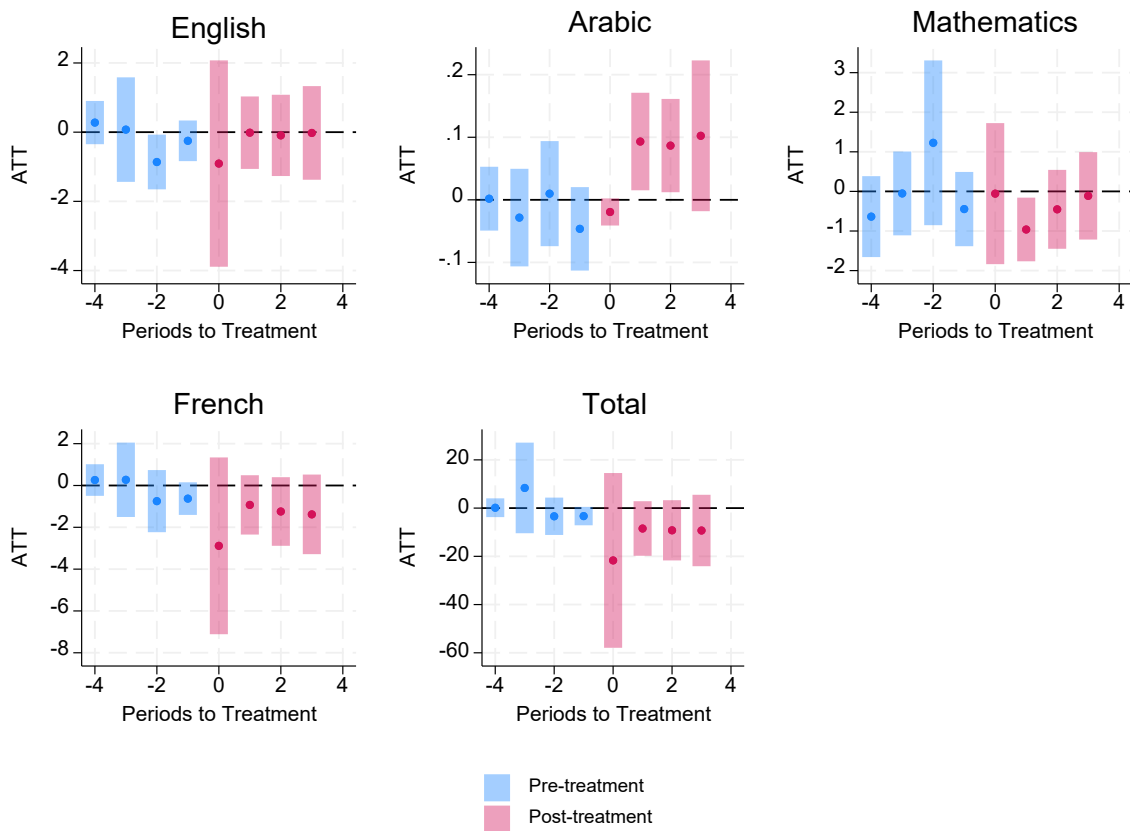
Figure 4: Dynamic treatment effects of Islamist violence on hours of instruction per class and week in schools that have stayed open.



Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant'Anna (2021) with 95% confidence intervals. Standard errors are clustered at the municipality level. Spillover communities are excluded from the estimation sample. Precise estimates, outcome means and number of observations in table A8. Estimated on observations from school years 2014/15 to 2020/21 since number of classes in the school is not available years 2013/14 and 2021/22.

<sup>11</sup> While there are also no statistically significant pre-trend coefficients, some are rather large. However, since they change signs between years, they do not suggest any persistent pre-trends in affected municipalities.

Figure 5: Dynamic treatment effects of Islamist violence on hours of instruction per 100 students and week in schools that have stayed open.



Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant’Anna (2021) with 95% confidence intervals. Standard errors are clustered at the municipality level. Spillover communities are excluded from the estimation sample. Precise estimates, outcome means and number of observations in table A9

There is a statistically significant increase in the number of hours of Arabic per class as well as per 100 students. While the immediate impact at the year of the first event is a small decrease for both outcomes, there are larger increases in the following years. The average effect for the period between the event and three years thereafter is about 0.2 more teaching hours of Arabic per class, statistically significant at the 1 percent level, or about 0.07 hours more per 100 students, statistically significant at the five percent level. These effects are small in absolute terms, but an increase of about 50% from the low baseline level. Estimated impacts on other subjects are in general statistically insignificant.<sup>12</sup>

<sup>12</sup> For English, the estimated average impact on hours per class is also positive and weakly statistically significant at the ten percent level. However, there is no statistically significant impact on hours per 100 students while there are rather large statistically significant pre-trend coefficients for both outcomes. For Mathematics there is a statistically significant reduction one year after the event on hours per 100 students, but no other statistically

It is also interesting to consider hours of instruction in new schools opening in affected municipalities after violence onset. Since they did not exist before violence onset, and since their very existence may be endogenous to the presence of violent Islamists and the resulting school closures, we cannot compare hours of instruction to any credible counterfactual for the hours that would have been taught at these schools in the absence of Islamist violence. We therefore make a simple descriptive comparison with all schools in the same municipalities before violence onset in Table 1.

Table 1: Teaching hours in schools opening after violence onset compared to teaching hours in the same municipalities before violence

	(1) English	(2) Arabic	(3) Mathematics	(4) French	(5) Total
New schools after violence onset	-0.138 (0.155)	0.098* (0.050)	0.025 (0.146)	-0.223 (0.214)	-1.849** (0.888)
Observations	5,446	5,446	5,446	5,446	5,446
R-squared	0.059	0.017	0.041	0.055	0.031
Outcome mean untreated	3.553	0.0402	4.344	5.014	23.98

Notes: There are 823 observations from schools that opened after violence onset. The comparison group is open schools in the same municipalities before violence onset. Regressions control for municipality fixed effects. Standard errors clustered at the municipality level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Newly opened schools in violence affected municipalities have fewer teaching hours per class in total, while they have more teaching hours per class in Arabic. While teaching in Arabic is still at a low level compared to teaching in core subjects, it is 2.5 times higher than the average in violence affected municipalities before violence onset. At the same time the schools opening after violence onset have about 1.8-1.9 hours less hours of teaching in total, (8% of the mean before violence).

#### 4.3. Impacts on transitions of teachers with different main subjects

Table 2: Average effect of Islamist violence in the municipality on teacher transitions

	(1) Stay at school	(2) Other school in municipality	(3) School in other municipality	(4) Not in sample
<b>Panel a: Main effect for all teachers</b>				
Islamist violence in municipality	-0.057 (0.048)	-0.015 (0.013)	0.085*** (0.032)	-0.013 (0.022)
Islamist violence in bordering municipality	-0.029* (0.017)	-0.004 (0.009)	0.020** (0.009)	0.013 (0.012)

significant effect. For total hours there is a weakly statistically significant reduction in total hours per class two years after the first event, but no other statistically significant effect.

Panel b: Interaction with French teacher				
Islamist violence in municipality	-0.036 (0.048)	-0.019 (0.012)	0.076** (0.033)	-0.022 (0.022)
× French teacher	-0.086*** (0.024)	0.015 (0.020)	0.036** (0.017)	0.034** (0.017)
Islamist violence in bordering municipality	-0.025 (0.018)	-0.005 (0.010)	0.015* (0.008)	0.015 (0.012)
× French teacher	-0.018 (0.016)	0.004 (0.011)	0.023** (0.010)	-0.009 (0.010)
Panel c: Interaction with English teacher				
Islamist violence in municipality	-0.063 (0.048)	-0.014 (0.014)	0.089*** (0.033)	-0.012 (0.022)
× English teacher	0.060 (0.037)	-0.010 (0.025)	-0.040 (0.027)	-0.010 (0.023)
Islamist violence in bordering municipality	-0.029 (0.018)	-0.005 (0.010)	0.021** (0.009)	0.013 (0.012)
× English teacher	-0.002 (0.020)	0.009 (0.015)	-0.009 (0.018)	0.002 (0.011)
Panel d: Interaction with Mathematics teacher				
Islamist violence in municipality	-0.047 (0.049)	-0.014 (0.014)	0.078** (0.031)	-0.017 (0.023)
× Mathematics teacher	-0.057* (0.030)	-0.006 (0.024)	0.040* (0.022)	0.023 (0.015)
Islamist violence in bordering municipality	-0.031* (0.018)	-0.005 (0.010)	0.021** (0.009)	0.014 (0.012)
× Mathematics teacher	0.008 (0.018)	0.005 (0.013)	-0.005 (0.012)	-0.008 (0.013)
Panel e: Interaction with Science teacher				
Islamist violence in municipality	-0.062 (0.049)	-0.018 (0.015)	0.089*** (0.034)	-0.009 (0.021)
× Science teacher	0.041 (0.048)	0.026 (0.038)	-0.035 (0.025)	-0.032* (0.017)
Islamist violence in bordering municipality	-0.034* (0.018)	-0.004 (0.010)	0.024*** (0.009)	0.014 (0.011)
× Science teacher	0.029 (0.020)	-0.000 (0.017)	-0.026* (0.014)	-0.003 (0.013)
Panel f: Interaction with History/Geography teacher				
Islamist violence in municipality	-0.063 (0.047)	-0.016 (0.014)	0.087*** (0.032)	-0.009 (0.021)
× History/Geography teacher	0.044 (0.034)	0.003 (0.018)	-0.017 (0.020)	-0.031 (0.023)
Islamist violence in bordering municipality	-0.028 (0.018)	-0.002 (0.010)	0.019** (0.008)	0.011 (0.011)
× History/Geography teacher	-0.013 (0.021)	-0.014 (0.013)	0.011 (0.011)	0.016 (0.012)

Control variables	Yes	Yes	Yes	Yes
Teacher FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Municipality FEs	Yes	Yes	Yes	Yes
Observations	65,541	65,541	65,541	65,541
R-squared	0.435	0.411	0.414	0.475
Teachers	21642	21642	21642	21642
Outcome mean	0.647	0.128	0.111	0.114

Notes: The estimation sample includes teachers observed at least twice who are observed at least once before the first Islamist event. All models include controls for the number of schools the teacher work at, the type of school, permanent contract, violence in a municipality with a common neighbor, and a binary indicator of whether the main subject is either French (panel b), English (panel c) Mathematics (panel d), Science (panel e) or History/Geography (panel f). Standard errors clustered at the municipality level in parenthesis.

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table 2 shows estimated average effects of Islamist violence in the municipality on teacher transitions to other schools. The results show that violence increases the probability that teachers move to other municipalities. There is a statistically significant increase in the probability of transitioning to a school in a different municipality when a municipality is subject to Islamist violence. The corresponding reduced probability of staying is, however, not statistically significant. There is a smaller reduction in the probability to stay when a municipality with a shared border is exposed to Islamist violence, with a corresponding increase in the probability to transition to a school in another municipality. Furthermore, contrary to our previous expectations, English teachers are not more likely to leave a school and municipality when violence hit. Indeed, their probability to stay does not seem to be much affected at all. Instead, teacher in French and Mathematics show stronger mobility responses. Teachers in the other core curricula subjects, Sciences (Physical science or Life/earth sciences) or History/Geography, are neither more or less likely to leave violent areas than the average teacher.

## 5. Conclusions

We examine how violent extremism affects the content of education, focusing on instructional time across subjects in secondary schools in Burkina Faso. While previous research has documented the effects of conflict on access to schooling and student outcomes, much less is known about how conflict reshapes what is actually taught in schools. We address this gap by combining nationwide school census data with geocoded conflict events and unique teacher-level panel data.

Our findings show that violent conflict significantly disrupts the supply of education by increasing school closures and reducing school openings, particularly among public schools. Interestingly, the estimated impact on openings of new public schools is larger than the estimated impact on public school closures. Weaker effects for other types of schools may partially be due to lower statistical power because of their lower numbers. Indeed, the composition of types of schools by providers does not seem to change much.

Within schools that remain open, we find a consistent increase in instructional time devoted to Arabic, while teaching hours in core subjects such as French, English, and Mathematics remain largely unchanged. Although small in absolute terms, the increase in Arabic instructional time represents a substantial relative change given its low baseline. While there is no natural counterfactual for schools that open after violence onset, newly opened schools in conflict-affected areas show a distinct instructional profile, with more emphasis on Arabic and fewer total teaching hours.

Conflict increases teacher mobility, particularly among teachers of core subjects such as French and Mathematics. A possible explanation is that Mathematics and French teachers have a better labor market. A consequence is that students will face larger teacher turnover in these core subjects.

These findings suggest that violent extremism affects educational content through multiple channels. Schools appear to adapt to insecurity by some reallocation of instructional time across subjects. Changes in the composition of schools and the movement of teachers may further shape the content of education. These adjustments occur even in the absence of formal changes to curricula, law or policies. Efforts to protect education in conflict-affected areas often focus on maintaining access to schooling. Our findings suggest that preserving access may not be sufficient if the content and quality of education change in response to insecurity.

Finally, in a broader perspective, this study highlights the need to better understand how conflict affects the provision of public services beyond access alone. Future research could explore whether similar patterns emerge in other contexts and examine the long-term consequences of shifts in educational content for student outcomes and social cohesion.

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## Appendix A: Additional figures and Tables

Figure A1: The spread of Islamist conflict events over time

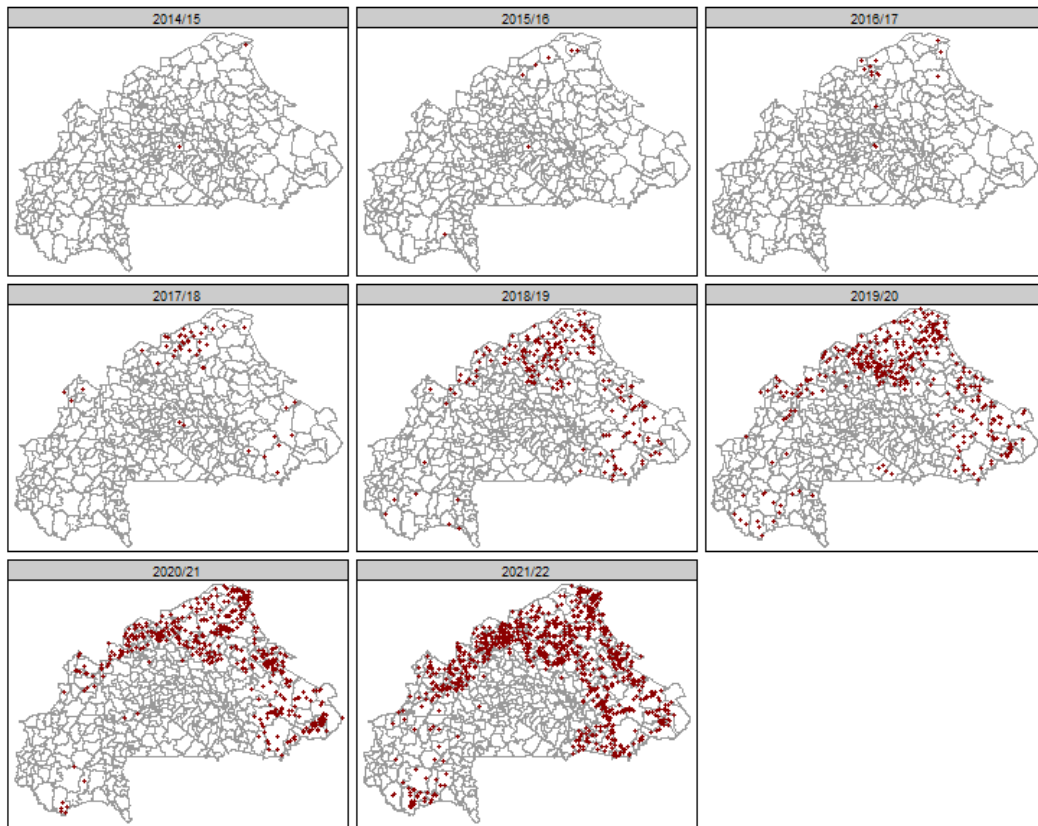


Figure A2: Open schools of different types over time in treated and control municipalities

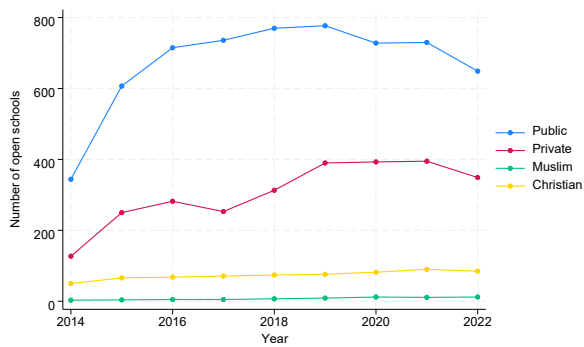
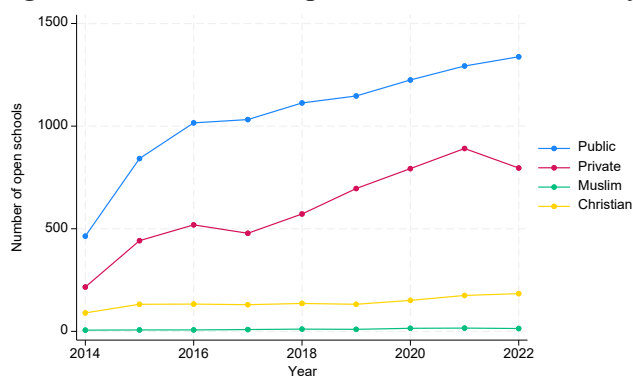


Figure A3: Number of open schools of different type in control municipalities



Note: Panel a) include schools in all municipalities exposed to Islamist violence until the school year 2021/22. Panel b) include all municipalities not exposed to Islamist violence up until the school year 2021/22.

Table A1: Teaching hours per class and week by type of school and exposure to Islamist violence

	All		Public		Private		Muslim		Christian	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
<b>Panel A: Schools in control municipalities</b>										
Hours	4.40	2.59	4.62	2.09	4.06	2.10	3.41	2.10	4.53	5.87
Hours French	5.11	2.31	5.64	2.21	4.44	2.26	4.27	1.86	4.55	2.05
Hours Arabic	0.04	0.52	0.01	0.19	0.02	0.23	3.94	5.02	0.01	0.18
Hours Total	24.72	10.16	24.96	8.11	24.23	8.76	25.15	8.35	26.73	22.29
Observations	11991		6885		4012		69		876	
<b>Panel B: Schools in violence affected municipalities before the first event</b>										
Hours	4.37	2.04	4.58	1.82	4.00	2.48	2.95	1.98	4.30	1.68
Hours French	5.04	2.30	5.36	2.20	4.54	2.44	4.41	2.40	4.38	1.94
Hours Arabic	0.04	0.51	0.02	0.25	0.03	0.40	2.81	4.74	0.00	0.03
Hours Total	24.10	12.57	23.91	7.18	24.79	21.21	23.89	6.98	25.37	6.39
Observations	4763		3131		1257		28		285	
<b>Panel B: Schools in violence affected municipalities after the first event</b>										
Hours	4.36	1.70	4.58	1.41	3.96	2.04	2.80	2.26	4.54	1.52
Hours French	5.06	2.07	5.60	1.75	4.27	2.27	5.09	2.24	4.41	2.17
Hours Arabic	0.06	0.72	0.02	0.29	0.05	0.46	3.73	5.72	0.02	0.15
Hours Total	24.55	6.89	24.78	5.67	23.78	8.56	26.01	6.93	26.07	6.93
Observations	2331		1360		770		21		176	

Table A2: Teacher Certificate Reporting by School Type

<i>School Type</i>	<i>% Schools Reporting Certificates</i>	<i>Avg. % Teachers Reported (if reporting)</i>
<i>Panel a) Full sample</i>		
<i>Public schools</i>	86.3%	64.4%
<i>Private non-confessional</i>	48.3%	57.9%
<i>Christian schools</i>	41.1%	49.3%
<i>Muslim schools</i>	38.6%	56.1%
<i>Panel b) Conflict affected sample</i>		
<i>Public schools</i>	91.7%	69.9%
<i>Private non-confessional</i>	52.3%	61.0%
<i>Christian schools</i>	45.8%	53.2%
<i>Muslim schools</i>	60.0%	70.2%

Note: The sample excludes Ouagadougou, in which reporting is similar for public schools, but lower for other schools.

Table A3: Descriptive statistics on teacher transitions

	<i>N</i>	<i>Mean</i>	<i>Std.</i>	<i>Min</i>	<i>Max</i>
Stay at same school	104,593	0.509	0.500	0	1
At other school in the same municipality	104,593	0.101	0.301	0	1
At school in other municipality	104,593	0.088	0.283	0	1
Not in the data	104,593	0.302	0.459	0	1

Note: The sample excludes Ouagadougou.

Table A4: Descriptive statistics of open secondary schools, one observation per school and year excluding Ouagadougou

<i>Variable</i>	<i>Obs</i>	<i>Mean</i>	<i>Std. dev.</i>	<i>Min</i>	<i>Max</i>
Number of teachers	26,572	13.037	16.831	1	1731
Number of pupils	26,261	326.4	332.9	2	4869
Number of classes*	19,097	5.898	4.548	1	119
Public school	26,038	0.602	0.490	0	1
Private school	26,038	0.317	0.465	0	1
Muslim school	26,038	0.006	0.079	0	1
Catholic school	26,038	0.039	0.195	0	1
Protestant school	26,038	0.035	0.185	0	1
New school	26,572	0.115	0.319	0	1
Year 2013/14	26,577	0.061	0.240	0	1
Year 2014/15	26,577	0.089	0.285	0	1
Year 2015/16	26,577	0.104	0.306	0	1
Year 2016/17	26,577	0.108	0.311	0	1
Year 2017/18	26,577	0.117	0.322	0	1
Year 2018/19	26,577	0.123	0.328	0	1
Year 2019/20	26,577	0.129	0.335	0	1
Year 2020/21	26,577	0.137	0.344	0	1
Year 2021/22	26,577	0.130	0.337	0	1

\*not available in 2014/15 and 2021/22

Table A5: Dynamic treatment effects of Islamist violence on number of schools closing in a municipality

	(1)	(2)	(3)	(4)	(5)
	Total	Public	Private	Muslim	Christian
Pre_avg	-0.075*	-0.027	-0.066***	-0.002	-0.004
	(0.039)	(0.016)	(0.020)	(0.002)	(0.005)
Post_avg	0.391***	0.436***	-0.017	0.006	-0.001
	(0.149)	(0.094)	(0.084)	(0.011)	(0.015)
Tm4	-0.176	-0.042	-0.070		-0.009
	(0.107)	(0.031)	(0.059)		(0.007)
Tm3	-0.095	-0.064	-0.074	-0.005	-0.016*
	(0.108)	(0.041)	(0.069)	(0.005)	(0.009)
Tm2	0.078	0.118**	-0.041	0.004	0.019**
	(0.093)	(0.052)	(0.061)	(0.004)	(0.009)
Tm1	-0.106	-0.119*	-0.078	-0.005	-0.010
	(0.133)	(0.069)	(0.069)	(0.005)	(0.015)
Tp0	-0.057	0.103	-0.015	-0.002	0.001
	(0.269)	(0.163)	(0.137)	(0.004)	(0.019)
Tp1	0.666**	0.688***	-0.030	-0.001	0.041
	(0.294)	(0.232)	(0.090)	(0.002)	(0.035)
Tp2	0.535*	0.378*	0.099	0.039	-0.013*
	(0.296)	(0.195)	(0.150)	(0.041)	(0.007)
Tp3	0.421	0.574***	-0.124	-0.013*	-0.031
	(0.322)	(0.199)	(0.202)	(0.007)	(0.040)
<i>N</i>	2191	2191	2191	2191	2191
Mean (all)	0.489	0.094	0.256	0.003	0.027
Mean (control)	0.454	0.064	0.249	0.002	0.027

Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant'Anna (2021). Spillover communities are excluded from the estimation sample. Standard errors clustered at the municipality level in parenthesis. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A6: Dynamic treatment effects of Islamist violence on number of new schools opening in a municipality

	(1)	(2)	(3)	(4)	(5)
	Total	Public	Private	Muslim	Christian
Pre_avg	0.077	0.064	-0.001	0.005	0.017
	(0.098)	(0.073)	(0.059)	(0.013)	(0.015)
Post_avg	-0.608**	-0.349*	-0.201	0.023	-0.089
	(0.262)	(0.204)	(0.153)	(0.016)	(0.058)
Tm4	-0.189	-0.189	-0.054	0.016	0.040
	(0.272)	(0.226)	(0.108)	(0.030)	(0.033)
Tm3	-0.035	0.078	-0.114	0.044	-0.013
	(0.253)	(0.195)	(0.125)	(0.050)	(0.036)
Tm2	0.234	0.136	0.062	0.015	0.014
	(0.223)	(0.125)	(0.156)	(0.019)	(0.029)
Tm1	0.298	0.230	0.102	-0.056	0.028
	(0.305)	(0.227)	(0.211)	(0.042)	(0.047)
Tp0	-0.176	-0.121	-0.050	-0.010*	-0.003
	(0.388)	(0.340)	(0.123)	(0.005)	(0.012)

Tp1	-0.467 (0.320)	-0.265 (0.260)	-0.167 (0.187)	0.031 (0.037)	-0.067 (0.098)
Tp2	-0.655* (0.349)	-0.494** (0.248)	-0.093 (0.200)	0.031 (0.034)	-0.110 (0.089)
Tp3	-1.132*** (0.419)	-0.518* (0.279)	-0.494* (0.264)	0.041 (0.043)	-0.175** (0.078)
<i>N</i>	2146	2146	2146	2146	2146
Mean (all)	1.246	0.647	0.515	0.010	0.068
Mean (control)	1.267	0.661	0.523	0.009	0.068

Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant'Anna (2021). Spillover communities are excluded from the estimation sample. Standard errors clustered at the municipality level in parenthesis. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A7: Dynamic treatment effects of Islamist violence on the share of open schools that are of different type

	(1) Public	(2) Private	(3) Muslim	(4) Christian
Pre_avg	0.002 (0.006)	-0.001 (0.006)	-0.000 (0.000)	-0.001 (0.002)
Post_avg	0.030 (0.024)	-0.032* (0.019)	0.002 (0.001)	-0.000 (0.012)
Tm4	-0.006 (0.012)	0.002 (0.012)	0.001 (0.002)	0.004 (0.004)
Tm3	0.003 (0.010)	0.001 (0.010)	0.000 (0.001)	-0.004 (0.005)
Tm2	-0.018 (0.018)	0.019 (0.018)	0.001 (0.001)	-0.002 (0.004)
Tm1	0.029 (0.030)	-0.026 (0.031)	-0.003 (0.002)	0.000 (0.006)
Tp0	0.019 (0.017)	-0.016 (0.016)	-0.000 (0.000)	-0.003 (0.003)
Tp1	0.031 (0.022)	-0.020 (0.021)	0.001 (0.001)	-0.011 (0.008)
Tp2	0.041 (0.029)	-0.035 (0.027)	0.002 (0.002)	-0.008 (0.011)
Tp3	0.028 (0.056)	-0.055 (0.039)	0.005* (0.003)	0.022 (0.039)
<i>N</i>	2146	2146	2146	2146
Mean (all)	0.752	0.207	0.002	0.040
Mean (control)	0.753	0.206	0.002	0.039

Note: Dynamic treatment effects estimated using the estimator by Callaway and Sant'Anna (2021). Spillover communities are excluded from the estimation sample. Standard errors clustered at the municipality level in parenthesis. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table A8: Dynamic treatment effects of Islamist violence on hours of instruction per class and week in schools that have stayed open.

	(1) English	(2) Arabic	(3) Mathematics	(4) French	(5) Total
Pre_avg	-0.028 (0.059)	-0.005 (0.007)	0.036 (0.075)	0.025 (0.067)	0.009 (0.366)
Post_avg	0.243* (0.129)	0.020*** (0.006)	0.157 (0.146)	0.076 (0.195)	-0.507 (1.107)
Tm4	0.276** (0.115)	0.008 (0.012)	0.037 (0.135)	0.130 (0.136)	0.713 (0.526)
Tm3	-0.027 (0.132)	-0.019 (0.023)	-0.010 (0.127)	0.034 (0.177)	0.562 (1.455)
Tm2	-0.293*** (0.110)	0.010 (0.014)	0.129 (0.152)	0.018 (0.157)	-0.760 (0.559)
Tm1	-0.070 (0.118)	-0.020 (0.015)	-0.013 (0.141)	-0.083 (0.151)	-0.479 (0.512)
Tp0	0.598** (0.285)	-0.009* (0.005)	0.352 (0.310)	0.190 (0.427)	0.733 (2.212)
Tp1	0.181* (0.107)	0.029** (0.014)	-0.084 (0.124)	0.027 (0.143)	-0.479 (0.613)
Tp2	0.162 (0.142)	0.033*** (0.010)	-0.046 (0.137)	-0.184 (0.167)	-1.302* (0.698)
Tp3	0.032 (0.233)	0.025** (0.011)	0.405* (0.240)	0.271 (0.318)	-0.979 (1.515)
Obs.	14890	14890	14890	14890	14890
Mean (all)	3.565	0.035	4.388	5.096	24.674
Mean (control)	3.566	0.036	4.391	5.094	24.638

Table A9: Dynamic treatment effects of Islamist violence on hours of instruction per 100 students and week in schools that have stayed open.

	(1) English	(2) Arabic	(3) Mathematics	(4) French	(5) Total
Pre_avg	-0.191 (0.185)	-0.016 (0.020)	0.023 (0.318)	-0.210 (0.204)	0.446 (1.744)
Post_avg	-0.260 (0.748)	0.066** (0.028)	-0.396 (0.457)	-1.609 (1.077)	-12.165 (9.319)
Tm4	0.276 (0.319)	0.002 (0.026)	-0.637 (0.522)	0.261 (0.386)	0.140 (1.985)
Tm3	0.074 (0.771)	-0.028 (0.040)	-0.051 (0.540)	0.272 (0.907)	8.355 (9.584)
Tm2	-0.863** (0.403)	0.010 (0.043)	1.228 (1.062)	-0.748 (0.758)	-3.396 (3.955)
Tm1	-0.250 (0.300)	-0.046 (0.034)	-0.446 (0.479)	-0.627 (0.398)	-3.317* (1.948)
Tp0	-0.907 (1.522)	-0.019* (0.011)	-0.056 (0.909)	-2.885 (2.157)	-21.690 (18.487)

Tp1	-0.015 (0.534)	0.093** (0.040)	-0.962** (0.410)	-0.928 (0.724)	-8.460 (5.770)
Tp2	-0.093 (0.599)	0.087** (0.038)	-0.452 (0.508)	-1.244 (0.838)	-9.219 (6.372)
Tp3	-0.022 (0.691)	0.102* (0.061)	-0.112 (0.563)	-1.380 (0.971)	-9.290 (7.559)
Obs.	19790	19790	19790	19790	19790
Mean (all)	8.468	0.134	10.428	11.847	60.390
Mean (control)	8.490	0.139	10.452	11.870	60.540

## Appendix B: Analyzing teacher migration patterns to define spillover municipalities

We expect spillover effects of Islamist violence on school supply in municipalities other than those directly affected for two reasons. First, teachers and school-age children may migrate out of violence-affected municipalities. Second, fear of violence may lead to outmigration from municipalities close to the directly affected ones. Since the analysis on teacher transitions in section 5.3 show that secondary teachers may respond to violence in a neighboring municipality, we will define municipalities with a shared border as spillover municipalities. We next investigate which municipalities should be expected to experience spillovers driven by in-migration of students and teachers from violence-affected municipalities, i.e. which municipalities do teachers who leave violence-affected municipalities relocate to?

To answer this question, we use data on migration flows of teachers between pairs of origin municipalities  $i$  and destination municipalities  $j$ . We create an annual panel of migration between municipality pairs using data on teachers with certificate. Since there are relatively few secondary school teachers compared to primary school teachers (there are 8,164 observed secondary school teacher migrations between municipalities and 48,280 for primary school teachers), we perform the analysis on both samples of teachers to create a more stable classification of spillover municipalities. To analyze migration flows, we use standard models in the migration literature as the point of departure. The classic migration model is the gravity model, which models migration flows as a function of population size and geographical distance between origin and destination (Stewart, 1941; Zipf, 1946). Subsequent models have included various economic and other characteristics, i.e. amenities, in both the origin and the destination locations as important factors in migratory decisions (Greenwood, 2015). However, if the aim is to predict migration flows rather than to investigate specific economic factors or amenities, past migration between origin-destination pairs is likely to outperform other variables (Greenwood, 1969).

We estimate equation B1 using the subsample of municipality pair - year observations where the origin municipality  $i$  is directly affected by Islamist violence. We do not impose any restrictions on destination municipalities  $j$ . The equation is thus:

$$y_{ijt} = \alpha_0 + \alpha_1 \ln(\text{totmigr})_t + \alpha_2 \ln(\text{totmigr})_{it} + \beta' \text{Dist}_{ij} + \gamma' \text{Hist}_{ijt} + \delta' X_{ij} + \theta \text{Viol}_{jt} + \pi' Z_j + \varepsilon_{ijt} \quad (\text{B1})$$

where  $y_{ijt}$  is the log number of migrant teachers +1 moving from  $i$  to  $j$ . We add 1 to the number of migrating teachers because there are many zero cells. Since we are not interested in interpreting the magnitude of the coefficients, the fact that coefficients are not directly interpretable in a log of zero model (Chen and Roth, 2024) will not have any practical consequences. The variables  $\ln(\text{totmigr})_t$  and  $\ln(\text{totmigr})_{it}$  measure total teacher migration across municipalities in year  $t$  and total teacher migration from municipality  $i$  in year  $t$ , respectively. These are included to control for scale of migration effects that affect flows to all destinations equally. <sup>13</sup> $\text{Dist}$  includes binary indicators of distances 92 km, 157, 128, 359 and 473 km between municipality centroids (less than 92 km being the reference), where the

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<sup>13</sup> We do not control for the total number of arrivals in the destination municipalities, i.e. for scale of migration to each destination affecting all origin municipalities equally. This is since we want to use the full variation across destinations to predict where teachers from violence affected municipalities migrate.

exact values come from division of municipality pair distances into quintiles. *Hist* includes binary indicators for historical migration odds of 1, 20, 40, and 70 (less than 1 being the reference) for secondary school teachers and 1, 3, 5, 10, 20 for primary school teachers. Historical migration odds are defined as the total number of earlier migrants from  $i$  to  $j$  divided by the average number of earlier migrants from  $i$  to all municipalities. Hence it measures the tendency of migrants from  $i$  to choose destination  $j$  over other possible alternative destinations. For example, a value of 20 indicates that the number of historical migrant teachers is twenty times larger than what we would expect if migrant teachers had no preference over destination municipalities and were therefore equally likely to migrate to any municipality in Burkina Faso. It should be noted though, that most dyad year observations have zeros, especially for secondary school teachers. For secondary school teachers, any migrating teacher at all makes the historical odds 2.3 or more.

We use binary indicators in *Dist* and *Hist* instead of standard logs of the continuous variables since our purpose is to classify each destination municipality  $j$  as likely or unlikely to experience migration spillovers from municipality  $i$ . Such classification is more straightforward if we directly use binary indicators to predict migration flows.  $X_{ij}$  includes binary indicators for having a shared border, having a common neighbor (i.e. being the neighbor of a neighbor), and whether municipality  $j$  is the closest city to  $i$ .  $Z_j$  include binary indicators for municipality  $j$  being the capital Ouagadougou and for being another city.  $Viol_{jt}$  is a binary indicator of Islamist violence in the destination municipality. To determine which indicators should be used to define spillover municipalities, we consider the relative size of coefficients, which can be directly compared since we consider only binary indicators.<sup>14</sup> In practice, we choose the indicators with the largest coefficients. The number of indicators used is constrained by the need to have at least some remaining control municipalities in the academic year 2021/22, the final year of observation. Hence, while we cannot guarantee that there are no spillovers to the remaining control municipalities, this procedure allows us to separate municipalities where migration spillovers can be expected to be larger from ones where possible migration spillovers should be much smaller.

Starting with secondary school teachers in Table B1, Column (1) shows the results from regression of equation B1. In terms of relative magnitudes, the coefficients on indicators of historical migration stand out as particularly relevant. Since historical migration patterns and geographic determinants of migration are correlated, geographic determinants may do a better job at predicting migration flows if indicators in *Hist* are excluded. In column 2 we test if this is the case. Indeed, coefficients increase substantially for all other predictors when historical migration indicators are not included. Closest city has the largest coefficient followed by the destination being Ouagadougou, the capital. The coefficient of distance indicators change sign to negative as would be expected. Still, historical migration indicators are clearly more predictive of migration flows than geography-based indicators in column (1), and R-square in column (2) is less than one third of the one in column (1). When only historical migration

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<sup>14</sup> We prefer to use relative coefficient size rather than measures of explanatory power such as R-squared since the latter will also be influenced by how common the variable is. For example, if the coefficient on the capital city and on other city is the same, the capital and any given other city are equally likely to receive migration spillovers, but other cities will have a higher explanatory power since there are more of them.

indicators are included in column (3), coefficients as well as R-squared are virtually unchanged relative to column (1). This suggests that historical migration patterns capture much of the impact of geography on migration flows, while geography alone does not capture historical migration patterns.

Table B2 shows results for primary school teachers. Again, historical migration indicators vastly outperform distance-based ones to predict where teachers who leave violence affected municipalities relocate. However, there is a larger spread in the size of the estimated historical odds coefficients, and higher historical odds clearly predict higher migration flows. We see this as an indication of more stable estimates when we have a larger sample of migrating teachers.

Taken together, we choose to define spillover municipalities as destination municipalities that either i) shares a border with a violence affected municipality or ii) historically received any migrant secondary school teachers and ten times as many migrant primary school teachers from a conflicted affected origin municipality compared to what we would expect if migration probabilities were equal across all municipalities in the country. That is, we remove municipalities that historically have received unusually many of both migrant secondary school teachers and migrant primary school teachers. The reason to use information about migrant primary school teachers in addition to information on migrant secondary school teachers is that the migration of one individual secondary school teacher influences the migration history of secondary school teachers, while this is not the case for primary school teachers since we have more observations. Table B3 shows the resulting number of directly affected, spillover and control municipalities each year.

Table B1: Predictors of secondary school teacher migration from municipalities affected by violent Islamist conflict.

	(1) log_outmigration	(2) log_outmigration	(3) log_outmigration
Historical odds>1	0.182*** (0.021)		0.186*** (0.021)
Historical odds>20	0.190*** (0.018)		0.195*** (0.019)
Historical odds>40	0.199*** (0.018)		0.208*** (0.019)
Historical odds>70	0.338*** (0.024)		0.367*** (0.025)
Shared border	0.031** (0.015)	0.075*** (0.019)	
Common neighbor	0.004 (0.003)	0.011** (0.005)	
Closest city	0.076*** (0.025)	0.247*** (0.032)	
Destination is capital	0.032* (0.018)	0.124*** (0.026)	
Destination is city	-0.000 (0.001)	0.018*** (0.003)	
Distance>92 km	-0.002 (0.002)	-0.007*** (0.002)	
Distance>157 km	0.001 (0.002)	-0.011*** (0.002)	
Distance>248 km	0.001 (0.002)	-0.013*** (0.002)	
Distance>359 km	0.002	-0.012***	

	(0.002)	(0.002)	
Distance>473 km	0.002	-0.012***	
	(0.002)	(0.002)	
Violence in neighboring municipality	-0.002**	-0.005***	
	(0.001)	(0.001)	
Observations	173,802	173,802	173,802
R-squared	0.276	0.079	0.271

All models also include the log of the total annual migration flow and the log of the total number of annual departures from the municipality and a constant. Standard errors clustered at the origin municipality level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B2: Predictors of primary school teacher migration from municipalities affected by violent Islamist conflict.

	(1)	(2)	(3)
	log_outmigration	log_outmigration	log_outmigration
Historical odds>1	0.131*** (0.006)	0.131*** (0.006)	
Historical odds>3	0.152*** (0.009)	0.153*** (0.009)	
Historical odds>5	0.201*** (0.010)	0.201*** (0.010)	
Historical odds>10	0.292*** (0.018)	0.295*** (0.018)	
Historical odds>20	0.638*** (0.030)	0.670*** (0.032)	
Shared border	0.050*** (0.013)		0.191*** (0.019)
Common neighbor	0.020*** (0.007)		0.073*** (0.011)
Closest city	0.286*** (0.047)		0.526*** (0.064)
Destination is capital	-0.015 (0.011)		0.052*** (0.013)
Destination is city	0.006*** (0.002)		0.008** (0.003)
Distance>92 km	0.010 (0.006)		-0.025*** (0.009)
Distance>157 km	0.019*** (0.006)		-0.049*** (0.009)
Distance>248 km	0.024*** (0.006)		-0.051*** (0.009)
Distance>359 km	0.026*** (0.006)		-0.050*** (0.009)
Distance>473 km	0.027*** (0.006)		-0.047*** (0.009)
Violence in destination	-0.009*** (0.001)		-0.017*** (0.002)
Observations	173,802	173,802	173,802
R-squared	0.306	0.297	0.094

All models also include the log of the total annual migration flow and the log of the total number of annual departures from the municipality and a constant. Standard errors clustered at the origin municipality level in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Table B3: Directly affected, spillover and control municipalities by academic year

<i>Year</i>	<i>Directly affected</i>	<i>Spill-over</i>	<i>Control</i>
2013/14	0	0	351
2014/15	2	9	340
2015/16	7	28	316
2016/17	13	37	301
2017/18	27	83	241
2018/19	74	142	135
2019/20	106	171	74
2021	114	193	44
2022	155	176	18