When Violence Strikes: Timing of Exposure and High-Stakes Exam Outcomes in Afghanistan*

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Abstract

Violent conflict disrupts human capital formation and performance at critical educational junctures, yet its impact on the transition from secondary to tertiary education remains understudied. We examine how the timing of conflict exposure affects university entrance exam outcomes in Afghanistan using administrative exam data from 2013 to 2018. Identification exploits two sources of variation: province-specific exam dates, which shift the pre-exam exposure window, and district-level differences in violence within province-years. Exposure to violence in the six months before the exam lowers test scores and admission likelihood, with effects largest closer to the exam date. These losses are heterogeneous across the score distribution. Violence in the window closest to the exam produces the largest declines among higher-performing students near the university admission threshold, while violence in the second closest window lowers scores throughout the distribution. The results suggest that exposure to conflict excludes marginal students from tertiary education rather than redirecting them to less selective programs. Regarding mechanisms, household data show that perceived insecurity reduces educational spending and is associated with occupational reallocation toward more vulnerable forms of work. Overall, the findings suggest that conflict constrains educational investment, weakens economic opportunities, and restricts access to higher education.

Keywords: conflict exposure, human capital, education, Afghanistan, gender

JEL Classifications: I20, O15, D74

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1 Introduction

Human capital plays a central role in shaping individual life outcomes and development trajectories (Hanushek and Woessmann, 2008; Angrist et al., 2021; Li et al., 2024; Card, 1999; Psacharopoulos and Patrinos, 2018). For young people, it serves as a critical pathway to economic opportunity, personal development, and civic participation. However, in many parts of the world, ongoing armed conflict continues to disrupt this vital pathway. While the consequences of war, conflict, and violence on educational attainment have been well documented (Akbulut-Yuksel, 2014; Shemyakina, 2011; Brown and Velásquez, 2017; Akresh and de Walque, 2008), much less is known about their short-run impacts at critical transition in the education life-cycle. In particular, exposure to conflict during late adolescence, especially during the transition from secondary to tertiary education, remain understudied. This stage is pivotal for shaping individuals' future trajectories, and disruptions at this juncture may have long-lasting consequences by hindering both individual opportunities and the accumulation of human capital at the societal level.

In this study, we assess the relationship between exposure to violent conflict and high-stakes exam outcomes during students' transition from high school to tertiary education. We ask: Does the proximity of armed conflict to exam dates affect test performance and university admission outcomes? We hypothesize that violence occurring closer to the exam date is associated with lower performance and admission likelihood, due to learning disruptions and heightened stress. To explore this question, we use administrative data on over one million students who participated in the public university entrance exam in Afghanistan from 2013 to 2018. We construct localized, district and year-specific measures of armed conflict by combining information on the date of the exam in each province with district-level fatality data.

The nature of armed conflict in Afghanistan presents both a relevant case for conflict-affected settings and a methodological challenge: unlike much of the previous literature, we conceptualize conflict not as a discrete, one-time event but a dynamic environment in which conflict intensity fluctuates over time, where most individuals face persistent exposure. Because we observe outcomes only for students who sit the exam, we exploit variation in province-specific exam dates to construct district-level measures of conflict exposure in the months preceding the test. During this period, students typically complete high school while intensely preparing for the Kankor, Afghanistan's annual nationwide entrance examination to public tertiary education. To identify period-specific exposed districts in a setting where

most areas experience baseline violence, we standardize conflict-related fatalities per 1,000 population within each pre-exam window and year, capturing deviations from expected levels. To compare students facing similar economic, educational, and security conditions, we exploit within-province-year variation in violence exposure across districts, thereby isolating period-specific shocks at the sub-provincial level.

We find that greater exposure to violent conflict significantly reduces both test scores and the likelihood of admission to public universities, with larger effects for female students. Timing of exposure is critical: conflict in the six months preceding the exam significantly lowers test scores and admission rates to both undergraduate programs and public tertiary education overall, while earlier exposure has no detectable effect, consistent with violence operating through proximate disruptions to exam preparation. A placebo test that exploits post-exam variation in violence confirms that these patterns are largerly driven by pre-exam exposure rather than unobserved district-level trends.

The effects also extend to the extensive margin: violence exposure reduces exam participation, particularly among women. Quantile regressions further show that recent violence exposure (within 90 days of the exam) disproportionately depresses scores among high-performing students near the selective four-year cutoff, while medium-horizon exposure depresses scores more uniformly across the conditional distribution. Because the most affected students are likely missing from the sample, intensive-margin estimates are attenuated toward zero under positive selection into participation. Province specific exam timing also plays an important role. Cold-calendar provinces drive the aggregate results in both pre- and post-2017 reform periods, while warm-calendar provinces show no negative effects in either period, however, limited variation and a smaller number of province-period cells in the warm calendar sub-sample reduce statistical power. Within cold-calendar provinces, exploiting a 2017 administrative reform that lengthened the gap between the end of twelfth grade and the exam, we find that the effect of earlier exposure (91-270 days) attenuates as it overlaps less with active schooling, while the immediate pre-exam effect (within 90 days) persists, consistent with the two windows operating through partially distinct channels.

Admission to public universities is determined solely by performance on the Kankor examination, and applicants rank up to five preferred fields of study on test day before learning their scores. Because we observe both test scores and admission outcomes, we implement an instrumental variable (IV) strategy that uses conflict exposure as an instrument for test scores. This approach allows us to examine whether conflict affects admissions through

its impact on test scores. The IV estimates suggest that violence depresses admissions by lowering test scores. The IV coefficient is larger for overall admission than for undergraduate admission, suggesting that conflict-driven declines in test scores push some students out of tertiary education altogether, rather than reallocating them from undergraduate to associate programs. These results point to a decrease in access to tertiary education for marginal students, rather than substitution across program types.

We further explore household-level mechanisms using the nationally representative Afghan Living Conditions Survey (ALCS) 2016–17[1]. Within districts, households whose heads perceive their district as insecure spend significantly less on education, more likely to report worsening economic conditions, and face greater exposure to economic and other shocks. In contrast, households headed by individuals with tertiary education spend more on education and demonstrate higher resilience to economic shocks. We find no association between perceived insecurity and household displacement status.

At the individual level, for the population at large, perceived insecurity is associated with a shift from salaried employment and day-labor toward self-employment, consistent with moving towards more vulnerable forms of work. However, this patterns mask substantial heterogeneity by education. Individuals with tertiary education retain a relative advantage in access to salaried and day-labor employment and are less likely to be employed under conditions of insecurity.

Taken together, these patterns suggest a reinforcing cycle: conflict reduces investment in education and disrupts exam preparation, while also reducing the quality of labor market opportunities. These results complement the broader literature examining the effect of conflict on education and human capital accumulation.

Our paper contributes to a large body of evidence that highlights the broad benefits of educational attainment, including monetary (Card, 1999; Tamborini, Kim, and Sakamoto, 2015; Psacharopoulos and Patrinos, 2018; Foltz and Gajigo, 2012; Peet, Fink, and Fawzi, 2015), non-monetary (Davies et al., 2018; Lager and Torssander, 2012; Clark and Royer, 2013; Conti, Heckman, and Urzua, 2010; Joshi, Subramanian, and Swaminathan, 2019), and inter-generational returns (Handa, 1999; Barrios-Fernández, Neilson, and Zimmerman, 2026; Hill and King, 1995). Despite these well-documented benefits, progression to secondary and tertiary education remains limited in developing countries (Glewwe and Kremer, 2006;

[1]The survey dates correspond to the year 1395 in the Afghan calendar. Henceforth, we refer to the survey as ALCS 2016–17.

Glewwe et al., 2011), especially in fragile and conflict-affected settings where insecurity may both constrain investment in education and limit the realization of returns to it. This is reflected in persistently low gross enrollment rates across primary, secondary, and tertiary education levels (Figure A.1–A.3).

Armed conflict is a central constraint in fragile settings, it undermines long-term economic development (Blattman and Miguel, 2010; Riaño and Caicedo, 2024), lowers educational attainment (Bertoni et al., 2019; Buvinić, Das Gupta, and Shemyakina, 2013; Valente, 2013; Kibris, 2015; Monteiro and Rocha, 2017; Chamarbagwala and Morán, 2011; Brück, 2006; Dabalen and Paul, 2014; Duque, 2024; Weldeegzie, 2017), and induces chronic stress that impairs cognitive functioning, decision-making and academic performance (Callen et al., 2014; Voors et al., 2012; Jakiela and Ozier, 2019; Brown et al., 2019; Michaelsen and Salardi, 2020).

A related strand of literature examines how short-run shocks around exam periods affect high-stakes assessments. Disruptions to nutrition assistance (Bond et al., 2022), the timing of cash transfers (Larrinaga and Reyes, 2025), and pre-exam heat exposure (Park et al., 2020) all have been shown to affect test scores. Similarly, Monteiro and Rocha (2017) find that drug-gang related violence depresses math achievement, with effects increasing closer to exam dates, while Koppensteiner and Menezes (2021) show that nearby homicides reduce attendance, scores, and educational aspirations.

However, evidence on how armed conflict affects performance on high-stakes examinations, key gateways to higher education in conflict-affected countries, remains limited. In particular, little is known about whether the timing of conflict exposure relative to exam dates matters, or how exposure at this critical juncture shapes access to tertiary education across admission margins. This study addresses this gap by examining how the proximity and intensity of pre-exam violence exposure affect test scores and university admission outcomes in Afghanistan, a setting where most students face some conflict exposure and the Kankor exam solely determines access to public higher education.

This study is most closely related to Brück, Di Maio, and Miaari (2019), Darwish and Wotipka (2022), and Kibris (2015), which examine the relationship between armed conflict and student performance on high-stakes examinations. However, existing work provides limited evidence of the role of timing and intensity of exposure around exam dates. Building on this work, we contribute to this literature in three ways.

First, we anchor exposure to each province's exam date rather than to the calendar year. Because exam dates vary across provinces and over time, this approach generates variation

in which calendar months fall within each pre-exam window. We exploit this variation by constructing district-level measures of conflict exposure across distinct pre-exam windows, using the universe of Kankor participants over six academic years matched to province-specific exam dates.

To capture dynamics in a setting where all students face some baseline conflict, we standardize fatalities per 1,000 population within each window-year, benchmarking districts against window-specific national conditions, to identify those experiencing unusually high violence.

Prior work typically relies on annual violence exposure measures that cannot distinguish recent from more distant violence, a limitation echoed in our results: violence within six months of the exam reduces scores, while exposure earlier than that does not. A post-exam placebo further supports the interpretation that these effects are driven by pre-exam exposure rather than unobserved district characteristics.

Second, we distinguish between admission to four-year undergraduate programs and overall admission to public tertiary education, a distinction not made in prior studies. Brück, Di Maio, and Miaari (2019) use the minimum required score as a proxy for university admission and Darwish and Wotipka (2022) classify all non-failed students as admitted, including those offered only nominal placement at discounted tuition in private institutions, we do not treat such placements as admissions. Separating these outcomes, we show that conflict affects access differently at two margins: entry into tertiary education and admission to selective four-year programs. In particular, marginal students are pushed out from tertiary education rather than redirected to less selective programs. Quantile regressions further reveal heterogeneous score effects: recent violence exposure lowers scores at the upper tail near the four-year admission cutoff, while medium-horizon reduces scores more uniformly across the distribution.

Third, we exploit a 2017 administrative reform that exogenously shifted the timing of pre-exam exposure windows relative to the twelfth-grade school year. The reform extended the gap between the end of schooling and the exam in cold-calendar provinces and compressed it in warm-calendar provinces, thereby changing how much pre-exam exposure windows overlaps with the school year. This variation allows us to assess how the effects of conflict depend on this overlap. Medium-term effects attenuate as exposure windows move further outside the school year, i.e. their overlap with active schooling declines. In contrast, effects in the 1–90 day window remain similar regardless of whether students are still in school at

the time of exposure. The pattern points to mechanisms operating outside active schooling, plausibly stress, mobility frictions, or exam-day logistics, which the design cannot disentangle.

Further, while we use the same underlying data as Darwish and Wotipka (2022), the two studies recover different estimands. Darwish and Wotipka (2022) rely on province-level annual variation in a two-way fixed-effects design, with time-varying province-level controls; while we exploit district-level variation within province-years, anchoring exposure to pre-exam windows rather than to the calendar year. Our province-by-year fixed effects absorb the province-level confounders targeted by their controls, and our exam-anchored windows isolate the timing through which violence plausibly affects performance, along a margin their annual measure cannot capture. We also distinguish undergraduate placement from overall tertiary admission, whereas they focus on a binary pass/fail outcome, classifying as admitted even students given only a referral to private institutions. Moreover, we provide a rich set of heterogeneity analyses, by gender, by regional high school completion rates, and by academic calendar type, that shed light on mechanisms such as differential preparation environments and schooling disruptions.

The paper proceeds as follows. Section 2 discusses the study setting and background information. Section 3 describes the data. Section 4 presents the empirical strategies implemented in the analysis. Results are presented in Section 5. Section 6 concludes.

2 Education Progression and Context

This section briefly reviews global trends in access to education and the role of entrance examinations in progression to higher education, and then describes the study context.

2.1 Progression to Higher Education

Significant progress has been made in expanding access to primary education in developing countries (Figure A.1). However, progression to higher levels of education, particularly secondary (Figure A.2) and tertiary education (Figure A.3), remains a challenge (Glewwe and Kremer, 2006; Glewwe et al., 2011). Addressing these gaps is a central priority of the Sustainable Development Goals. Specifically, educational targets 4.3, 4.4, and 4.5 under SDG-4, "Quality Education," emphasize providing equal opportunities at all levels of education to ensure that students gain relevant skills for employment and decent jobs.

Several mechanisms are used by countries to evaluate and select students for progression to higher levels of education. Such evaluations are most common at critical junctures, such as the transition from primary to secondary schooling and from secondary to postsecondary education. In many countries, such as Brazil, China, Iran, Nigeria, Türkiye, and Afghanistan, high-stakes exams play central role in determining admission to universities.

High-stakes examinations play a pivotal role in shaping students' academic and career trajectories, with outcomes often determining access to higher education and future employment opportunities. Disruptions during the preparation period, whether from political violence, armed conflict, or other emergencies, can therefore significantly impair performance on these assessments. Because such exams typically represent a gateway to tertiary education, even temporary disruptions at this juncture may have lasting consequences for educational progression.

Among educational transitions, the final year of secondary education is particularly critical, as students are simultaneously completing coursework and preparing for high-stakes exams. Disruptions at this stage can impair learning, compromise exam preparation, and lower performance. These challenges are especially pronounced in fragile and conflict-affected settings (Figure A.4), where higher educational costs, limited access to resources, and restricted mobility create significant obstacles—particularly for women and girls, who face disproportionately negative impacts that widen gender gaps in postsecondary education and the labor market.

2.2 Study Context

In Afghanistan, public universities admit applicants based on their performance on the Kankor, a nationwide entrance examination for public undergraduate and associate degree programs. Prospective students are permitted a maximum of two attempts at this exam over their lifetime (Sherzad, 2016).

On the day of the exam, students rank up to five fields of study prior to learning their test scores. In our dataset, we observe only the program to which each applicant was admitted, not their submitted rankings. Admission is granted to applicants whose scores exceed the cutoff threshold for their chosen field in that year. These cutoffs are determined competitively: high-scoring students are admitted until all seats in a given field are filled, so thresholds vary

by field and year depending on the number of candidates and availability of seats.[2] The Kankor exam is held annually nationwide, though not on the same date in all provinces. Eligible participants include high school graduates from the current academic year who register for the test, as well as applicants from previous years who have not yet used their second and final attempt. The exam must be taken in the province where the student completed high school, and most test-takers register in that province.

There are two registration windows for the exam. The first, known as the regular registration window, typically lasts a couple of weeks and opens several months before the initial exam date. During this window, most twelfth-grade students complete registration forms[3] through their high schools, which then submit all documents to the relevant government agency. The second window opens several weeks before the exam date to accommodate students who were unable to register in the first round. These students must visit the provincial capital or the central administering agency in Kabul to register for the test in their high school's province. In exceptional cases, applicants may be permitted to register and take the exam in Kabul.

Figure A.5 shows high school completion rates by province among adults aged 18 to 25 in 2016. Badghis has the lowest rate, at 3.8%, while Kabul (the capital) has the highest, at 40%, with a national median of 18%. Between 2013 and 2018, on average about 14% of high school graduates did not participate in the Kankor exam, with 21% of female high school graduates opting out (Najam, 2024). While systematic data on the characteristics of non-participants are limited (Shayan, 2015), anecdotal evidence suggests they are more likely to come from remote regions and poorer households. These patterns are consistent with findings from the ALCS 2016–17 survey data in Table A.1 on the determinants of high school completion. Among adults aged 18 to 25, higher household income and greater education spending are positively associated with completion. Females and individuals from rural and nomadic backgrounds are significantly less likely to complete high school. In contrast, household displacement is not significantly associated with high school completion.

[2] In 2016, the Afghan government introduced a gender quota, making competition in certain fields of study gender-specific while still basing admissions decisions on gender-specific exam scores. For further details on this policy, see Najam (2024).

[3] The registration fee was AFN 200 (≈ $3) until 2015, after which it was increased to AFN 400 (≈ $6). The Kankor examination plays a pivotal role in shaping higher education opportunities in Afghanistan, prompting the establishment of numerous private preparation centers across the country. However, access to these programs is highly uneven, as they are predominantly concentrated in large cities. Kankor preparation courses typically last nine months and cost around $250 (Hayward and Karim, 2019), which represents about 1/10 of the annualized income of salaried workers (Najam, Patrinos, and Kattan, 2026), though prices vary from $100 to $350 depending on course popularity.

Another distinctive feature of Afghanistan's education system is its dual academic calendar, which applies to all levels of education and is divided into warm- and cold-climate schedules (Figure 1).

The cold-climate calendar, followed by most regions, runs from early March to early December, with schools closed during the winter months (the second week of December to the first week of March) due to extreme cold, insufficient heating infrastructure, and safety concerns. In these regions, midterm examinations are typically held in July, with final exams taking place in the last week of November and the first week of December.

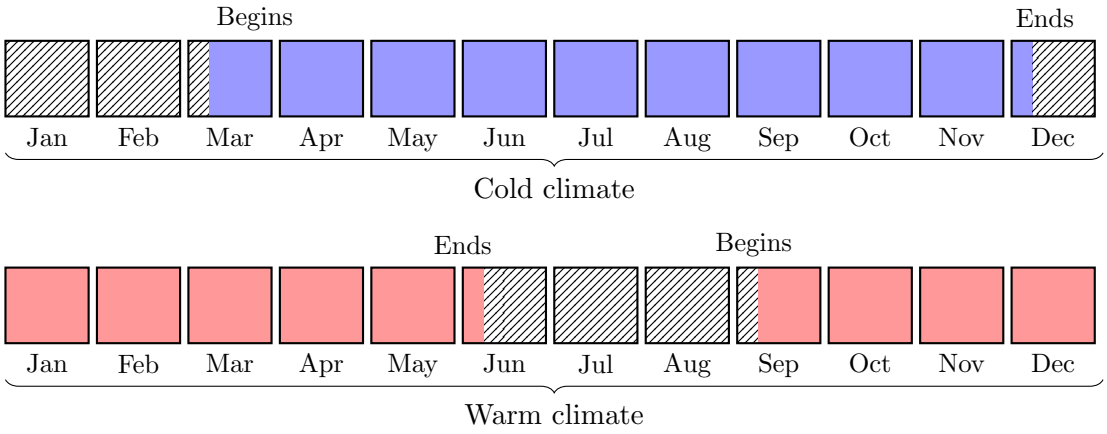
The warm-climate calendar, primarily adopted in the eastern and southwestern regions, runs from early September to early June of the following year, with schools closed during the summer months (the second week of June to the first week of September) to avoid extreme heat. In these regions, midterm exams are administered in December, and final exams are held in the last week of May and the first week of June.

This calendar structure has implications for the gap between high school completion and the Kankor exam: students in cold-climate regions typically sit the exam shortly after completing high school, while graduates in warm-climate regions must wait several months before the exam. This pattern has shifted since 2017. Prior to 2017, the Kankor exam was typically held between late November and early March (Figure A.6). In 2017, an administrative reform shifted exam scheduling to the spring and early summer months,[4] with considerable cross-province dispersion persisting within each year (Figure A.6).

Because exam dates vary by province and year, aggregating exposure to violent conflict at the annual level is likely to be imprecise and may obscure how the timing of exposure relates to the exam date. To address this, we recenter local exposure measures relative to each province's exam date, allowing us to identify when exposure occurs within the pre-exam period and assess its effect on students' performance and admission outcomes. The 2017 reform also mechanically altered the gap between the end of twelfth grade and the exam date: it lengthened the gap in cold-calendar provinces, where students previously taken the exam shortly after completing high school, and shortened it in warm-calendar provinces, where students previously waited several months after graduation (Figure A.7). We later exploit this variation to assess how the effects of violence exposure differ across pre-exam windows.

[4]In 2022 (outside the study period), the Kankor exam was held on consecutive days nationwide for the first time. However, starting in 2022, women were excluded from participating in the exam due to the Taliban's ban on education for women and girls beyond grade 6 (secondary and tertiary) (Najam, Patrinos, and Kattan, 2026).

Figure 1: Academic and Holiday Months by Calendar Type

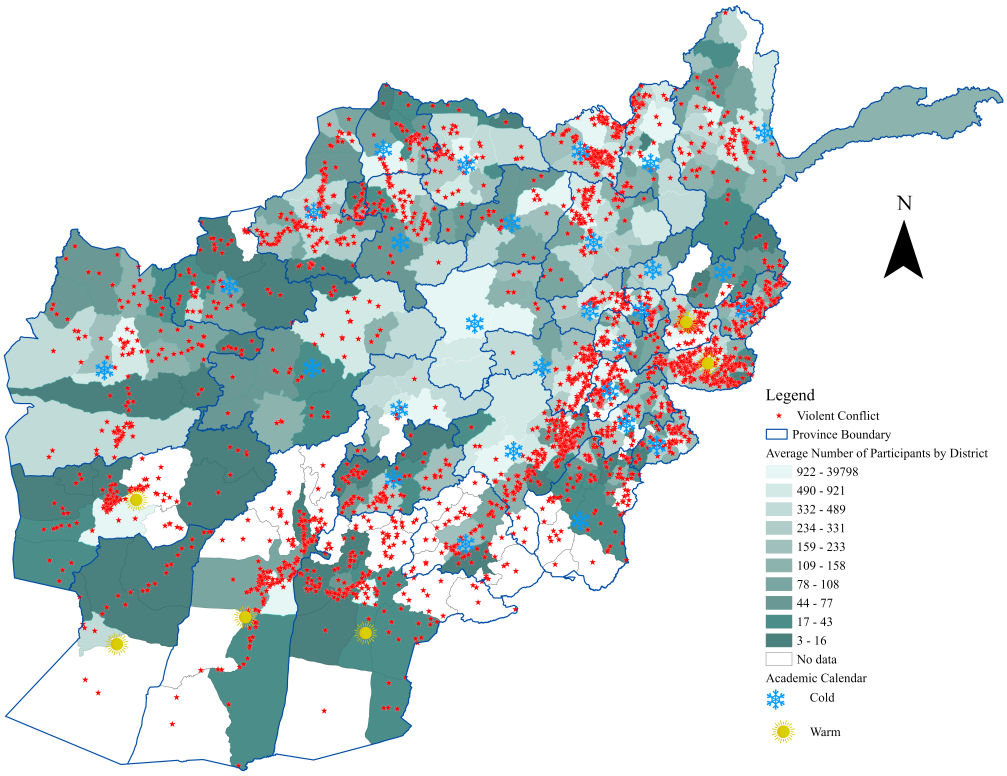


Note: This figure shows the academic calendar timeline. Solid boxes indicate academic months; dashed boxes indicate holiday months Majority of the regions operate on a cold calendar, while schools in Nangarhar, Laghman, Kandahar, Helmand, Farah, and Nimruz provinces operate on warm climate schedule.

Figure 2 illustrates the average number of test-takers across districts and years, with darker colors indicating lower participation.[5] A clear geographic divide is evident: provincial capitals and larger districts, such as Kabul and Mazar-e-Sharif, show the highest exam participation, consistent with higher school attendance in these areas. Participation is higher in the central, northern, eastern, and Kabul province regions, while the southern and southwestern regions exhibit lower levels, excluding provincial centers (depicted in lighter colors). Low participation is particularly concerning given the importance of role models in shaping educational aspirations (Winston, Zimmerman, and Dee, 2004; Barrios-Fernández, 2022; Barrios-Fernández, Neilson, and Zimmerman, 2026; Bobonis and Finan, 2009) among youth, and may discourage households from investing in education, particularly for girls.

[5]The 'No data' category does not represent zero participation; rather, it reflects cases where we could not match applicants due to inconsistencies between in reported province and high school name, preventing identification of their district of origin. Applicants in these cases may still have participated in the Kankor exam.

Figure 2: Kankor participation by district, school calendar and violent conflict incidents, 2012–2018



Note: This map illustrates the average participation in the Kankor exam by district, the spatial distribution of violent conflict incidents, and the academic calendars. These findings provide suggestive evidence for the matched applicants, who constitute 93% of total test takers for 2013–2018. For further details, see Najam (2024). We urge caution in interpreting these values as the absolute participation rate by district, as a small portion of students could not be matched to their district of origin, and some may be participants from these provinces. Data sources: Conflict incidents (Sundberg and Melander, 2013), number of participants based on authors' calculation from the Kankor datasets, and academic calendar type constructed by the authors from the schools census list 2019 (Ministry of Education, 2019).

In Figure 2, red markers depict the spatial distribution of violent conflict incidents reported in the UCDP across Afghanistan from January 2012 to December 2018 (Sundberg and Melander, 2013). At the national level, conflict is concentrated in the southern and eastern provinces. Within provinces, however, districts exhibit meaningful variation in conflict intensity, and it is this localized within-province variation that the empirical strategy exploits. Figure A.8 further shows monthly levels of nationwide violent incidents and associated fatalities during the 2012–2018 period, revealing an upward trend in both, conflicts and fatalities.

3 Data and Descriptive Statistics

3.1 Data Sources and Measurement of Key Variables

To address our research questions, we draw on several sources of data, which we describe below.

3.1.1 Kankor Test Takers Data

Our first dataset contains individual-level information on all Kankor test takers for the 2013–2018 academic years. It was compiled from open data sources and students were matched to their respective districts using high school names (for more details, see Najam (2024)). The dataset includes information on applicants' test scores, gender, school name and district, high school graduation year, the province where exam was taken, and the fields of study and postsecondary institutions to which applicants were admitted. The analytical sample is restricted to test takers whose district of origin can be matched based on their high school name.[6]

Our main variables of interest are test scores and admission outcomes. For admission outcomes, we differentiate between overall admission to any public postsecondary program and admission to a four-year undergraduate program. Over the six-year period, the average exam scores are 236.47 for men and 235.66 for women, on a scale of 0–360. Women account for 33 percent of Kankor tests takers on average, with shares ranging from 30 percent in 2013 to 34 percent in 2018.

3.1.2 Conflict Data and Timing of Kankor Exam

Data on conflict come from the Uppsala Conflict Data Program (UCDP) (Sundberg and Melander, 2013), from which we obtain geo-referenced, incident-level information on armed conflict events, including reported fatalities, for 2012–2018. Using geolocation of each incident, we match events to district boundaries to construct a district-by-day panel of conflict incidents.

[6]We compare applicants with matched and unmatched records and find that the unmatched sample has a lower proportion of female test takers, less likely to be admitted to tertiary education programs, and more likely to have taken a gap year. The differences in unmatched sample by province are minimal, as seen in Table A.2.

Province-level exam dates are identified through a web search of historical schedules from the examination authority and news outlets, specifically from the education section of Bakhtar News Agency,[7] the official state news agency. The three 90-day pre-exam windows span most of the academic year in cold-climate regions and most of the latter half of the academic year in warm-climate areas, as shown in Figure 1. Provincial exam dates vary substantially across both provinces and years, as shown in Figure A.9.[8]

In 2013–2016, most provinces administered the Kankor exam between November and February, while a 2017 administrative reform shifted scheduling to the spring and early summer months, with considerable cross-province dispersion persisting within each year. This variation is central to our identification strategy and shapes how we measure conflict exposure. Because the same calendar month can fall into different pre-exam windows depending on a province's exam date, measuring conflict in calendar time would assign identical numbers of violence events to different exposure periods across districts and years, conflating the timing of conflict with its position in the preparation window. We therefore anchor all conflict exposure measures to each province's exam date rather than to the calendar year.

To examine how the timing of violence exposure relative to the exam date affects student performance and admission outcomes, we construct district-level measures of violence intensity for the period leading up to each province's exam date. Specifically, we divide the 270 days preceding the exam into three 90-day windows (1–90, 91–180, and 181–270 days before the exam) and compute the total number of conflict-related fatalities per 1,000 population in each district-window, using WorldPop 2013 district-level population estimates (WorldPop and CIESIN, 2018). We use 90-day windows to reduce collinearity among violence exposure measures and to avoid finer bins dominated by zeros, which would inflate noise without adding identifying variation. We then standardize the measure within each year and window by subtracting the year-window mean and dividing by the standard deviation, yielding a Z-score that captures a district's exposure to violence relative to other districts within the same exam year and window. This within-year, window-specific standardization ensures that exposure measures are directly comparable across the three pre-exam windows: a Z-score of one in the 1–90 day window carries the same interpretation as a Z-score of one in the 91–180

[7]Bakhtar News Agency. https://bakhtarnews.af/en/category/news/education/ Last Accessed: 12/27/2024.

[8]This feature of exam scheduling has received limited attention in the literature. For example, studies such as (Darwish and Wotipka, 2022) measure exposure to violence exposure at the calendar-year level, which may introduce measurement error and fails to capture the precise timing of violence in the period immediately preceding high-stakes examinations.

day window. This comparability is essential for assessing whether the timing of conflict exposure within the pre-exam period differentially affects outcomes.

This normalization serves several purposes, each addressing a distinct dimension.[9]

First, the standardization and the province × year fixed effects play complementary roles throughout: the fixed effects absorb differences in *levels*, while standardizing within each window × year equalizes differences in *scale*, so that a one-standard-deviation increase carries a common meaning across relevant comparison.

Second, the normalization ensures comparability across cohorts. The cross-district distribution of conflict intensity widens over the sample period, with fatalities nearly tripling between 2013 and 2018 (Figure A.8). Province × year fixed effects absorb differences in average violence across province-year cohorts, while year-specific standardization equalizes changes in the cross-district dispersion. Consequently, a one-standard-deviation increase denotes the same relative shock in an early, low-dispersion cohort as in a later cohort with high-dispersion.

Third, the normalization ensures comparability across districts. Standardizing within each window × year places every district on a common scale relative to others in the same window-year, so that cross-district comparisons capture relative exposure intensity; the province × year fixed effects complement this by absorbing province-specific conditions, including seasonal factors.

Finally, the normalization ensures comparability across windows. Standardizing separately within each of the three pre-exam windows makes the coefficients directly comparable: a one-standard-deviation increase in the 1–90 day window has the same interpretation as a one-standard-deviation increase in the 91–180 day window. This comparability is central to the timing analysis at the core of our design.

3.1.3 Weather Controls

Next, because exams are often conducted in provincial capitals during the winter and spring months, when temperatures are low and precipitation is high, it is important to control weather variations. These conditions can significantly hinder students' travel from their districts to provincial capitals and may affect performance, particularly as some exams are held in open spaces. We therefore include district-level weather controls drawn from

[9]Table A.3 shows an example of the normalized violence exposure construction.

the ERA5-Land Hourly ECMWF Climate Reanalysis (Hersbach et al., 2023), accessed via Google Earth Engine, covering temperature, humidity, precipitation, and wind speed. For each district, we compute the average value of each indicator over the seven days preceding the exam date and subtract the district-level historical mean, so that the controls capture deviations from expected local conditions rather than absolute levels.

3.1.4 Afghan Living Conditions Survey 2016/17

Finally, we use the nationally representative Afghan Living Conditions Survey 2016–17 to examine the mechanisms through which conflict exposure affects student performance. Specifically, we assess how perceived insecurity is associated with household educational spending, economic hardship, and the likelihood of displacement. Using individual-level data, we also examine the role of tertiary education in buffering households against economic shocks and improving labor market outcomes for working-age adults.

3.1.5 Descriptive Statistics

We report summary statistics for our key variables in Table 1. On average, nearly 190,000 applicants take the Kankor exam each year. Following the exam, 45 percent of test-takers are admitted to public tertiary education programs, including both associate and undergraduate programs, while only about 30 percent are admitted to undergraduate programs. One-third of test-takers are female, and about 21 percent of all test-takers have a gap between high school graduation and exam participation: 16 percentage points take a one-year gap and the remaining 5 percentage points take a gap of two years or more. About 10 percent of test-takers are from regions with a warm-climate academic calendar. Exposure to violence varies across districts and over time, as reflected in the standardized measures.

Table 1: Descriptive statistics for 2013–2018

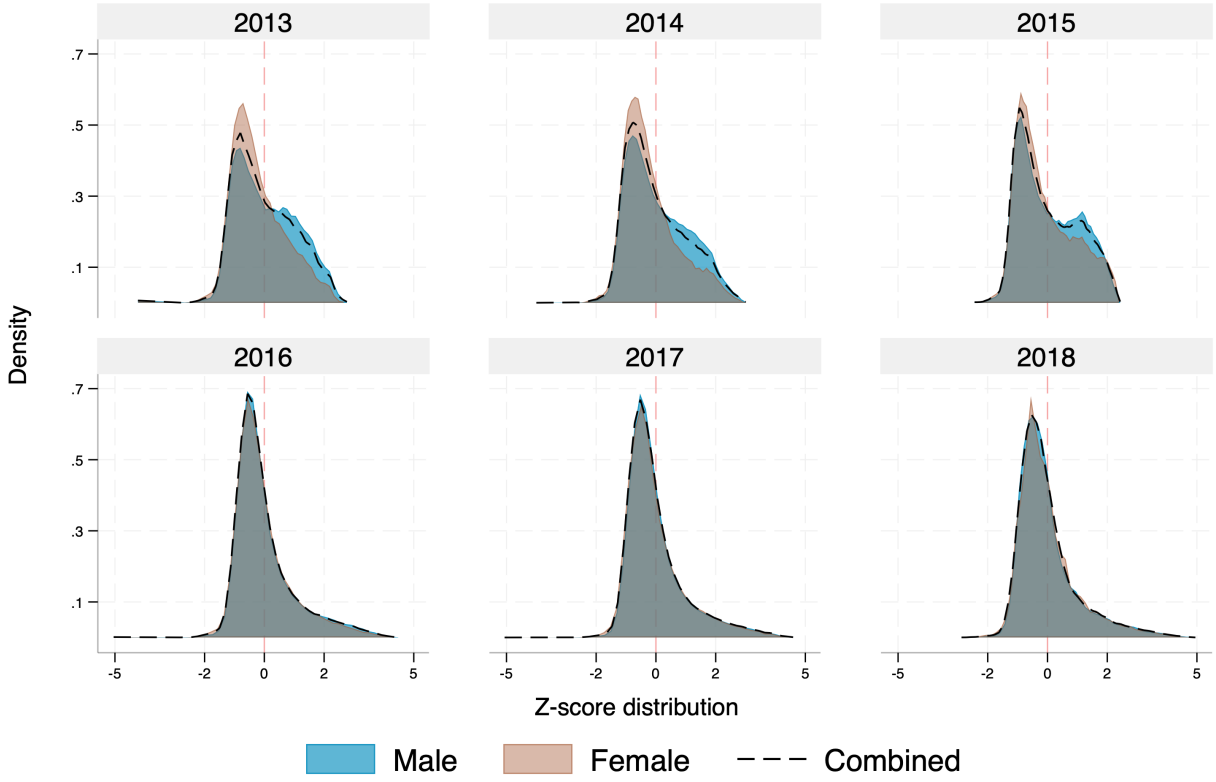
	Mean	SD	Min	Max	N
Admission outcomes					
Associate & undergraduate degrees	0.447	0.497	0	1	1,061,726
Undergraduate degree only	0.301	0.459	0	1	1,061,726
Applicant characteristics					
Female	0.331	0.471	0	1	1,061,726
Age	18.319	0.037	18.259	18.363	1,061,726
Gap year	0.212	0.409	0	1	1,062,025
Warm Climate	0.099	0.299	0.000	1.000	1,062,025
Pre-exam Total					
Incidents (1–270 days)	8.650	11.069	0	103	1,061,726
Fatalities (1–270 days)	64.998	127.821	0	992	1,062,025
Standardized violence exposure					
1–90 days	-0.169	0.478	-0.379	16.772	1,061,726
91–180 days	-0.154	0.385	-0.373	18.178	1,061,726
181–270 days	-0.175	0.525	-0.364	17.036	1,061,726
Standardized violence exposure (winsorized at 99th percentile)					
1–90 days	-0.173	0.429	-0.379	4.656	1,061,726
91–180 days	-0.156	0.347	-0.373	4.280	1,061,726
181–270 days	-0.184	0.390	-0.364	4.231	1,061,726
District-level control variables					
Dev. 7-day precipitation (inches)	0.004	0.026	-0.046	0.189	1,061,726
Dev. 7-day temperature (°C)	-8.069	10.293	-23.904	15.108	1,061,726
Dev. 7-day relative humidity	9.816	14.472	-27.917	45.939	1,061,726
Dev. 7-day wind speed	2.440	2.192	-12.507	13.364	1,061,726

Note: This table reports the descriptive statistics for the analytical sample of test-takers. The "Gap year" indicates the share of test takers who participated in the exam a year or more after graduation—an estimated difference between high school graduation year and the year the applicant participated in the Kankor exam. Standardized violence exposure variables are z-scores of fatalities per 1,000 population in the district, standardized by year within each window. Winsorized measures trim the top 1% of the distribution. The weather variables (7-day precipitation, temperature (°C), relative humidity, and wind speed (km/hour)) reflect the differences between district-specific 7-day averages leading up to the exam date and the corresponding historical daily averages. For example, a positive value in precipitation indicates more rainfall than the expected historical daily average.

Our primary outcome, applicants' test scores, is standardized annually to facilitate interpretation. Each score is expressed in standard deviations from the national average in that year, and the resulting distributions are shown in Figure 3. Prior to 2016, there were notable gender differences in the score distribution, with male applicants generally scoring higher on average. From 2016 onward, however, the distributions by gender become more similar. These changes may reflect systematic reforms, including the introduction of a

biometric verification system in 2016 to confirm test-taker identity.

Figure 3: Kankor Z-score distribution by year and gender



Note: This figure shows the yearly distribution of standardized scores, both pooled and by gender. Before 2016, gender-based differences were evident; from 2016 onward, however, the distributions become more similar.

4 Empirical Strategy

4.1 Estimation Model

In our analysis, we examine the relationship between the timing of exposure to elevated violence and high-stakes exam outcomes, specifically, test scores and admission probabilities. We also explore heterogeneity by gender and institutional characteristics, including regional variation in high school completion rates and differences in academic calendars.

We exploit variation in exposure to violence across districts within a given province-year and estimate an ordinary least squares (OLS) specification in Equation 1, controlling for time-

varying district characteristics. This approach helps isolate the effects of violent conflict by comparing students living broadly under similar economic, educational, cultural and security conditions within provinces. Our main estimates capture the effects of violence exposure on applicants' test scores and their probability of admission to (i) public undergraduate programs and (ii) any public tertiary programs (associate or undergraduate).

$$Y_{idpt} = \alpha + \sum_{k \in \{\text{1-90, 91-180, 181-270}\}} \beta_k \cdot \text{Violence exposure}_{dptk} + \lambda X_{dpt} + \rho \text{female}_{idpt} + \mu_t + \gamma_{pt} + \epsilon_{idpt} \tag{1}$$

The Y_{idpt} is the outcome—test score or probability of admission—for an individual i in district d in province p and year t. The term $\sum_{k \in \{\text{1-90, 91-180, 181-270}\}} \beta_k \cdot \text{Violence exposure}_{dptk}$ captures the effect of violence exposure over the three 90-day windows prior to the exam date. $\text{Violence exposure}_{dptk}$ is defined as fatalities per 1,000 population in district d of province p during window k, standardized within window-year so that the distribution has mean zero and unit standard deviation. Each β_k measures the effect of a one-standard-deviation increase in conflict intensity on student outcomes. The term X_{dpt} is a vector of district-level time-variant controls, including deviations in weather conditions, such as temperature, precipitation, humidity, and wind speed, from the district-specific historical mean during the seven days preceding the exam. Female is an indicator variable equal to one if the test taker i is female and zero otherwise. μ_t denotes exam-month fixed effects, which capture the calendar month in which the exam was administered, and γ_{pt} denotes province-by-year fixed effects. Because each province administers the Kankor exam in a single month within a given year, μ_t is collinear with γ_{pt} and is therefore absorbed in estimation. The inclusion of province-year fixed effects accounts for potential confounding factors correlated with violence both across provinces (Figure 2) and over time (Figure A.8), allowing us to exploit variation in conflict exposure across districts d within province p in year t. This enables comparisons across districts within the same province-year, effectively holding constant economic, educational, and security conditions. We cluster standard errors at the district level following Bertrand, Duflo, and Mullainathan (2004).

4.2 Threats to Identification Strategy

In this subsection, we explore several threats to our identification strategy. The first set concerns the validity of the assumption that conflict events are quasi-random relative to the

exam date.

The first concern in this set is whether insurgent groups systematically increase violence in the period leading up to the exam date. To assess this, we aggregate conflict to the provincial level, where exam dates vary both within and across years, and test whether violence exposure, measured by incidents and reported fatalities, rises discontinuously around the exam date. Figures A.10 and A.11 show no discontinuous increase in either conflict incidents or fatalities around the exam date. Formal RD estimates at the exam-date cutoff are small and statistically indistinguishable from zero for both measures, consistent with the assumption that local conflict is not systematically timed in anticipation of the exam.

The second concern is whether exam administrators respond to elevated conflict by rescheduling exams. To assess this, we conduct a predetermination test that avoids the mechanical circularity of anchoring violence exposure windows to the realized exam date. We measure each province's exam timing as the number of days between November 1 of the prior year and the exam date (as in Figure A.9), providing a common anchor that predates all exams. Conflict exposure, measured by incidents and fatalities, is then aggregated within 90-day windows counted backward from November 1, while year fixed effects absorb the structural shift in the exam calendar between the pre-reform (2013–2016) and post-reform (2017–2018) periods. We regress exam-date deviations on conflict incidents and fatalities in the 1–90, 91–180, and 181–270 day windows, absorbing province and year fixed effects and clustering standard errors at the province level. As shown in Figure A.12, the coefficients for the 91–180 and 181–270 day windows are small and statistically insignificant for both incidents and fatalities. The 1–90 day window (August–October, corresponding to peak violence in most years: Figure A.8) shows a significant association for both incidents and fatalities, although the magnitudes remain small. At the sample mean of approximately 2.8 incidents and 22.4 fatalities, the implied shifts in exam timing are about 3.5 and 1.5 days, respectively, consistent with limited short-term logistical adjustments rather than systematic rescheduling in response to local security conditions. Together with the null results for the 91-180 and 181-270 day windows and with the RD continuity evidence (Figures A.10 and A.11), these results suggest any conflict-related adjustments in exam timing are modest in magnitude and do not appear to reflect systematic rescheduling in response to local conflict conditions.

We next consider two additional concerns and assess their empirical relevance.

The first concern is that conflict exposure affects selection into test-taking. Table A.4

examines the extensive margin by estimating the effect of pre-exam conflict exposure on district-level exam participation. Higher conflict exposure is significantly associated with a reduction in overall participation and the female share of participants, with weaker evidence for female absolute counts. These effects point to broader selection concern: conflict shapes which students remain in school, complete high school, and ultimately sit for the exam. Table A.1 shows that students from higher-income households with greater educational spending are more likely to complete high school, suggesting that those who persist through conflict are positively selected on both, resources and motivation. This selection is likely to be particularly pronounced among women, given their lower baseline participation. As a result, our estimates of conflict's effect on test scores and admission outcomes are likely biased toward zero due to positive selection at the participation margin, where the students most adversely affected by conflict are less likely to appear in the exam-taking sample, leading to attenuation, especially among women.

Furthermore, conflict frequently induces displacement, which may alter the composition of test takers. However, student migration out of conflict-affected areas is likely limited given the strong constraints on mobility in Afghanistan. Switching schools during the final year is costly, high school options outside large cities are limited, and difficult terrain restricts inter-district travel. Consistent with this, Table A.1 shows that household displacement status is not significantly associated with high school completion.

Taken together, the results of these four tests support our identification strategy. The scheduling validation tests indicate that the timing of the exposure window is quasi-random: conflict does not rise discontinuously around the exam date, and exam scheduling is not systematically predicted by prior conflict levels. Given the inclusion of province $\times$ year fixed effects, however, identification relies on cross-district variation in conflict intensity within a province-year, specifically, which districts experience relatively high violence during the administratively determined exposure window. The plausibility of this spatial variation as a source of identification is supported by the post-exam placebo test (Table A.5), which shows that exposure to violence immediately after the exam in the same districts does not predict outcomes. Any remaining selection bias from conflict-induced exam non-participation would bias estimates against finding negative effects. While we cannot rule out all district-level time-varying confounders, province-year fixed effects absorb the most consequential sources of omitted variable bias.

4.3 IV Estimation Strategy: Test Scores and Admission Outcomes

We hypothesize that conflict affects admission outcomes through test scores. To assess the magnitude of this effect, we employ an instrumental variable approach that uses standardized violence exposure measures as instruments for standardized test scores, as specified in Equation 2. The validity of this approach rests on the exclusion restriction: that conflict affects admission outcomes only through test performance. The institutional setting lends plausibility to this condition: admission decisions are determined solely by Kankor exam performance, and applicants select their field-of-study preferences on the day of the exam without knowledge of their scores. That said, the exclusion restriction cannot be tested directly. Conflict could, in principle, affect admission through channels other than test performance, for example, if violence leads students to change their field of study preferences or if household financial constraints influence program choices independently of test scores. However, these channels likely operate, at least in part, through anticipated test performance. Because students rank fields on the day of the exam without knowing their scores, any adjustments in rankings by conflict-affected students plausibly reflect expectations of lower performance due to disrupted preparation. Similarly, if conflict-induced household financial constraints reduce educational investment, the primary pathway to worse admission outcomes is likely through diminished preparation and lower test scores rather than through a direct effect on placement conditional on scores. The exclusion restriction is therefore more plausible than a simple enumeration of potential threats might suggest, although we cannot rule out all direct channels and accordingly interpret the IV estimates as suggestive evidence on the test score mechanism rather than as strictly causal.

$$Y_{idpt} = \alpha + \beta \left(Score_{idpt} = \sum_{k \in \{\text{1-90}, \text{91-180}, \text{181-270}\}} \beta_k \cdot \text{Violence exposure}_{dptk} \right) + \lambda X_{dpt} + \rho \text{female}_{idpt} + \mu_t + \gamma_{pt} + \epsilon_{idpt} \tag{2}$$

In Equation 2, the outcome variable is the likelihood of admission to a four-year public undergraduate program or to any public tertiary program. The term $Score_{idpt} = \sum_{k \in \text{1-90},,\text{91-180},,\text{181-270}} \beta_k \cdot \text{Violence exposure}_{dptk}$ denotes applicant i's standardized test score, instrumented by the three 90-day pre-exam conflict exposure measures. The remaining terms are as defined in Equation 1.

5 Results

This section presents the main findings on the relationship between pre-exam violence exposure and high-stakes exam outcomes, followed by heterogeneity analyses and a discussion of potential mechanisms.

5.1 Main Results

Table 2 reports our primary specification across three academic outcomes: standardized Kankor test scores (Column 1), admission to public undergraduate programs (Column 2), and overall admission to public tertiary education, which combines associate and undergraduate programs (Column 3). Each column presents a separate regression of the outcome on the three pre-exam violence exposure windows, 1–90, 91–180, and 181–270 days before the exam date, along with a female indicator. All specifications include district-level controls, province $\times$ year fixed effects, and exam-month fixed effects, with standard errors clustered at the district level.

Female test-takers perform significantly worse than their male counterparts on all three outcomes. They score 0.149 SD lower on the standardized exam, are 7.2 percentage points less likely to matriculate into a public undergraduate program, and 4.3 percentage points less likely to be admitted to any public tertiary program. Relative to outcome means of 0.301 and 0.447, these gender gaps correspond to roughly 24 percent and 10 percent of the mean admission rate, respectively.

Higher district-level violence intensity in the months preceding the Kankor is associated with worse outcomes across all three dependent variables, with the strongest and most consistent effects in the 91–180 day window. A one-standard-deviation increase in population-normalized fatalities during this period is associated with a 0.055 SD decline in test scores, a 1.0 percentage points reduction in undergraduate admission, and a 2.0 percentage points reduction in overall tertiary admission—equivalent to about 3.3 percent and 5.0 percent of the respective outcome means. Exposure during the most recent window (1–90 days before the exam) is also statistically significant but smaller in magnitude across all three outcomes. By contrast, exposure 181–270 days before the exam is not statistically distinguishable from zero in any specification, consistent with the effects of earlier conflict dissipating by the time of exam.

Table 2: Effects of pre-exam violence exposure on academic outcomes

	Test Score (Standardized)	Undergraduate Admission	Overall Admission
	(1)	(2)	(3)
Violence exposure pre-exam date			
1–90 days	-0.027**	-0.008**	-0.008*
	(0.011)	(0.004)	(0.004)
91–180 days	-0.055***	-0.010**	-0.020***
	(0.014)	(0.004)	(0.006)
181–270 days	-0.014	-0.002	-0.005
	(0.011)	(0.004)	(0.005)
Female	-0.149***	-0.072***	-0.043***
	(0.039)	(0.009)	(0.013)
$\beta_1 = \beta_2$ (P-value)	0.136	0.742	0.111
$\beta_2 = \beta_3$ (P-value)	0.036	0.294	0.077
Joint F-test (p-value)	0.000	0.002	0.000
N	1,061,725	1,061,726	1,061,726
Outcome average		0.301	0.447
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

Note: Each column reports estimates from a separate regression of the indicated outcome on pre-exam violence intensity measures (Z-scored fatalities per 1,000 population by time window within a year). Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

The joint F-test rejects the null of no effect of pre-exam violence exposure for all three outcomes, indicating that such exposure meaningfully depresses academic performance.

Pairwise equality tests provide insight into the timing of these effects. The 1–90 and 91–180 day coefficients are each statistically significant across all outcomes, and we cannot reject their equality in any specification. However, persistent within-district conflict limits power to detect modest differences across adjacent windows. As such, this should not be interpreted as evidence that the two coefficients are equal in magnitude—but rather that we cannot reject their equality. By contrast, the 181–270 day coefficient is statistically indistinguishable from zero in all specifications. For test scores, we reject equality between the 91–180 and the

181–270 day coefficients, with overall admission showing a similar pattern. Taken together, the individual coefficients and pairwise tests support a two-window pattern: conflict within roughly six months of the exam depresses performance, while conflict 6–9 months prior has no detectable effect on exam-day outcomes. This attenuation with temporal distance from the exam is consistent with violence operating, at least in part, through proximate channels.

These two proximate windows correspond to substantively distinct phases of the pre-exam period for cold-calendar students, who account for the majority of the sample. The 91–180 day window falls within the school year and overlaps with the post-midterm exam intensive preparation phase, when students are engaged in classroom instruction and structured Kankor review. The midterm examination, while not required for high school completion, is widely regarded as a benchmark for students registering for the Kankor. Instruction in this phase focuses on twelfth-grade material, which the Kankor weights most heavily, drawing predominantly from grades ten through twelve with particular emphasis on the final year. Disruptions during this window plausibly operate by interrupting ongoing schooling and structured preparation. The 1–90 day window overlaps with the end of formal instruction for pre-2017 cohorts and falls entirely after active schooling period for post-2017 cohorts. Students in this window are largely in self-directed review and managing exam-day logistics; disruptions here plausibly operate through more proximate channels such as stress, constraints on mobility, and logistical interference with the exam itself. The reduced-form design cannot separately identify these channels in the pooled specification. We therefore exploit the 2017 reform to provide examine each channel more directly.

5.2 Robustness Checks

To evaluate the credibility of our identification strategy, we perform several additional robustness checks. We begin with a falsification test, where we add to the main model a set of controls for post-exam exposure to conflict. Next, we assess the robustness of our results to alternative measures of violence exposure.

Table A.5 reports results of a falsification test estimating the effects of both pre- and post-exam violence exposure within the same specification. The coefficients on violence in two pre-exam windows closest to the exam, the 1–90 and the 91–180 day windows, remain significant across all three outcomes, consistent with the main results. The pre-exam windows are jointly significant at the one percent level for all outcomes, and the combined pre-exam effect (days 1–270) is large and precisely estimated. By contrast, the post-exam windows

are jointly insignificant for both admission outcomes. For test scores, while the post-exam joint test is statistically significant at the 95 percent level, this effect is driven by a single coefficient in the most distant window with exposure occurring seven to nine months after the exam.

This asymmetry supports the interpretation that exposure to violence in the period leading up to the exam drives the results, rather than unobserved district-level trends correlated with both conflict and academic performance. Regarding district-level confounding, if persistent characteristics such as remoteness, state capacity, or chronic conflict intensity were driving the estimated effects, they would raise coefficients across all pre-exam windows similarly. A time-invariant confounder cannot plausibly generate a pattern in which only the immediate pre-exam windows are affected, while coefficients for more distant pre-exam and post-exam windows remain near zero.

Next, we assess sensitivity of our results to the operationalization of violence exposure. For that, we re-estimate our main specification using three alternative measures. The first conflict measure used in Table A.6 is set-up to limit the influence of extreme outliers, where the z-scored exposure is winsorized at the 99th percentile. For the second conflict measure, in Table A.7, we replace the continuous measure of conflict with a set of binary indicators equal to one if district-level fatality intensity exceeds the 75th percentile within each exposure window. For the third measure, in Table A.8, we use raw fatalities per 1,000 population without standardization as a measure of conflict exposure. Across all three specifications, the joint F-test remains significant for every outcome, and the sign on the coefficients is consistent with the main results.

In the specifications in Table A.6 using a winsorized measure, the 91–180 day window consistently yields the largest and most robust negative coefficient. The estimated coefficient for the 181–270 day window remains not statistically significant, confirming the two-window pattern observed in the main results.

The specification in Table A.8 which uses raw fatalities data, yields a broadly similar pattern for overall admission: the 91–180 day window produces the largest coefficient, but the results differ from our main results in two respects. For test scores, all three windows produce coefficients of comparable magnitude and statistical significance, including the 181–270 day window, suggesting that without within-year standardization the measure does not as sharply distinguish effects attributed to recent versus distant exposure. For undergraduate admission, only the 1–90 day window yields the largest and individually significant coefficient, with a

smaller and not statistically significant coefficient for the 91–180 exposure window. Beyond these differences in pattern, the raw fatalities coefficients also differ in interpretation: each coefficient captures the effect of an additional fatality per 1,000 population in absolute terms, whereas the z-scored measure reflects the effect of a one-standard-deviation increase relative to the national distribution within the same window and year. The raw measure therefore conflates the timing variation of interest with secular escalation and seasonal patterns in fatalities, factors that the z-score normalization is designed to absorb and that likely explain the less distinct timing gradient in this specification. For this reason, magnitudes are not directly comparable across the two operationalizations.

5.3 Beyond the Baseline: Distributional and Temporal Patterns

In this subsection, we present three additional analyses to assess whether the main results vary across distributional and temporal dimensions.

First, we examine the heterogeneity in responses to conflict across the score distribution (Figures A.13 and A.14). Figure A.13 shows that admission probabilities increase steeply with test scores: admission to four-year programs rises from 0.2% in the bottom score quintile to 84.9% in the top quintile, with the sharpest gains between the third and top quintiles; while admission to any program approaches saturation in the top quintile.

Figure A.14 presents the effect of violence on test scores across the conditional score distribution. The 91–180 day effect is negative and statistically significant at every percentile, ranging from -0.045 at the 10th percentile to -0.067 at the 90th, indicating a broadly uniform downward shift. By contrast, the 1–90 day effect is not statistically different from zero at the 95% significance level at the 10th and 25th percentiles, but becomes larger in absolute value and statistically significant (at least at the 95 percent level) for the top three percentiles, peaking at -0.053 at the 90th percentile. The 181–270 day effect is null throughout. The two windows thus affect different parts of the distribution: medium-horizon exposure depresses scores broadly, while recent violence exposure disproportionately penalizes high-performing students, consistent with disruption to intensive exam-preparation in the weeks preceding the test.

In our second analysis, we exploit the 2017 exam reform by splitting the sample at the reform. Tables A.9 and A.10 report results separately for the earlier (2013–2016) and later (2017–2018) exam cohorts. Across both periods, violence exposure within roughly six months

of the exam is negatively associated with test scores. In Panel A of Table A.9, the coefficients for the 1–90 and 91–180 day windows are both negative and individually significant, while the 181–270 day coefficient is small and not statistically significant. The joint F-test rejects the null for all three outcomes. For test scores, Panel B shows negative point estimates across all three pre-exam windows. The coefficients for the 1–90 and 181–270 day windows are similar in magnitude and at least marginally significant; indicating a less sharply defined distinction between recent and more distant exposure in this cohort, although the joint F-test result remains significant. Two factors likely contribute to this attenuation. First, with only two post-2017 years, identification relies on fewer province × year cells, reducing statistical power. Second, the 2017 reform mechanically altered the mapping between pre-exam windows and periods of active schooling. Consistent with this, Table A.10 shows that in cold-calendar provinces the 91–180 day coefficient attenuates as the window's overlap with active schooling declines, whereas the 1–90 day coefficient remains stable across sub-samples.

Finally, we estimate year-specific effects. Figures A.15–A.17 report estimates for each pre-exam exposure window across the three outcomes by exam year. The majority of estimates in the 1–90 and 91–180 day windows are negative across years, indicating that the pooled results are not driven by any single year. Individual year estimates are naturally less precise, but the overall pattern reinforces the main findings: violence exposure in the two windows closest to the exam reduces both test scores and admission probabilities.

5.4 Heterogeneity Analysis

In this section (Table 3) we present heterogeneity results along three dimensions: (i) gender, (ii) provinces with above- and below-median high school completion rates and (iii) calendar type. Results are reported for three outcomes: test scores (Panel A), the probability of undergraduate admission (Panel B), and the probability of overall admission (Panel C).

In Panel A, the results reveal an asymmetry consistent with positive selection into exam-taking among women. For women (Column 1), we observe a sizable negative impact of violence on test scores only in in the 91–180 day window (-0.098, significant at the 5% level). For men, the test scores (Column 2) are negatively and statistically significantly associated with violence in both the immediate and medium-term windows, albeit with smaller coefficients (-0.032 and -0.051, significant at the 5 and 1% percent respectively). If conflict deters lesser-prepared female candidates from sitting the exam, the remaining female sample is positively selected, attenuating estimated effects. Nevertheless, the larger

medium-term effect for women suggests that even among positively selected test-takers, sustained disruption to schooling is difficult to offset.

In Columns 3–4, we stratify provinces by high school completion rates. Test scores in both subgroups are significantly negatively affected, with comparable magnitudes for exposure in the medium-term window.

Effects of violence in the other two windows vary across groups: in above-median provinces, violence in the immediate window for has a sizable and statistically significant effect, whereas in below-median provinces, the most distant window shows a marginally significant effect. Importantly, the effects of pre-exam violence exposure are not concentrated in educationally weaker districts, instead, they indicate that conflict disrupts academic performance across different educational environments.

We next examine heterogeneity by calendar type. We find significant effects in the near- and medium-term windows for cold-calendar provinces (Column 5), which cover most of the country, but no detectable impact in the pooled specification for warm-calendar provinces. The cold-calendar subsample is nearly ten times larger, providing substantially greater statistical power. In addition, warm-calendar provinces largely experience higher baseline levels of violence (as shown in Figure 2), which may attenuate the marginal impact of additional exposure. However, interpreting this pooled comparison requires caution, as it conflates variation across windows: the 2017 reform shifted the timing of pre-exam windows relative to active schooling.

As noted earlier, the 2017 administrative reform shifted exam scheduling from winter–spring to spring–summer, mechanically altering the alignment between pre-exam windows and active twelfth-grade schooling in opposite directions across the two calendar types (Figure A.7). For cold-calendar provinces, the reform extended the gap between the end of twelfth grade and the exam, shifting the 91–180 day window from largely within twelfth grade to mostly after completion. For warm-calendar provinces, the reform compressed this gap, moving the pre-exam windows from the post-graduation period to the final months of twelfth grade. Splitting the calendar-type heterogeneity by reform period (Table A.10) provides a sharper test of whether the cold-warm contrast reflects differences in calendar-window timing or other regional factors.

Within cold-calendar provinces, the 91–180 day coefficient on test scores attenuates as the window's overlap with active schooling declines post-reform, consistent with this window partly operating through disruption to ongoing schooling. By contrast, the 1–90 day coefficient

remains essentially unchanged across sub-periods, despite the window straddling the end of twelfth grade in the pre-reform period and occurring several months later post-reform.

The persistence of this proximate-window effect long after active schooling has ended suggests it operates through channels other than schooling disruption, such as stress, mobility frictions, and logistical interference with the exam itself. Warm-calendar provinces, by contrast, show no negative effects in either period, including post-reform, when students take the exam at or shortly after the end of twelfth grade. Limited statistical power in the warm-calendar cells warrants caution: the warm-post cell includes only six provinces over two years. Nevertheless, the overall pattern suggests that region-specific factors, such as higher baseline violence, conflict composition, or selective exam-participation, rather than calendar-window timing, drive the warm-cold contrast in the pooled estimates. To summarize, the medium-term and proximate windows appear to operate through partially distinct mechanisms, while the cold-warm contrast likely reflects regional features that the design cannot fully isolate.

Table 3: Heterogeneity in the effects of pre-exam violence exposure

	Gender		HS Completion Rate		Calendar Type	
	Female	Male	Below	Above	Cold	Warm
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Standardized test score						
Violence exposure pre-exam date						
1–90 days	-0.014	-0.032**	-0.001	-0.033***	-0.032***	0.025
	(0.016)	(0.013)	(0.018)	(0.012)	(0.012)	(0.017)
91–180 days	-0.098**	-0.051***	-0.047***	-0.056**	-0.055***	-0.017
	(0.047)	(0.013)	(0.018)	(0.022)	(0.017)	(0.016)
181–270 days	-0.015	-0.017	-0.019*	-0.004	-0.012	0.004
	(0.028)	(0.010)	(0.010)	(0.016)	(0.013)	(0.019)
Female			-0.202***	-0.137***	-0.154***	-0.063
			(0.036)	(0.043)	(0.042)	(0.038)
$\beta_1 = \beta_2$	0.097	0.321	0.089	0.362	0.289	0.102
$\beta_2 = \beta_3$	0.165	0.060	0.210	0.095	0.060	0.425
Joint F-test	0.064	0.000	0.006	0.001	0.000	0.412
N	351,390	710,333	267,366	794,359	956,256	105,469
Panel B: Undergraduate admission						
Violence exposure pre-exam date						

Continued on next page

Table 3 (continued)

	Gender		HS Completion Rate		Calendar Type	
	Female	Male	Below	Above	Cold	Warm
	(1)	(2)	(3)	(4)	(5)	(6)
1–90 days	-0.005	-0.008*	0.002	-0.009**	-0.009**	0.008
	(0.006)	(0.004)	(0.007)	(0.004)	(0.004)	(0.008)
91–180 days	-0.026**	-0.008**	-0.006	-0.012*	-0.009*	-0.000
	(0.013)	(0.004)	(0.005)	(0.007)	(0.005)	(0.004)
181–270 days	-0.004	-0.003	-0.007	0.004	-0.001	0.001
	(0.009)	(0.004)	(0.004)	(0.007)	(0.005)	(0.007)
Female			-0.056***	-0.075***	-0.078***	0.052**
			(0.020)	(0.010)	(0.010)	(0.026)
$\beta_1 = \beta_2$	0.141	0.994	0.389	0.736	0.951	0.409
$\beta_2 = \beta_3$	0.238	0.371	0.942	0.193	0.381	0.868
Joint F-test	0.057	0.012	0.119	0.015	0.008	0.697
N	351,390	710,334	267,366	794,360	956,257	105,469
Outcome average	0.242	0.331	0.382	0.274	0.293	0.378
Panel C: Overall admission (associate and/or undergraduate)						
Violence exposure pre-exam date						
1–90 days	-0.002	-0.010*	0.001	-0.010*	-0.009*	0.007
	(0.006)	(0.005)	(0.007)	(0.005)	(0.005)	(0.008)
91–180 days	-0.041**	-0.019***	-0.013*	-0.026***	-0.022***	-0.003
	(0.016)	(0.006)	(0.007)	(0.009)	(0.007)	(0.005)
181–270 days	-0.007	-0.006	-0.007	-0.000	-0.004	0.000
	(0.014)	(0.005)	(0.005)	(0.008)	(0.006)	(0.008)
Female			-0.049***	-0.042***	-0.047***	0.033*
			(0.017)	(0.015)	(0.015)	(0.018)
$\beta_1 = \beta_2$	0.032	0.261	0.197	0.125	0.133	0.351
$\beta_2 = \beta_3$	0.137	0.110	0.531	0.066	0.074	0.742
Joint F-test	0.050	0.000	0.039	0.002	0.001	0.819
N	351,390	710,334	267,366	794,360	956,257	105,469

Outcome average	0.402	0.469	0.526	0.420	0.441	0.500
District level controls	✓	✓	✓	✓	✓	✓
Province × year FE	✓	✓	✓	✓	✓	✓
Exam-month FE	✓	✓	✓	✓	✓	✓

Note: HS stands for High School. Each column within each panel reports estimates from a separate regression. Violence intensity is measured as Z-scored fatalities per 1,000 population by time window within a year. All regressions include district-level weather controls, province × year fixed effects, and exam-month fixed effects. Regressions in columns (3)–(6) include a female indicator. Equality test rows report p-values. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Turning to Panel B, Table 3 shows that the heterogeneity patterns for undergraduate admission closely mirror those for test scores. The gender asymmetry persists: the female subsample again shows a larger point estimate concentrated in the 91–180 day window, while the male subsample shows smaller but more precisely estimated effects in both the immediate and medium-term windows. Notably, the female coefficient on the medium-term window is more than three times its male counterpart. Consistent with the selection pattern observed in Panel A, among women who do sit the exam, exposure to sustained pre-exam violence substantially worsens placement into undergraduate programs.

The high school completion dimension shows a somewhat different pattern from test scores. Above-median provinces exhibit significant effects in both the immediate and medium-term windows, whereas below-median provinces display no individually significant coefficients, and no significant joint effect. Two mechanisms may explain this pattern. First, in provinces with low completion rates, students who reach the Kankor are already highly selected, having completed high school in more challenging environments, and may therefore be more resilient to violence shocks at the admission margin. Second, these students may sort into less competitive programs. Provinces with below-median violence have higher baseline undergraduate admission rate (0.382 versus 0.274), consistent with applicants in these areas concentrating in fields with lower marginal cutoffs. For students with scores well above such cutoffs, modest score declines are unlikely to push them below program thresholds, attenuating the translation of score effects into placement outcomes. The contrast between cold- and warm-calendar provinces persists, with cold-calendar provinces showing significant effects in both conflict windows.

Finally, Panel C (Table 3), which broadens the outcome to include associate-degree admission, reinforces patterns observed above. Results by gender (Columns 1-2) and calendar

type (Column 5-6) closely mirror those in Panel B. For the provinces with below-median conflict, we now observe a marginally significant negative effect of medium-term violence on overall admission probabilities.

5.5 Mechanisms

In this section, we examine potential mechanisms through which conflict may affect test scores and admission outcomes.

5.5.1 Standardized Test-Score and Admission

Admission decisions made by the National Examination Authority (formerly within the Ministry of Higher Education) are based solely on applicants' test scores. We therefore assess whether the impact of violent conflict on admission operates through its effect on test performance. Two institutional features support this approach: test-takers rank their preferred fields of study on the exam date, prior to receiving their scores, and final admission offers are strictly score-based. We acknowledge, however, that this exercise provides suggestive evidence, as other channels may also be at play.

To assess whether the effects of violent conflict on admission likelihood operate through test scores, we employ an instrumental variable (IV) approach.

Specifically, we compare OLS estimates of the relationship between standardized test scores and admission likelihood with IV estimates that instrument standardized test scores using pre-exam violence exposure. Table 4 reports the results: Columns 1–4 present estimates for admission to undergraduate programs, and Columns 5–8 for overall admission (associate or undergraduate). Table A.11 reports heterogeneity by gender, regional high-school completion rates, and academic calendar type. We note that the Kleibergen-Paap F-statistics, which test instrument strength, fall below conventional thresholds, particularly in the interaction specifications, suggesting potential weak identification. Given that weak instrument bias pulls IV estimates toward their OLS counterparts, the attenuation the attenuation we estimate likely represents a conservative lower bound. We therefore interpret these IV results as suggestive evidence of the test-score channel rather than precisely identified causal effects.

For admission to public undergraduate programs, the OLS estimate (Column 1) indicates that a one SD increase in test scores is associated with a 32.0 percentage-point increase in

admission likelihood, while women are 2.5 percentage points less likely to be admitted on average. The IV estimate (Column 3) is substantially smaller: a one SD increase in test scores raises admission likelihood by only 20.5 percentage points, and the gender gap widens to 4.1 percentage points.

The OLS coefficient captures the score–admission gradient across all sources of score variation, whereas the IV estimate isolates variation driven by pre-exam violence exposure, identifying a local average treatment effect. The attenuation likely reflects, in part, selection into the complier population. This interpretation is consistent with evidence that higher levels of violence reduce the number of test-takers (Table A.4), suggesting that the more vulnerable students are deterred from sitting the exam altogether. Positive selection at the participation margin therefore shapes the test-taker pool. As a result, the smaller IV estimate at the undergraduate margin may reflect a flatter score-to-admission gradient for compliers or unobserved compositional differences among those who persist to take the exam. The interaction model (Column 4) yields broadly similar estimates.

For overall admission to public tertiary education (Columns 5–8), the OLS-IV gap largely disappears: the IV estimate is, if anything, slightly larger than the OLS estimate, in sharp contrast to the substantial attenuation observed for undergraduate admission. This pattern is consistent with the test-score channel operating across both tiers of admission. While the Kleibergen-Paap F-statistic of 8.56 falls below conventional thresholds for ruling out weak-instrument concerns, implying that the specific point estimates should be interpreted cautiously, the qualitative pattern is informative. The OLS-IV gap is concentrated at the competitive undergraduate margin, while at the overall-admission margin OLS and IV estimates largely coincide. This pattern is consistent with conflict-induced declines in scores reducing access to associate-degree programs as well, suggesting that violence affects admission throughout the tertiary system rather than reallocating applicants across program tiers.

Intuitively, if displaced undergraduate applicants were simply absorbed into associate programs, the score effect on overall admission would attenuate similarly; the absence of such attenuation indicates that marginal students are lost from both tiers. The quantile regression evidence (Figure A.14) supports this interpretation: recent violence mainly affects the upper part of the conditional distribution, where the four-year cutoff binds, while medium-term violence lowers scores more broadly. This observation helps explain why IV effects attenuate more at the four-year margin than at the overall tertiary margin. The baseline gender

disadvantage observed in the undergraduate IV estimates disappears for overall admission, indicating that conditional on conflict-instrumented test scores, women face no additional penalty at the associate-degree margin. By contrast, a significant gender gap persists at the more competitive undergraduate level.

Table A.11 examines heterogeneity across sub-samples defined by gender (Panel A), district-level high-school completion rates (Panel B), and academic calendar type (Panel C)—though some specifications yield less precise IV estimates and warrant caution. Two patterns are consistent across panels. First, for undergraduate admission, the IV estimates are uniformly smaller than their OLS counterparts, reinforcing that the test-score channel is strongest at the competitive-admission margin. Second, for overall tertiary admission, the OLS-IV gap largely disappears—and in several cases reverses.

Table 4: Effects of standardized test scores on admission likelihood

	Undergraduate only				Overall (associate or undergraduate)			
	OLS		IV		OLS		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Z-test score	0.320***	0.323***	0.205***	0.207***	0.339***	0.333***	0.355***	0.360***
	(0.012)	(0.011)	(0.032)	(0.032)	(0.007)	(0.007)	(0.041)	(0.040)
Female	-0.025***	-0.010	-0.041***	-0.034**	0.007*	0.011**	0.010	0.006
	(0.008)	(0.011)	(0.007)	(0.015)	(0.004)	(0.005)	(0.007)	(0.014)
Z-test score × female		-0.011*		-0.026		0.019***		-0.047
		(0.006)		(0.059)		(0.005)		(0.065)
N	1,061,725	1,061,725	1,061,725	1,061,725	1,061,725	1,061,725	1,061,725	1,061,725
Outcome average	0.30	0.30	0.30	0.30	0.45	0.45	0.45	0.45
Set of interaction		✓		✓		✓		✓
District level controls	✓	✓	✓	✓	✓	✓	✓	✓
Province × year FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Cragg-Donald F-stat			278.03	134.65			278.03	134.65
Kleibergen-Paap F-stat			8.56	3.66			8.56	3.66

Note: This table reports the effect of standardized test scores on the likelihood of admission, estimated by OLS and IV. In the IV specifications, standardized test scores are instrumented using three pre-exam violence intensity windows (1–90, 91–180, and 181–270 days). All models control for district-level weather deviations (precipitation, temperature, relative humidity, and wind speed) from historical means, province × year fixed effects, and exam-month fixed effects. The set of interactions indicates models where the female indicator is interacted with all explanatory variables, including the instruments. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

5.5.2 Household Level

Next, we explore descriptive household- and individual-level channels through which insecurity may affect academic preparation and subsequent tertiary education outcomes, using the nationally representative Afghan Living Conditions Survey 2016-2017. Tables A.12 and A.13 report summary statistics at the household and individual levels for the samples used in the analysis below. Importantly, the survey includes household heads' assessments of the security situation in their district.

Within the same district, households whose heads perceive the area as insecure spend less on education, are more likely to report worsening economic conditions, and higher exposure to both economic and violent shocks (Tables A.14 and A.15). We find no corresponding association with household displacement status. Households headed by individuals with tertiary education, in contrast, spend substantially more on education and are less likely to report economic hardship or exposure to shocks. These patterns indicate that tertiary-educated households are generally better positioned along a range of socioeconomic dimensions.

At the individual level, for the overall population, perceived insecurity is associated with a shift away from salaried work and day labor toward self-employment (Table A.16). Individuals with tertiary education are more likely to be self-employed and less likely to be engaged in unpaid family employment.

When perceived insecurity is interacted with education, we find that individuals with tertiary education in insecure districts are more likely to hold salaried and day-labor jobs and less likely to be self-employed. These patterns suggest that tertiary education may help buffer individuals from occupational reallocation into self-employment associated with insecurity.

In terms of sectoral allocation, individuals reporting higher insecurity are more likely to be employed in agriculture and less likely to work in manufacturing, construction, and services. However, when perceived insecurity is interacted with education, individuals with tertiary education in insecure areas remain less likely to work in agriculture and more likely to work in non-agricultural sector jobs such as manufacturing and construction (Table A.17).

Taken together, these descriptive patterns illustrate a broader cycle: violence is associated with lower investment in education and reduced access to higher education, while also contributing to occupational shifts toward more vulnerable forms of work. At the same time, even in insecure environments, tertiary education continues to be associated with greater likelihood to hold salaried and non-agriculture jobs, suggesting that it provides some

protection in the labor market impacted by insecurity.

6 Conclusion

This study examines how the timing of violent conflict exposure relative to high-stakes examinations affects academic outcomes in Afghanistan. Exploiting plausibly exogenous variation in timing of exposure to conflict relative to the provincial Kankor exam date, we find that conflict exposure within roughly six months of the exam significantly reduces standardized test scores, undergraduate admission rates, and overall public tertiary admission, with effects attenuating at longer horizons. A post-exam placebo provides supporting evidence that these patterns are driven by pre-exam exposure rather than unobserved district-level trends.

We further exploit a 2017 administrative reform that shifted the relationship between pre-exam windows and active twelfth-grade schooling. We find that the medium-term effects attenuate as their overlap with active schooling diminishes, while effects in the most proximate window persist. This pattern is consistent the partially distinct mechanisms: disruption to active schooling in the medium-term, and more immediate effects such as stress, mobility frictions, or exam-day logistics in the window closest to the exam.

The effects are heterogeneous along several dimensions. Female test-takers exhibit larger point estimates in the medium-term window. Because conflict is likely to deter less-prepared women from sitting the exam, selective participation implies that the estimated effects among those who persist represent a lower bound on their true vulnerability. Cold-calendar provinces drive the aggregate results in both pre- and post-reform periods, while warm-calendar provinces show no negative effects in either period. This pattern suggests that cold vs warm province contrast reflects region-specific factors such as higher baseline violence, differential selection into exam-taking, or other unobserved characteristics rather than differences in calendar-window timing. Limited statistical power in the warm-calendar sub-samples, however, warrants caution.

Instrumental variable estimates suggest that violence depresses admission primarily through reduced test scores, with effects spanning both undergraduate and associate-degree margins. Marginal students are excluded from tertiary education altogether rather than being redirected toward less selective programs. Quantile results further support this interpretation: recent violence disproportionately lowers scores among high-performing students near the

selective margin, while medium-horizon violence exposure reduces performance more uniformly across the distribution.

At the household level, perceived insecurity is associated with reduced educational spending and greater economic hardship. In the labor market, perceived insecurity is associated with reallocation of working age population away from salaried work and day labor and toward self-employment. At the same time, tertiary education appears to provide some protection. Tertiary educated household exhibit lower exposure to economic hardship, and tertiary educated individuals are more likely to maintain access to paid employment in insecure environments.

These patterns illustrate a reinforcing cycle: violence constrains human capital accumulation and appears to weaken labor market opportunities, even if tertiary education partially buffers households from these challenges.

These findings convey implications for education policy in conflict-affected settings. Within cold-calendar provinces, the attenuation of medium-term effects following the 2017 reform suggests that a longer gap between the end of twelfth grade and the exam may reduce the adverse impacts of conflict exposure. Targeted interventions that help students remain academically engaged in heavily affected districts during the school year may partially mitigate the medium-term academic costs of conflict.

The persistence of proximate-window effects across both periods suggests that violence close to the exam dates operates through additional channels including but not limited to stress, mobility frictions, and exam-day disruptions. Importantly, these factors remain relevant even after active schooling has ended. This implies that interventions such as safe transit to exam centers and secure testing environments could be effective.

More broadly, the results show that conflict exposure during the secondary-to-tertiary transition can hinder access to undergraduate education. This underscores the importance of maintaining educational continuity during conflict, particularly for women, who face costs at both the participation and performance margins.

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Data availability

The dataset used in this study consists of Kankor exam results for all test-takers from the 2013–2018 academic years, compiled from publicly available online sources. The dataset was assembled by the author (Najam) and the process is discussed here. De-identified final individual-level data can be made available for empirical reproducibility purposes and can be shared upon request on a case-by-case basis; please contact the authors for further information.

Disclosure statement

None

Declaration of Generative AI and AI-assisted technologies in the writing process

During the preparation of this work, the authors did not use generative AI tools to produce the main text. The use of generative AI tools was limited to addressing grammatical errors in drafts of the paper prior to rigorous proofreading.

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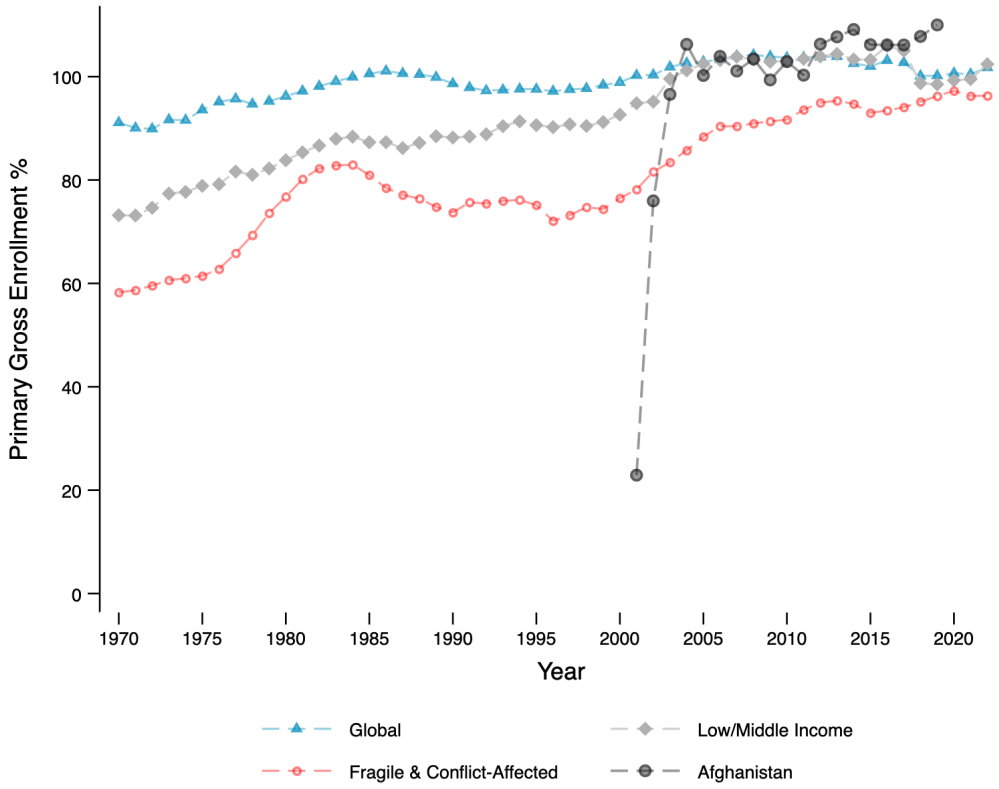
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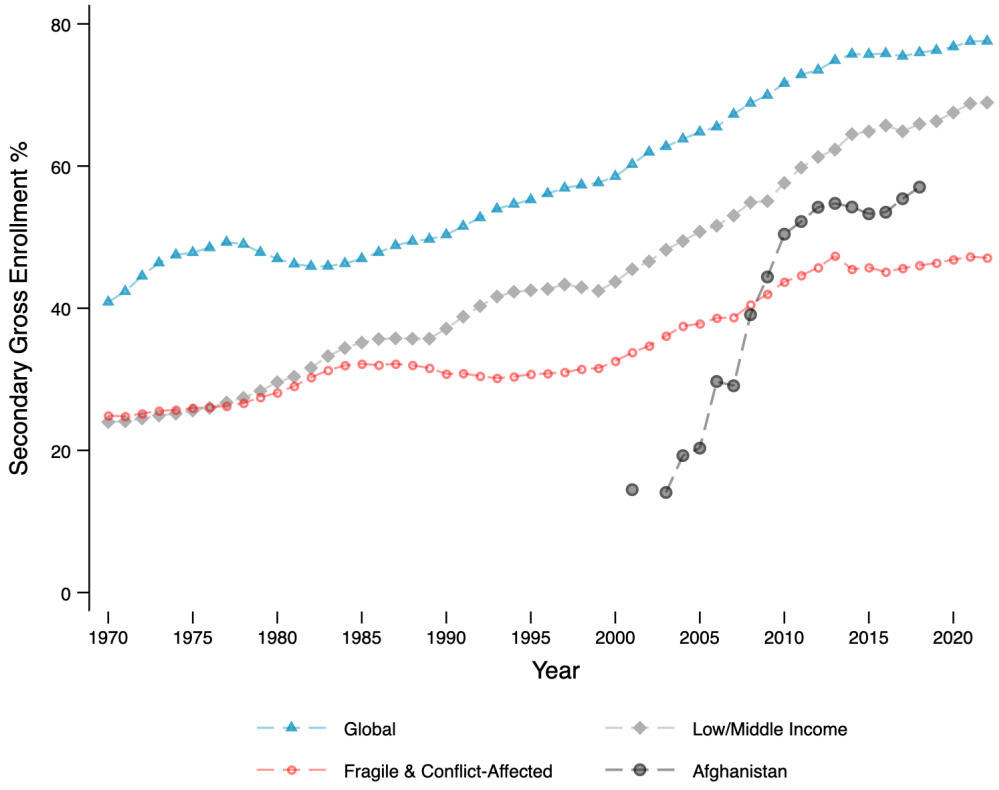
A Appendix

Figure A.1: Primary education gross enrollment



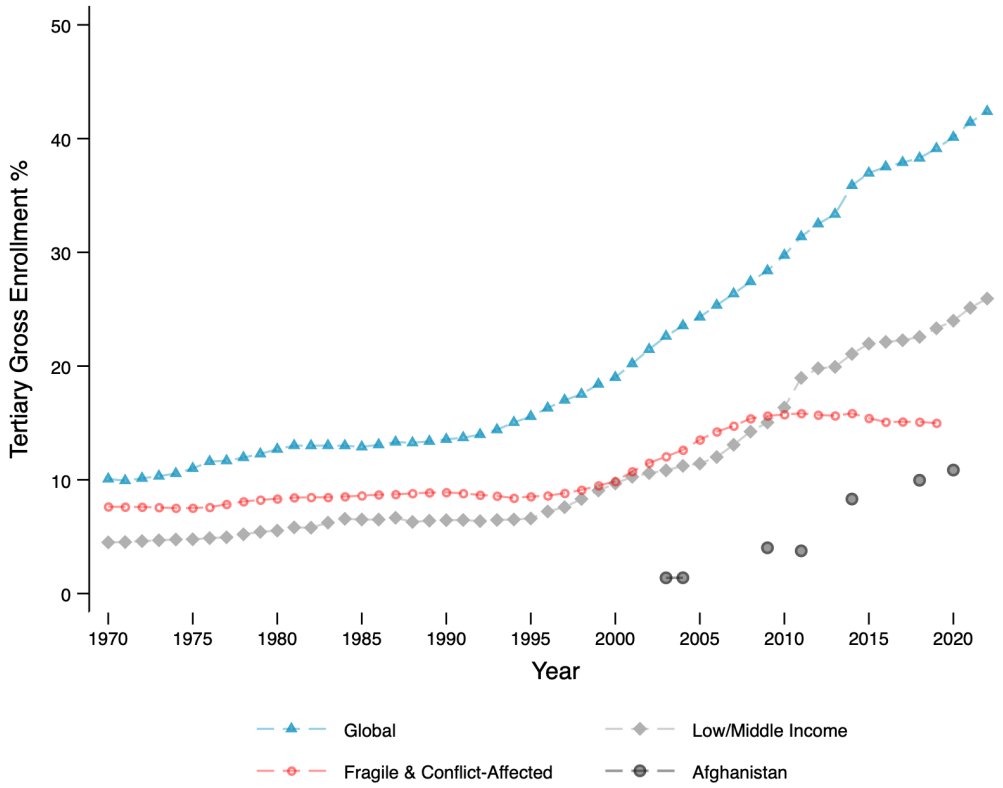
Note: This figure shows primary school gross enrollment for the specified categories, based on data retrieved on 11/1/2024 from the World Bank Open Data portal. The trends may not reflect historical averages for countries classified as FCS in 2024.

Figure A.2: Secondary education gross enrollment



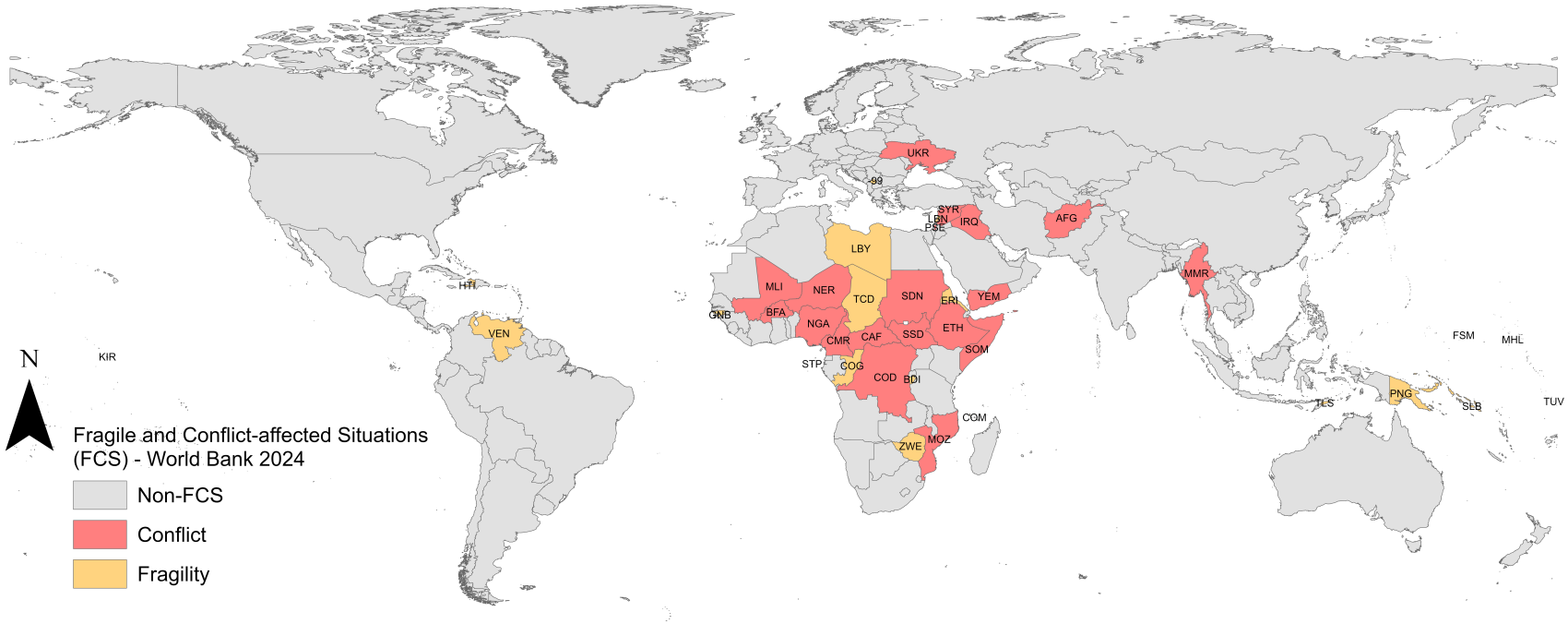
Note: This figure shows secondary school gross enrollment for the specified categories, based on data retrieved on 11/1/2024 from the World Bank Open Data portal. The trends may not reflect historical averages for countries classified as FCS in 2024.

Figure A.3: Tertiary education gross enrollment



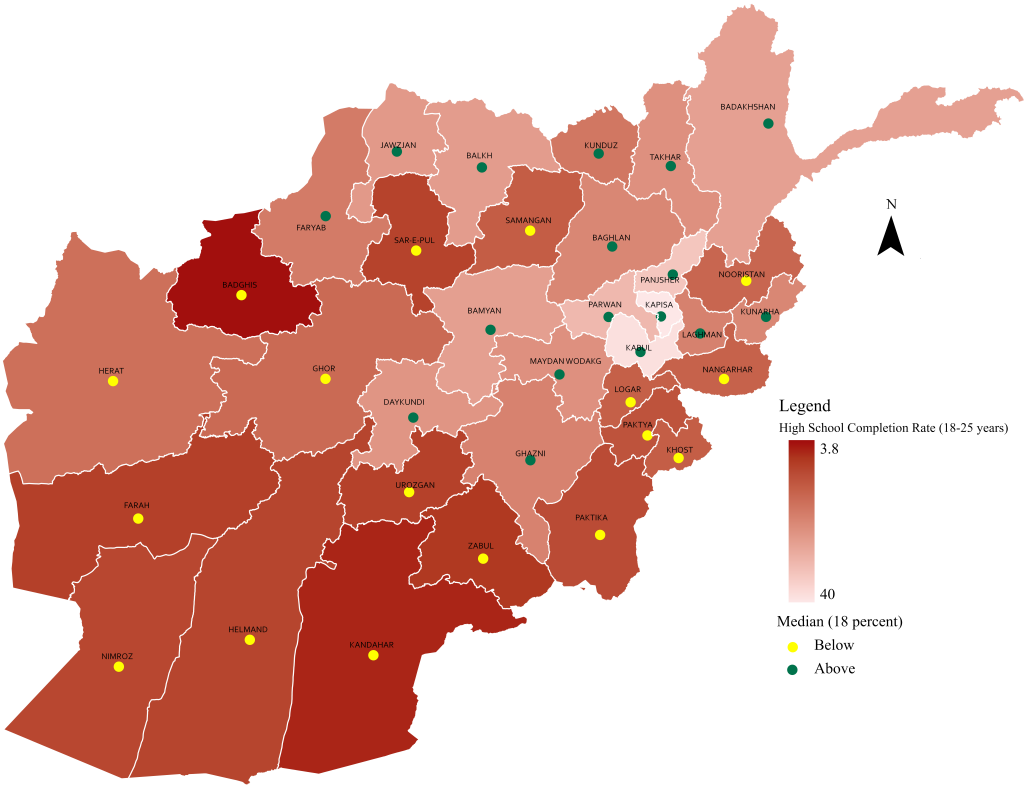
Note: Note: This figure shows tertiary school gross enrollment for the specified categories, based on data retrieved on 11/1/2024 from the World Bank Open Data portal. The trends may not reflect historical averages for countries classified as FCS in 2024.

Figure A.4: Fragile and conflict-affected situations in the 2024 fiscal year - World Bank



Note: This figure produced by authors shows contexts classified as fragile and conflict-affected situations by the World Bank in 2024 fiscal year (https://thedocs.worldbank.org/en/doc/608a53dd83f21ef6712b5dfef050b00b-0090082023/original/FCSListFY24-final.pdf), with a total population of 1.068 billion, according to population estimates from the World Bank in 2024. The base shape-file is obtained from World BanK Official Boundaries (https://datacatalog.worldbank.org/search/dataset/0038272/World-Bank-Official-Boundaries), a public dataset.

Figure A.5: High school completion rate among adults aged 18 to 25 in 2016 by provinces



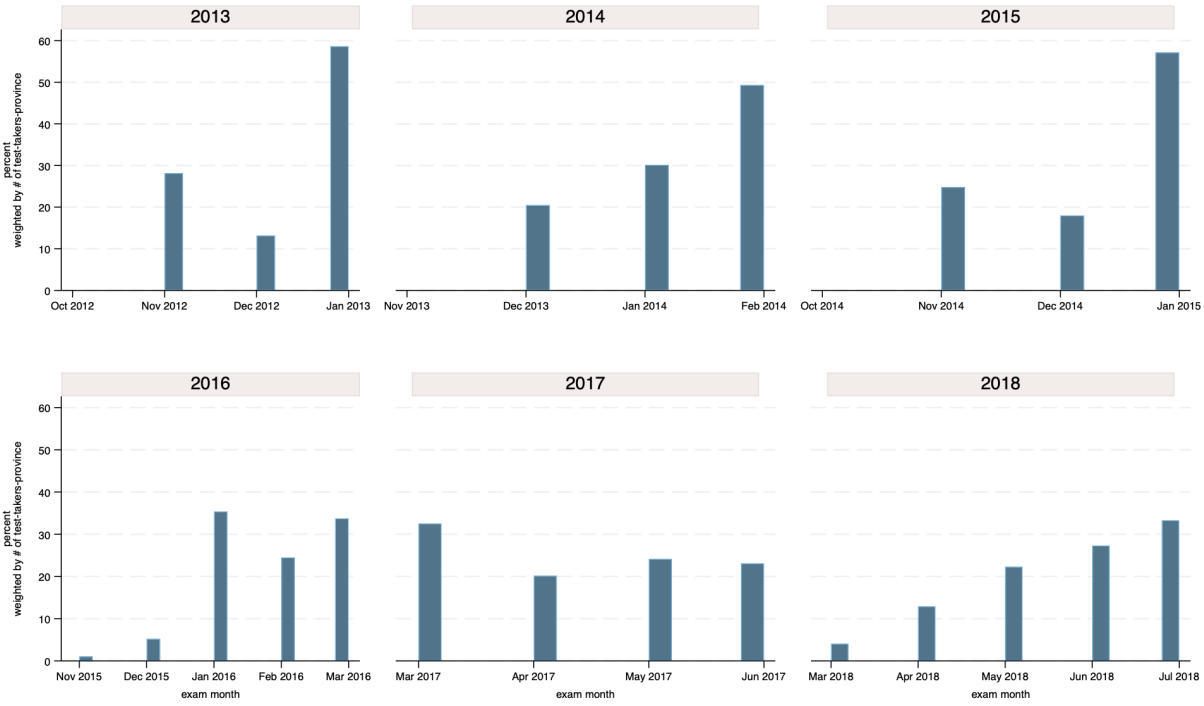
Note: This figure shows high school completion rates among adults aged 18 to 25 in 2016 at the province level based on estimates from the ALCS 2016–17 dataset, and our classification of provinces as being above or below the median rate of 18 percent.

Table A.1: Determinants of high school completion among adults aged (18–25) in 2016

	Completed High School
Female	-0.154***
	(0.010)
Income (ref. Q1)	
Q2 (25-50%)	0.017*
	(0.009)
Q3 (50-75%)	0.024**
	(0.010)
Q4 (Top 25%)	0.054***
	(0.020)
Locality (ref. Urban)	
Rural	-0.156***
	(0.021)
Nomad/Kuchi	-0.203***
	(0.024)
ln(Edu Spending)	0.027***
	(0.001)
Households Displaced	0.014
	(0.012)
R-squared	0.193
N	23128
Outcome average	0.207

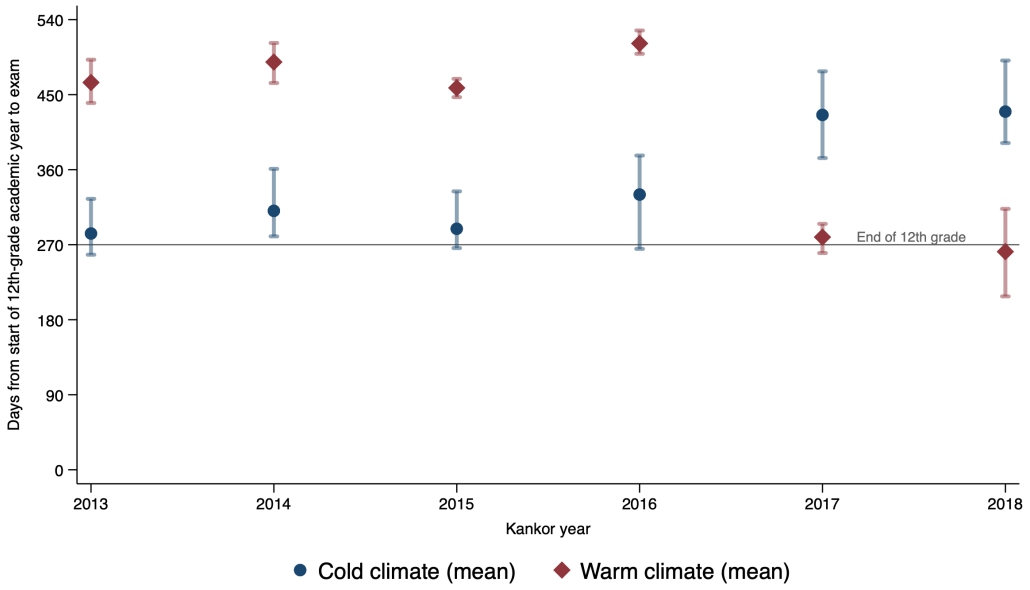
Note: This table shows the relationship between demographic variables and the likelihood of high school completion among adults aged 18 to 25 in 2016. Standard error clustered at the district level in parentheses. The median rate of completion for this population across provinces is 18%.
Significance level: * 0.10 ** 0.05 *** 0.01

Figure A.6: Kankor exam months by academic year



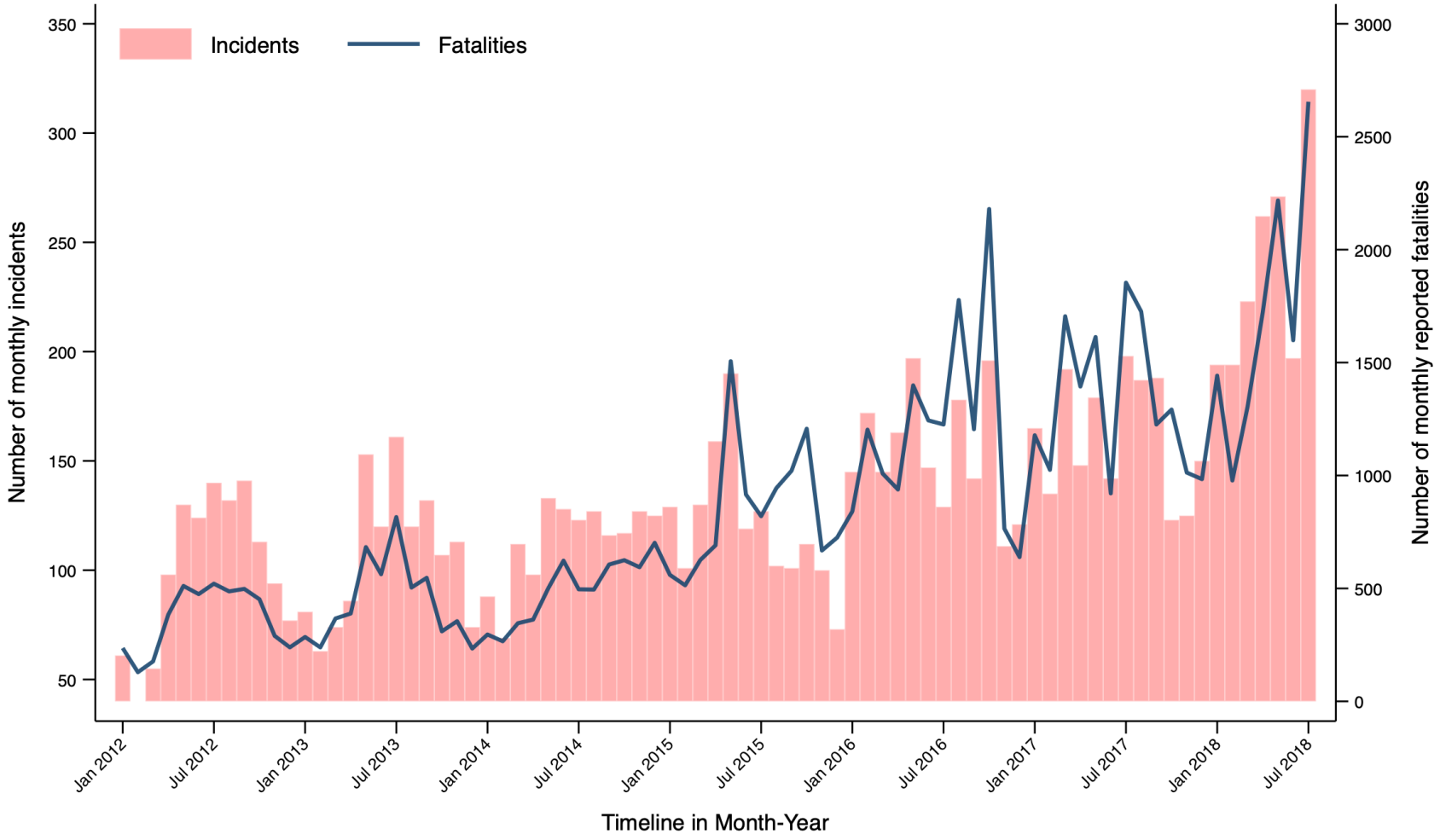
Note: This figure shows the distribution of Kankor exam months across academic years. In each year the exam is concentrated in a handful of consecutive months, but the specific months vary: in 2013–2016 the exam falls in the winter (roughly November–February), while in 2017–2018 it falls in the spring and summer (March-July).

Figure A.7: Days from the Start of Twelfth Grade to the Kankor Exam, by Cohort and Calendar Type, 2013–2018



Note: This figure shows the number of days between the start of the twelfth-grade academic year and the Kankor exam date, by exam cohort and calendar type. For cold-climate provinces, the twelfth-grade academic year is anchored to March 1 of the relevant calendar year; for warm-climate provinces, to September 1. Markers show the unweighted mean distance across provinces within each cohort × calendar cell (28 cold-climate provinces and 6 warm-climate provinces per cohort); while whiskers indicate the minimum and maximum values across provinces in that cell. The horizontal reference line at 270 days marks the approximate end of the twelfth-grade academic year in both calendars. Markers below the line indicate that, on average, students sat the exam during the twelfth-grade academic year. Markers above the line indicate that the exam occurred the academic year ended, with the vertical distance above the line corresponding to the average number of days between the end of school and the exam date. The 2017 reform shifted the national exam schedule from winter to spring/summer, mechanically increasing the school-to-exam gap for cold-climate provinces while reducing it for warm-climate provinces.

Figure A.8: Monthly levels of violent conflict and associated fatalities for 2012–2018



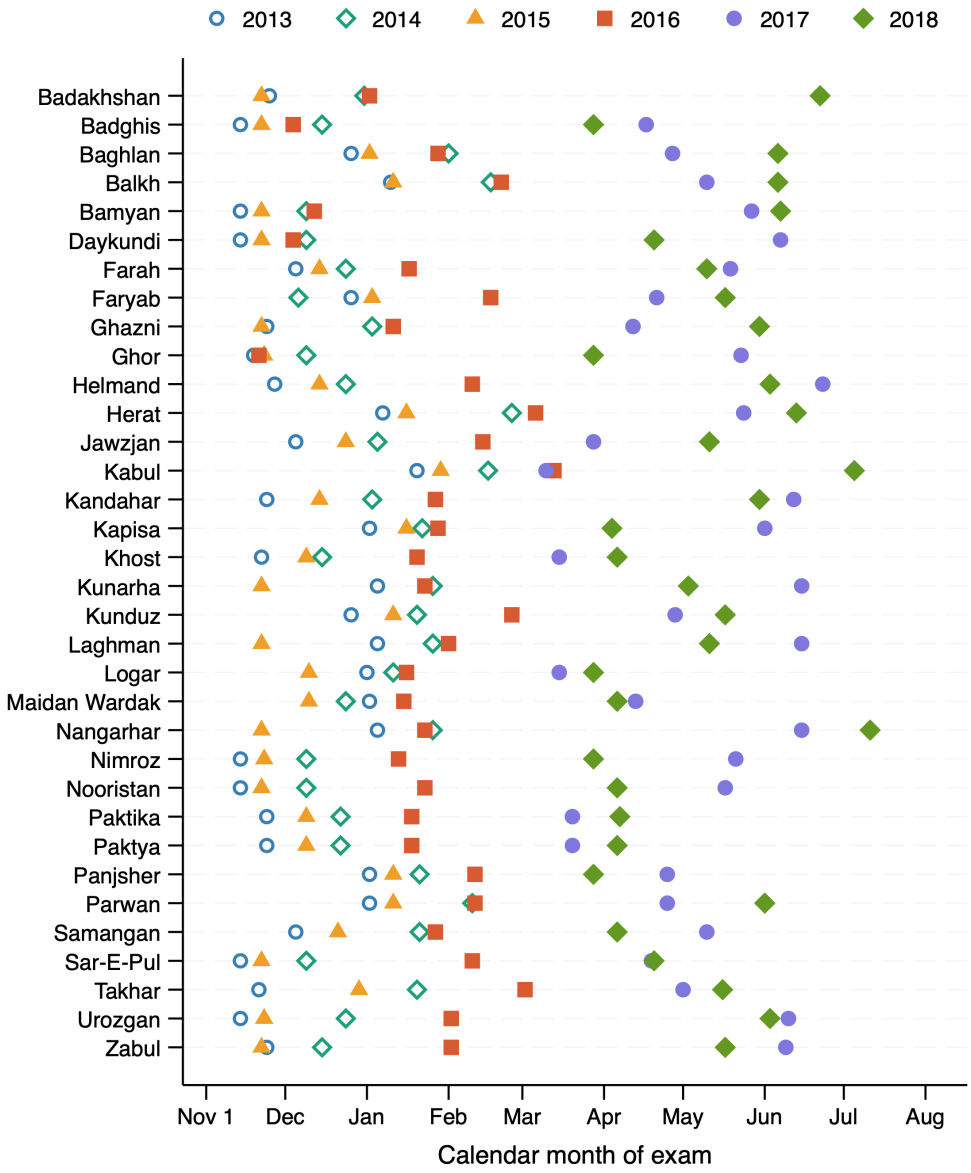
Note: This figure shows the monthly time-series of violent incidents and associated fatalities. The figure is produced by authors' based on the UCDP data (Sundberg and Melander, 2013).

Table A.2: Balance table for matched (analytical sample) and unmatched applicants

	Not-matched (1)		Matched (2)		Pairwise t-test difference (1)-(2)
Variable	N	Mean	N	Mean	N
Female	67808	0.218	1073182	0.331	1140990	-0.113***
Result					
Admit (undergrad)	67808	0.202	1073182	0.300	1140990	-0.098***
Admit (associate)	67808	0.115	1073182	0.145	1140990	-0.030***
Didn't pass	67808	0.683	1073182	0.555	1140990	0.128***
Gap year#	55483	0.470	1072037	0.303	1127520	0.167***
Province						
Badghis	65742	0.003	1073181	0.003	1138923	0.000*
Badkhashan	65742	0.058	1073181	0.049	1138923	0.009***
Baghlan	65742	0.056	1073181	0.038	1138923	0.017***
Balkh	65742	0.056	1073181	0.082	1138923	-0.026***
Bamyan	65742	0.023	1073181	0.024	1138923	-0.001*
Daykundi	65742	0.011	1073181	0.019	1138923	-0.008***
Farah	65742	0.004	1073181	0.006	1138923	-0.002***
Faryab	65742	0.021	1073181	0.026	1138923	-0.005***
Ghazni	65742	0.021	1073181	0.049	1138923	-0.028***
Ghor	65742	0.009	1073181	0.010	1138923	-0.001***
Helmand	65742	0.007	1073181	0.007	1138923	-0.001*
Herat	65742	0.041	1073181	0.066	1138923	-0.026***
Jawzjan	65742	0.023	1073181	0.023	1138923	0.000
Kabul	65742	0.270	1073181	0.243	1138923	0.028***
Kandahar	65742	0.008	1073181	0.012	1138923	-0.004***
Kapisa	65742	0.018	1073181	0.021	1138923	-0.004***
Khost	65742	0.022	1073181	0.023	1138923	-0.001
Kunar	65742	0.022	1073181	0.016	1138923	0.007***
Kunduz	65742	0.051	1073181	0.037	1138923	0.015***
Laghman	65742	0.017	1073181	0.012	1138923	0.005***
Logar	65742	0.013	1073181	0.017	1138923	-0.004***
Nangarhar	65742	0.086	1073181	0.060	1138923	0.026***
Nimruz	65742	0.002	1073181	0.002	1138923	-0.000*
Noristan	65742	0.001	1073181	0.001	1138923	0.000***
Paktia	65742	0.022	1073181	0.014	1138923	0.007***
Paktika	65742	0.006	1073181	0.004	1138923	0.002***
Panjshir	65742	0.006	1073181	0.009	1138923	-0.003***
Parwan	65742	0.035	1073181	0.035	1138923	0.000
Samangan	65742	0.018	1073181	0.011	1138923	0.006***
Sar-e-pul	65742	0.011	1073181	0.009	1138923	0.002***
Takhar	65742	0.039	1073181	0.047	1138923	-0.008***
Uruzgan	65742	0.002	1073181	0.003	1138923	-0.001***
Wardak	65742	0.015	1073181	0.020	1138923	-0.005***
Zabul	65742	0.003	1073181	0.002	1138923	0.001***

Significance: *0.10, ** 0.05, *** 0.01. #The difference between high school graduation year and the year applicant participated in the exam in years.

Figure A.9: Kankor Exam Schedule by Province, 2013–2018



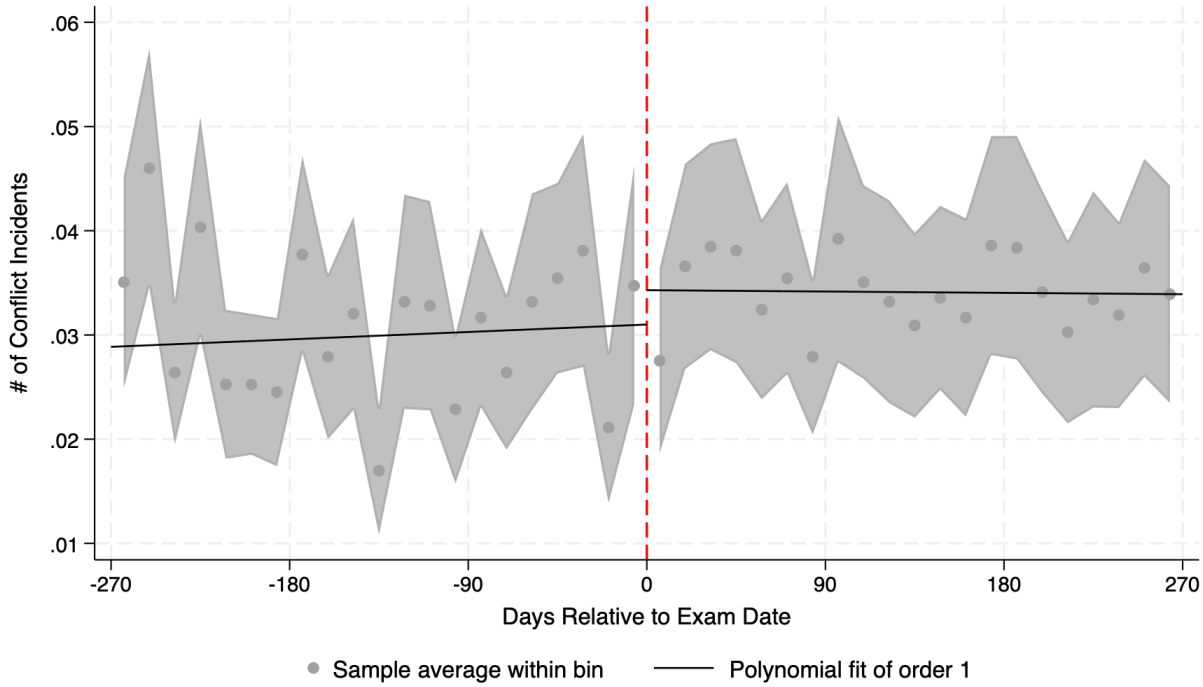
Note: Each marker shows the start date of the Kankor university entrance exam in a given province and year. To make dates comparable across years, timing is measured as the number of days elapsed since November 1 of the *preceding* year (e.g., day 0 corresponds to November 1, day 61 to January 1, day 151 to April 1). Provinces are listed on the vertical axis in alphabetical order. Each year is represented by a distinct symbol and color. The figure reveals two dimensions of scheduling variation: *cross-province spread*, the gap between the earliest and latest province within a given year (averaging 90_{cross} days), and *within-province annual shift* the year-to-year shift in a province's own exam date (averaging 46_{within} days). The 2016–2017 transition is particularly pronounced, reflecting a nationwide rescheduling of the exam cycle.

Table A.3: Illustrative example: construction of z-scored violence exposure

	Districts within province p in year t			
	A	B	C	D
Step 1: Raw data				
Population	10,000	10,000	30,000	50,000
Fatalities (1–90 days pre-exam)	0	10	50	2
Step 2: Population normalization				
Fatalities per 1,000 population	0	1.00	1.67	0.04
Step 3: Within-window-year standardization				
Window-year mean and SD		$\bar{x} = 0.48$	$\sigma = 0.27$	
Z-scored violence exposure	−1.78	1.93	4.41	−1.63

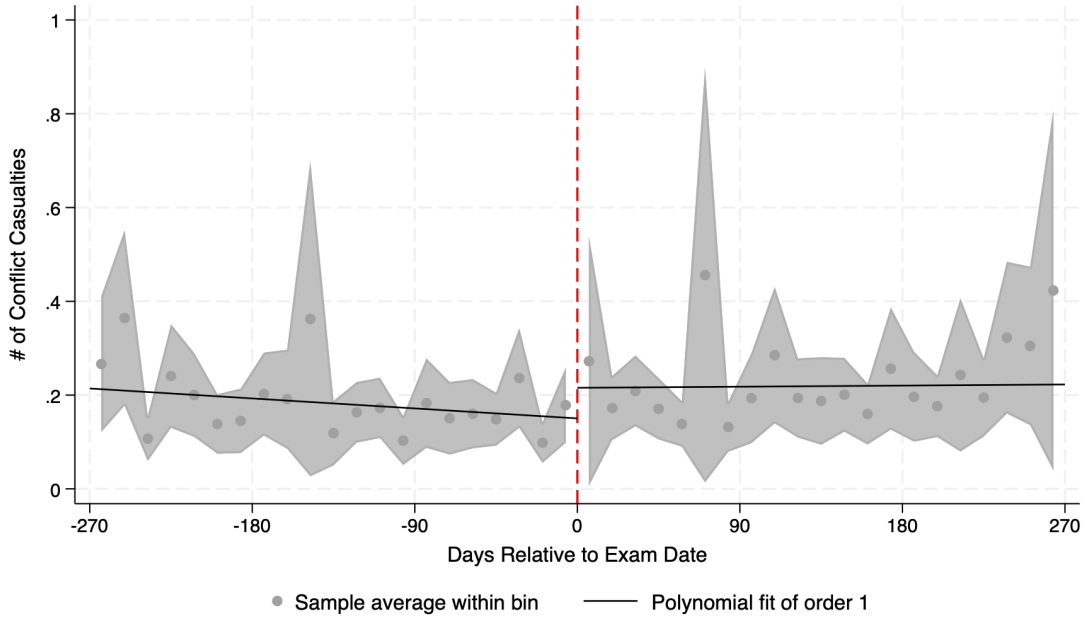
Note: This table illustrates the construction of the z-scored violence exposure measure for a hypothetical province-year using the 1–90 day pre-exam window. Fatality counts are first normalized by district population, then standardized within the window-year to produce a mean-zero, unit-variance measure. The same procedure is applied separately to each of the three exposure windows (1–90, 91–180, and 181–270 days before the exam) by year. The mean and standard deviation used in Step 3 are calculated from the full national distribution for the relevant year-window, rather than from the four illustrative districts shown in Step 2.

Figure A.10: Daily province level violence incidents



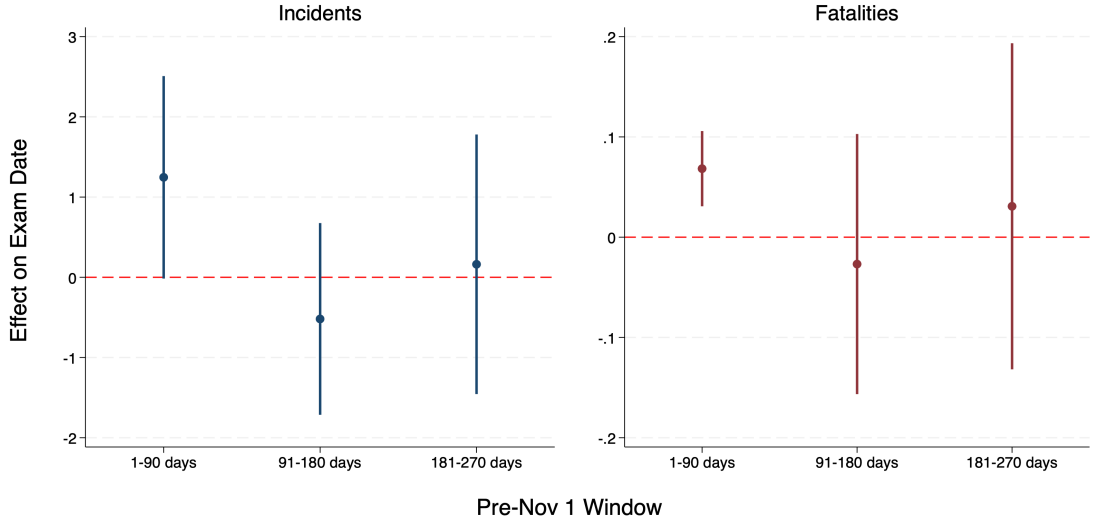
Note: This figure presents an RD-style plot of the daily number of conflict incidents at the province level around the provincial exam date, pooled across 2013–2018. The running variable is days relative to the exam date, with negative values indicating the pre-exam period and the observations shown within a 270 days bandwidth on either side of the cutoff. Dots represent average number of incidents within 21 evenly spaced bins on each side of the cutoff. Solid lines are first-order polynomial fits estimated separately on each side using an Epanechnikov kernel, with shaded area denoting the 95% confidence interval. The dashed red line marks the exam date. The absence of a discontinuity at the cutoff supports the assumption that conflict incidents do not systematically respond to exam scheduling

Figure A.11: Daily province level reported fatalities



Note: This figure presents an RD-style plot of the daily number of conflict casualties at the province level around the provincial exam date, pooled across 2013–2018. The running variable is days relative to the exam date, with negative values indicating the pre-exam period and the observations shown within a 270 days bandwidth on either side of the cutoff. Dots represent average casualties within 21 evenly spaced bins on each side of the cutoff. Solid lines are first-order polynomial fits estimated separately on each side using an Epanechnikov kernel, with shaded area denoting the 95% confidence interval. The dashed red line marks the exam date. The absence of a discontinuity at the cutoff supports the assumption that conflict casualties do not systematically respond to exam scheduling.

Figure A.12: Exam-date deviation: days between November 1 and the actual exam date



Note: In this figure, we examine whether the timing of the Kankor exam is systematically related to prior armed conflict exposure, which would raise concerns about the assumption that exam dates are set independently of security conditions. The dependent variable is the province-specific exam-date deviation, defined as the number of days between the province's exam start date and November 1 of the prior year. November 1 serves as a fixed anchor because it precedes the earliest exam date observed in any province-year; conflict measured up to this date therefore predates every exam in the sample and cannot be affected by the realized exam date. The regressors are total conflict incidents (left panel) and fatalities (right panel) aggregated within three windows—1–90, 91–180, and 181–270 days before November 1. Because the dependent variable is the exam date itself, the conflict windows are anchored to the fixed November 1 benchmark rather than to the exam date. This construction ensures that variation in conflict measures does not mechanically arise from variation in the outcome being explained. The unit of observation is the province-exam cohort. All regressions absorb province and year fixed effects, with standard errors clustered at the province level. A positive coefficient would indicate that higher prior conflict exposure is associated with a later exam date relative to the November 1 benchmark, raising concerns that exam timing may be systematically related to local security conditions. In contrast, coefficients close to zero provide no evidence of that observed conflict predicts subsequent exam scheduling, consistent with the identifying assumption that exam timing is not systematically driven by prior conflict. The dashed red line marks zero.

Table A.4: Effects of normalized violence intensity on test takers

	Test Takers (1)	Female (2)	Share Female (3)
Violence exposure pre-exam date			
1–90 days	-23.153**	-7.629*	-0.004
	(9.935)	(4.143)	(0.004)
91–180 days	-26.797*	-5.152	-0.006*
	(15.296)	(5.746)	(0.003)
181–270 days	-23.630*	-10.193**	-0.007**
	(12.221)	(5.149)	(0.003)
R-squared	0.06	0.06	0.44
N	2101	2101	2101
Outcome average	505.344	167.250	0.160
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

Note: This table reports estimates of the effects of normalized pre-exam violence intensity on district-level test-taker outcomes. The unit of observation is the district-year. Outcomes are the total number of test takers (column 1), the number of female test takers (column 2), and the female share of test takers (column 3). Explanatory variables are Z-scored fatalities per 1,000 population. All regressions include district-level weather controls (precipitation, temperature, humidity, and wind speed) and province × year fixed effects. Standard errors clustered at the district level are reported in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table A.5: Effects of pre- and post-exam violence exposure

	Test score	Admission	
		undergrad	overall
Pre-exam violence exposure			
1–90 days	-0.021**	-0.008**	-0.007*
	(0.010)	(0.004)	(0.004)
91–180 days	-0.051***	-0.010**	-0.020***
	(0.014)	(0.004)	(0.006)
181–270 days	-0.011	-0.002	-0.005
	(0.010)	(0.004)	(0.005)
Post-exam violence exposure			
1–90 days	-0.012	-0.001	-0.001
	(0.008)	(0.003)	(0.003)
91–180 days	-0.001	0.005	0.001
	(0.010)	(0.003)	(0.004)
181–270 days	-0.021**	-0.005	-0.006*
	(0.009)	(0.003)	(0.003)
Female	-0.149***	-0.072***	-0.043***
	(0.039)	(0.009)	(0.013)
Joint F-test Pre-exam	0.000	0.001	0.000
Joint F-test Post-exam	0.065	0.299	0.265
Post-estimation linear combination			
Combined Pre-exam (Days 1–270)	-0.083***	-0.020***	-0.032***
	(0.017)	(0.005)	(0.007)
Combined Post-exam (Days 1–270)	-0.034**	-0.001	-0.006
	(0.016)	(0.005)	(0.006)
R-squared	0.15	0.10	0.12
N	1061725	1061726	1061726
Outcome average		0.301	0.447
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

Note: This table reports estimates of the effects of violence exposure on Kankor exam outcomes, separately for pre- and post-exam windows. The dependent variables are the standardized test score (column 1), a binary indicator for undergraduate admission (column 2), and a binary indicator for overall admission to either an undergraduate or associate program (column 3). Violence intensity is measured as Z-scored fatalities per 1,000 population. The post-exam windows serve as placebo tests. Joint F-test rows report p-values from tests of joint significance of all pre-exam (post-exam) window coefficients. The post-estimation linear combinations report the sum of the three pre-exam (post-exam) coefficients with their standard errors. All regressions include a female indicator, district-level weather controls, province × year fixed effects, and exam-month fixed effects. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table A.6: Effects of pre-exam violence exposure on academic outcomes (winsorized exposure)

	Test Score (Standardized)	Undergraduate Admission	Overall Admission
	(1)	(2)	(3)
Violence exposure pre-exam date			
1–90 days	-0.031**	-0.008*	-0.009*
	(0.013)	(0.005)	(0.005)
91–180 days	-0.066***	-0.012**	-0.025***
	(0.016)	(0.005)	(0.006)
181–270 days	-0.023	-0.004	-0.009
	(0.016)	(0.007)	(0.008)
Female	-0.149***	-0.072***	-0.043***
	(0.039)	(0.009)	(0.013)
$\beta_1 = \beta_2$ (P-value)	0.104	0.631	0.061
$\beta_2 = \beta_3$ (P-value)	0.093	0.385	0.146
Joint F-test (p-value)	0.000	0.003	0.000
N	1,061,725	1,061,726	1,061,726
Outcome average		0.301	0.447
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

Note: Each column reports estimates from a separate regression of the indicated outcome on pre-exam violence intensity measures winsorized at the 99th percentile (fatalities per 1,000 population within province-year, winsorized). The three exposure windows aggregate daily fatality counts over 1–90, 91–180, and 181–270 days prior to the exam date. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table A.7: Effects of pre-exam violence exposure on academic outcomes (Above 75%)

	Test Score (Standardized)	Undergraduate Admission	Overall Admission
	(1)	(2)	(3)
Violence exposure pre-exam date			
1–90 days	-0.028	-0.012	-0.018**
	(0.026)	(0.007)	(0.008)
91–180 days	-0.061***	-0.013**	-0.019**
	(0.020)	(0.006)	(0.008)
181–270 days	-0.011	0.004	-0.004
	(0.023)	(0.007)	(0.009)
Female	-0.148***	-0.072***	-0.043***
	(0.039)	(0.009)	(0.013)
$\beta_1 = \beta_2$ (P-value)	0.356	0.895	0.943
$\beta_2 = \beta_3$ (P-value)	0.090	0.070	0.180
Joint F-test (p-value)	0.007	0.033	0.005
N	1,061,725	1,061,726	1,061,726
Outcome average		0.301	0.447
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

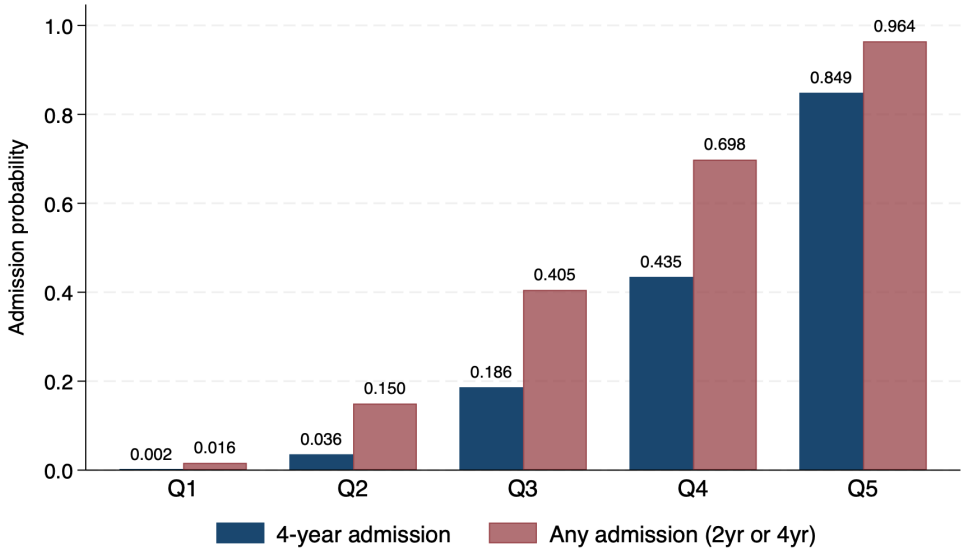
Note: Each column reports estimates from a separate regression of the indicated outcome on pre-exam violence intensity measures defined as binary indicators equal to one if the fatality intensity (fatalities per 1,000 population within window for district) exceeds the 75th percentile of the distribution within each exposure window. The three exposure windows cover 1–90, 91–180, and 181–270 days prior to the exam date. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table A.8: Effects of pre-exam violence exposure on academic outcomes (fatalities per 1,000 population)

	Test Score (Standardized)	Undergraduate Admission	Overall Admission
	(1)	(2)	(3)
Violence exposure pre-exam date			
1–90 days	-0.048**	-0.014**	-0.010
	(0.021)	(0.006)	(0.008)
91–180 days	-0.057**	-0.009	-0.026*
	(0.027)	(0.007)	(0.013)
181–270 days	-0.048**	-0.012	-0.014
	(0.022)	(0.009)	(0.011)
Female	-0.148***	-0.072***	-0.043***
	(0.038)	(0.009)	(0.013)
$\beta_1 = \beta_2$ (P-value)	0.797	0.581	0.330
$\beta_2 = \beta_3$ (P-value)	0.798	0.809	0.500
Joint F-test (p-value)	0.004	0.033	0.046
N	1,061,725	1,061,726	1,061,726
Outcome average		0.301	0.447
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

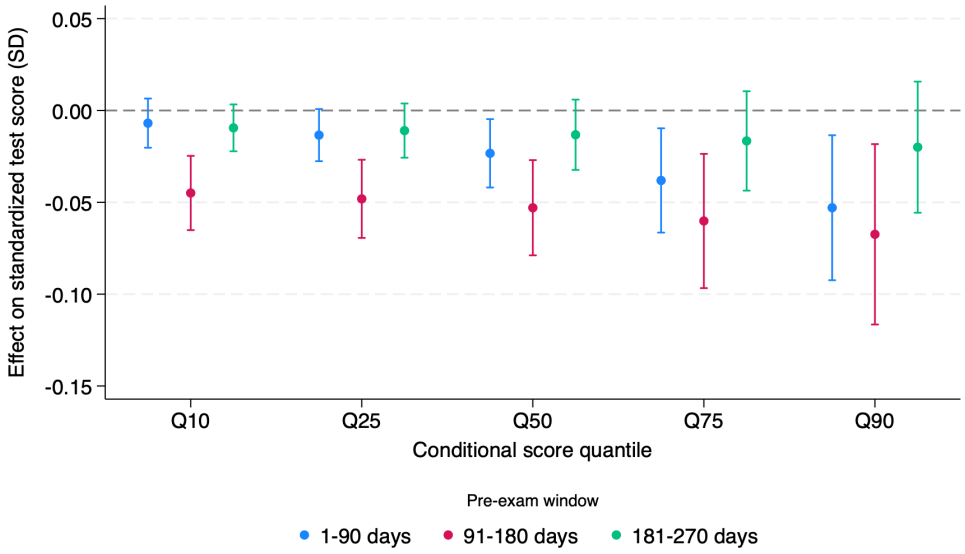
Note: Each column reports estimates from a separate regression of the indicated outcome on pre-exam violence intensity measured as cumulative fatalities per 1,000 population within district over each exposure window, using WorldPop 2013 district-level population estimates (WorldPop and CIESIN, 2018). The three exposure windows cover 1–90, 91–180, and 181–270 days prior to the exam date. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Figure A.13: Admission probability by Kankor score quintile



Note: Sample mean admission probability by quintile of the within-year standardized Kankor score, pooled across the estimation sample. Outcomes are admission to a four-year program and admission to any program (two-year or four-year). The gap between the two bars at each quintile represents two-year-only admissions.

Figure A.14: Quantile regression effects of pre-exam violence on standardized test scores



Note: Coefficient estimates from method-of-moments quantile regressions (Machado and Santos Silva, 2019) of the standardized Kankor score on three pre-exam violence windows (1–90, 91–180, and 181–270 days), each measured as the within-year z-score of district-level conflict intensity. Regressions include province × year and exam-month fixed effects and control for sex and pre-exam weather differences. Markers report point estimates at the 10th, 25th, 50th, 75th, and 90th conditional quantiles; whiskers denote 95% confidence intervals based on standard errors clustered at the district level.

Table A.9: Effects of pre-exam violence exposure on academic outcomes by periods

	Test Score (Standardized)	Undergraduate Admission	Overall Admission
	(1)	(2)	(3)
Panel A: 2013–2016			
Violence exposure pre-exam date			
1–90 days	-0.027*	-0.007	-0.012*
	(0.015)	(0.006)	(0.006)
91–180 days	-0.059***	-0.013***	-0.024***
	(0.016)	(0.005)	(0.007)
181–270 days	-0.008	0.000	-0.004
	(0.013)	(0.005)	(0.006)
Female	-0.169***	-0.080***	-0.049***
	(0.034)	(0.009)	(0.012)
$\beta_1 = \beta_2$ (P-value)	0.170	0.494	0.233
$\beta_2 = \beta_3$ (P-value)	0.025	0.134	0.066
Joint F-test (p-value)	0.000	0.009	0.000
N	752,036	752,037	752,037
Outcome average		0.301	0.447
Panel B: 2017–2018			
Violence exposure pre-exam date			
1–90 days	-0.026**	-0.008**	-0.004
	(0.011)	(0.004)	(0.004)
91–180 days	-0.041*	0.002	-0.002
	(0.024)	(0.008)	(0.009)
181–270 days	-0.025*	-0.006	-0.004
	(0.014)	(0.005)	(0.005)
Female	-0.103**	-0.053***	-0.028
	(0.050)	(0.009)	(0.017)
$\beta_1 = \beta_2$ (P-value)	0.544	0.268	0.805
$\beta_2 = \beta_3$ (P-value)	0.559	0.440	0.883
Joint F-test (p-value)	0.019	0.074	0.569
N	309,689	309,689	309,689
Outcome average		0.301	0.447
District level controls	✓	✓	✓
Province × year FE	✓	✓	✓
Exam-month FE	✓	✓	✓

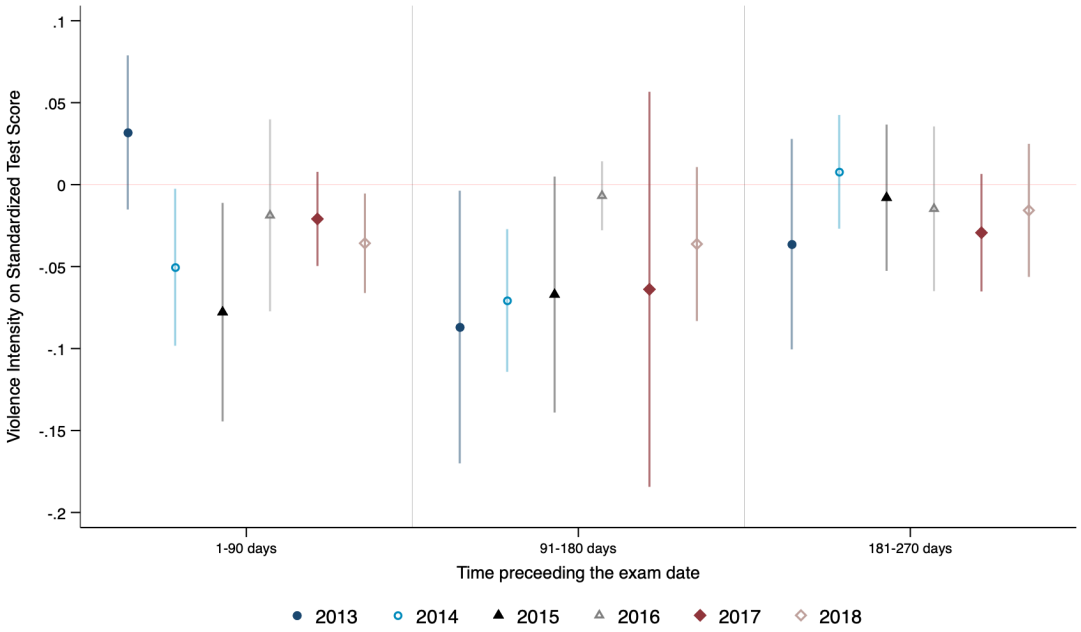
Note: Each column reports estimates from a separate regression of the indicated outcome on pre-exam violence intensity measures (Z-scored fatalities per 1,000 population by time window within a year). Panel A restricts the sample to exam cohorts from 2013–2016; Panel B to 2017–2018. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table A.10: Effects of pre-exam violence exposure on academic outcomes by periods and calendar type

	Cold-climate			Warm-climate		
	Test Score (Standardized) (1)	Undergraduate Admission (2)	Overall Admission (3)	Test Score (Standardized) (4)	Undergraduate Admission (5)	Overall Admission (6)
Panel A: 2013–2016						
Violence exposure pre-exam date						
1–90 days	-0.031**	-0.009*	-0.013**	0.036	0.014	0.012
	(0.015)	(0.006)	(0.006)	(0.039)	(0.018)	(0.019)
91–180 days	-0.061***	-0.012**	-0.026***	-0.019	-0.003	-0.003
	(0.019)	(0.006)	(0.008)	(0.016)	(0.005)	(0.005)
181–270 days	-0.005	0.001	-0.003	-0.001	0.001	-0.003
	(0.015)	(0.006)	(0.007)	(0.019)	(0.008)	(0.008)
Female	-0.174***	-0.084***	-0.053***	-0.073**	0.029	0.035
	(0.036)	(0.010)	(0.013)	(0.035)	(0.031)	(0.023)
$\beta_1 = \beta_2$ (P-value)	0.255	0.766	0.199	0.239	0.388	0.470
$\beta_2 = \beta_3$ (P-value)	0.035	0.206	0.046	0.517	0.683	0.970
Joint F-test (p-value)	0.001	0.015	0.000	0.546	0.806	0.779
N	687,520	687,521	687,521	64,516	64,516	64,516
Outcome average		0.266	0.418		0.376	0.504
Panel B: 2017–2018						
Violence exposure pre-exam date						
1–90 days	-0.034***	-0.009*	-0.004	0.020**	0.002	0.003
	(0.013)	(0.005)	(0.005)	(0.009)	(0.003)	(0.005)
91–180 days	-0.044	0.006	0.006	0.008	0.013	0.003
	(0.028)	(0.011)	(0.012)	(0.033)	(0.011)	(0.013)
181–270 days	-0.036	-0.005	-0.004	0.023	0.003	0.007
	(0.023)	(0.007)	(0.007)	(0.017)	(0.005)	(0.007)
Female	-0.108*	-0.064***	-0.033*	-0.075*	0.076***	0.027*
	(0.056)	(0.010)	(0.020)	(0.044)	(0.025)	(0.016)
$\beta_1 = \beta_2$ (P-value)	0.725	0.207	0.416	0.712	0.324	0.969
$\beta_2 = \beta_3$ (P-value)	0.830	0.436	0.474	0.719	0.346	0.724
Joint F-test (p-value)	0.007	0.179	0.719	0.122	0.645	0.763
N	268,736	268,736	268,736	40,953	40,953	40,953
Outcome average		0.363	0.500		0.381	0.495
District level controls	✓	✓	✓	✓	✓	✓
Province × year FE	✓	✓	✓	✓	✓	✓
Exam-month FE	✓	✓	✓	✓	✓	✓

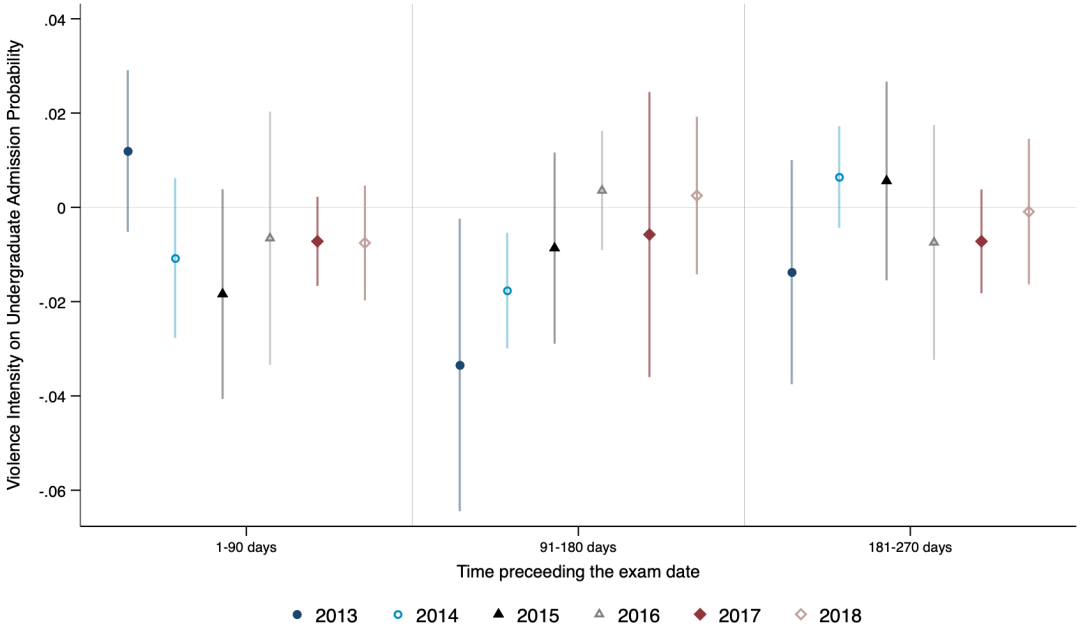
Note: Each column reports estimates from a separate regression of the indicated outcome on pre-exam violence intensity measures (Z-scored fatalities per 1,000 population by time window within a year). Columns 1–3 restrict the sample to cold-climate provinces; columns 4–6 to warm-climate provinces. Panel A covers exam cohorts from 2013–2016; Panel B covers 2017–2018. The 2017 reform shifted exam scheduling: cold-climate students sat the exam shortly after twelfth grade ended pre-reform but several months later post-reform, while warm-climate students sat it several months after twelfth grade ended pre-reform but close to the end of the academic year post-reform. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Figure A.15: Effects of violence exposure on standardized test score - by year



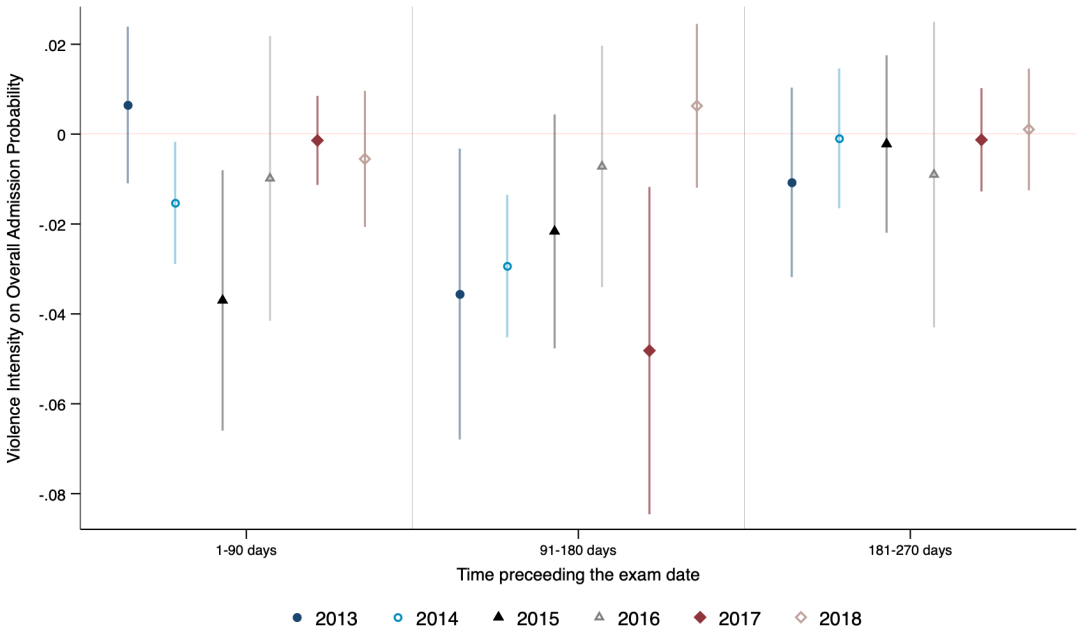
Note: This figure shows the effect of violence exposure on students' standardized test scores from year-specific regressions. The year-specific coefficients are derived from a single regression where admission likelihood is regressed on the three periods of exposure interacted with year indicators, controlling for province fixed effects and district-level time-varying variables. The whiskers indicate 95% confidence intervals.

Figure A.16: Effects of violence exposure on undergraduate admission - by year



Note: This figure shows the effect of violence exposure on students' admission likelihood to undergraduate programs. The year-specific coefficients are derived from a single regression where admission likelihood is regressed on the three periods of exposure interacted with year indicators, controlling for province fixed effects and district-level time-varying variables. The whiskers indicate 95% confidence intervals.

Figure A.17: Effects of violence exposure on overall admission (associate and/or undergraduate) - by year



Note: This figure shows the effect of violence exposure on students' overall admission (associate and/or undergraduate). The year-specific coefficients are derived from a single regression where admission likelihood is regressed on the three periods of exposure interacted with year indicators, controlling for province fixed effects and district-level time-varying variables. The whiskers indicate 95% confidence intervals.

Table A.11: Effects of standardized test score on admission—Heterogeneity

	Undergraduate only				Overall (associate or undergraduate)			
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Panel A: Gender								
	Female		Male		Female		Male	
Standardized test score	0.315***	0.273***	0.322***	0.192***	0.352***	0.389***	0.334***	0.345***
	(0.014)	(0.043)	(0.011)	(0.034)	(0.010)	(0.065)	(0.007)	(0.041)
N	351,390	351,390	710,333	710,333	351,390	351,390	710,333	710,333
Outcome average	0.24	0.24	0.33	0.33	0.40	0.40	0.47	0.47
Cragg-Donald F-stat		61.96		247.69		61.96		247.69
Kleibergen-Paap F-stat		2.44		7.85		2.44		7.85
Panel B: High School Completion Rate								
	Below		Above		Below		Above	
Standardized test score	0.337***	0.169***	0.315***	0.205***	0.334***	0.295***	0.340***	0.378***
	(0.008)	(0.055)	(0.014)	(0.045)	(0.013)	(0.058)	(0.009)	(0.058)
N	267,366	267,366	794,359	794,359	267,366	267,366	794,359	794,359
Outcome average	0.38	0.38	0.27	0.27	0.53	0.53	0.42	0.42
Cragg-Donald F-stat		59.89		153.72		59.89		153.72
Kleibergen-Paap F-stat		4.39		5.61		4.39		5.61
Panel C: Calendar Type								
	Cold		Warm		Cold		Warm	
Standardized test score	0.318***	0.178***	0.349***	0.257	0.337***	0.356***	0.359***	0.244
	(0.012)	(0.040)	(0.017)	(0.163)	(0.007)	(0.050)	(0.019)	(0.172)
N	956,256	956,256	105,469	105,469	956,256	956,256	105,469	105,469
Outcome average	0.29	0.29	0.38	0.38	0.44	0.44	0.50	0.50
Cragg-Donald F-stat		195.46		12.48		195.46		12.48
Kleibergen-Paap F-stat		6.96		1.02		6.96		1.02
District level controls	✓	✓	✓	✓	✓	✓	✓	✓
Province × year FE	✓	✓	✓	✓	✓	✓	✓	✓
Exam-month FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: Each column within each panel reports estimates from a separate regression. Standardized test scores are instrumented using three pre-exam violence intensity windows (1–90, 91–180, and 181–270 days). All models include a female indicator (except gender-split regressions in Panel A), district-level weather controls, province × year fixed effects, and exam-month fixed effects. The Cragg-Donald F-statistic assumes i.i.d. errors; the Kleibergen-Paap F-statistic is robust to clustering at the district level. Standard errors clustered at the district level in parentheses. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table A.12: Household level summary statistics - ALCS 2016–17

	Mean	SD
Edu expenditure (12m)	1763.505	(4928.859)
Economic situation worsened	0.519	(0.500)
Displacement	0.096	(0.294)
Any shock (12m)	0.521	(0.500)
Economic shock	0.338	(0.473)
Agriculture/livestock shock	0.182	(0.386)
Drinking-water shock	0.160	(0.366)
Health shock	0.147	(0.354)
Insecurity/violence shock	0.133	(0.339)
Natural shock	0.072	(0.258)
Other shock	0.018	(0.133)
School-age children (6–18)	2.820	(2.095)
College-age children (19–24)	0.773	(1.026)
Female	0.011	(0.104)
Age	42.893	(13.787)
No schooling	0.653	(0.476)
Primary (1–6)	0.118	(0.322)
Secondary (7–12)	0.158	(0.365)
Tertiary	0.066	(0.248)
Islamic	0.005	(0.069)
Household size	7.848	(3.490)
Urban	0.193	(0.395)
Rural	0.779	(0.415)
Kuchi	0.028	(0.164)
N	19,838	

Notes: This table presents summary statistics for the ALCS 2016–17 data. Means are reported with standard deviations in parentheses.

Table A.13: Summary statistics: working-age sample (Ages 25–65) ALCS 2016–17

	Mean	SD	N
Female	0.502	(0.500)	47,365
Age	39.214	(11.257)	47,365
Experience	31.049	(12.547)	47,099
Experience squared	1121.442	(841.017)	47,099
No schooling	0.772	(0.420)	47,099
Primary (1–6)	0.073	(0.260)	47,099
Secondary (7–12)	0.102	(0.303)	47,099
Tertiary	0.051	(0.219)	47,099
Islamic	0.003	(0.050)	47,099
Employed at all	0.495	(0.500)	47,143
Day labourer	0.154	(0.361)	23,592
Salaried worker	0.176	(0.381)	23,580
Self-employed	0.462	(0.499)	23,616
Unpaid family worker	0.200	(0.400)	23,617
Agriculture	0.442	(0.497)	23,614
Mining & electricity	0.003	(0.052)	23,614
Manufacturing	0.055	(0.228)	23,614
Construction	0.093	(0.291)	23,614
Retail	0.107	(0.309)	23,614
Transport & communication	0.067	(0.250)	23,614
Finance & real estate	0.010	(0.101)	23,614
Other services	0.203	(0.402)	23,614

Notes: Means with standard deviations in parentheses; N is the number of non-missing observations per variable. Sample restricted to individuals aged 25–65. The employment-status variables (day labourer, salaried worker, self-employed, unpaid family worker) and the sector variables (agriculture through other services) are defined only for women reporting any work, so their statistics are computed over that subsample.

Table A.14: Household educational expenditure, economic situation, and displacement

	ln(Ed spending) (1)	Economic hardship (2)	Displaced (3)
District is insecure	-0.157*	0.053***	0.001
	(0.088)	(0.015)	(0.007)
# school age children	0.752***	0.006**	0.003***
	(0.033)	(0.003)	(0.001)
# college age children	0.198***	-0.001	0.005*
	(0.027)	(0.005)	(0.002)
Household head characteristic			
Female	0.213	0.058	0.008
	(0.217)	(0.066)	(0.012)
Age	0.017***	0.000	0.001*
	(0.002)	(0.000)	(0.000)
Primary (grade 1-6)	0.588***	0.020	0.019***
	(0.080)	(0.013)	(0.006)
Secondary (grade 7-12)	0.995***	-0.077***	0.021*
	(0.085)	(0.011)	(0.012)
Tertiary	1.881***	-0.184***	-0.002
	(0.106)	(0.014)	(0.008)
Islamic	-0.036	-0.041	0.022
	(0.340)	(0.066)	(0.036)
Household size	0.022	-0.005***	-0.002
	(0.015)	(0.002)	(0.001)
Rural	-1.235***	-0.034*	-0.017
	(0.136)	(0.019)	(0.012)
Kuchi/Nomads	-4.009***	-0.029	0.111**
	(0.246)	(0.030)	(0.049)
R-squared	0.433	0.140	0.223
N	19,624	19,611	19,587
District FE	✓	✓	✓

Note: This table shows the association between perceived insecurity and household-level outcomes. ln(Ed spending) is the log of household educational spending, Economic hardship indicates whether the household reports worsening economic conditions, and Displaced captures whether the household was displaced at the time of the survey. All models include district fixed effects and use survey weights, with standard errors clustered at the district level. Source: ALCS 2016–17, obtained under restricted use from NSIA–Afghanistan. Significance level: * 0.10 ** 0.05 *** 0.01.

Table A.15: District subjective security ratings and the likelihood of household reporting negative shocks

	Any shock	Economic	Agriculture / livestock	↓ drinking water	Health	Insecurity / violence	Natural	Others
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
District is insecure	0.060***	0.050***	-0.001	0.032**	0.009	0.107***	-0.008	-0.006
	(0.015)	(0.015)	(0.016)	(0.015)	(0.013)	(0.021)	(0.008)	(0.004)
# school age children	0.006***	0.007**	0.003	0.005**	-0.002	0.002	0.003*	-0.001
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
# college age children	-0.005	-0.006*	-0.002	-0.000	-0.006*	0.003	0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)
Household head characteristic								
Female	-0.028	-0.037	-0.093***	-0.037	0.099	-0.021	0.006	-0.006**
	(0.049)	(0.037)	(0.029)	(0.026)	(0.069)	(0.021)	(0.014)	(0.003)
Age	0.000	-0.001*	0.001***	-0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Primary (grade 1-6)	-0.018	-0.016	0.001	-0.035**	-0.002	0.011	0.004	-0.003
	(0.018)	(0.019)	(0.009)	(0.015)	(0.008)	(0.009)	(0.006)	(0.004)
Secondary (grade 7-12)	-0.053***	-0.059***	-0.014	-0.037***	-0.010	0.005	0.001	-0.002
	(0.016)	(0.013)	(0.009)	(0.010)	(0.010)	(0.008)	(0.007)	(0.004)
Tertiary	-0.126***	-0.142***	-0.029**	-0.048***	-0.040**	-0.008	0.009	-0.000
	(0.030)	(0.032)	(0.014)	(0.011)	(0.019)	(0.010)	(0.008)	(0.006)
Islamic	-0.189***	-0.049	-0.095***	-0.100**	-0.080***	-0.022	-0.053	-0.010
	(0.065)	(0.065)	(0.035)	(0.039)	(0.028)	(0.026)	(0.039)	(0.014)
Household size	0.001	-0.003	0.005***	-0.004***	0.004**	0.000	-0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Rural	0.087***	0.013	0.169***	0.062***	-0.032	0.052	0.005	0.002
	(0.031)	(0.026)	(0.045)	(0.022)	(0.021)	(0.048)	(0.007)	(0.009)
Kuchi/Nomads	0.141***	0.028	0.209***	0.114**	-0.044	0.003	-0.032**	0.117***
	(0.041)	(0.036)	(0.051)	(0.050)	(0.034)	(0.049)	(0.015)	(0.029)
R-squared	0.330	0.268	0.289	0.317	0.110	0.417	0.290	0.119
N	19,624	19,624	19,624	19,624	19,624	19,624	19,624	19,624
Outcome mean	0.522	0.339	0.182	0.160	0.147	0.133	0.072	0.018
District FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: This table shows the association between perceived insecurity and household-level shocks. Shock categories are not mutually exclusive; households may report more than one type of shock, which are grouped into these broader categories. All models include district fixed effects and use household survey weights, with standard errors clustered at the district level. Source: ALCS 2016–17, obtained under restricted use from NSIA–Afghanistan. Significance level: * 0.10 ** 0.05 *** 0.01.

Table A.16: Determinants of adult (25–65 years old) labor force participation by type of work

	Any work	If Any work at all = 1			
		Day laborer	Salaried worker	Self-employed	Unpaid family worker
	(1)	(2)	(3)	(4)	(5)
District insecure	-0.003	-0.031***	-0.015*	0.048***	-0.003
	(0.010)	(0.010)	(0.008)	(0.015)	(0.009)
Primary 1-6	-0.023	0.001	-0.114***	0.158***	-0.045***
	(0.021)	(0.017)	(0.019)	(0.021)	(0.011)
Secondary 7-12	-0.020	0.028	-0.178***	0.231***	-0.079***
	(0.036)	(0.025)	(0.034)	(0.038)	(0.016)
Tertiary	0.040	0.027	0.054	0.108*	-0.187***
	(0.065)	(0.034)	(0.043)	(0.056)	(0.027)
Islamic	-0.086	-0.017	0.081	0.085	-0.155***
	(0.057)	(0.049)	(0.076)	(0.085)	(0.030)
Insecurity × Primary	-0.003	0.020	-0.051***	0.014	0.015
	(0.023)	(0.023)	(0.018)	(0.030)	(0.020)
Insecurity × Secondary	-0.011	0.064***	-0.044*	-0.025	0.005
	(0.018)	(0.018)	(0.023)	(0.028)	(0.015)
Insecurity × Tertiary	0.022	0.094***	0.054**	-0.114**	-0.032
	(0.025)	(0.031)	(0.026)	(0.044)	(0.028)
Insecurity × Islamic	-0.005	0.081	-0.011	-0.125	0.056
	(0.136)	(0.061)	(0.146)	(0.142)	(0.056)
Female	-0.575***	-0.218***	-0.057***	-0.303***	0.572***
	(0.015)	(0.014)	(0.014)	(0.022)	(0.034)
Age	0.007**	-0.020***	0.038***	-0.020***	0.002
	(0.003)	(0.002)	(0.003)	(0.004)	(0.002)
Experience	0.013***	0.017***	-0.036***	0.027***	-0.008***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Experience square	-0.000***	-0.000	-0.000	-0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
HH size	-0.002***	-0.007***	-0.004***	0.002	0.008***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Rural	-0.014	-0.009	-0.070***	0.021	0.060***
	(0.019)	(0.026)	(0.014)	(0.036)	(0.016)
Kuchi/Nomads	0.133***	-0.070**	-0.107***	-0.025	0.206***
	(0.025)	(0.028)	(0.017)	(0.044)	(0.028)
R-squared	0.42	0.16	0.35	0.21	0.49
N	46,671	23,198	23,186	23,222	23,223
Outcome mean	0.496	0.156	0.177	0.465	0.202
District FE	✓	✓	✓	✓	✓

Note: This table shows the association between perceived district insecurity and individuals' labor market outcomes for working-age adults (25–65). All models include district fixed effects, use survey weights, and report standard errors clustered at the district level. Data source: Afghan Living Conditions Survey (ALCS) 2016–2017; the data was obtained under restricted use from the National Statistics and Information Authority (NSIA), Afghanistan. Significance level: * 0.10 ** 0.05 *** 0.01.

Table A.17: Determinants of adults (25–65 years old) labor force participation by sector

	Agriculture (1)	Utilities (2)	Manufacturing (3)	Construction (4)	Retail (5)	Transport/Com (6)	Finance (7)	Services (8)
District insecure	0.074***	-0.001	-0.023***	-0.030***	-0.002	-0.002	-0.002	-0.017**
	(0.015)	(0.001)	(0.008)	(0.007)	(0.008)	(0.006)	(0.002)	(0.008)
Primary 1-6	-0.086***	-0.003	0.040***	0.064***	0.069***	0.049***	-0.005	-0.118***
	(0.016)	(0.002)	(0.008)	(0.015)	(0.014)	(0.012)	(0.007)	(0.020)
Secondary 7-12	-0.106***	-0.010**	0.025*	0.075***	0.145***	0.058***	0.009	-0.164***
	(0.031)	(0.005)	(0.013)	(0.022)	(0.025)	(0.021)	(0.012)	(0.035)
Tertiary	-0.205***	-0.014**	-0.015	0.106***	0.047	0.028	-0.006	0.101***
	(0.038)	(0.006)	(0.018)	(0.030)	(0.036)	(0.028)	(0.012)	(0.038)
Islamic	-0.176**	0.008	0.028	0.026	0.009	0.017	-0.008	0.109
	(0.073)	(0.022)	(0.018)	(0.039)	(0.032)	(0.025)	(0.013)	(0.072)
Insecurity × Primary	0.012	0.000	-0.010	-0.032*	0.029	0.039**	0.003	-0.038*
	(0.035)	(0.003)	(0.010)	(0.018)	(0.025)	(0.018)	(0.006)	(0.020)
Insecurity × Secondary	-0.054**	0.004	0.054***	0.030**	0.012	-0.008	-0.012**	-0.018
	(0.025)	(0.003)	(0.015)	(0.014)	(0.024)	(0.014)	(0.006)	(0.024)
Insecurity × Tertiary	-0.160***	0.008	0.038**	0.053**	0.017	-0.007	0.002	0.040
	(0.027)	(0.006)	(0.015)	(0.020)	(0.024)	(0.013)	(0.007)	(0.029)
Insecurity × Islamic	-0.118	-0.021	0.019	-0.009	0.034	0.021	-0.004	0.109
	(0.134)	(0.019)	(0.014)	(0.062)	(0.035)	(0.018)	(0.008)	(0.100)
Female	0.272***	-0.003***	0.159***	-0.142***	-0.105***	-0.087***	-0.009**	-0.083***
	(0.025)	(0.001)	(0.021)	(0.008)	(0.013)	(0.006)	(0.004)	(0.017)
Age	0.002	0.001	-0.005***	-0.016***	-0.009***	-0.008***	0.000	0.031***
	(0.004)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
Experience	-0.002	-0.001	0.003*	0.015***	0.010***	0.010***	-0.001	-0.030***
	(0.004)	(0.000)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
Experience square	0.000***	-0.000**	0.000**	-0.000	-0.000	-0.000***	0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
HH size	0.005***	0.000**	-0.000	-0.003***	0.001	0.001	-0.000	-0.005***
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)
Rural	0.331***	-0.001	-0.050***	-0.005	-0.172***	-0.024	-0.007	-0.059***
	(0.050)	(0.003)	(0.013)	(0.018)	(0.024)	(0.015)	(0.005)	(0.021)
Kuchi/Nomads	0.567***	0.001	-0.112***	-0.011	-0.213***	-0.071***	-0.013**	-0.125***
	(0.052)	(0.004)	(0.016)	(0.023)	(0.026)	(0.016)	(0.006)	(0.023)
R-squared	0.41	0.04	0.15	0.10	0.10	0.07	0.02	0.27
N	23,238	23,238	23,238	23,238	23,238	23,238	23,238	23,238
Outcome mean	0.444	0.003	0.056	0.094	0.108	0.067	0.010	0.204
District FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: This table shows the association between perceived district insecurity and employment sector among working individuals (aged 25–65). The estimation sample is restricted to the employed; each outcome is an indicator equal to one if the individual's main job is in that sector and zero otherwise, so the Outcome mean gives each sector's employment share. Sectors are defined from the industry-of-work classification: Agriculture; Utilities (mining and electricity); Manufacturing; Construction; Retail (wholesale and retail trade); Transport/Com (transport and communication); Finance (financing, insurance, and real estate); and Services (community, social, and personal services). All models include district fixed effects and use survey weights, with standard errors clustered at the district level. Source: Afghan Living Conditions Survey (ALCS) 2016–2017, obtained under restricted use from NSIA–Afghanistan. Significance level: * 0.10 ** 0.05 *** 0.01.