Active Conflict and Access to Education

Evidence from a Series of Conflict-Related Shocks in the Republic of Yemen

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Abstract

Using a high-frequency survey in the Republic of Yemen, this paper demonstrates how school attendance responds to a series of conflict-related shocks. First, there are plausibly exogenous changes in violence that have limited impacts on school attendance but do affect other dimensions of well-being. And second, consequences of conflict aside from

living in close proximity to violence can impact attendance. The importance of a wide variety of conflict shocks suggests that an understanding of all shocks is needed before attributing the cause of attendance changes in such tumultuous settings, and these results have implications for the delivery of education assistance in conflict settings.

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Active Conflict and Access to Education: Evidence from a Series of Conflict-Related Shocks in the Republic of Yemen

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1. Introduction

Conflict and fragile settings are becoming increasingly important contexts in which education assistance is needed. In addition to an increase in conflict in the past decade, the world's worst conflicts are also beginning to last longer (e.g., World Bank 2017a). Given the priority of humanitarian assistance in many conflict contexts (e.g., OCHA 2019a), there is a risk that poor education during these longer-lasting conflicts will have permanent impacts on later life outcomes of school-aged children and will also have large impacts on the characteristics of the society that emerges from conflict (e.g., UNICEF 2018).

There is a large literature motivating the need to support education during conflict. One set of findings demonstrates that the onset of violence or living in close proximity to violence often has a negative impact on the educational attainment of at least a subset of children, oftentimes drawing findings from household surveys conducted long after conflict has ended.¹ However, there is a wide range of conflict-related shocks aside from living in close proximity to violence that can impact access to education (e.g., Chavez et al. 2018), and a more limited but growing set of analyses investigate other types of conflict-related shocks (e.g., Di Maio and Nandi 2013; etc.). More evidence on the effect these other types of conflict shocks have on access to education, and how these effects compare to the effect of living close to violence, would help inform the design and targeting of needed interventions.

This paper seeks to investigate the impact of a series of conflict-related shocks on school attendance in the Republic of Yemen using a novel high-frequency mobile phone survey collected during active conflict. The setting is interesting for a number of reasons. First, the country has been enduring a protracted conflict for over five years that has resulted in a large increase in the share of children not attending school (e.g., OCHA 2020). Second, in addition to the violence from the conflict, there are a number of other conflict-related shocks that have affected the ability of children to attain an adequate education, such as the suspension of salary payments to teachers, the inability to acquire teaching materials, and the substantial increase in the share of households that are financially struggling (e.g., OCHA 2020). And lastly, there are a number of education interventions currently being implemented in Yemen that need up-to-date information on how and where to best support schools and parents (e.g., OCHA 2019a).

This analysis utilizes a monthly mobile phone survey conducted by the World Food Programme (WFP) that tracks school attendance, food security, and access to health care for a panel of households beginning in March 2019.² The survey initially reached households through random digit dialing in the first month, and then each subsequent month contacted a mix of both previous and new respondents to reach a monthly sample size of approximately 2,400 households that is distributed across the entire country according to population. Prior to the beginning of the COVID-19 crisis in March 2020, the survey reached approximately 13,500 households an average of two times each. The survey is one of the most complete data sources describing the evolution of school attendance during active conflict and offers a unique

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¹For a survey of the literature, see Blattman and Miguel (2010), Justino (2012), and Verwimp et al. (2019).

²The monthly survey tracking only food security began in August 2015, but an expanded module tracking access to basic services was inserted in March 2019.

opportunity to track how attendance responds to a variety of conflict-related shocks.

However, in order to accurately identify the factors that most affect schooling during active conflict, it is necessary for mobile phone penetration to be high and for access to mobile phones to be relatively unaffected by important conflict-related shocks. Evidence from the last nationally-representative survey in 2014 suggests that access to mobile phones was high prior to the conflict, and evidence from the WFP mobile phone surveys suggests that access has remained high during the conflict. Additionally, there is no evidence that any of the shocks analyzed here affect either the number of active mobile phone numbers in a household or other household characteristics that are unlikely to rapidly change. Thus, although the survey misses households without access to mobile phones who potentially have worse access to education, the relatively high penetration of mobile phones allows an analysis that is representative of the majority of the population.³

Using the WFP surveys, we illustrate a number of patterns in access to schooling during the conflict in Yemen. First, we demonstrate that sudden changes in violence and fragility (e.g., conflict fatalities) need not lead to a change in access to schooling in all cases. In particular, we estimate the change in school attendance in response to southern secessionists capturing the capital of the internationally-recognized government (IRG), a partial ceasefire unofficially declared by the de facto authorities in the north (DFA) and Saudi Arabia, and in response to a sudden and violent campaign led by the DFA to capture a northern governorate from the IRG. Each event was unexpected, caused a substantial change in violence, and one of these events had a significant impact on peoples' livelihoods. However, in all cases, there was little change in school attendance in affected regions. These findings are further corroborated by a lack of correlation between violence in the district of residence and school attendance for the entire sample of households.

And second, we demonstrate that other conditions associated with conflict aside from the location of violence can have a large impact on access to education. The distrust of the IRG, insufficient salaries for teachers, and overall poor conditions in schools led to a sudden and voluntary teacher strike in regions controlled by the IRG during the survey period. The strike was directly in response to factors that have been caused by the conflict and have impacted the entire country to a differing degree, but the strike was not directly related to any specific violent event. Importantly, the event is further entwined with the conflict as it has been reported that southern secessionists had been exploiting these frustrations to help incite the strike.⁴ The duration of the strike was longer than many of the rapid escalations of violence experienced during the conflict, and like many incidents of violence, the shock impacted the ability of schools to remain open.

Following the strike, there was a more than doubling of households in IRG-held areas that had school-aged children not attending school. In specifications comparing the impacts

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³ Additionally, there is the possibility that selection bias in who is more likely to respond to a phone survey could impact the results. However, the mobile phone survey has been able to detect significant changes in access to education, food security, and access to basic services that have been identified by independent sources.

⁴ For example, see <http://aden-tm.net/NDetails.aspx?contid=110777>.

of the strike to the impacts of the violent campaigns described above, one can reject the hypothesis that the impacts are equal at conventional significance levels. Overall, this event caused attendance to go from better than other more conflict-affected regions to significantly worse in a matter of 2 months, and the regions with the worst school attendance actually had the least violence at the end of the survey period analyzed (February 2020). Importantly, in specifications that do not properly account for the strike, the strong impacts of the strike can actually be misattributed to particular types of violence in this setting that have a strong geographic overlap with regions affected by the strike.

There are three primary contributions of these results. First, although the majority of articles addressing education during conflict focus on the relationship between proximity to violence and education outcomes, this setting also incorporates other conflict-related shocks. These findings also demonstrate that conflict-related shocks aside from proximity to violence can be more important to explaining variation in school attendance, and the effects of these other shocks can be misattributed to the local impact of violence if not properly incorporated. Given the possibility that other unexplored events could be obscuring the relationship between violence and schooling outcomes in other settings as well, these results offer an additional explanation for significant variations in the impact violence has on education across contexts, where the magnitude and sign of the impact often depends on the type of violence, the characteristics of children, and the conflict setting (e.g., Blattman and Annan 2010; Chamarbagwala and Morn 2010; Shemyakina 2011; Leon 2012; Akbulut-Yuksel 2014; Justino et al. 2014; Swee 2015; Monterio and Rocha 2017; Michaelsen and Paolo 2018; Bertoni et al. 2019; Bruck et al. 2019; etc.).⁵

Second, these results demonstrate that the sensitivity of school attendance to living in close proximity to violence can change during conflict. Relative to pre-conflict levels, school attendance in Yemen had significantly declined by the early years of the conflict (e.g., OCHA 2017; WFP 2019; etc.). Evidence suggests that at least a portion of this decline was due to households being in close proximity to violence, where the most often cited reason parents took their children out of school was safety and there is evidence that violence had damaged school buildings in the country (e.g., OCHA 2020; World Bank 2020a).⁶ However, as the violence in the country surged to levels much higher than at the beginning of the conflict, there was surprisingly little sensitivity of school attendance to changes in violence. This is further in line with aggregate displacement patterns, where the vast majority of displacement occurred at the beginning of the conflict when violence was far from its peak (e.g., TFPM 2017).

And lastly, even in the conflict in Yemen, where news and humanitarian organizations are reporting many events in real time, these results demonstrate that there can be consequential events that have a strong impact on the ground that are not widely reported. The teacher

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⁵Additionally, the possibility that conflict can significantly affect school attendance beyond the geographic location of violence is consistent with a growing literature demonstrating similar results with food security outcomes (e.g., D' Souza and Jolliffe 2013; Martin-Shields and Stojetz 2018; etc.).

⁶As of 2020, 32 percent of school buildings in the 16 largest cities in Yemen were partially damaged and 3 percent were destroyed (e.g., World Bank 2020a).

strike that more than doubled the incidence of children not attending school in affected regions, and even led to an increase in children not attending school nationally, has received little-to-no attention by the international media.⁷ Given the difficulty of identifying all the significant events to which interventions need to be sensitive in a conflict setting that is being actively followed, it is likely even more difficult to try and identify many of the effects of conflict-related shocks using data on educational attainment by cohorts collected decades after conflict has ended. In order to better inform possible education interventions in the midst of protracted conflicts, it is important to perform more data collection during conflict and to further closely monitor reports from key informants, news organizations, and humanitarian organizations to fully understand the conflict-related events happening on the ground.

The rest of the paper is structured as follows. Section 2 describes the background of the conflict and education in Yemen; Section 3 describes the data; Section 4 reports the correlation between school attendance and violence in the district of residence of the entire sample; Section 5 reports the impacts of the four major shocks on school attendance; Section 6 provides an example illustrating how conflict shocks aside from the proximity to violence can potentially be misattributed; and Section 7 concludes.

Section 2. Background

Houthi forces from the north of the country, upset with the outcome of a transition to a new constitution following the Arab Spring, began an assault on the national government in 2014 (e.g., Baron 2015). They captured the capital Sana'a, and then captured much of the south of the country. Beginning in March 2015, the conflict significantly escalated after a coalition of nine countries began an extensive bombing campaign to recover the territory captured by Houthi forces (e.g., World Bank 2017b). During the period under analysis, Houthi forces were the de facto authorities (DFA) in most of the north of the country, and the IRG controlled most of the south of the country. Aside from the IRG, the DFA, and the coalition of nine countries supporting the IRG, there were also a variety of other actors that were involved in the conflict during the time period under analysis, including southern secessionists and terrorist organizations (e.g., Amnesty International 2019).

There were a number of ways the conflict potentially affected access to education in the country. Increased violence and fragility- from both ground troops and airstrikes- limited the ability of children to safely attend school in regions that experienced the most violence. Previous surveys and key informant interviews conducted by UNICEF identified declines in safety as one of the many reasons why children might not be attending school (e.g., OCHA 2020; etc.). The manner in which safety declined varies across the different regions and over time. For example, in Taizz, the third largest city in Yemen, the front lines of the war bisected the city and children had to make their way to school amid active fighting and snipers (e.g., Doucet 2020); in some instances, there were active ground battles surrounding military

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⁷This is in contrast to events that are widely reported but have limited effects on access to education and other dimensions of welfare, such as the capture of the IRG capital by secessionists or the currency depreciation following the Houthis no longer accepting new bank notes printed by the IRG (e.g., WFP 2019a; WFP 2020).

installations on the outskirts of cities (e.g., Al Jazeera 2020); and in other cities, there were intermittent air strikes and terrorist attacks (e.g., Amnesty International 2019). Furthermore, some regions were being captured by different parties to the conflict (e.g., Al Jazeera 2019), which interrupted or completely changed the governance of regions and further affected the safety of children attending school and the ability of schools to remain open.

Related to the increased violence, a number of school buildings have been damaged in the country that make it more difficult for children to attend school. In the most recent Damage and Needs Assessment performed by the World Bank in the 16 largest cities in the country, 32 percent of schools have been partially damaged, and 3 percent of schools have been destroyed as of 2020 (World Bank 2020a). In response to schools not being operational, some school officials have reported either setting up temporary structures for schools nearby, or sending children to other nearby schools (e.g., World Bank 2020b). Given the added difficulty in attending school in these circumstances, it is possible that violence can have a persistent effect in some regions.

However, there were a variety of other ways the conflict affected access to education in the entire country and not just in regions affected by the most severe violence. First, beginning in 2016, public sector salaries were completely halted in DFA-held territory in the north of the country (e.g., World Bank 2017b). Given the lack of any or consistent payment for teachers in the north, and given the lack of adequate payment for the teachers who were receiving a salary in the rest of the country, there was an increase in teacher absenteeism in the country (e.g., OCHA 2019a). There were reports of volunteer teachers making up the teacher deficit in some regions, but not all needs could be covered in this way and many claimed that the quality of the volunteer teachers was poor (e.g., World Bank 2020b).

Second, in addition to the lack of teachers, the conflict exacerbated the poor condition of schools and problems with the limited amount of teaching materials (e.g., OCHA 2019a). These were already problems prior to the conflict (e.g., World Bank 2017b), but physical damage to schools since the start of the conflict and a near complete freeze on the ability to repair schools and teaching materials significantly worsened these problems (e.g., World Bank 2020b; etc.). There were reports of school construction in a limited number of districts, and some grades in a portion of IRG-held territory received textbooks, but these instances were limited and did not cover the entire country (e.g., OCHA 2019b).

And third, there was a large shock to household incomes in the country. Dramatic increases in underemployment and significant inflation in the price of key commodities likely had significantly limited the ability of households to purchase items necessary for children to go to school (e.g., OCHA 2019a). Additionally, there were reports that older children were taken out of school to help financially support households (e.g., World Bank 2020b). Along with this lack of income, there were significant deprivations in food security and access to health care, which both limited the ability of children to attend school and to acquire a quality education if they did attend (e.g., OCHA 2019a).

Given all these factors that limited the ability of children to attend school or the ability

of schools to remain open, it is clear that access to schooling likely declined since the start of the conflict across the entire country (e.g., WFP 2019b). However, there has not been a thorough analysis investigating the relative importance of each of these possible factors that limited access to schooling, which makes it difficult to prioritize factors and regions in an intervention. As discussed above, the relationship between the location of violence and poor education outcomes has been well-documented in other settings. But given the lack of data collection in the midst of such fragile and volatile circumstances, the differential effects of living in close proximity to violence versus conflict shocks that affect regions beyond those experiencing violence are unknown.

Section 2a. Shocks during the Survey Period

Between March 2019 and February 2020, the 12 months during which the WFP survey included questions on school attendance prior to the closure of schools due to COVID-19 in March 2020, there were a number of unexpected and plausibly exogenous conflict-related shocks that could have significantly affected access to schooling. The timeline of these shocks is illustrated in figure 1. First, in August 2019, southern secessionists captured Aden, the capital of the IRG, and the capitals of two additional governorates. The stated aims of the group, which were supporters of the Southern Transition Council, were to overthrow the IRG and claim South Yemen as a separate country from the north as it was prior to unification in 1990 (e.g., OCHA 2020). The capture was arguably a surprise,⁸ and figure 2 demonstrates that there was a large increase in both violent incidents and fatalities in the districts that were captured.

The capture of the territory began in August 2019, which is prior to the date that schools opened on September 15, 2019. However, the occupation continued through the start of the school year until there was a negotiated transition of power back to the IRG on October 15, 2019 (e.g., Al Jazeera 2019; Peoples' Dispatch 2019; etc.). Despite the capture having occurred during August, figure 2 further shows that a higher number of fatalities occurred during the occupation than prior to the assault and this violence continued after the school year began.

The second major shock during the time period was a partial ceasefire that occurred in September 2019. Days after a drone strike on oil infrastructure in Saudi Arabia, DFA forces unilaterally announced a ceasefire without any agreement from coalition forces. Shortly after this, Saudi Arabia announced a ceasefire in Al Hudaydah, Al Jawf, Sana'a, and Sa'ada (e.g., Hatem 2019). Although there were continual reports of the partial ceasefire failing to hold, especially in Al Hudaydah where violence did not decline, figure 2 demonstrates that there was a large average decrease in violence in the regions covered by the ceasefire. After the ceasefire broke down at the beginning of January, there was a significant increase in violence.

The third major shock was the unexpected capture of the oil-rich governorate Al Jawf from the IRG by DFA forces beginning in February 2020 and completing on March 1, 2020 (e.g.,

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⁸Immediately prior to the capture in July 2019, the World Bank sent a delegation of senior officials to Aden to begin discussions with the IRG to re-open the World Bank office in the city given the relative safety of the city prior to the capture (e.g., World Bank 2019).

Al Jazeera 2020). This capture almost immediately followed the breakdown of the partial ceasefire and was unexpected by the IRG. As demonstrated in Figure 2, there was a decline in violence in Al Jawf following the ceasefire described above, and violence rapidly increased back to the pre-ceasefire levels during the assault in February. Thus, although violence suddenly and unexpectedly increased, the violence was at levels experienced by the population in the past year.

And lastly, the fourth major shock was the unexpected teacher strike in regions completely controlled by the IRG beginning in February 2020 in response to distrust of the IRG, low salaries for teachers, poor conditions of the schools, and inadequate teaching materials (e.g., World Bank 2020b). Each of these factors has either been caused or significantly exacerbated by the conflict and has affected the entire country rather than affecting only the most violent regions (e.g., OCHA 2019a). Further tying the strike to the conflict, there are reports that the strike was being instigated by secessionist forces in an attempt to use public frustration against the IRG.⁹ Although the strike was voluntary, there were reports of many teachers participating in a number of governorates. However, there were also reports of portions of some IRG-held governorates not fully participating in the strike- parts of Shabwah and Al Maharah.¹⁰ Importantly, figure 2 demonstrates that there was little change in violence in the time period of the strike.

Surprisingly, the strike was not widely reported in the international news media or by humanitarian organizations, but was covered by a number of local news organizations.¹¹ One potential reason was the capture of Al Jawf and other events happening at the same time, which could have dominated the news cycle and captured the attention of humanitarian agencies.¹² Despite limited coverage in the local media, the World Bank performed a number of key informant interviews with teachers, principals, and district and governorate officials in charge of education to discuss the strike (e.g., World Bank 2020b).

Using these four strong and arguably exogenous conflict-related shocks during the survey period that might significantly impact access to education, we investigate the impact each had on school attendance. In addition to investigating the impact each had individually, the effects of the four shocks are compared. Furthermore, the impacts of each shock on a variety of other household measures are investigated to better understand the range of potential impacts on households.

Section 3. Data

Given the tumultuous environment during active conflict, it is very challenging to find data

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⁹For example, see <http://aden-tm.net/NDetails.aspx?contid=110777>.

¹⁰The districts that border the semi-autonomous governorate of Marib were reported to not participate, and districts in Al Maharah were also reported to not all participate. Al Maharah, which borders Oman, is removed from much of the conflict.

¹¹For example, see <http://aden-tm.net/NDetails.aspx?contid=110777>, <https://almawqeaqpost.net/news/47124>, <http://adengad.net/news/435004/>, <https://www.alkhabarayemeni.net/2020/01/08/75137/>.

¹²At the same time, the north announced they would no longer accept new bank notes printed by the IRG beginning in 2016. However, reports suggest that this change had little change on markets or household welfare (including education) during the period under analysis (e.g., WFP 2020).

that assess how school attendance and other education outcomes respond to conflict-related shocks in real time. Yemen is no exception, where the poor security situation (e.g., Sundberg and Melander 2013), the budgetary problems of the government and the Central Statistical Organization (e.g., World Bank 2017b), and the significant upheaval in population movement since the last census conducted in 2004 all make it very difficult to conduct traditional household surveys (e.g., TFPM 2017). Furthermore, given the large and frequent shocks that could potentially affect school attendance (e.g., Tandon and Vishwanath 2020), high-frequency data collection would be necessary to differentiate between potential mechanisms affecting school attendance.

Given the substantial constraints on traditional data collection and the need for high-frequency data, we turn to rare, non-traditional survey data. This paper utilizes the most geographically complete survey covering Yemen following the start of the conflict- the mobile Vulnerability and Assessment Mapping Survey (mVAM) conducted by the WFP. The monthly survey began in August 2015 and has been conducted nearly every month since. However, the survey was originally focused on food security, and only began incorporating school attendance in March 2019 (e.g., WFP 2019b). The data collected in the monthly survey since March 2019 provide a rare opportunity to analyze schooling decisions in an active and volatile conflict, with important implications for governments, and humanitarian and development organizations.

On average, each mVAM interview takes about 15 minutes to complete. The survey is conducted via mobile phone and respondents are reached via random digit dialing. The survey is stratified by governorate/capital city, where potential respondents continue to be contacted until a sufficient number of completed responses have been met in each governorate aside from the island of Socotra.¹³ The number of minimum responses per governorate are determined by the governorate's share of the total population, and the total number of respondents surveyed each month is approximately 2,400.

The survey is a rolling panel, where the call center initially acquires a list of 2,400 phone numbers via random digit dialing in the first month. In the second month, the same 2,400 phone numbers are contacted for an identical survey. Of these 2,400, approximately 400 on average will not be able to be reached and will have to be replaced for the second survey via random digit dialing, and approximately 200 will not respond for two months in a row. However, those that are not reached will continue to be listed in the bank of possible phone numbers to be tried each month, and the same household is potentially contacted for a number of survey rounds.¹⁴

The analysis uses surveys between March 2019 and February 2020. This sample includes 13,577 households interviewed approximately two times each. Of these households, 6,114 were interviewed only once, and the rest of the households were interviewed between 2 and 12 times spanning the 12 monthly surveys. All education results are restricted to the 11,313 households that have school-aged children and were interviewed while school was in session.

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¹³Yemen has 333 districts that fall within 21 governorates and one municipality (capital city).

¹⁴The WFP is unable to observe whether a phone number exists in the random digit dialing procedure, and thus we are unable to identify the share of non-responses related to active phone numbers.

The survey collects information on whether all school-aged children in the household attended school in the last 30 days. This high-frequency measure of attendance is in contrast to school enrollment data compiled at a single point in time and allows for attendance to vary over time and in response to the many conflict-related shocks. Additionally, the survey collects a full food security module (food consumption score and reduced coping strategies index), whether the household had access to health care when needed, whether the household hosted displaced individuals, and whether the household itself had been displaced since the start of the conflict. These indicators make up a multidimensional welfare measure tracked by the WFP and the World Bank (e.g., WFP 2019b).¹⁵ We further merge the month and district of households with conflict data obtained from the Armed Conflict and Event Data (ACLED), which give the number of conflict incidents and the number of conflict fatalities in the district for the months around the survey.

Figure 3 reports summary statistics of school attendance. The top panel reports the share of households where not all school-aged children attend school by month of the survey for months in which school was in session; and the bottom panel uses the panel aspect of the data and reports the share of household, by number of surveys answered, in which there was at least one survey in which not all school-aged children attended school in the previous month. In any given survey approximately 20 percent of households report not sending all their children to school in the past month. However, there is a significant amount of instability within each household over time given such a tumultuous environment. The second panel of figure 3 demonstrates that for each additional survey to which households respond, there is a significant increase in the share of households that report not sending all children to school in at least one survey. By four surveys and above, over half of respondents report not sending all children to school in at least one survey.

The data further illustrate the dire food security situation for the entire mobile phone-using population over the course of the survey. Over 30 percent of households had inadequate Food Consumption Scores (FCS), which is defined as an FCS of less than 42;¹⁶ food assistance was the primary source of food for nearly 25 percent of households; and approximately 48 percent of households reported their primary difficulty being underemployment and low wages.

However, there are important caveats to note about the data. First, the survey can only be representative of the mobile phone-using population following the beginning of the conflict. Although it is difficult to identify exactly how access to mobile phones has changed since the onset of the conflict, all evidence suggests that access has remained high. The share of the population that lived in a household that owned at least one mobile phone was high prior to the conflict across the entire country and among vulnerable groups,¹⁷ there is anecdotal evidence

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¹⁵ Additionally, the survey tracks the number of active mobile phone numbers to which the household has access, primary sources of income in the past month, and the primary difficulty faced by the household in the past month.

¹⁶ Food Consumption Scores are the weighted sum of the number of times households consume from specific food groups. They capture both quantity and quality of food consumption.

¹⁷ Prior to the escalation of the conflict, 85 percent of the total population lived in a household that owned at least one mobile phone and would be represented by the household-level survey conducted by the WFP.

that the share with access to mobile phones remained high following the start of the conflict (although some households reported sharing a mobile phone),¹⁸ the geographic coverage of the survey reaches the vast majority of the country,¹⁹ and the WFP survey itself demonstrates that the number of mobile phones owned by households has largely not changed at the national and governorate levels.²⁰ Importantly, the shocks on which this analysis focuses all affect regions where the number of mobile phones has not changed since the last nationally-representative estimate (e.g., Abyan, Al Jawf, Shabwah, etc.).²¹

Another potential challenge to the generalizability of results is sample selection (i.e., non-random non-response). For example, in random digit dialing phone surveys in the United States of political preferences, the surveys are generally good at predicting party affiliation and many other political attributes relative to traditional household surveys but over-predict the amount of civic engagement due to differences in who is most likely to respond to a phone survey (e.g., Abraham et al. 2009).

To the degree that we are able to assess this issue in such a data- and evidence-scarce environment, we validate the WFP survey's representativeness of the broader mobile phone-using population by demonstrating that the survey is capturing trends that are independently corroborated by other sources. We demonstrate that the mobile phone survey does in fact detect large declines in many welfare outcomes and access to basic services that are consis-

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Furthermore, this figure remained high for the rural population (81 percent), the population living below the poverty line (77 percent), and the population of all governorates (over 60 percent for each). Authors' calculations using the 2014 Household Budget Survey.

¹⁸Registration for the World Bank's cash transfers program being implemented by UNICEF, which covers approximately one-quarter of the total population and is aimed at relatively poorer households, demonstrates the vast majority of households can be reached via phone (see (accessed September 2018): <http://www.worldbank.org/en/news/press-release/2017/05/19/new-world-bank-support-to-address-food-insecurity-in-yemen-aims-to-reach-9-million-yemenis>); evidence from different WFP surveys of food aid beneficiaries suggests that the share of food aid recipients that can be reached via mobile phone has remained very high (see (accessed September 2018): <http://documents.wfp.org/stellent/groups/internal/documents/projects/wfp278006.pdf>); and evidence from the WFP mobile phone survey itself suggests that particularly struggling households are well represented in the sample with nearly 40 percent of the sample being displaced and approximately 41 percent receiving food assistance.

¹⁹There are only 16 districts of 333 that do not have any respondents in any of the surveys. Alternatively, the vast majority of districts are represented in any single month of the survey. For example, in the March 2019 survey, the survey reached respondents in 264 of the 333 districts in the country- nearly 80 percent of the total. It is important to note that the districts for which there are zero respondents tend to be less populated and in the east of the country. Only 9.2 percent of the country's 2017 population resided in the 69 districts for which there are zero respondents in the March 2019 survey; and the districts overwhelmingly came from the governorates of Hadramaut and Al Mahrah, where only 39 and 33 percent of districts in each respective governorate had at least one respondent. Importantly, neither of these governorates is the primary focus of the analysis here.

²⁰For each survey, the 95 percent confidence interval for the average number of mobile phones owned by the household is contained in the 95 percent confidence interval prior to the conflict (based on data from the 2014 Household Budget Survey). Despite the large shock to household incomes and energy sources needed to charge mobile phones, the confidence intervals overlap in approximately 52 percent of the governorate-month observations. However, in the cases where there was not broad overlap between the WFP survey and pre-conflict data, much of the lack of overlap is in part due to the changes in the composition of the populations due to large-scale migration of IDPs (e.g., TFPM 2017), and also due to the five years that has passed since the last nationally-representative survey.

²¹All estimates are qualitatively identical when the analysis only focuses on regions where mobile phone ownership has not changed since the last nationally-representative estimate.

tent with the reports of humanitarian and news agencies on the ground.²² Furthermore, the regions that the WFP survey identifies as receiving the most assistance roughly align with the population-level estimates of the prevalence of food assistance (e.g., Tandon and Vishwanath 2019).

And lastly, for the empirical analysis, it is important to verify that access to mobile phones and the likelihood of responding to the survey does not change in response to the shocks that are the focus of the analysis. Table 1 reports summary statistics of respondent characteristics and the number of active mobile phone numbers owned by the respondent before and after each shock in the affected regions. For each shock, there is little difference in each variable following the shock- the magnitude of each difference is small, and only one coefficient out of 16 estimated is marginally statistically significant. Furthermore, none of the differences between pre- and post-shock is jointly significant (p-values of 0.518, 0.769, 0.202, and 0.615 respectively).²³ Combined, there is little evidence that the shocks significantly affected the characteristics of those who responded to the WFP mobile phone survey.

Section 4. Correlation between School Attendance and Violence in the District of Residence

Using the WFP survey, we first analyze the correlation between poor school attendance and violence in the district of residence. Specifically, we estimate the following specification:

$$(1) \text{ Poor\_School\_Attend}_{hgdmy} = \psi_h + \tau_{my} + \rho_g + \beta V \text{ violence\_Severity}_{gdmy} + \alpha X_{hgdmy} + \epsilon_{gdmy}$$

where h denotes household, g denotes governorate, d denotes district, m denotes month, and y denotes year; *Poor_School_Attend* denotes an indicator equaling one if not all school-aged children attended school in the past month; *X* denotes a vector of household control variables²⁴; and *V violence_Severity* denotes a number of measures of severity used in different empirical specifications, including an indicator equaling one if there was a conflict fatality in the month of the survey in the district, the number of conflict incidents in the district, and the number of conflict fatalities in the district.

Estimates of β are reported in Table 2. In all specifications, there is little evidence of a relationship between school attendance and the location of violence. Column (1) demonstrates that there is no relationship using the indicator equaling one if there was a conflict fatality in the district as the measure of severity, column (2) demonstrates there is no relationship using

²²Comparisons between the WFP survey to estimates of mobile phone users in the 2014 HBS data demonstrate that every single indicator of food security collected that is replicable in the 2014 HBS dramatically declined, consistent with the reports of widespread food insecurity (e.g., IPC 2017; FEW NET 2018; etc.); and access to a wide range of other basic services also declined, which is consistent with the reporting of humanitarian needs (e.g., OCHA 2020).

²³The p-values were obtained from a regression of the post indicator on each of the variables reported in table 1. For each shock, the sample is restricted to observations from the two months before and the months during each shock in the region that the shock affected.

²⁴Control variables include the gender of the household head, the age of the respondent, and the household size.

the number of conflict incidents, and column (3) demonstrates there is no relationship using the number of conflict fatalities. Importantly, these estimates are all precisely estimated and preclude large effects. Using the upper bound of the confidence interval estimated in column (3), one can reject the hypothesis at the 5 percent significance level of the increase in poor school attendance being larger than 0.015 in response to a doubling of conflict fatalities.²⁵

Furthermore, the lack of a relationship between poor school attendance and the location of violence is robust to a number of different empirical specifications. Column (4) re-estimates the relationship in column (3) using a logit specification²⁶; and column (5) includes lagged violence and future violence in the specification. In each case, none of the estimated coefficients is statistically significant either individually or jointly.

Additionally, there is little evidence of a correlation between school attendance and the location of violence when estimating the impact of different types of violence separately. Column (6) separates conflict fatalities into those from remote violence (e.g., air strikes), violence against civilians, ground battles, and all other conflict fatalities (captured by the coefficient on the total number of fatalities). Additionally, column (7) estimates the relationship separately for violence in Taizz and violence in the rest of Yemen. As discussed in the background section, Taizz is divided in two by the conflict and people are living amid active fighting and snipers. Again, in both cases, there is little evidence of any relationship between school attendance and violence.

Related to potential differences in the effect violence has based on the type of violence, it is also possible that separating the violence severity by the actors involved or in the region of the country could also help to uncover an underlying relationship between school attendance and the severity of violence. However, there is little evidence of this being the case. Column (8) separates fatalities into those involving Al-Qaeda in the Arabian Peninsula (AQAP), a prominent terrorist organization operating in Yemen (e.g., OCHA 2019a), and the rest of conflict fatalities; and column (9) estimates the impact violence has separately in regions controlled by the IRG and in regions controlled by DFA forces. However, again, there is little relationship between school attendance and the location of violence in either case.

Combined, there appears to be little evidence of school attendance being correlated with the location of violence in the year of surveys analyzed. However, it is important to note that following the onset of conflict, school attendance did significantly drop across the entire country relative to the last nationally-representative estimate from the 2014 Household Budget Survey (e.g., WFP 2019b). Thus, these results do not imply that the location of violence has no effect

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²⁵When using the number of fatalities as the independent variable instead of the natural logarithm, there is a positive relationship that is statistically significant at the 5 percent level. However, this is entirely driven by 39 observations in one district that had 613 conflict fatalities in one month, which is the highest number of district fatalities for any month in the sample. If these 39 observations are dropped, the variable is right-censored at the second greatest number of fatalities in a month (483), or the independent variable is the natural logarithm of conflict fatalities, there is little evidence of any relationship between school attendance and the severity of violence. Importantly, this district with 613 conflict fatalities in one month is well-represented in every other survey in addition to the survey in which there were 613 conflict fatalities, and in each of those other surveys, the district remained one of the most violent in the country.

²⁶All logit specifications estimated use only the most recent survey each household completes.

on school attendance, but rather that after 4 years of a conflict in which there is broad economic suffering in locations that are not necessarily affected by violence (e.g., Tandon and Vishwanath 2020), school attendance does not vary in response to additional changes in violence in the district of residence.

However, it is possible that other factors correlated with violence or shocks causing violence and school attendance to co-move are obscuring the relationship. Furthermore, another reason that there is potentially little correlation between school attendance and the location violence is that other conflict-related shocks are more important to school attendance in this context. Investigating these possibilities, we further investigate the impact of sudden conflict-related shocks on school attendance using the WFP mobile phone survey. In particular, we investigate the impact each of the four conflict-related shocks described above on school attendance—three of them are sudden and unexpected changes in the severity of violence, and another was caused by the conflict but was not related to any specific violent incidents.

Section 5. Response of Attendance to Each Type of Conflict-Related Shock

The basic empirical strategy is illustrated in figure 4, which presents the evolution of school attendance in regions affected by each of the four shocks and indicates when the shock occurred. We estimate the change in school attendance immediately after the shock happened in the affected regions and compare this change to the change at the same time in all the districts of Yemen that were unaffected by any of the four major shocks analyzed.

The first three panels of figure 4 demonstrate that there was little change in the share of households where all school-aged children attended school following the secessionist capture of regions in the south from the IRG, the onset of the partial ceasefire,²⁷ or the capture of Al Jawf from the IRG by DFA forces. However, the fourth panel illustrates how school attendance significantly worsened following the beginning of the teacher strike in the governorates fully controlled by the IRG.²⁸

Formalizing this comparison between shock-affected and unaffected districts, we estimate the following difference-in-difference linear probability model for each shock²⁹:

$$(2) \quad \text{Poor-School-Attend}_{hgdmy} = \psi_h + \tau_{my} + \rho_g + \gamma \text{Post}_{gdmy} \quad \text{Shock Region}_{gdmy} \\ + \beta \text{Post}_{gdmy} + \rho \text{Shock Region}_{gdmy} + \alpha X_{hgdmy} + \epsilon_{hgdmy}$$

where all variables are as above, *Post* denotes an indicator if the household was surveyed

²⁷There appears to be an increase in the share of children not attending school following the reduction in violence from the partial ceasefire, which is the opposite sign that one might expect based on evidence from other settings. However, the timing of this shock coincides with the beginning of the school year and the increase in the share of households with children not attending school at the beginning of the school year is qualitatively identical to the change observed in districts with no conflict-related shock.

²⁸Figure 4a does not report attendance for the months of January and February because all districts were significantly affected by the teacher strike, which is reported in figure 4c.

²⁹Given that the dependent variable is binary, we also estimate a logit specification for each shock using only the most recent survey each household completed.

during the shock,³⁰ and *Shock Region* denotes an indicator equaling one if households were in districts affected by the shock. The samples are restricted to regions affected by each individual shock analyzed and households from the rest of Yemen that was not affected by any of the four major shocks analyzed.³¹ Furthermore, the sample is restricted to the months prior to the shock, the months during the shock, and if possible, the month following the shock.

Estimates of γ represent how much more the share of households with poor school attendance increased in regions affected by the shock than in districts of Yemen that did not experience any of major shocks over the entire time period. A positive estimate of γ would be consistent with the shock having an adverse impact on school attendance. Additionally, specifications are reported that estimate month-to-month differences between shock-affected regions and districts not impacted by any of the shocks to better illustrate whether school attendance was trending differently in the two regions leading up to the shock, and to further identify the persistence of the shock if post-shock rounds are available.

Section 5a. Impacts of Shocks to the Severity of Violence

Estimates of specification (2) for each of the shocks to the severity of violence are presented in Table 3. Consistent with the patterns presented in figure 4 and with the estimates from table 2, there is little evidence of a significant change in school attendance in any of the regions affected by the sudden and large changes in violence.

First, columns (1)-(3) demonstrate that there is little evidence of there being a large decline in school attendance following the capture of the southern capital by secessionist forces.³² Column (1) demonstrates that there was little difference in school attendance following the capture of the southern capitals by secessionists relative to the prior school year. The estimate in column (1) is negative and small in magnitude. Importantly, the estimate in column (1) also bounds the possible decline in school attendance, where using the upper bound of the 95 percent confidence interval, one can reject the hypothesis that the decline in school attendance was larger than 7.0 percentage points at standard significance levels.

The lack of a significant change in school attendance survives a number of different specifications. Column (2) demonstrates the estimates are qualitatively identical when using a logit specification; and column (3) demonstrates that when breaking up the pre- and post-periods by month, school attendance was not trending prior to the capture and there was little change either during the capture or in the month-and-a-half following the transition back to the IRG.

Second, columns (4)-(6) also demonstrate that there is very little evidence of a large decline in school attendance following the partial ceasefire.³³ Similar to the secessionist capture, the

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³⁰For the shocks for which there are completed surveys following the resolution of the shock, households are denoted as non-treated.

³¹There are 5,967 households that completed a total of 11,200 surveys across 166 districts (of a total of 333 in the country) that were not affected by any of the four major shocks described above.

³²The sample for estimating the impact of the secessionist capture includes all surveys completed during the school year between April 2019 and November 2019. The sample stops short of the later months given the onset of the teacher strike in the regions captured by secessionist forces in February 2020.

³³The sample for estimating the impact of the partial ceasefire includes all surveys completed during the school year between April 2019 and February 2020.

change in school attendance following the ceasefire was small in magnitude and precludes a large decline. Using the upper bound of the 95 percent confidence interval, one can reject the hypothesis that the decline in school attendance was larger than 4.3 percentage points at standard significance levels.

The lack of a significant change in school attendance following the ceasefire also survives a number of different specifications. Column (5) demonstrates the estimates are qualitatively identical when using a logit specification; and column (6) demonstrates that when breaking up the pre- and post-periods by month, school attendance was not trending prior to the ceasefire, did not change during the ceasefire, and also did not change after the ceasefire fell apart.³⁴

And third, we demonstrate a qualitatively identical pattern with the capture of Al Jawf from the IRG by DFA forces in column (7)-(9).³⁵ Despite the rapid increase in violence in the region described in figure 2, there is little evidence suggesting that there was a large impact on school attendance. Column (7) demonstrates that there was little difference in school attendance following the capture. Although the estimate in column (7) is small in magnitude, it is also imprecisely estimated and cannot rule out larger impacts on school attendance.

The lack of a significant change in school attendance in Al Jawf also survives different specifications. Column (8) demonstrates the estimates are qualitatively identical when using a logit specification with an imprecisely estimated but positive impact; and column (9) demonstrates that when breaking up the pre- and post-periods by month, school attendance was not trending prior to the capture and there was little change either during the capture or in the month-and-a-half following the transition back to the IRG. Importantly, estimates in column (9) also demonstrate that there was little change in school attendance following a limited ceasefire signed in late September that led to significantly less violence in the governorate between October and January, which further corroborate estimates in columns (4)-(6).

Although all results in table 3 are consistent with the summary statistics of school attendance presented in figure 4 and consistent with the lack of a relationship between the severity of violence and school attendance presented in table 2, the lack of a change in school attendance following any of these sharp conflict events is surprising. Both the secessionist capture and the capture of Al Jawf led to significant increases in violence and a change in governance, each of which could significantly impact attendance. Furthermore, news and humanitarian sources emphasized the negative impact each event had on the population (e.g., Al Jazeera 2020; OCHA 2020; etc.). Alternatively, the partial ceasefire led to decline in violence was large enough that the total amount of violence in the country followed a nearly identical pattern, and some of the most violent regions in the country were much calmer for the first half of the 2019-2020 school year (figure 2). However, despite the significant changes in violence when the partial ceasefire went into effect and when it broke down, there were no changes in school attendance.

Additionally, these results demonstrate that the impact of sudden changes in violence is

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³⁴In results not shown, the lack of a change is identical in all four governorates affected by the ceasefire.

³⁵The sample for estimating the impact of the capture of Al Jawf includes all surveys completed during the school year between September 2019 and February 2020.

similar in regions that have had very different experiences with violence during the conflict. For example, more violent regions might have more school buildings closed by previous fighting and some of the students that might have been affected by the violence might have already been taken out of school; or households might be less affected by changes in violence if they have experienced more of it over the course of the conflict. However, the results demonstrate that the sudden change in violence did not affect attendance in regions that have experienced relatively little violence over the course of the conflict (e.g., Aden) and also did not affect attendance in regions that have experienced some of the most intense violence (e.g., Sa'ada; etc.).

Importantly, although the shocks to violence analyzed here had little effect on school attendance, there is evidence that at least one of these shocks did significantly impact households. Table 4 re-estimates the month-to-month changes estimated in table 3 (columns 3, 6, and 9), but for each shock the table uses the household's subjective assessment of safety and primary sources of income as the dependent variables.³⁶

Although not all shocks had a significant impact on households, the capture of regions by secessionist forces did have an immediate impact on subjective well-being and sources of income that lasted throughout the capture. Columns (1)-(3) demonstrate that in the districts captured by secessionists, there was a significant increase in the share of households that reported the primary difficulty that they had faced in the past month was violence; there was a significant decrease in the share of households that reported receiving their primary income from wage labor; and there was a significant increase in the share of households that reported having no income. Importantly, all of these impacts only began in the month that the regions were captured.

However, columns (4)-(9) demonstrate that the other two shocks- the partial ceasefire and the capture of Al Jawf- did not have similar impacts on households. In the regions that were affected by these shocks, there was little evidence that there was any change to households' primary income source or subjective well-being. Only in one of the specifications can one reject the hypothesis that there was no change in the months leading up to and following the shocks (change in share reporting wage labor was primary income source in Al Jawf).

Section 5b. Teacher Strike in IRG-Held Territory

Estimates of specification (2) following the teacher strike in regions controlled by the IRG are presented in table 5.³⁷ Despite the lack of change in any measure of violence described in figure 2, the teacher strike in response to conflict-related issues had a large impact on school attendance. Column (1) demonstrates that there was a 19.5 percentage point increase in the share of households in which not all children attended school prior to before the strike. This change is a more than doubling of the share of households with poor school attendance prior

³⁶Given the non-schooling outcomes analyzed, the specifications also include summer months when children were not attending school.

³⁷The sample for estimating the impact of the teacher strike includes all surveys completed during the school year between September 2019 and February 2020.

to the strike.

The large change in attendance survives a number of different specifications. Column (2) demonstrates the estimates are qualitatively identical when using a logit specification. Additionally, column (3) demonstrates that when breaking up the pre- and post-periods by month, school attendance was not trending prior to the strike, and the large decline in attendance only occurred after the strike began in February.

However, as one might expect given the difficult governance in the country and the possibility that many teachers might not want to impact students by joining the strike, the strike was not uniform across all regions controlled by the IRG. As discussed in the data section, there were two governorates where it was reported that the entire governorate was not taking part in the strike. When estimating the impact excluding these two governorates where it was reported that not all districts were participating in column (4), the magnitude and the precision of the estimate both increase. And when estimating the impact of the strike in the two governorates with only partial teacher strikes in column (5) and dropping all other striking regions from the specification, the estimates are less precise and smaller in magnitude.³⁸

Furthermore, there is evidence consistent with the strike being the driving factor behind the increase in poor school attendance. Columns (6)-(8) of table 5 re-estimates specifications estimated in column (3) but uses the same non-school outcomes as in table 4. None of the indicators of employment, subjective assessments of difficulties, or indicators of welfare changed following the strike. This is consistent with the change in school attendance not being driven by other factors that potentially affect other aspects of life beyond school attendance.³⁹

The strong impact of a teacher strike is surprising in the current context. First, as discussed above, the event was not extensively reported on in the international media or by humanitarian organizations. Rather, only careful monitoring of Yemeni news agencies and key informant interviews by the World Bank uncovered the strike and its extent. Second, given the emphasis of the conflict literature on the impacts of the severity of violence and the significant reporting on the poor access to schooling in DFA-held regions (e.g., Majidyar 2018; World Bank 2020b; etc.), it is unexpected that such a large shock could affect regions where schooling was supposed to be better. And lastly, it is likely difficult for teachers to plan and execute a broad and effective strike in a volatile conflict context.

However, the estimates are consistent with the summary statistics of school attendance presented in figure 4. Furthermore, the importance of shocks aside from the location of violence is one of the explanations for why there is little relationship reported in table 2. By the end of the time period analyzed, figure 5 demonstrates that the share of households with poor

³⁸These patterns are identical when including a triple interaction term in the baseline specification.

³⁹There are some coefficients that are borderline significant in table 5 in the specification using subjective feelings of safety as the dependent variable (column 6). Although not jointly significant, these borderline significant coefficients are being driven by the fact that the base period is September, and some of the regions affected by the teacher strike were in the midst of the secessionist capture in that month and there was a spike in the dependent variable in September and October. Consistent with these experiences subsiding after the secessionist capture was over, the borderline significant coefficients demonstrate a decline in people citing violence as their most difficult challenge as things got back to normal in the regions affected by the secessionist capture.

school attendance was actually higher in regions controlled exclusively by the IRG than in regions where the IRG controlled part of the governorate (the regions on the front battle lines with the most violence) and regions controlled exclusively by DFA forces (which also had more violence on average than IRG-held regions). However, attendance in IRG-controlled regions only became worse than more conflict-affected areas after the beginning of the teacher strike.

In addition to demonstrating the strike had a strong impact on school attendance, we further compare the impact of the strike to the impacts of the shocks to the severity of violence described above. Table 6 estimates the following variant of specification (2):

(3)

$$Poor\_School\_Attend_{hgdmj} = \psi_h + \tau_{mj} + \rho_g + \sum_j [\gamma^j During\_Shock^j_{gdmj} Shock\_Region^j_{gdmj} + \beta_j During\_Shock^j_{gdmj} + \rho_j Shock\_Region^j_{gdmj}] + \alpha X_{hgdmj} + \epsilon_{hgdmj}$$

where all variables are as above, j denotes the four shocks to school attendance, and *During Shock* ^{j} is an indicator equaling one if shock j was occurring during month m and year y . The specifications include all surveys over the course of the 12 months conducted during the school year and separately estimates the impact of each shock.

In all specifications, the impact of the teacher strike continues to have the largest impact on school attendance and is the most precisely estimated coefficient. Columns (1)-(4) estimate the impact each shock has individually using the entire sample as a comparison group; and columns (5)-(7) estimate the full specification comparing the impacts of each of the shocks. Columns (1)-(4) again demonstrate that only the strike has a large and unambiguous impact on school attendance, and that one cannot reject the hypothesis that the shocks to the severity of violence had no impact on school attendance at standard significance levels.

When comparing the impact of each shock directly in column (5), one can reject the hypothesis that the impacts of all three shocks were equal at conventional significance levels (p-value of 0.016). The results in column (5) - the stronger and more precisely estimated impact of the strike, and the statistical difference between each of the four shocks having an equal impact - survive a number of different specifications. Column (6) demonstrates the same pattern using a logit specification; and when dropping governorates where it was reported that not all districts joined the strike in column (7), the magnitude and the precision of the estimated impact of the strike increases.

Section 6. Example Illustrating the Importance of Incorporating All Conflict Shocks

As has been demonstrated above, oftentimes active conflict is a tumultuous environment that impacts access to education through many different channels. However, the results described above have two separate implications for understanding the relationship between conflict and education more generally. First, one of the many reasons why the impact of living in close proximity to violence has on education might vary significantly across contexts is the fact that there are conflicts in which the relationship might not be universal over the entire

course of the conflict.

The lack of the relationship between the location of violence and poor attendance is independent of the strike that occurred in regions controlled by the IRG. Column (1) of table 7 re-estimates the relationship between the share of households with poor school attendance and the number of conflict fatalities in the district of residence (specification 1) but estimates the impact of violence separately for each month of the sample. One cannot reject the hypothesis that there is no relationship in the base month (March 2019), and one cannot reject the hypothesis that there is no difference in the relationship in the base month and any other month in the sample (individually or jointly). Thus, consistent with the lack of a change in schooling due to sudden changes in violence described in table 3, the onset of the strike does not appear to be obscuring a relationship between violence and poor school attendance in this setting.

Second, this setting offers another potential implication for the relationship between conflict and access to education. In a variety of other settings, there is not a relationship between living in close proximity to violence and access to education for all children. Rather, many articles report that the severity of certain types of conflict violence might affect children, or conflict might affect girls but not boys, or older children and not younger children (e.g., Chamarbagwala and Morn 2010; Shemyakina 2011; Justino et al. 2014; Swee 2015; Monterio and Rocha 2017; etc.).

However, without a thorough understanding of all the conflict-related shocks aside from the severity of violence, it might be difficult to make strong inferences. Demonstrating this point in more detail, table 7 re-estimates the specification reported in column (1) but breaks up conflict into all violence and violence involving the largest terrorist group in the country, Al-Qaeda in the Arabian Peninsula (AQAP).

Column (2) demonstrates that one cannot reject the hypothesis that there was a strong relationship that emerged between violence involving AQAP in February 2020. However, AQAP is only active and holding territory in regions controlled by the IRG (e.g., Amnesty International 2019), and the relationship only emerged after the teacher strikes in IRG-held regions began. Furthermore, only when non-IRG territory is included as a comparison group, where there are no fatalities involving AQAP and where there is not a teacher strike, does this relationship emerge.

Thus, without detailed knowledge of events on the ground, one might mistakenly infer that there could be an adverse relationship between particular types of violence (terrorist groups in this case) and poor school attendance. This problem would be exacerbated if the COVID-19 pandemic did not cause all schools to shut down in the country and there were more post-strike rounds to analyze. In such a setting, an average relationship between violence involving AQAP across all survey rounds could appear. Furthermore, this issue in the current setting would not necessarily be solved by looking for plausibly exogenous increases in terrorist violence given the regions affected would again only be those that are experiencing the teacher strike.

Importantly, these non-conflict issues could also potentially make inference for different age groups and different genders difficult. For example, the IRG has been providing some school

materials only to younger grades, and humanitarian agencies have been providing some gender-specific interventions to some regions more than others (e.g., OCHA 2019; World Bank 2020b; etc.). Although the education interventions in this setting are not extensive and there were no reported disruptions to these operations during the time period under analysis, disruptions to education interventions in other settings could mistakenly be attributed to the severity of violence.⁴⁰

Section 7. Conclusion

We demonstrated how a series of sharp, conflict-related incidents impacted school attendance in one of the most volatile and protracted conflicts in the world. There is little evidence that living in close proximity to violence- either the onset of sharp and unexpected violent campaigns, or the severity of all violence- had strong impacts on school attendance. Rather, we demonstrated that other conflict-related issues that affect the country more broadly than the location of violence can have a stronger impact on school attendance. Combined, these results suggest that targeting of limited resources for education should also consider factors other than the location of violence.

However, there are a number of issues unexplored by this analysis. First, the analysis focuses on a measure of school attendance and there are a number of other issues that impact the quality of schooling that cannot be analyzed in this setting. For example, it might still be the case that children are attending school in more violent regions, but the quality of schooling is still worse in those regions than the schooling received in less violent regions. However, in the current environment, it is more difficult to capture measures of the quality of schooling using available data and this is an issue that needs to be addressed in future work.

Second, these findings are representative of the mobile phone-using population, and segments of the population that potentially have worse access to mobile phones might be underrepresented. It is possible that the impacts of violence could be different for children at different points of the welfare distribution, and these results might not generalize to those underrepresented populations. However, given the inability of conducting traditional face-to-face surveys in the current environment, it is difficult to incorporate these populations directly. There are some attempts at performing key informant interviews (e.g., OCHA 2019a; World Bank 2020b; etc.), but more alternative survey methodologies need to be utilized to better understand the challenges facing these populations.

And lastly, it is important to emphasize that these results do not imply that violence is unimportant to school attendance in Yemen. Rather, these findings suggest that marginal and temporary changes in the severity of violence after prolonged conflict do not have a direct impact on school attendance. It is possible that the households that are most sensitive to violence had already taken their children out of school before the survey period, or it is possible that households will not let their children attend school until they feel the decline in violence is permanent and the conflict is over. Thus, it is possible that interventions that reduce

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⁴⁰That is not to say that violence could not also cause a disruption to these services. But that it is possible that non-violent conflict related shocks could also be responsible.

the distance children travel to school or other interventions that make it safer for children to attend school might indeed have a positive impact on attendance rates. However, given the lower attendance rates in the least violent regions at the end of the survey period analyzed here, these results suggest that it is not necessarily true that interventions aimed at more violent regions would have a larger impact on attendance than other types of interventions aimed at improving schools in less violent regions (e.g., increases in teacher salaries, provision of school materials, etc.).

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Table 1. Change in Respondent Characteristics and Access to Mobile Phones Following Shocks

	Secessionist Capture			Partial Ceasefire			Al Jawf Capture			Teacher Strike		
	Month Before Shock	Month After Shock	Difference	Month Before Shock	Month After Shock	Difference	Month Before Shock	Month After Shock	Difference	Month Before Shock	Month After Shock	Difference
Share of Households with Male Heads	0.967	0.953	-0.014	0.965	0.970	0.005	0.983	0.983	0.000	0.969	0.966	-0.003
	[0.009]	[0.011]	[0.014]	[0.005]	[0.003]	[0.006]	[0.008]	[0.012]	[0.015]	[0.005]	[0.007]	[0.009]
Average Respondent Age	41.306	40.226	-1.080	38.001	38.477	0.476	38.749	39.052	0.303	39.44	40.851	1.411**
	[0.618]	[0.563]	[0.836]	[0.325]	[0.233]	[0.400]	[0.817]	[1.133]	[1.396]	[0.336]	[0.490]	[0.594]
Average Number of Mobile Phones	2.301	2.388	0.087	2.150	2.101	-0.050	2.315	2.138	-0.177	2.111	2.100	-0.011
	[0.075]	[0.088]	[0.116]	[0.047]	[0.032]	[0.057]	[0.096]	[0.142]	[0.171]	[0.045]	[0.075]	[0.088]
Household Size	8.38	8.465	0.085	10.350	10.344	-0.006	12.030	12.474	0.444	9.312	9.269	-0.043
	[0.174]	[0.202]	[0.267]	[0.156]	[0.101]	[0.186]	[0.382]	[0.618]	[0.726]	[0.136]	[0.196]	[0.238]
P-value from a joint test of significance	-	-	0.518	-	-	0.769	-	-	0.202	-	-	0.615
Observations	389	402	791	1217	2390	3607	235	116	351	1248	610	1858

Notes: This table reports summary statistics of respondent characteristics and access to mobile phones in the months before each shock and the month during each shock in the affected regions. For each shock, the difference between the pre- and post- characteristics are reported; and for each shock, p-values from a joint test of significance are reported as well (regression of post indicator on each variable, sample restricted to pre- and post-months). For the secessionist capture, months before are June-July 2019 and the months during are August-September 2019; for the ceasefire, months before are July-August 2019 and the months during are September-December 2019; for the capture of Al Jawf, the months before are December 2019-January 2020 and the month during is February 2020; and for the strike, the months before are December 2019-January 2020 and the month during is February 2020. Robust standard errors reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Table 2. Correlation Between Conflict Intensity and Poor School Attendance

	Dependent Variable:								
	Indicator Equaling One if Not All School-Aged Children in the Household Attended School in the Past Month								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Indicator for Any Fatality in Survey Month	0.016 [0.015]	-	-	-	-	-	-	-	-
ln(Incidents _t)	-	0.004 [0.010]	-	-	-	-	-	-	-
ln(Fatalities _t)	-	-	0.004 [0.006]	0.036 [0.030]	0.006 [0.007]	-0.000 [0.017]	0.004 [0.006]	0.014 [0.012]	0.006 [0.006]
ln(Fatalities _{t-1})	-	-	-	-	-0.007 [0.006]	-	-	-	-
ln(Fatalities _{t-2})	-	-	-	-	0.008 [0.006]	-	-	-	-
ln(Fatalities _{t-3})	-	-	-	-	-0.004 [0.007]	-	-	-	-
ln(Fatalities _{t-4})	-	-	-	-	-0.006 [0.006]	-	-	-	-
ln(Fatalities _{t-5})	-	-	-	-	0.004 [0.007]	-	-	-	-
ln(Fatalities _{t+1})	-	-	-	-	0.001 [0.008]	-	-	-	-
ln(Fatalities _{t+2})	-	-	-	-	-0.001 [0.006]	-	-	-	-
ln(Fatalities _{t+3})	-	-	-	-	-0.003 [0.006]	-	-	-	-
ln(Fatalities _{t+4})	-	-	-	-	-0.007 [0.006]	-	-	-	-
ln(Fatalities _{t+5})	-	-	-	-	0.002 [0.008]	-	-	-	-
ln(Fatalities from Remote Violence _t)	-	-	-	-	-	-0.004 [0.013]	-	-	-
ln(Fatalities from Violence Against Civilians _t)	-	-	-	-	-	0.037 [0.030]	-	-	-
ln(Fatalities from Ground Battles _t)	-	-	-	-	-	0.008 [0.015]	-	-	-
Taiz Indicator x ln(Fatalities _t)						-	0.021 [0.053]		
ln(Fatalities Involving AQAP _t)	-	-	-	-	-	-	-	-0.008 [0.009]	-
DFA Indicator x ln(Fatalities _t)	-	-	-	-	-	-		-	-0.017 [0.021]
Logit Specification	N	N	N	Y	N	N	N	N	N
P-value of a test of all violence coefficients jointly equaling zero	0.286	0.694	0.508	0.240	0.740	0.558	0.734	0.503	0.558
Observations	22,071	22,071	22,071	11,651	22,071	22,071	22,071	22,071	22,071

Notes: This table reports the correlation between violence intensity in the district in which the household resides and an indicator for whether all school-aged children attended school in the past month. The sample is restricted to households surveyed during the school year. Household control variables include the gender of the household head, the age of the respondent, and the household size. All OLS specifications include household fixed effects, governorate fixed effects, time fixed effects and household control variables. The Logit specification includes only the most recent time the household was interviewed, governorate indicators, time indicators, and household control variables. Standard errors clustered at the district are reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Table 3. Change in School Attendance Following Shocks to Violence

	Dependent Variable: Indicator Equaling One if All School-Aged Children Did Not Attend School in Past Month								
	Secessionist Capture			Partial Ceasefire			Capture of Al Jawf		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post-Secession Indicator x Secession Indicator	-0.088 [0.080]	0.416 [0.301]	-	-	-	-	-	-	-
May Indicator x Secession Indicator	-	-	-0.022 [0.104]	-	-	-	-	-	-
June Indicator x Secession Indicator	-	-	0.011 [0.086]	-	-	-	-	-	-
September Indicator x Secession Indicator	-	-	-0.056 [0.136]	-	-	-	-	-	-
Pre-Handover October Indicator x Secession Indicator	-	-	-0.158 [0.198]	-	-	-	-	-	-
Post-Handover October Indicator x Secession Indicator	-	-	0.069 [0.124]	-	-	-	-	-	-
November Indicator x Secession Indicator	-	-	-0.045 [0.118]	-	-	-	-	-	-
Post-Ceasefire Indicator x Ceasefire Indicator	-	-	-	-0.007 [0.025]	0.094 [0.135]	-	-	-	-
May Indicator x Ceasefire Indicator	-	-	-	-	-	-0.047 [0.065]	-	-	-
June Indicator x Ceasefire Indicator	-	-	-	-	-	-0.050 [0.061]	-	-	-
September Indicator x Ceasefire Indicator	-	-	-	-	-	-0.009 [0.075]	-	-	-
October Indicator x Ceasefire Indicator	-	-	-	-	-	-0.034 [0.057]	-	-	-
November Indicator x Ceasefire Indicator	-	-	-	-	-	-0.050 [0.070]	-	-	-
December Indicator x Ceasefire Indicator	-	-	-	-	-	-0.053 [0.066]	-	-	-
January Indicator x Ceasefire Indicator	-	-	-	-	-	-0.064 [0.067]	-	-	-
February Indicator x Ceasefire Indicator	-	-	-	-	-	-0.031 [0.070]	-	-	-
Post-Al Jawf Capture Indicator x Al Jawf Indicator	-	-	-	-	-	-	0.059 [0.162]	0.711 [0.514]	-
October Indicator x Al Jawf Indicator	-	-	-	-	-	-	-	-	-0.114 [0.134]
November Indicator x Al Jawf Indicator	-	-	-	-	-	-	-	-	-0.018 [0.095]
December Indicator x Al Jawf Indicator	-	-	-	-	-	-	-	-	-0.037 [0.124]
January Indicator x Al Jawf Indicator	-	-	-	-	-	-	-	-	-0.027 [0.134]
February Indicator x Al Jawf Indicator	-	-	-	-	-	-	-	-	0.014 [0.153]
P-value from test of all higher-order terms jointly equaling zero	0.274	0.167	0.581	0.786	0.489	0.986	0.718	0.167	0.422
Logit Specification	N	Y	N	N	Y	N	N	Y	N
Observations	7,517	5,414	7,517	14,806	7,830	14,806	7,049	3,474	7,049

Notes: This table reports by how much more school attendance changed in districts affected by shocks to violence from the conflict relative to districts unaffected by the shocks. Columns (1)-(3) estimate the change in response to the capture of Aden and the capture of the capitals of two neighboring governorates by secessionist in August 2019; columns (4)-(6) estimate the change in response to the implementation of a partial ceasefire during September 2019; and columns (7)-(9) estimate the change in Al Jawf as de facto authority forces in the north captured the governorate from the internationally-recognized government. The sample is restricted to households surveyed during the school year, and includes the months immediately prior to the changes and the months during and after (if available). All OLS specifications include household fixed effects, governorate fixed effects, time fixed effects and household control variables. Household control variables include the gender of the household head, the age of the respondent, and the household size. The Logit specification includes only the most recent time the household was interviewed, governorate indicators, time indicators, and household control variables. Standard errors clustered at the district are reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Table 4. Non-Schooling Changes Following Shocks to Violence

	Secessionist Capture			Partial Ceasefire			Al Jawf Capture		
	Indicator for Primary Difficulty Faced Being Violence	Indicator for Primary Income from Wage Labor	Indicator for No Sources of Income	Indicator for Primary Difficulty Faced Being Violence	Indicator for Primary Income from Wage Labor	Indicator for No Sources of Income	Indicator for Primary Difficulty Faced Being Violence	Indicator for Primary Income from Wage Labor	Indicator for No Sources of Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
May Indicator x Secession Indicator	-0.030 [0.027]	-0.093 [0.064]	-0.026 [0.064]	-	-	-	-	-	-
June Indicator x Secession Indicator	-0.014 [0.021]	-0.062 [0.076]	-0.020 [0.055]	-	-	-	-	-	-
July Indicator x Secession Indicator	-0.006 [0.024]	-0.039 [0.047]	0.014 [0.050]						
August Indicator x Secession Indicator	0.098** [0.038]	-0.088 [0.083]	0.009 [0.052]						
September Indicator x Secession Indicator	0.115** [0.049]	-0.140** [0.067]	0.090* [0.049]	-	-	-	-	-	-
Pre-Handover October Indicator x Secession Indicator	-0.017 [0.030]	-0.089 [0.119]	0.026 [0.068]	-	-	-	-	-	-
Post-Handover October Indicator x Secession Indicator	0.025 [0.034]	-0.176*** [0.067]	0.083* [0.046]	-	-	-	-	-	-
November Indicator x Secession Indicator	-0.008 [0.030]	-0.154 [0.097]	0.018 [0.087]	-	-	-	-	-	-
May Indicator x Ceasefire Indicator	-	-	-	-0.047 [0.030]	0.034 [0.042]	0.000 [0.045]	-	-	-
June Indicator x Ceasefire Indicator	-	-	-	-0.031 [0.027]	-0.005 [0.051]	0.014 [0.038]	-	-	-
July Indicator x Ceasefire Indicator				-0.016 [0.019]	0.037 [0.053]	0.015 [0.036]			
August Indicator x Ceasefire Indicator				-0.040 [0.027]	0.020 [0.045]	-0.005 [0.040]			
September Indicator x Ceasefire Indicator	-	-	-	-0.036* [0.021]	-0.020 [0.045]	0.056* [0.032]	-	-	-
October Indicator x Ceasefire Indicator	-	-	-	-0.025 [0.029]	-0.010 [0.051]	0.037 [0.036]	-	-	-
November Indicator x Ceasefire Indicator	-	-	-	-0.019 [0.023]	-0.015 [0.049]	0.034 [0.035]	-	-	-
December Indicator x Ceasefire Indicator	-	-	-	-0.039 [0.026]	-0.013 [0.049]	0.009 [0.046]	-	-	-
January Indicator x Ceasefire Indicator	-	-	-	-0.034* [0.019]	-0.013 [0.056]	0.025 [0.041]	-	-	-
February Indicator x Ceasefire Indicator	-	-	-	-0.028 [0.024]	0.040 [0.052]	-0.003 [0.045]	-	-	-
October Indicator x Al Jawf Indicator	-	-	-	-	-	-	0.009 [0.055]	-0.009 [0.114]	-0.003 [0.047]
November Indicator x Al Jawf Indicator	-	-	-	-	-	-	0.013 [0.057]	0.024 [0.109]	0.025 [0.026]
December Indicator x Al Jawf Indicator	-	-	-	-	-	-	-0.025 [0.051]	0.087 [0.111]	-0.030 [0.056]
January Indicator x Al Jawf Indicator	-	-	-	-	-	-	0.001 [0.020]	0.189* [0.100]	-0.068 [0.056]
February Indicator x Al Jawf Indicator	-	-	-	-	-	-	0.049 [0.114]	0.229 [0.230]	-0.126 [0.143]
P-value from test of all higher-order terms prior to shock jointly equaling zero	0.000***	0.003***	0.089*	0.783	0.853	0.629	0.884	0.000***	0.482
Observations	11,472	11,472	11,472	20,095	20,095	20,095	8,012	8,012	8,012

Notes: This table reports by how employment indicators and subjective well-being indicators of safety changed in districts affected by shocks to violence from the conflict relative to districts unaffected by the shocks. Columns (1)-(3) estimate the change in response to the capture of Aden and the capture of the capitals of two neighboring governorates by secessionist in August 2019; columns (4)-(6) estimate the change in response to the implementation of a partial ceasefire during September 2019; and columns (7)-(9) estimate the change in Al Jawf as de facto authority forces in the north captured the governorate from the internationally-recognized government. The sample includes the months immediately prior to the changes and the months during and after (if available). All OLS specifications include household fixed effects, governorate fixed effects, time fixed effects and household control variables. Household control variables include the gender of the household head, the age of the respondent, and the household size. The Logit specification includes only the most recent time the household was interviewed, governorate indicators, time indicators, and household control variables. Standard errors clustered at the district are reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Table 5. Change in School Attendance and Non-Attendance Outcomes Following the Teacher Strike in Regions Controlled by the Internationally-Recognized Government

	Dependent Variable:							
	Indicator Equaling One if All School-Aged Children Did Not Attend School in Past Month					Indicator for Primary Difficulty Faced Being Violence	Indicator for Primary Income from Wage Labor	Indicator for No Sources of Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Indicator x Strike Indicator	0.195*** [0.066]	1.216*** [0.219]	-	-	-	-	-	-
October Indicator x Strike Indicator	-	-	0.010 [0.069]	0.024 [0.080]	-0.008 [0.086]	-0.054* [0.032]	-0.036 [0.050]	-0.006 [0.043]
November Indicator x Strike Indicator	-	-	0.036 [0.055]	0.022 [0.058]	0.063 [0.076]	-0.060* [0.036]	0.010 [0.048]	-0.022 [0.038]
December Indicator x Strike Indicator	-	-	0.064 [0.069]	0.076 [0.086]	0.042 [0.065]	-0.048 [0.042]	0.032 [0.052]	-0.054 [0.043]
January Indicator x Strike Indicator	-	-	0.055 [0.056]	0.089 [0.072]	-0.009 [0.060]	-0.065* [0.036]	0.005 [0.057]	-0.047 [0.045]
February Indicator x Strike Indicator	-	-	0.236*** [0.082]	0.314*** [0.083]	0.091 [0.077]	-0.051 [0.041]	0.001 [0.056]	-0.000 [0.045]
P-value from test of all higher-order terms jointly equaling zero	0.004***	0.000***	0.023**	0.000***	0.434	0.229	0.718	0.514
Logit Specification	N	Y	N	N	N	N	N	N
Regions without Reports of Partial Strike Dropped from the Sample	N	N	N	Y	N	N	N	N
Regions where Strike is Not Reported Partial Dropped from the Sample	N	N	N	N	Y	N	N	N
Observations	9,560	4,291	9,560	8,500	7,475	11,079	11,079	11,079

Notes: This table reports by how much more school attendance and non-attendance outcomes changed in regions affected by the large teacher strike relative to the rest of Yemen unaffected by any of the four main shocks analyzed. The strike began in February 2020 and continued through the end of the survey period analyzed here. The sample analyzing school attendance is restricted to households surveyed during the 2019-2020 school year, and includes surveys conducted between September 15, 2019 and February 29, 2020. The sample analyzing non-attendance outcomes includes all surveys conducted between September 1, 2019 and February 29, 2020. Household control variables include the gender of the household head, the age of the respondent, and the household size. All OLS specifications include household fixed effects, governorate fixed effects, time fixed effects and household control variables. The Logit specification includes only the most recent time the household was interviewed, governorate indicators, time indicators, and household control variables. Standard errors clustered at the district are reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Table 6. Impacts of All Education Shocks on School Attendance

	Dependent Variable: Indicator Equaling One if All School-Aged Children Did Not Attend School in Past Month						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Secession Time Indicator x Secession Districts Indicator	-0.067 [0.061]	-	-	-	-0.051 [0.060]	-0.099 [0.326]	-0.084* [0.050]
Ceasefire Time Indicator x Ceasefire Districts Indicator	-	0.008 [0.021]	-	-	-0.002 [0.021]	0.045 [0.110]	-0.002 [0.022]
February Indicator x Al Jawf Indicator	-	-	0.076 [0.092]	-	0.124 [0.091]	0.840* [0.446]	0.124 [0.091]
February Indicator x Strike Indicator	-	-	-	0.180***	0.185***	1.364***	0.255***
P-value from test of all higher-order terms being equal	-	-	-	-	0.016**	0.000***	0.000***
Logit Specification	N	N	N	N	N	Y	N
Regions without Reports of Partial Strike Dropped from the Sample	N	N	N	N	N	N	Y
Observations	22,071	22,071	22,071	22,071	22,071	11,683	20,208

Notes: This table reports by how much more school attendance changed in regions affected by each of the large education shocks during the survey period. All surveys during the school year between March 2019 and February 2020 are included in the main sample. The secessionist capture began at the beginning of August and ended on October 15, 2019; the partial ceasefire began during September and broke down during the beginning of January; the capture of Al Jawf began during February and continued through the end of the survey period; and the teacher strike began in February 2020 and continued through the end of the survey period analyzed here. Household control variables include the gender of the household head, the age of the respondent, and the household size. All OLS specifications include household fixed effects, governorate fixed effects, time fixed effects and household control variables. The Logit specification includes only the first time the household was interviewed, governorate indicators, time indicators, and household control variables. Standard errors clustered at the district are reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Table 7. Illustrative Example of How Other Conflict-Related Shocks Can Obscure the Relationship Between Violence and Access to Education

	Dependent Variable: Indicator Equaling One if Not All School-Aged Children in the Household Attended School in the Past Month	
	(1)	(2)
ln(fatalities)	-0.003 [0.013]	0.004 [0.006]
April Indicator x ln(fatalities)	0.010 [0.019]	-
May Indicator x ln(fatalities)	0.008 [0.021]	-
June Indicator x ln(fatalities)	0.005 [0.017]	-
September Indicator x ln(fatalities)	0.009 [0.022]	-
October Indicator x ln(fatalities)	0.001 [0.016]	-
November Indicator x ln(fatalities)	-0.008 [0.017]	-
December Indicator x ln(fatalities)	0.012 [0.015]	-
January Indicator x ln(fatalities)	0.015 [0.014]	-
February Indicator x ln(fatalities)	-0.002 [0.021]	-
ln(fatalities involving AQAP)		0.082 [0.101]
April Indicator x ln(fatalities involving AQAP)		-0.122 [0.104]
May Indicator x ln(fatalities involving AQAP)		-0.029 [0.110]
June Indicator x ln(fatalities involving AQAP)		-0.108 [0.111]
September Indicator x ln(fatalities involving AQAP)		-0.113 [0.105]
November Indicator x ln(fatalities involving AQAP)		-0.062 [0.142]
January Indicator x ln(fatalities involving AQAP)		-0.083 [0.110]
February Indicator x ln(fatalities involving AQAP)		0.321*** [0.102]
P-value of a test of all higher-order coefficients jointly equaling zero	0.906	0.000***
Observations	22,071	22,071

Notes: This table reports the correlation between violence intensity in the district in which the household resides and an indicator for whether all school-aged children attended school in the past month. The sample is restricted to households surveyed during the school year. Household control variables include the gender of the household head, the age of the respondent, and the household size. All specifications include household fixed effects, governorate fixed effects, time fixed effects and household control variables. Standard errors clustered at the district are reported in parentheses; *** denotes statistical significance at the one percent level, ** denotes statistical significance at the five percent level, and * denotes statistical significance at the ten percent level.

Figure 1. Timeline of Conflict-Related Shocks

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Figure 2. Violence Surrounding Each Conflict Related Shock

Figure 3. Summary Statistics of School Attendance

Figure 4. Evolution of Attendance in Shock-Affected Regions

Notes: This figure reports the share of households without all school-aged children attending school in each of the four regions affected by conflict-related shocks. The figure further reports the upper and lower bounds of the 95 percent confidence interval for each estimate.

Figure 5. Share of Households with Poor School Attendance by Regions