alternatif politika

Effects Of Armed Conflict On Educational Outcomes In Turkey

Kaynak: Alternatif Politika, Cilt 6. Sayı 2. (Eylül 2014) Sayfa. 195-253.

Yazar: **Ali BERKER** (Doç. Dr., Abant İzzet Baysal Üniversitesi, İktisat Bölümü)

Alternatif Politika; Worldwide Political Science Abstracts, Scientific Publications Index, Scientific Resources Database, Recent Science Index, Scholarly Journals Index, Directory of Academic Resources, Elite Scientific Journals Archive, Current Index to Scholarly Journals, Digital Journals Database, Academic Papers Database, Contemporary Research Index, Ebscohost, Index Copernicus ve Asos Index'de taranmaktadır.

EFFECTS OF ARMED CONFLICT ON EDUCATIONAL OUTCOMES IN TURKEY[1]

Ali BERKER

ABSTRACT

This study is the first attempt to evaluate the educational consequences of a 30-year-old internal armed conflict between the Turkish state and Kurdish insurgents, one manifestation of Turkey's prolonged Kurdish problem. To tackle the endogeneity of conflict and omitted variable bias, I implement a difference-in-differences estimation method, in which I exploit variations in exposure to conflict across provinces and over time. Estimation results suggest the presence of a robust negative association between exposure to conflict and children's educational outcomes. This estimated negative association appears to be stronger for female children, for upper-secondary school outcomes, and for those who are exposed to conflict for a longer time period. The robustness analyses purport that the conflict's estimated negative impacts are unlikely to be driven by either conflict-induced migration or underlying province-specific trends.

Keywords: **Armed conflict, educational attainment, Turkey.**

[1] I conducted this research during my postdoctoral studies at two institutions: Max Planck Institute for Demographic Research (Germany) and Population Studies Center, University of Michigan (USA). My studies at these institutions were supported by TÜBİTAK-Deutsche ForschungsGemeinschaft (DFG)-2225: European Scientific Exchange Scholarship Program and TÜBİTAK-2219: Postdoctoral Research Schoarship Program. I am grateful Ahmet İçduygu, Rainer Heuer, David Lam, İnsan Tunalı for their support and helpful comments. Any errors are mine.

INTRODUCTION

In the past twenty years there has been a growing research interest in both the causes and consequences of internal armed conflict in economic literature (Blattman and Miguel 2009). One line of research on conflict devotes significant effort to understanding the link between exposure to conflict and human capital outcomes.[2] Studying such a link is important because disruptions in human capital accumulation are unlikely to be confined to the generations exposed to it; future generations are likely to be affected as well. Thus verifying contemporaneous and intergenerational causal impacts of conflict on human capital outcomes could be a critical policy input for stakeholders in conflict-afflicted countries in their efforts to end conflict and design a sustainable and lasting post-conflict period.

This study aims to contribute to this growing literature on the empirical front by estimating the educational consequences of internal armed conflict in Turkey. In the last 30 years, conflict that has been clustered in Turkey's provinces located in the East and Southeast Anatolia Regions[3] has deteriorated the social and development for both these conflict-exposed provinces and Turkey as a whole. Armed conflict occurring between state military forces and Kurdish insurgents, the Kurdistan Workers Party (PKK), resulted in 29,607 individuals killed and 18,644 injured during the 1984-2004 time period (Bila, 2004). Furthermore, 945 village and 2,021 hamlets had been evacuated for security reasons, and, consequently, about one million individuals were forced to leave their places of residence in the East Region (TBMM, 1998; HUNEE, 2006). Armed conflict in Turkey may also have affected human capital outcomes for individuals in the conflict region by worsening conditions that determine the demand and supply of human capital investments. To evaluate whether and to what extent the 30 years of armed conflict in Turkey has altered human capital outcomes, I examine educational outcomes, which have been proved to be most important determinant of individuals' economic and social well-being, as well as that of their offspring.

As observed in Turkey, when examining the educational consequences of armed conflict concentrated in a particular region, several methodological problems must be addressed to estimate the causal impact of conflict. First,

[2] For studies on the impacts of conflict on children's educational outcomes, see: Akbulut-Yüksel (2009), Swee (2009), Chamarbagwala and Moran (2008), Akresh and Walque (2008), Merrouche (2006), and Olga (2006); for studies on its impacts on children's health outcomes, see: Akbulut-Yüksel (2009), Guerrero-Serdan (2009), and Bundervoet et al. (2009).

[3] For the sake of brevity, the East and Southeast Anatolia Regions of Turkey are hereafter referred to as the East Region.

regions are not randomly selected to suffer from the horrors of conflict in the sense that the conflict region may significantly and permanently differ from other regions of the country. Even before the conflict in Turkey, for example, the East Region had been construed as the least developed region, and it featured unorganized and widely scattered settlements, a lower level of urbanization, and a lower level of infrastructure investments for basic needs, such as water, electricity, and transportation; it also had a unique ethnic structure, in that the majority of the population is composed of Kurds. Because these region-specific characteristics may be related simultaneously to both the onset of conflict and lower educational outcomes in the region, failure to account for region-specific characteristics may lead to an omitted variable bias, contaminating the estimates of the conflict's impact on educational outcomes.

Somewhat related to the first problem, the second problem is that the causality between armed conflict and educational outcomes may run in opposite directions. While conflict reduces educational outcomes, lower educational outcomes may also cause the onset of conflict. For example, lower educational outcomes for individuals in the conflict region may hinder their success in the labor market, thereby reducing their opportunity cost of uprising against the central government.[4]

To remedy problems associated with an omitted variable bias and the endogeneity of the conflict's occurrence, I use time and geographical variations in exposure to conflict that has been ongoing since 1984. First, to measure time variations in exposure to conflict, I utilize information from the 1985 Turkish Census to identify 16-20 and 18-20 year-old individuals who were not exposed to conflict. Similarly, using the 1990 and 2000 Turkish Censuses, I extract information for the same age group but who were exposed to conflict. Second, to measure geographical variations in exposure to conflict, provinces in the East Region of Turkey that had been governed by the state emergency law are defined as provinces exposed to conflict (conflict provinces), while the remaining provinces are defined as provinces not exposed to conflict (non-conflict provinces). I then calculate the extent to which differences in educational outcomes of the specified age group between 1985 and 1990 and between 1985 and 2000 differ between conflict and non-conflict provinces. These difference-in-differences (DD) estimates may capture the causal impacts of conflict on children's educational outcomes. The first-differencing between different time periods sweeps out the province-specific fixed effects, whereas the second-

[4] It is noteworthy that while poor economic conditions and low level of resource endowment may cause researchers to observe lower educational outcomes, these same factors may also depress incentives to uprise, thus weakening the link stated above between the onset of conflict and lower educational outcomes (Blattman and Miguel, 2009).

differencing between provinces sweeps out the time--specific (cohort-specific) fixed effects.

Consistent through the main regression analysis and its robustness checks, I provide evidence that exposure to conflict may reduce the educational attainment of children residing in conflict provinces. In particular, despite improvements in educational outcomes observed for the last three decades in all provinces regardless of their different exposure to conflict, among 16-20 year olds, the DD estimates of the baseline specification suggest that those who were exposed to conflict are less likely to have completed lower-secondary school in both 1990 and 2000 than in 1985, compared to their peers who are not exposed to conflict. Exhibiting a similar pattern, among the 18-20 year olds, the adverse consequences of conflict are much more pronounced for the likelihood of completing upper-secondary school. For both educational outcomes, the conflict's negative estimated effects are greater for 2000, after 16 years of exposure to conflict, than 1990, after 6 years of exposure to conflict, which is consistent with the expectation that the longer exposure to the conflict, the greater is the adverse impact of armed conflict on children's educational outcomes. Furthermore, these estimated effects are found to be greater for female children than their male peers, confirming the findings of both labor and development economics, in the sense that negative shocks are likely to reduce females' educational outcomes more than males'.

The remainder of the paper is structured as follows. The next section provides a brief historical account of armed conflict in Turkey. The third section explains the possible channels through which exposure to conflict may influence children's education outcomes and conveys basic findings of previous studies. The fourth section introduces the DD estimation strategy. The fifth section presents gender differences in the conflict's estimated impacts and discusses the results of a robustness analysis to determine whether the conflict's estimated negative impacts are driven by conflict-induced changes in cross-province migration or whether they are artifacts of province-specific underlying trends. The final section concludes. In addition, for the sake of brevity, I design a Web Appendix that provides further information on the empirical analysis performed for this study.

1. BACKGROUND INFORMATION ON ARMED CONFLICT IN TURKEY

Different from non-conflict provinces, conflict-provinces located in the East Region of Turkey have been densely populated by Kurds, which constitutes the second-largest ethnic group in Turkey. As Turkish Statistical Institute no longer published information on individuals' ethnic background after the 1965 Turkish

Census and removed ethnicity-related questions from the census questionnaire starting in the 1990 Census, there has been no consensus on the size of Turkey's Kurdish population. Estimates range from about 5 to 16 million, given that Turkey's total population is about 71.5 million according to the latest official statistics (Izady, 1992: Mutlu, 1996; Koç et al., 2008).

This varying ethnic composition of the population, however, does not constitute the only difference between conflict and non-conflict provinces. As documented in Table A1 of the Web Appendix, even during the period prior to the conflict's occurrence, conflict provinces have more characteristics of a rural economic and social setting than non-conflict provinces. Specifically, descriptive statistics obtained from the 1985 Census data indicate that while among the working-age population (ages 25-64) employment opportunities were higher in conflict provinces (72.3%) than non-conflict provinces (64.8%), agricultural employment seems to the main source of income-generating activities, with 74.3% of the working-age population being employed in conflict provinces, compared with 52.1% in non-conflict provinces. Accordingly, only a negligible portion of the employed working-age population, 3.3%, performed any sort of activity in the manufacturing sector in conflict provinces, compared to 12.5% in non-conflict provinces. Not surprisingly, echoing differences in the distribution of employment activities, conflict provinces were endowed with less human capital than conflict provinces. For instance, for the population over 25 years old, residents of conflict provinces (5.4%) are less likely to have an upper-secondary school degree or higher than their counterparts in non-conflict provinces (10%). Finally, consistent with differences in employment and educational outcomes, conflict provinces are more likely to be composed of rural areas than non-conflict provinces.

For the period preceding the armed conflict in Turkey, these differences in basic indicators can be seen as verifying the historical fact that during the twentieth century, the East Region, that is, where conflict provinces are located, has benefited the least from Turkey's modernization attempts (Izady, 1992, Mutlu, 1996, 2002; Kirişci and Winrow, 1997; Bozarslan, 2001; Heper, 2007, McDowal, 2007). It could be argued that grievances and mistrust that emerged between the region's individuals and the state because of the Kurdish uprising and their suppression during the early period of the Republic of Turkey, the persistence of the region's feudal structure, its lower geographical endowments, the low level of private investments, and government sanctions on Kurdish population in performing their social and cultural rights have prevented the East Region's economic and social development.

Within such an economic and social environment, in 1984, the PKK, an armed separatist and illegal group founded in 1978, launched an armed struggle

against the central government by attacking Turkish military establishments in Eruh and Şemdinli, which are located in the Southeast Region of Turkey. While the PKK's objectives appear to have changed over time, ranging from establishing an independent Kurdish state in the Middle East to seeking more enhanced political and cultural rights for Turkey's Kurdish population, this armed conflict continued between state military forces and the PKK until the PKK's leader, Abdullah Öcalan, was captured in 1999. Although the armed conflict ceased between 1999 and 2003, since 2004 it has resurged, though at a much lower intensity. As a result of armed conflict in Turkey, during the 1984-2004 period, 29,607 individuals lost their lives, including 5,619 members of state security forces, 19,453 members of the PKK, and 4,535 civilians (Bila, 2004). Furthermore, to weaken the government authority in the East Region, until 1994, the PKK made several attacks on public personal and institutions, including those that provide educational services (Kirisci and Winrow, 1997). In the conflict province, 128 teachers lost their lives between 1984 and 1994; and 5,210 school were closed down between 1992 and 1994.

To effectively suppress the PKK forces, the Turkish government implemented an important regulation, which plays a critical role in determining the structure of the DD framework applied in this study. Specifically, it introduced the state emergency law in 1987 for provinces that experienced intensive clashes between military forces and the PKK (Kirisci and Winrow, 1997; Jacoby, 2005).[5] Similar to martial law under military authority, the state emergency law vested significant authority in the governor of the region of state emergency to control mass media coverage, to change individuals' places of residence, and to prohibit public meetings, demonstrations, as well as strikes. The state emergency law was entirely abolished in 2002 in all provinces where it had been implemented.

[5] State emergency law had governed conflict provinces under two specific statuses. Those that are categorized as the first status are strictly subject to this law. The remaining provinces, defined as adjacent/contiguous (mucavir) provinces, are also regulated by state emergency law, but with less stringent rules. During the time period in which the state emergency law was effective with varying length across provinces, the provinces' legal status may have switched between these two statuses. Without being mutually exclusive, conflict provinces governed in the first status include Batman, Bingöl, Bitlis, Diyarbakır, Elazığ, Hakkari, Mardin, Siirt, Şırnak, Tunceli, Van; those who are defined only as the contiguous provinces are Adıyaman and Muş. Therefore, while a total of 13 provinces were effectively governed by the state emergency law during the 1987-2002 period, in order to be able identify each provinces across three years of the Census data, Batman, Hakkari, Mardin, Şırnak, and Siirt are all grouped together and recorded as Hakkari, yielding a total of 9 conflict province out of 65 recoded provinces. See Table A2 in the Web Appendix for further details regarding the recoding of provinces. Finally, it is important to note that although conflict provinces exhibited variations in enforcing the state emergency law and its effective time period, the empirical method implemented this study treats all conflict provinces uniformly in conceptualizing the implementation of the state emergency law within the framework of the DD estimation method.

In addition to introducing the state emergency law, the Turkish government institutionalized the village guard system in rural areas of conflict provinces to break down the PKK's dominance within rural settlements; to cut down its logistic support from the local rural population in terms of providing food and shelter; and to benefit from local individuals who know the characteristics of the geographically difficult conflict region in military operations (Kurban et al., 2006; McDowal, 2007). As of 2006, 57,174 individual served as village guards in conflict provinces.

In conflict provinces, the civil population, particularly those living in rural areas, had been under crossfire from both sides of the armed conflict (Kirişci and Winrow, 1997; Gunter, 1997; Bozarslan, 2001). From one side, the PKK oppressed the civil population to extract financial, material, and human resources for their movement, perpetrating human right violations in these provinces. From the other side, in response to the PKK's attacks, while the Turkish state militarily defended the integrity of its territory, at least some state apparatuses committed human rights violations in conflict provinces, including mistreatment of prisoners held in detention; the disappearances and illegal killings of politicians and important social figures, journalists, and other dissidents; and increased political and cultural sanctions. More importantly to destroy the PKK's local infrastructure in which food, shelter, and economic and political supports are provided, the Turkish government evacuated several villages and hamlets settlements, particularly those that refused to join the village guard system. Given that conflict provinces have historically been construed as migrant-giving regions, the conflict caused conflict-induced migrants, some who voluntarily left their local residences because of their security concerns and others who were forcedly displaced from their local residences. While it has been reported that 945 villages and 2,021 hamlets were evacuated between 1986 and 2005, there is no consensus yet on the magnitude of conflict-induced migrants, particularly for that of displaced population, whose estimates range from 385,000 to 4 million individuals. In this regard, the most reliable estimates can be obtained from the Turkey Migration and Internally Displaced Population Survey: 2004-2006 (TMIDPS), which was conducted to determine the size and characteristics of the displaced population. The results of the TMIPDS suggest that between 953,680 and 1,202,200 individuals left conflict provinces for security reasons during the 1986-2005 period.

In summary, the 30-year-old armed conflict in Turkey has devastating consequences in terms of physical destruction and socio-economic deprivation for the conflict-exposed population. Furthermore, as observed in Turkey's case, the conflict's negative consequences are aggravated by human-rights violations and conflict-induced population movements. In this regard, after outlining the

main pathways through which the conflict may affect individuals' educational investments, the empirical section of the paper turns to examining whether and to what extent exposure to conflict alters individuals' educational outcomes.

2. THEORETICAL BACKGROUND AND PREVIOUS EMPIRICAL LITERATURE

An armed conflict occurring within a country may influence educational outcomes, mainly through its effects on both the demand and supply of education (Denininger, 2003; Shemyakina, 2006; Justino, 2007; Lai and Thyne, 2007; Blattman and Miguel, 2009). The conflict may damage school buildings, facilities, and other education-related infrastructure and may lead to an insecure environment in which academic and administrative personnel cannot efficiently provide educational services, interrupting the supply of educational services.

The conflict may also depress the demand for education, further exacerbating its negative consequences for educational outcomes. With the onset of conflict, for example, increasing outflows of economic activities and investments from the conflict region, and thereby deteriorating labor market opportunities, may generate a negative economic shock at different levels of decision-making units, such as a province, a community, and a household. Considering that education is a normal good, this shock may reduce the demand for education. Indeed, to mitigate the adverse effects of conflict-induced economic shock--that is, to be able smooth their consumption behavior--parents may take their children out of school and send them into the labor market. In contrast to such a negative income effect, however, the substitution effect may operate in the opposite direction. In particular, conflict-induced reductions in both the likelihood of being employed and wages may lower the opportunity cost of schooling, alleviating the negative consequences of the income effect for the schooling behavior of conflict-exposed children.

In addition to these contemporaneous effects, if the conflict itself and its negative economic consequences are anticipated to last for a longer time period, exposure to conflict may further depress the demand for education by diminishing the expected economic return to education. More importantly, such a negative intertemporal effect of conflict may be intensified, because conflict shortens the time horizon in which individuals can reap the benefits of their educational investments. Furthermore, increases in difficulties associated with access to educational services and decreases in the quality of education received may raise both the financial and psychic costs of acquiring education for children in the conflict region, thereby lowering children's educational outcomes.

Finally, the negative educational consequences of conflict may also operate through its impacts on the household's demographic composition: The conflict-induced rearrangement of the labor division within the household brings more responsibilities for children in both home production and income-generating activities, forcing them to leave school. The loss of an adult wage earner in the household because of exposure to conflict, for example, may compel children to work, reducing their educational outcomes. Along with reductions in household income and the reallocation of tasks within the household, the loss of a family member, particularly a parent or a sibling, may hinder children's psychological development, which is closely related to their educational success.

Other channels, however, may weaken these negative impacts of armed conflict on educational outcomes that arise from simple demand and supply considerations. One possible channel is that families originally from conflict provinces and that are exposed to conflict, particularly Kurdish families, may increase their children's educational investment to mitigate the negative consequences of conflict-induced discrimination against them and/or those of the misperception that all individuals from conflict provinces are associated with the insurgent group, the PKK.[6] It is also possible that to the degree returns to physical capital suffer more from the conflict than returns to human capital, families are more likely to divert their resources for their children's human capital (educational) investments than physical capital investments.

When considering these possible channels altogether, it is difficult to claim that the direction of causation is unilateral, running from the conflict to educational outcomes (Collier and Hoeffler, 1998, 2001; Deininger, 2003; Blattman and Miguel, 2009; Miguel et al., 2004). For the region with the lowest economic and social development, the East Region of Turkey in this study, resulting, in part, from its low level of human capital stock, the conflict-exposed region may fail to retain and attract economic investments that generate income-generating opportunities and necessary instruments, such as more human capital investments, to achieve a higher economic and social well-being, falling into a poverty trap. Along with this poverty trap, as observed for Turkey in the last three decades, with the introduction of market-oriented reforms, relative improvements of economic and social conditions in other regions of the country cause residents of the least developed region to feel that they may be discriminated against and disfavored by the state. If this is the case, then the regions' lower economic and social development, which could be reflected in its

[6] Note that such conflict-induced discrimation can also negatively affect the demand for education by reducing its expected economic returns. Therefore, the net effect of conflict-induced discrimation hinges on the relative magnitudes of these two channels that operate in opposite directions.

lower human capital stock, may spark conflict in the region, making the causal direction of relation between conflict and educational outcomes bilateral.

Taking this possible bilateral causation structure into account, previous studies exploit geographical and time (cohort) variations in exposure to conflict to estimate the causal impacts of conflict on educational outcomes. Geographical variations in the exposure to the conflict enable them to control for cohort-specific fixed effects, whereas its cohort variations allow them to control for region-specific fixed effects. Focusing on school-age children during World War II, for example, Inchino and Winter-Ember (2004) compare differences in long-term educational and earning outcomes between those who were citizens of Germany and Australia, which participated in the war, and those of Sweden and Switzerland, which did not. They find that 40 years after the war, war-exposed individuals still suffer from lower educational outcomes and an associated loss in earnings. Using a similar method but instead exploiting variations in exposure to conflict within a given country, several studies have attempted to assess the educational cost of conflict: Miguel and Roland (2006) for the U.S. bombing of Vietnam during the Indochina War; Merrouche (2006) for the conflict in Cambodia between 1970 and 1998; Shemyakina (2006) for the conflict between 1992 and 1998 in Tajikistan; Chamarbagwala and Moran (2008) for the most intensified period of the conflict (1979-1984) in Guatemala, the country that had to cope with conflict between 1960 and 1996; Akresh and Walque (2008) for the 1994 Rwandan Genocide; Swee (2009) for the Bosnian War between 1992 and 1995; and Akbulut-Yuksel (2009) for the allied forces bombing of Germany during World War II. With the exception of Miguel and Roland (2006), who found no effects of U.S bombing on the literacy rate measured after 30 years in Vietnam, these studies document robust findings of a negative association between conflict and educational outcomes. Differentiating the conflict's estimated impacts by the gender of children who were exposed to the conflict during their schooling period generate differential results. While Shemyakina (2006) and Chamabagwala and Moran (2008) convey evidence that the adverse educational consequences of conflict are greater for female children than their male counterparts, Akresh and Walque (2008) and Swee (2009) find an opposite pattern.

Although this study also utilizes a similar approach, it is necessary to note that except for Inchino and Winter-Ebmer's (2004) study, the micro-level studies cited above, which rely on a partial equilibrium analysis, ignore the spillover effects of the conflict across regions regardless of their exposure to conflict, underestimating the cost of conflict for a country as a whole. Using time variation across countries in the incidence of conflict, Lai and Thynee (2007) provide evidence for the conflict's negative effects on educational outcomes,

even when its general equilibrium effects are taken into account. When assessing macro-level evidence on the possible consequences of exposure to conflict, however, researchers should be concerned about potential econometric problems associated with macro-level analysis, such as finding an instrumental variable to break down the joint determination of the conflict and educational outcomes.

3. ESTIMATING THE IMPACT OF ARMED CONFLICT ON EDUCATIONAL OUTCOMES

Data

The data required for this study must satisfy two conditions. First, it must have a sufficient sample size for each province, enabling me to obtain province-level estimates and make statistical inferences. Given that this sample size requirement is satisfied, the second condition is that the data should include information on individuals' residential history, including where they resided during their school-age years. For Turkey, the Census appears to have the only data set that contains a sufficient number of observations for each province. It also provides, among data sets available in Turkey, the most comprehensive residential information from which individuals' place of residence during their school-age years can be approximately identified. In particular, it enables me to determine where individuals were residing at the time of the Census and five years prior to the Census, as well as their birthplace. Therefore, because the Census provides the most suitable and available data set to measure individuals' exposure to conflict, and thereby to study the educational consequences of armed conflict in Turkey, I use a randomly drawn 5% sample of the 1985, 1990, and 2000 Turkish Censuses. I tailor these raw data in three dimensions to be able to use them in the empirical analysis. First, I exclude the non-institutional population from the analysis sample.[7] Second, I drop individuals who reported countries other than Turkey as a birthplace or place of residence at the Census time and five years prior to the Census. Finally, in an attempt to observe provinces in each year of the Census data, I regroup provinces to account for changes in Turkey's administrative structure in which provinces are governed. Mostly because of political motivations rather than administrative and economic necessities, the number of provinces, which was recorded as 67 in the 1985 Census, increased to 73 by the time of the 1990 Census and to 81 by that of the 2000 Census. Because the Census data do not make it possible to identify

[7] As a result of this restriction, the sample is confined to children who were residing in a household setting. In an effort to examine the sensitivity of estimation results to this restriction, three specifications similar to those reported in Table 1 were estimated by removing this restriction and using only children's characteristics and residence information. The results remain both qualitatively and quantitatively the same. The estimation results of this empirical exercise are reported in Table A3 of the Web Appendix.

precisely both urban and rural areas that made up a given new province, this regrouping results in 65 provinces that can consistently be identified throughout the three years of the Census (Coulibaly et al., 2007; Filiztekin and Gökhan, 2008).[8]

The most important challenge in constructing a data set that can be used to analyze the consequences of armed conflict in Turkey comes from the fact that because the Census data record one move for a fixed time period, such as within the last five years prior to the Census or since birth. Such limited information on individuals' residential history may not capture precisely their migration behavior, making difficult to determine individuals' place of residence during their school-age years. This problem may be compounded because individuals are more likely to move multiple times across multiple locations during conflict. Also, regarding individuals' educational outcomes, the Census data contain only their highest degree completed, reflecting their educational success that occurred during a certain time period in the past. To some extent, the adverse effects of this problem can be avoided by focusing on a younger population as an analysis sample in the empirical analysis. The Turkish pre-tertiary educational system consists of two distinct educational levels in which children aged 6-14 are required to participate in lower-secondary education (grades 1-8), whereas those aged 15 and older are expected to attend upper-secondary schools (grades 9-11).[9] Within this institutional framework, when the focus centers on lower-secondary education outcomes (hereinafter, lower education), the sample is restricted to those aged 16-20 who are expected to have completed their lower education degree. Likewise, the sample is restricted to those aged 18-20 when the focus is shifted to upper-secondary education outcomes (hereinafter, upper education).

Examining educational outcomes for such a restricted age group may minimize the adverse effects of the incomplete mobility information from the Census. In fact, although I utilize the head of household residential information

[8] It is noteworthy that the Census data do not provide information that enables us to identify individuals' rural place of residence, such as small towns, villages, and hamlets. Furthermore, as new provinces, Batman and Sırnak, which appear for the first time in the 2000 Census, were formed by combining rural and urban settlements that used to be parts of administrative structure of Hakkari, Mardin, and Siirt. Consequently, since it is not possible to detect these five provinces individually for the 1985 and 1990 Census data, they are recoded as Hakkari in the data used in the empirical analysis. Thus there are 65 instead of 67 provinces when the administrative structure of provinces that were effective in the 1985 Census are taken as a reference point to reclassify provinces in the three years of Census data. Coulibaly et al. (2007) adopt a similar reclassification. This reclassification of provinces is illustrated in Table A2 of the Web Appendix.

[9] Note that compulsory education in Turkey was extended from 5 to 8 years by a change in the law enacted in 1997, reducing the three-tier system, which consists of primary school, middle school, and high school, to a two-tier system, which includes the lower-secondary education, which combines primary- and middle-school education, and the upper-secondary education. Furthermore, the educational period in upper-secondary schools was extended from 3 years to 4 years in 2006.

to derive that of children, for the age group of children I focus on in this study, the Census data are likely to provide sufficient information to match provinces where children had spent their schooling years with those where conflict had taken place.[10] For example, restricting the sample of 16-20 year olds to those who had not moved within a 5-year period prior to the Census, I can identify precisely their places of residence from a maximum of 3-4 years of their lower education period for the youngest members (16 years old) to a minimum of 0-1 years for their oldest counterparts (20 years old). Given the narrowed age interval for the 18-20 year olds, the determination of whether children had spent their schooling years in conflict provinces or not can be made more precisely for their upper educational period. Therefore, focusing on these two narrowed age groups may reduce concern about the difficulty of measuring individuals' whereabouts during the conflict, particularly in the Population Census conducted by the government.[11]

Observing these two specific age groups over three Censuses provides a time variation in exposure to conflict. Observations from the 1985 Census convey information on educational outcomes before the occurrence of conflict; observations from the 1990 and 2000 Censuses are used to measure the corresponding outcomes as the conflict continued. In addition to this time variation in exposure to conflict, the geographical concentration of conflict in Turkey enables me to identify a cross-sectional variation in exposure to conflict. Specifically, as illustrated in Table A2 of the Web Appendix, provinces that had been governed by the state of emergency law, in which the state forces were equipped with enhanced authority and individuals were denied from exercising their basic rights, are defined as conflict provinces. Accordingly, all other provinces are defined as non-conflict provinces.

Specification

By using data structured as outlined above, I rely on variations in exposure to conflict across provinces and over time to estimate the conflict's causal effects on educational outcomes in Turkey. On the one hand, differencing between before and during conflict exploits time variations within a given province in its exposure to conflict, thus eliminating province-specific fixed effects. Differencing across provinces, on the other hand, exploits geographical variations within a

[10] In the robustness analysis below, I convey evidence that the estimation results remain same when I use the residential information of children rather than that of their head of household to determine whether they were residing in conflict provinces or not.

[11] In a companion paper, using the 2000 Census data and focusing on individuals ages 18-50, I measure the years of exposure to conflict for each individual in his or her schooling period. In this paper, I determine the specific cohort at which the conflict's negative effects were phased in and provide evidence that the conflict's negative estimated effects are heightened with longer exposure to the conflict.

given time period, thus eliminating the time- or cohort-specific fixed effects. Using the former differencing over the latter yields the DD estimates that can be obtained by running the following regression model:

$$Educ_{ijt} = \alpha + \beta_1(Conflict\_province_j * Year\_1990) + \beta_2(Conflict\_province_j * Year\_2000) + X_{ijt}\delta + \pi_1 Year\_1990 + \pi_2 Year\_2000 + \theta \Pr ovince_j + \varepsilon_{ijt}$$

Where *i, j* and *t* index for individuals, provinces, and time, respectively; $Educ_{ijt}$ measures children's outcomes; And X_{ijt} measures denotes a vector of both child- and household-level variables. The former includes dummy variables indicating a child's age, gender, and whether she is a child of the head of household; the latter includes head of household characteristics, such as education, age, urban status, and household size.[12] Given that 1985 is chosen as a reference year, $Year\_1990$ and $Year\_2000$ denote year dummies for 1990 and 2000, respectively, accounting for time-specific effects that are common to all provinces in Turkey. $\Pr ovince_j$ denotes a vector of province dummy variables, controlling for province-specific, time-invariant characteristics. Finally, ε_{ijt} is an idiosyncratic error term. To account for heteroscedasticity across province-year cells and for the possibility that individuals drawn from the same province-year cell are likely to have a common error term, leading to the serial correlation problem, standard errors are adjusted for the clustered data by province and Census year.

Coefficients on the interaction terms, β_1 and β_2, measure the effects of exposure to armed conflict on educational outcomes. The estimate of the coefficient on $Conflict\_province_j * Year\_1990$ measures the educational consequences of conflict six years after the onset of conflict by comparing differences in educational outcomes between before conflict and six years after the onset of conflict in conflict provinces, relative to those in non-conflict provinces. The coefficient on $Conflict\_province_j * Year\_2000$ provides similar information by measuring the educational consequences of 16 years of conflict. Here, the natural hypothesis to test is that the adverse impact of conflict on children's educational outcomes may be heightened with the duration of the conflict, which turns out to be supported by the empirical evidence provided below.

Although it is not presented here for space considerations, Table A6 in the Web Appendix gives a descriptive account of what exactly the DD estimation

[12] For each Census year, the descriptive statistics of both children and head of household characteristics are shown in Tables A4 and A5 of the Web Appendix.

method accomplishes in terms of measuring the conflict's impact on children's educational outcomes. Negative DD estimates reported in Table A6 can be interpreted as indicating adverse educational consequences for children exposed to conflict. It is noteworthy that these negative consequences are felt more by children who are exposed to 16 years of conflict, compared to those who are exposed to it for 6 years. Furthermore, among those with longer exposure to conflict, the conflict's negative estimated impacts are greater for upper-secondary school outcomes than lower-secondary school outcomes. These main findings are also confirmed with the regression analysis of the DD framework presented in subsequent sections. It is also important to note that as previously mentioned, because the Census data do not contain information on individuals' ethnicity, I cannot analyze the educational consequences of conflict along ethnic lines. But, as demonstrated in Mutlu (1996), since Kurds have constituted the majority of the population in conflict provinces, the DD estimates presented in this study can be used, at least indirectly, to infer how the conflict may influence the course of ethnic differences in educational outcomes in Turkey, as well as those in behavioral outcomes in which education may play an important role, such as labor market earnings, fertility, and crime.[13]

Main Results

Because the likelihood of being both exposed to armed conflict and included in the analysis sample are jointly determined with both individual and household characteristics, it might be extremely difficult, if not impossible, to find determinants of educational success that are not endogenously determined. For this reason, as shown in Table 1, I begin the empirical analysis by estimating the specification with no control variables except province and year dummies (Column 1). I re-estimate this basic specification by adding first the characteristics of children (Column 2), then those of head of household characteristics (Column 3), and finally the full interaction terms of these two sets of control variables with year dummies for 1990 and 2000 (Column 4). In particular, the specification used to obtain estimation results reported in Column (3) constitutes the baseline specification. I run all these specifications for both lower-secondary (Panel A) and upper-secondary (Panel B) school outcomes.

[13] Because in addition to the Turkish and Kurdish populations, conflict provinces, as well as non-conflict provinces, have also hosted Arabic and Caucasian population, this interpretation must be made with a caution and must be augmented with future studies that can use individuals' ethnicity information to measure the human capital consequences of conflict. In this regard, it is important to note that Turkey's diverse ethnic structure calls for well-designed research to examine how all ethnic groups' economic and social outcomes, including human capital outcomes such educational success, are influenced by the conflict.

Table 1 Estimates of the impacts of conflict on two educational attainments

	(1)	(2)	(3)	(4)
	A. Lower-secondary school			
Conflict_provinces*Year1990	-0.024* (0.014)	-0.021 (0.014)	-0.021*** (0.008)	-0.016* (0.009)
Conflict_provinces*Year 2000	-0.040** (0.017)	-0.039** (0.017)	-0.047*** (0.010)	-0.042*** (0.013)
R-squared	0.082	0.112	0.254	0.260
Number of observations	803186	803186	802487	802487
	B. Upper-secondary school			
Conflict_provinces*Year1990	-0.021* (0.013)	-0.019 (0.013)	-0.018* (0.010)	-0.010 (0.009)
Conflict_provinces*Year 2000	-0.076*** (0.016)	-0.075*** (0.016)	-0.083*** (0.012)	-0.051*** (0.013)
R-squared	0.083	0.098	0.240	0.242
Number of observations	466316	466136	465732	465732
Control variables				
Province dummies	YES	YES	YES	YES
Year dummies (1990, 2000)	YES	YES	YES	YES
Children's characteristics	NO	YES	YES	YES
Head of household characteristics	NO	NO	YES	YES
Children's and head's characteristics interacted with year dummies	NO	NO	NO	YES

Note: Panel A presents the estimation results of lower-secondary school outcomes for the 16-20-years-old children, whereas Panel B presents those of upper-secondary school outcomes for the 18-20-year-old individuals. Each column shows the estimation of results of a separate specification whose independent variables are indicated in the respective column. For specifications in columns (3) and (4), children's characteristics include the child's age (dummy variables for each age group), gender, and a binary variable indicating whether she is a child of the head of household, whereas head of household characteristics include her education, age, urban status, and household size. Tables A4 and A5 given in the Web Appendix provide descriptive statistics of variables that are used in estimating specification in column (3), which constitutes the baseline specification in the rest of the analyses executed in the paper. For this baseline specification, the full estimation results are given in Table A7 in the Web Appendix. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

For the sake of brevity, although tables in the paper contain only the coefficients on interactions terms between a dummy variable for conflict provinces and dummy variables for the Census years, Table A7 located in the Web Appendix provides information on the estimated coefficients for the full set of control variables that are included in the baseline specification. When the positive estimated coefficients on individual year time dummy variables for 1990 and 2000 are evaluated, it appears that educational outcomes improved over time in all provinces, regardless of whether they were exposed to conflict or not during the 1985-2000 period. As documented in Table 1, however, the negative estimates of the interaction terms--that is, DD estimates of the conflict's impact on educational outcomes--suggest that the presence of conflict may slow down the occurrence of these improvements for children who were exposed to conflict during their school-age years. In other words, the estimation results mainly demonstrate that exposure to conflict is negatively associated with children's educational outcomes. Furthermore, the conflict's negative estimated effects for 2000 are more pronounced than those for 1990. Specifically, among the estimates of the specification reported in column (3) of Table 1, which will serve as a baseline specification throughout the paper,[14] the coefficients on the interaction terms for 1990 and 2000 suggest that children who were residing in conflict provinces are less likely to have a lower-secondary school degree than their peers in non-conflict provinces by 2.1 percentage points in 1990 and 4.7 percentage points in 2000, relative to the base year 1985.[15] When examining the conflict's estimated impacts on an upper-secondary school outcome, the corresponding estimates reported in the same column are 1.8 and 8.3 percentage points, respectively.

As reported in Table 1, among eight specifications, while the estimated effects for 1990 are statistically significant except in three specifications, the corresponding estimates for 2000 are statistically significant in all specifications. In addition to being more likely to be significant, the latter estimates are found to be larger than their former counterparts. These differences in estimated effects could be interpreted as evidence that the adverse consequences of armed conflict may become stronger if children have been exposed to conflict for a longer time period during their schooling years. This finding is also consistent with literature that has established significant links between conditions in individuals' early

[14] In the Web Appendix, Table A7 presents the entire regression results of this baseline specification for both educational outcomes. The results suggest, not surprisingly, that children's educational outcomes are positively related to age, being a child of the head of the household, household head's age, and living in an urban area, whereas it is negatively related to being a female and coming from a crowded household.

[15] For the entire econometric analysis performed in the paper, I estimate four basic specifications reported in Table 1. Regardless of the baseline specification chosen, estimation results report similar quantitative and qualitative patterns in the relation between exposure to conflict and educational outcomes with some minor exceptions.

childhood period and their success later in life in terms of education, health, and labor market outcomes.[16] In fact, when the focus is centered on lower-secondary school outcome, in contrast to their peers in 1990, those aged 16-20 in 2000 had spent almost their entire early childhood and schooling years in the shadow of armed conflict. A similar difference in the length of exposure to conflict also exists for the upper-secondary school sample--those aged 18-20--who were observed in 1990 and 2000, respectively. This positive association between the length of exposure to armed conflict and the magnitude of the conflict's impact on children's educational outcomes bolsters confidence in the DD estimates, which capture the causal relation between exposure to conflict and educational outcomes.

The other main finding derived from Table 1, which also remains unchanged through the robustness analysis performed below, is that the adverse impact of conflict is greater for the likelihood of being a upper-secondary school graduate than that of being a lower-secondary school graduate. A change in the compulsory education law in 1997, which requires the school-age population to complete their lower-secondary education, may account for the relatively lower effect of conflict on children's lower-education outcomes. In addition, compared to the lower-secondary school-age population, the upper-secondary school-age population is more likely to be able to assist their families by working in the labor market or undertaking more responsibilities in household production to mitigate the adverse consequences of conflict-induced shocks at the household level. These factors may therefore contribute to conflict having a larger adverse impact on children's upper-secondary education outcomes.

Because these two main findings hold across all specifications in Table 1, when I perform the robustness analysis below, I proceed with a specification in Column (3) where I control for the characteristics of both children and head of household, along with both province- and year-specific fixed effects.

4. ROBUSTNESS ANALYSIS

Migration and Gender Differences in Estimates of the Conflict's Effects

Similar to the study presented here, the non-random distribution of individuals across locations may constitute an important threat for studies whose identification strategy utilizes information on individuals' location to differentiate those who are exposed to a shock (treatment group) from those who

[16] See, for example, Almond (2006), Almond et al. (2007), and Gould et al. (2007).

are not (control group).[17] This might be particularly true when examining consequences of conflict, since families may migrate to conflict-free locations to secure their economic and social well-being, as well as that of their offspring.[18] This conflict-induced migration is likely to be selective to the extent that families' migration propensities may vary with children's abilities and their familial economic and social resources. When more able children and those with more familial resources leave conflict provinces, for example, the remaining children who are exposed to conflict are likely to be less able and to come from families that are financially constrained to both migrate and send their children to school. This type of heterogeneity in migration propensities may cause an overestimation of the negative impact of conflict. In addition to this voluntary, conflict-induced migration with its selective nature, involuntary migration--forced migration--from conflict provinces took place during the conflict. While there is no information on the volume of the former type of migration, it has been estimated that about one million individuals were forcedly displaced from their settlement in the conflict region, many of whom resided in rural areas (HUNEE, 2006). Unlike voluntary conflict-induced migration, because forced migration does not discriminate among individuals based on their ability or resources, the effects of migration's selective nature on estimates of conflict coefficients is expected to be less significant. Because Census data do not allow for differentiating between these two types of migrant flows from conflict provinces, I perform the sensitivity analysis of main results to the presence of cross-province migration by using an aggregated measure of individuals' migration behavior.

In this regard, to examine whether the negative estimated effects of conflict on children's educational outcomes are influenced by the non-random distribution of the population through migration, I define two distinctive migration behaviors that can be detected in the Census data. First, I use individuals' information on the province of residence both at the time of the Census and five years prior to the Census, capturing individuals' short-term,

[17] See, for example, Borjas (2003), who explains how cross-state migration may threaten an identification strategy that relies on the spatial distribution of immigrants to detect causal effects of immigrant inflows on natives' labor market success.

[18] It is important to note that in this study I focus exclusively on internal migration as a possible threat to the identification strategy of the DD framework. But it is possible that armed conflict may also cause international migration. In fact, about one million Kurds from conflict provinces are living in Europe, building a diaspora that also has important ramifications for the course of conflict in Turkey (Hassanpour and Mojab, 2005). To the degree that episodes of international migration are more likely than those of internal migration to be driven by higher familial resources or higher ability or greater preferences for education, the conflict's negative impacts might be overestimated in the DD framework. An additional caveat should be made for the fact that some Kurdish children in conflict provinces may select to join the insurgent group, thus not appearing the analysis sample. There exists no study in Turkey to trace down the nature of such a selection mechanism that might bias estimation results, however.

cross-province migration behavior. Individuals who did not move across provinces within a five-year period are defined as short-term non-movers; the remaining individuals are defined as short-term movers. Second, to capture individuals' long-term, cross-province migration, I apply a similar procedure, in which individuals are differentiated on the basis of whether the province where they were born is matched with the province where they resided at the time of the Census. If the match is occurred, then these individuals are defined as long-term non-movers; otherwise, they are defined as long-term movers.[19]

As shown in Table 2, columns (1) and (3) present the conflict coefficients for immobile individuals: short-term and long-term non-movers, respectively. Compared to the benchmark specifications in column (3) of Table 1, restricting the sample to non-movers increases the estimated impact of conflict, particularly for long-term non-movers. I also run the same specifications for their mover counterparts: short-term and long-term movers, respectively. As reported in columns (2) and (4), the conflict coefficients are much smaller and statistically nonsignificant, except the estimate for 2000 in the upper-secondary school specification. As a result, the evidence that the adverse impacts of conflicts are most observed among non-movers and that within the non-mover population, the conflict coefficients are larger for long-term non-movers than their short-term peers is consistent with the expectation that as individuals are exposed to conflict for a longer time, its adverse impacts on their educational outcomes increases. Moreover, as shown in column (5), the estimates of the conflict coefficient are robust to using information on provinces where individuals were born to split provinces into conflict and non-conflict provinces.

[19] For individuals who may have been residing in conflict provinces at least at one time of the three points of time that can be identified in the Census data, such groupings for long- and short-term movers entail four distinct migrant groups: i) those who had moved from one conflict province to another one; ii) those who had moved from one non-conflict province to another one; iii) those who had moved from a conflict province to a non-conflict province; and iv) those who had moved from a non-conflict province to a conflict province. While this level of aggregation may influence the estimation results, the main aim of this empirical exercise laid out above is to verify that the conflict's negative estimated impacts are indeed caused by the effects of the exposure to conflict that is anticipated to be more pronounced for non-mover residents of conflict provinces, but not changes in migration behavior over the course of the conflict. As explained in the text, it turns out that the results of this empirical exercise confirm its predictions. Furthermore, examining separately these four distinct groups of both long- and short-term movers may introduce other selection issues in the sense that, for instance, those who had moved between conflict provinces are likely to be significantly different than those who had moved out from conflict provinces to non-conflict provinces in terms of both their observable and unobservable characteristics. While addressing these issues is beyond the scope of the paper, in a future study I will conduct a detailed examination of whether and to what extent conflict-induced migration is related to educational outcomes.

Table 2 Robustness analysis of the impacts of conflict to cross-province migration and use of different residential information

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	A. Lower-secondary school										
Conflict_provinces*Year1990	-0.024***	-0.002	-0.027***	0.006	-0.027***	-0.022***	-0.027***	0.010	-0.026***	0.016	-0.020***
	(0.010)	(0.015)	(0.010)	(0.014)	(0.009)	(0.009)	(0.010)	(0.013)	(0.009)	(0.018)	(0.009)
Conflict_provinces*Year2000	-0.052***	0.027	-0.072***	-0.005	-0.071***	-0.046***	-0.052***	0.010	-0.062***	0.018	-0.068***
	(0.011)	(0.018)	(0.011)	(0.016)	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.017)	(0.010)
R-Squared	0.251	0.295	0.257	0.226	0.255	0.253	0.251	0.280	0.262	0.238	0.257
Number of observations	748436	54051	560239	242248	802487	79995	738855	61140	634654	165341	799995
	B. Upper-secondary school										
Conflict_provinces*Year1990	-0.020*	-0.003	-0.019*	-0.0006	-0.019*	-0.019**	-0.021**	-0.027	-0.021**	0.012	-0.02*
	(0.010)	(0.027)	(0.011)	(0.015)	(0.011)	(0.010)	(0.010)	(0.021)	(0.010)	(0.019)	(0.010)
Conflict_provinces*Year2000	-0.084***	-0.045	-0.096***	-0.027*	-0.093***	-0.082***	-0.084***	-0.052***	-0.093***	-0.005	-0.093***
	(0.012)	(0.031)	(0.014)	(0.016)	(0.013)	(0.012)	(0.013)	(0.019)	(0.013)	(0.020)	(0.013)
R-squared	0.233	0.310	0.229	0.241	0.241	0.240	0.233	0.296	0.240	0.248	0.243
Number of observations	432577	33155	324138	141594	465732	464035	424723	39312	362511	101524	464035

Note: See note to Table 1. All specifications use the same set of variables as used in the baseline specification presented in column (3) of Table 1 with a different analysis sample with the exception of the specification whose results are given in column (6), in the sense that the analysis sample reported in Table 1 is used. For each column, the analysis sample with the corresponding column number is given as follows: (1) Short-term non-mover individuals: The sample is restricted to individuals who had not moved between provinces within the last five years prior to the Census; (2) Short-term mover individuals: The sample is restricted to individuals who had moved between provinces within the last five years prior to the Census; (3) Long-term non-mover individuals: The sample is restricted to individuals whose provinces where they were born are same as their province of residence at the time of the Census; (4) Long-term mover individuals: The sample is restricted to individuals whose provinces where they were born are not same as their province of residence at the time of the Census; (5) Information on individuals' provinces where they were born is used to categorize provinces as conflict and non-conflict provinces. The specification in column (6) replicates the one reported in column (3) of Table 1, and those in columns (7)-(11) replicate the specifications in Columns (1)-(5) by using children's information on the provinces of residence. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Thus far, I use residential information on the head of the household to differentiate among children with different statuses regarding exposure to conflict. Instead, in Table 2, the specification in column (6) replicates the benchmark specification in column (3) of Table 1, and those in columns (7)-(11) replicate specifications reported in columns (1)-(5) by using children's residential information to identify whether they were residing in conflict or non-conflict provinces. The results of this empirical exercise suggest that conflict's estimated negative impacts do not seem to hinge on how children's residential information are extrapolated from the Census data to measure their exposure to conflict.

Migration may not only invalidate the non-selective distribution of individuals across provinces with different degree of exposure to conflict, but also spillover the adverse impacts of conflict from conflict provinces to non-conflict provinces, leading to a downward bias in the conflict coefficient's estimates. In fact, Berker (2009) provides evidence for Turkey that a higher rate of migrant inflows to a given province may hurt the educational success of local residents who did not move within the five-year period prior to the Census and those who did not move since birth, while these adverse effects are much larger for the latter group. For this reason, to probe the extent to which the estimated conflict coefficients are contaminated by spillover effects through migration, I exclude provinces that were not exposed to conflict, but experienced larger migrant inflows from the conflict region.[20] The results reported in column (1) of Table 3 provide evidence that the negative estimated effects of conflict remain robust to the exclusion of these provinces from the analysis sample. Besides, when I only keep these provinces as non-conflict provinces along with conflict provinces in the sample for the specification in column (2), the conflict's negative effects for both 1990 and 2000 are estimated to be much larger and more statistically significant. These findings contradict the expectation that spillover effects caused by conflict-induced migration may adversely influence education outcomes for the local population of non-conflict provinces with a higher density of conflict-induced migrant inflows. The lack of such a negative impact can be attributed to the fact that these non-conflict provinces are historically the most developed provinces, which also benefited enormously from market-oriented reforms implements since the early 1980s. Furthermore, as conflict-induced migrants have chosen to be reallocated to provinces that are densely populated with previous migrants from conflict provinces, further segregation and sorting with respect to ethnic origin, as well as the province of origin, within non-conflict provinces may mask the negative effects of conflict-induced migration in this empirical setting.

[20]The results of Turkey Migration and Internally Displaced Population Survey illuminate that most conflict-induced migrants chose to settle in provinces where they have historically hosted a larger Kurdish population (HUNEE, 2006). These excluded provinces are Adana, Ankara, Antalya, Bursa, İçel (Mersin), İstanbul, İzmir, Kocaeli, Malatya, and Manisa.

Table 3 The robustness analysis of the impacts of conflict for different sets of non-conflict provinces

	(1)	(2)	(3)	(4)
	A. Lower-secondary school			
Conflict_provinces*Year1990	-0.022** (0.009)	-0.026** (0.011)	-0.026*** (0.009)	-0.009 (0.014)
Conflict_provinces*Year2000	-0.061*** (0.011)	-0.040*** (0.012)	-0.059*** (0.011)	-0.008 (0.018)
R-Squared	0.255	0.246	0.248	0.255
Number of Observations	470217	359280	668469	160938
	B. Upper-secondary school			
Conflict_provinces*Year1990	-0.013 (0.011)	-0.030*** (0.010)	-0.022** (0.010)	-0.005 (0.017)
Conflict_provinces*Year2000	-0.077*** (0.013)	-0.094*** (0.013)	-0.093*** (0.012)	-0.030 (0.021)
R-Squared	0.229	0.240	0.233	0.213
Number of Observations	271557	209566	386077	95046

Note: See note to Table 3. All specifications use the same set of variable as used in the baseline specification presented in column (3) of Table 1 with a different analysis sample. For each column, while the analysis sample includes the same set of the conflict provinces, it includes a different set of non-conflict provinces. These non-conflict provinces with the associated column number are defined as follows. (1) The non-conflict provinces exclude provinces that experienced the highest migrant inflows from the conflict provinces; (2) The non-conflict provinces are restricted to provinces that experienced the highest migrant inflows from conflict provinces; (3) The non-conflict provinces exclude provinces that are located in the East Region of Turkey but were not governed by the state emergency law; (4) The non-conflict provinces are restricted to provinces that are located in the East Region of Turkey but were not governed by the state emergency law. For the list of non-conflict provinces that experienced the highest migrant inflows from conflict provinces, see footnote 19; for the list of non-conflict provinces that are located in the East Region of Turkey, see Table A2 in the Web Appendix. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Up to this point, non-conflict provinces are restricted to provinces that were not governed by the state emergency law, including those located in the East Anatolia and Southeast Anatolia Regions that also contain conflict provinces. Spillover effects may occur, however, between conflict and non-conflict provinces that are located in the same region. To investigate the sensitivity of the negative estimated effects of conflict to this restriction, I run two different specifications. The first specification excludes these provinces from the sample. The results of this empirical exercise, which are presented in column (3) of Table 3, suggest that the negative estimated impacts of conflict increase. The second specification, in contrast, keeps only those previously excluded provinces as non-conflict provinces in the sample; hence both conflict provinces and non-conflict provinces come from the East and Southeast Anatolia Regions. As reported in column (4), the conflict coefficients become much smaller and statistically nonsignificant. When the results in both columns (3) and (4) are evaluated together, it seems that the conflict's adverse impact may spill over significantly to non-conflict provinces in the East Region where conflict provinces are also located. Indeed, when the analysis sample is restricted to only Turkey's East Region, which contains both conflict and non-conflict provinces, it is not surprising to find no impacts of conflict on educational outcomes. One explanation is that these non-conflict provinces in the region constitute the first set of geographic locations where clashes between state forces and the PKK may physically spill over. It is also possible that given the fact that provinces within the same region are not only geographically attached but also connected through the provision of public services, such electricity and transportation, as well as through economic linkages, any adverse economic effects or disruptions in the provision of public services caused by the conflict may have been immediately felt by the other provinces in the region that were not exposed to conflict. Likewise, the conflict's onset may have severely disrupted economic linkages between the East Region as a whole and other regions of Turkey. Finally related to the first explanation, because non-conflict provinces in the East Region inhibit a significant number of clustered Kurdish populations, such a population composition may have facilitated the spread of armed clashes in these provinces or may have made them potential insurgents from the state's point view, thus contributing to similar adverse impacts of conflict for the educational outcomes of children who were residing in the East Region's non-conflict provinces.

The specification analyses whose results are presented in Tables 2 and 3 convincingly illustrate that the negative estimated impacts of armed conflict on children's educational outcomes may not be driven by how individuals' residential information is used to identify geographical variations in exposure to conflict. Furthermore, the results provide evidence that the conflict's effects may spill over to non-conflict provinces in the East and Southeast Anatolia Regions.

Having established that evidence of a negative association between exposure to conflict and educational outcomes does not vary by individuals' residential information, I proceed to the remaining robustness analysis by confining the sample to individuals who reported the same province of residence when the Census was conducted and 5 years prior to the Census. Information on 5-year immobility is most likely to capture the location-specific experiences of children in terms of both being exposed to conflict and schooling. However, because gender differences in the conflict's estimated impact may exhibit heterogeneity with respect to the ways through which individuals' geographical exposure to conflict is defined, a different account of individuals' residential information continues to be utilized to examine the extent to which the impacts of conflict may vary by children's gender.

Gender differences in the conflict's impacts on educational outcomes can be attributed to several factors. As widely established in both development and labor literature, one general factor is that in their efforts to mitigate adverse consequences of conflict-induced shocks, parents may opt not to send their female children to school, in part because females are more likely than males to be disadvantaged in terms of reaping benefits from their educational investments in the labor market. Another factor that is more specific to Turkey's institutional setting is that every male individual is required to perform military service, for which he becomes eligible between the first day of the year in which he turns 20 and that of the year in which he becomes 41. In this setting, individuals' school attendance status and educational credential may play critical roles in determining the time at which they are required to join military forces, the length of military service, and their rank and types of assignments during military service. Age-specific, eligible individuals who continue their education, for example, can postpone their military service until the completion of their educational program. Furthermore, those with a 4-year university degree and those without such a degree are subject to different military regulations regarding the length of military service. Depending on the relative magnitude of the number of individuals with a university degree and the demand for reserve officers in the army, as of 2001, university graduates can serve either 16 months as a reserve officer or 8 months as a short-term private. Those without a 4-year university degree are required to serve 18 months as a long-term private.[21] Finally, there is some anecdotal evidence that both reserve officers and short-

[21] In 2003, time periods of military service were reduced to 12 months for reserve officers, 6 months for short-term privates, and 15 months for long-term privates. It is also noteworthy that Turkish citizens who live and work abroad for at least three years can qualify to fulfill their compulsory military service in one month of basic military training by paying necessary fees. There have many episodes in which university graduate males, particularly those from middle- and upper-income families, opt to go abroad to pursue postgraduate studies, which enables them to defer their military service so that they can legally leave the country. Following the completion of their studies, they work until they become eligible for this buy-out option.

term privates who are required to have a 4-year university degree are less likely than their long-term private counterparts to be assigned to military operations in the armed conflict in Turkey's East Region. Therefore, because male children's educational success may dramatically influence their status in performing compulsory military service, families may have strong motivations to provide further education to their male children, reducing the likelihood that their children will be given combat assignments.

When the conflict's estimated impacts are differentiated by children's gender in Table 4, the estimation results suggest that these two possible causal channels explained above may indeed contribute to observe greater and significant negative impacts of conflict on females' both educational outcomes regardless of whether the focus is placed on i) the entire population of children, including all types of movers and of non-movers (column 1); ii) short-term non-movers (column 2); and iii) long-term non-movers (column 4). In addition, as reported in the Panel (A2) of Table 4, for females' lower-secondary school outcomes, being in line with the main results, the conflict's negative impacts are estimated to be larger for the year 2000; and they are slightly larger for long-term non-movers than for short-term non-movers. Similar patterns with reduced magnitudes are observed for males in Panel (A1) when their sample is restricted to long-term non-movers (column 4) or when their information on province of birth is utilized to map geographical exposure to conflict (column 6). Unlike their female peers, for male children the negative estimated effects of the conflict do not appear to be significant when the identification of geographical exposure to conflict is based on the head of household's province of residence at the time of the Census (column 1). This last finding, however, could not be replicated for males' upper-secondary school outcomes in the same column of Panel (B1), where the conflict coefficient for the year 2000 is negative and statistically significant. Indeed, such an estimated effect for male children is found consistently except for long-term movers (column 5). As observed for lower-secondary school outcomes, particularly for the year 2000, the conflict's impacts on upper-secondary outcomes are greater for females than males, with the exception of short-term movers. Overall, with some exceptions as noted above, breaking down the conflict's estimated impacts by gender provides further evidence that the DD estimation method exhibits a good performance in measuring the educational consequences of conflict. In fact, as found for the entire sample of children reported in Table 1, gender-specific results presented in Table 4 continue to illustrate that the negative estimated impacts of conflict are greater 16 years after the onset of conflict--measured by the interaction term for 2000--than 6 years after the onset of conflict--measured by the interaction term for 1990--and that increases in individuals' propensity to be immobile, as measured in the Census data, are associated with increases in the conflict's negative impacts on children's educational outcomes.

Table 4 Estimates of the impacts of conflict on educational attainments by gender

	(1)	(2)	(3)	(4)	(5)	(6)
	A1. Males-Lower-secondary school					
Conflict_provinces*Year1990	-0.015 (0.011)	-0.019* (0.011)	0.033 (0.029)	-0.027*** (0.011)	0.025 (0.023)	-0.026*** (0.010)
Conflict_provinces*Year2000	-0.008 (0.012)	-0.012 (0.012)	0.002 (0.027)	-0.036*** (0.012)	-0.001 (0.024)	-0.044*** (0.010)
R-squared	0.197	0.192	0.2785	0.199	0.191	0.196
Number of observations	367076	341891	25185	294413	117663	367076
	A2. Females-Lower-secondary school					
Conflict_provinces*Year1990	-0.026*** (0.010)	-0.027*** (0.010)	-0.024 (0.024)	-0.025* (0.013)	-0.007 (0.011)	-0.028*** (0.011)
Conflict_provinces*Year2000	-0.083*** (0.013)	-0.089*** (0.013)	0.045 (0.029)	-0.100*** (0.017)	-0.015 (0.014)	-0.099*** (0.014)
R-squared	0.280	0.277	0.310	0.272	0.258	0.283
Number of observations	435411	406545	28866	310826	124585	435411
	B1. Males-Upper-secondary school					
Conflict_provinces*Year1990	-0.016 (0.014)	-0.017 (0.014)	-0.027 (0.061)	-0.021 (0.015)	-0.015 (0.017)	-0.019 (0.013)
Conflict_provinces*Year2000	-0.040** (0.013)	-0.042*** (0.016)	-0.065** (0.032)	-0.063*** (0.017)	0.010 (0.018)	-0.070*** (0.014)
R-squared	0.210	0.199	0.342	0.194	0.234	0.208
Number of observations	200982	186667	14315	136013	64969	200982
	B2. Females-Upper-secondary school					
Conflict_provinces*Year1990	-0.019** (0.010)	-0.020** (0.010)	0.0005 (0.027)	0.018 (0.010)	0.009 (0.017)	-0.019* (0.011)
Conflict_provinces*Year2000	-0.120*** (0.012)	-0.122*** (0.013)	-0.037 (0.033)	-0.124*** (0.013)	-0.059*** (0.018)	-0.118*** (0.014)
R-squared	0.269	0.264	0.308	0.253	0.269	0.271
Number of observations	264750	245910	18840	188125	76625	264750

Note: See note to Table 1. Panels (A1) and (A2) illustrate the estimation results for lower-secondary school outcomes with respect to children's gender. Panels (B1) and (B2) perform a similar task for upper-secondary school outcomes. While a specification in each column includes the same control variables that are also used for the specification reported in column (3) of Table 1, the analysis sample used in each column differs. With the attached column number, they are given as follows: (1) The entire sample used for the specification in Column (3) of Table 1 with no restriction introduced; (2) Short-term non-mover individuals; (3) Short-term mover individuals; (4) Long-term non-mover individuals; (5) Long-term mover individuals; and (6) Information on the head of household's provinces where they were born is used to categorize provinces as conflict and non-conflict provinces. For the definitions of long- and short-term movers and non-movers, see note to Table 2. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Province-Specific Time Trends

Even though the Census provides the only data set to obtain province-level estimates for Turkey, it was not originally designed to measure individuals' migration behavior, particularly not for those who have been exposed to conflict. Moreover, as noted before, because Census data record only one move for a fixed time period--such as within 5 years prior to the Census--based on individuals' retrospective information, it is unlikely that they could capture a comprehensive account of individuals' migration behavior, particularly for individuals exposed to conflict, who are likely to have multiple moves with multiple destinations. For this reason, I perform complementary empirical analyses to those implemented in the preceding section in order to further circumvent the contaminated effects of cross-province migration. In particular, I conduct several experiments that pool conflict provinces with non-conflict provinces from the single non-conflict region, which is chosen sequentially in descending order of its socio-economic development level. In other words, the first experiment considers provinces of the Marmara Region, which is the most developed region and is regarded as being comprised of non-conflict provinces; and the final experiment analyses the Black Sea Region, the least developed region, which constitutes non-conflict provinces in the sample.

These empirical exercises can also be seen as a sensitivity analysis of the estimates to the presence of province-specific time trends, which might be correlated with both educational outcomes and the onset and duration of conflict. In fact, it is possible that in addition to their time-invariant characteristics, provinces' time-varying characteristics may also systematically differ between conflict provinces and non-conflict provinces before the occurrence of conflict and during the ongoing conflict. Therefore, it is necessary to examine whether province-specific time trends may plague the conflict's estimated effects. Because the identification strategy of the DD framework relies on variations in exposure to conflict across provinces over time, we cannot simply add province-year interaction terms in a specification. In this regard, multiple comparisons of conflict provinces with different sets of non-conflict provinces provide another robustness analysis of the negative estimated impacts of conflict.[22]

[22] Alternatively, including province-age interaction terms in the regression model may account for province-specific trends. As shown in Table A8 of the Web Appendix, the estimation results of such a regression model convey evidence that a negative relation between exposure to conflict and educational outcomes may remain robust to the presence of province-specific trends. It is noteworthy, however, that because the analysis sample includes individuals in the narrowed age group, province-age interaction terms may not fully capture differential trends between conflict and non-conflict provinces.

As reported in Table 5, I begin this section of the robustness analysis by pooling conflict provinces with provinces of the Marmara and Aegean Regions that constitute the most developed parts of Turkey and that have experienced an influx of conflict-induced migrants because they have a greater stock of previously migrated individuals from conflict provinces. The conflict coefficients reported in columns (1) and (2) of Table 5 are very similar to those reported for the baseline specification in column (3) of Table 1. Moreover, these estimation results do not provide evidence for the presence of a channel through which a higher conflict-induced migration may adversely affect educational outcomes for these two regions' residents, reducing the conflict's negative estimated impacts. This table also shows that when non-conflict provinces are separately confined to those located in the Mediterranean Region (column (3)) and those in the Central Region (column (4)), the conflict coefficients remain robust, except the estimate for 1990 in the experiment, where provinces of the Mediterranean Region are included as non-conflict provinces in the sample.

Among these experiments, the most reliable one may come from the comparison in which provinces of the Black Sea Region serve as non-conflict provinces. Because after the East and Southeast Anatolia Regions that include conflict provinces, the Black Sea Region has the lowest socio-economic development level. In addition, its provinces were least affected by the inflow of conflict-induced migrants, owing to their low density of previous migrants from conflict provinces. As shown in column (5) of Table 5, keeping the Black Sea Region as non-conflict provinces increases the conflict coefficient, particularly for lower-secondary school outcomes, with the exception of the estimate for 1990. Overall, when the reported estimates from columns (1) to (5) are evaluated together, it seems that the conflict's estimated negative effects become somewhat larger and more significant when the conflict and non-conflict provinces resemble each other more in terms of their economics and social development, ruling out the possibility that the negative association between exposure to conflict and children's educational outcomes could be an artifact of differences in time trends across provinces.

In an effort to have a more direct test for the sensitivity of the estimated effects to differential, underlying province-specific time trends, I compare conflict provinces with non-conflict provinces in terms of their educational success measured in 1985. This allows for an assessment of whether the conflict coefficient is contaminated by having non-conflict provinces that serve as a control group with different initial educational outcomes at the beginning of the armed conflict for the relevant school-age children analyzed here. Matching conflict provinces to other provinces with similar initial educational success may provide more robust estimates of the conflict's impact, to the extent which cont-

Table 5 Robustness analysis of the impacts of conflict for non-conflict provinces of different regions

	(1)	(2)	(3)	(4)	(5)
	A. Lower-secondary school				
Conflict_provinces*Year1990	-0.026* (0.015)	-0.022** (0.009)	-0.011 (0.011)	-0.038*** (0.011)	-0.026*** (0.010)
Conflict_provinces*Year2000	-0.046** (0.011)	-0.046*** (0.011)	-0.054*** (0.013)	-0.070*** (0.012)	-0.076*** (0.012)
R-squared	0.246	0.249	0.245	0.289	0.275
Number of observations	236662	167953	178432	217658	191648
	B. Upper-secondary school				
Conflict_provinces*Year1990	-0.026** (0.011)	-0.018* (0.011)	-0.014 (0.011)	-0.030*** (0.011)	-0.014 (0.011)
Conflict_provinces*Year2000	-0.091*** (0.013)	-0.075*** (0.012)	-0.091*** (0.014)	-0.104*** (0.014)	-0.093*** (0.015)
R-squared	0.226	0.221	0.219	0.264	0.233
Number of observation	138904	98044	104316	127106	111891

Note: See note to Table 1. All specifications use the same set of variables as used in the baseline specification presented in column (3) of Table 1 with a different analysis sample. For each column, while the analysis sample includes the same set of conflict provinces, it includes a different set of non-conflict provinces. These non-conflict provinces with the associated column number are defined as follows. Non-conflict provinces are confined to those located in (1) the Marmara Region; (2) the Aegean Region; (3) the Mediterranean Region; (4) the Central Anatolian region; and (5) the Black Sea Region. The list of non-conflict provinces for each region is given in Table A2 of the Web Appendix. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

rolling for these initial differences may account for their differences in determinants of children's schooling behavior, which may also lead provinces to have differential education outcomes even in the absence of conflict over the last 30 years. When the 16-20 year old group is examined for lower-secondary outcomes, its provincial average of lower-secondary school completion rates in 1985 are used to match conflict provinces with non-conflict provinces with respect to their educational success. A similar procedure is applied to examine the upper-secondary outcome, except the provincial average of upper-secondary school completion rates is used as a measure of educational success of the 18-20 year old group for each province in 1985. To perform this empirical exercise for each educational outcome, I classify provinces into three groups based on their rank in the corresponding measure of their educational success: (i) lower-education provinces: those that rank in the bottom one-third of educational success; (ii) medium-education provinces: those that rank in the second one-third of educational success; and (iii) higher-education provinces: those that rank in the top one-third of educational success.[23]

Table 6 displays estimation results for this empirical exercise where non-conflict provinces in each column are chosen sequentially in the ascending order of provinces with respect to their baseline educational success measured in the 1985 Census. These results may convey enhanced support for negative estimates of the DD of the conflict's impacts if there is pattern in which the estimated impacts become more pronounced as I match conflict provinces to non-conflict provinces with similar baseline educational success. In contrast, the opposite pattern emerges in Table 6. Given the fact that all conflict provinces except Elazığ and Tunceli fall in the group of lower educational success, matching conflict provinces with other provinces with lower educational success but not exposed to the conflict in column (1) generates a statistically significant, negative link between exposure to conflict and children's educational outcomes. Yet, columns (2) and (3) illustrate that the estimates become larger, particularly for upper-secondary school outcomes, when non-conflict provinces are chosen from those with higher baseline educational success. One possible way to interpret this differential estimated impact of the conflict is that if the conflict is presumed to be a main driving force in differential educational outcomes between provinces after accounting for their differences in the DD framework, conflict may contribute to widening the gap in educational success between conflict provinces with lower initial educational success and non-conflict provinces with higher initial educational success.

[23] See Table A2 in the Web Appendix for further information on how each province is labeled with respect to how its educational success is measured using the 1985 Census data.

Table 6 Estimates of the impacts of conflict using different sets of non-conflict provinces with different levels of educational success in 1985: The robustness analysis of results to the existence of underlying province-specific time trends

	(1)	(2)	(3)
	A. Lower-secondary school		
Conflict_provinces*Year1990	-0.020** (0.009)	-0.015 (0.009)	-0.026** (0.011)
Conflict_provinces*Year 2000	-0.032*** (0.013)	-0.062*** (0.011)	-0.046*** (0.013)
R-squared	0.256	0.248	0.249
Number of observations	246420	289870	433023
	B. Upper-secondary school		
Conflict_provinces*Year1990	-0.014 (0.012)	-0.012 (0.011)	-0.028*** (0.011)
Conflict_provinces*Year2000	-0.046*** (0.015)	-0.087*** (0.013)	-0.010*** (0.013)
R-squared	0.211	0.216	0.239
Number of observation	132804	170819	226676

Note: See note to Table 1. All specifications use the same set of variables used in the baseline specification presented in column (3) of Table 1 with a different analysis sample. For each column, while I use the same set of conflict provinces, I include different sets of non-conflict provinces with varying degrees of educational success measured in 1985. These non-conflict provinces with the assigned column number are given as follows: (1) those with the lowest level of educational outcomes of interest; (2) those with the middle level of educational outcomes of interest; (3) those with the highest level of educational outcomes of interest. Note that the educational outcomes of interest are the lower-secondary school outcome for Panel A and the upper-secondary school outcome for Panel B. See the text for details. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

As a final robustness analysis to verify that the findings of a negative link between exposure to conflict and educational outcomes are not driven by province-specific preexisting trends that may be correlated with both the onset of conflict and educational outcomes, plaguing the conflict's estimated impacts, I repeat a similar DD estimation method for cohorts who had completed their schooling-age period before the onset of conflict. If the maintained identification assumption of the DD framework is valid in the sense that in the absence of conflict, there would be no differences in educational outcomes between conflict and non-conflict provinces, the conflict's estimated impacts, which are measured by interaction terms in specifications estimated above, are predicted to be zero. To perform this empirical test for the validity of the identification assumption, the sample is confined to individuals who were 21-25 years old in 1984 when the conflict begun, and thus were recorded as 37-41 years old in the 2000 Census, 16 years of the conflict. However, it should be noted that unlike the younger cohorts in the original analysis, for the 37-41 year olds it is more difficult, if not impossible, to derive their exact residential histories from the Census data to match their provinces of residences during their school years with those exposed to conflict. To be able to capture, at least partially, the mobility of these relatively older cohorts and account for its effects on estimates, I estimate the benchmark specification in column (3) of Table 1 by using different definitions of province-level mobility, a robustness analysis that is also performed for the 16-20 and 18-20 year old groups.

Table 7 reports the DD estimates of the conflict's impact on educational outcomes for the 37-41 year olds. As reported in column (1), where estimates are obtained with the use of information on individuals' province of residence at the time of the Census, there is no evidence of an association between exposure to conflict and educational outcomes for individuals who had already completed their school-age period for the educational outcomes studied before the onset of conflict in the provinces of Turkey's East Region. This evidence of no effect is also observed for both the short- and long-term non-movers in columns (2) and (4), respectively. Furthermore, for the long-term movers who were not exposed to conflict in their schooling years, the conflict's positive estimated impact provides supportive evidence that the main findings reported above cannot be attributed to the presence of province-specific preexisting trends. Therefore, with the exception of those shown in columns (3) and (6), the results of this placebo experiment provide evidence that the estimated negative impacts of conflict on the educational outcomes of younger cohorts who were exposed to conflict during their schooling period may not be attributed to differences in underlying trends in educational outcomes between conflict and non-conflict provinces.

Table 7 Placebo/Falsification experiment for the sample of individuals ages 37-41

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Lower-secondary school					
Conflict_provinces*Year1990	-0.004 (0.097)	0.002 (0.010)	-0.049*** (0.022)	-0.004 (0.009)	0.030* (0.017)	-0.009 (0.011)
Conflict_provinces*Year2000	-0.001 ((0.011)	-0.0002 (0.011)	-0.017 (0.022)	-0.017* (0.010)	0.033** (0.016)	-0.030*** (0.011)
R-Squared	0.097	0.099	0.086	0.123	0.076	0.104
Number of Observations	525793	489184	36609	353381	172412	525793
	B. Upper-secondary school					
Conflict_provinces*Year1990	-0.005 (0.008)	-0.002 (0.008)	-0.050*** (0.022)	-0.007 (0.008)	0.034*** (0.014)	-0.010 (0.009)
Conflict_provinces*Year2000	-0.004 (0.009)	0.002 (0.009)	-0.017 (0.022)	-0.012 (0.009)	0.038*** (0.013)	-0.026*** (0.010)
R-Squared	0.068	0.068	0.086	0.083	0.059	0.070
Number of Observations	525793	489184	36609	353381	172412	525793

Note: See notes in Tables 1 and 2. In each column except column (6), whereas the control variables include individuals' age (dummy variables for each age group), gender, and dummy variables for the provinces of residence at the time of the Census, two educational outcomes serve as dependent variables. Keeping in mind differences in the control variables across specifications, column (1) attempts to replicate the estimation results in column (3) of Table 1; similarly, columns (2)-(6) attempt to replicate those reported in columns (1)-(5). The analysis sample used in each column differs. With the respective column numbers, they are given as follows: (1) The entire sample used for a specification in Column (3) of Table 1 with no restriction introduced; (2) Short-term non-mover individuals; (3) Short-term mover individuals; (4) Long-term non-mover individuals; (5) Long-term mover individuals: The sample is restricted to individuals whose provinces where they were born are not same as their province of residence at the time of the Census; and (6) Information on individuals' provinces where they were born is used to categorize provinces as conflict and non-conflict provinces. For definitions of long- and short-term movers and non-movers, see note to Table 2. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

5. CONCLUSION

Mainly because of the political climate and data availability issues in Turkey, this study is the first attempt to answer the questions of whether and to what extent Turkey's 30 years of armed conflict may causally affect children's human capital outcomes, specifically, their educational attainments. The underlying identification strategy of the study is to relate geographical and time variations in exposure to conflict to those variations in educational outcomes. As a result of applying this identification strategy empirically, I provide robust evidence that exposure to conflict is negatively associated with the likelihood of completing lower-secondary school for individuals ages 16-20 years and that of completing upper-secondary school for those ages 18-20. In terms of heterogeneity of the estimated effects, I provide further evidence that the conflict's estimated impacts are greater for children exposed to conflict for a longer period, for female children, and for the likelihood of completing upper-secondary school. I also offer a credible set of suggestive evidence that the negative, significant link between exposure to conflict and children's educational outcomes are not driven by contemporaneous changes in migration or by the presence of province-specific trends that might be correlated with both the onset of conflict and educational attainment.

Several caveats must be taken into account, however, when interpreting the main finding of a negative association between exposure to conflict and educational outcomes as a causal impact of armed conflict in Turkey. The most important challenge to a causal interpretation of this finding comes from the possibility that the conflict's negative estimated effects may have arisen because of province-specific trends. To respond to this empirical challenge, I run several experiments in which conflict provinces are matched with non-conflict provinces that have varying degree of resemblances to conflict provinces in terms of their socio-economic development level and their education success during the pre-conflict period. Furthermore, I also apply the DD estimation strategy for the group of individuals who had completed their schooling years far before the time period in which the conflict occurred. As a result of these experiments, I provide evidence that a negative relation between exposure to conflict and education is not sensitive to the choice of non-conflict provinces with different underlying province-specific trends. It should be noted, however, that there exists no historical information--time-series data--on educational attainment measured at the province level from previous censuses or other sources of data. Such a data limitation compels me to apply somewhat ad-hoc criteria to design matching schemes in which conflict and non-conflict provinces with similar characteristics are categorized together. For this reason, it could be argued that the matching schemes used in the empirical analysis may not fully permit me to isolate the

conflict's causal impacts on educational outcomes from province-specific time trends that threaten the validity of the DD estimation method's identification assumption. Therefore, with this caveat in mind, evidences presented in this study should at least be considered as an empirical evaluation of the educational success of relatively younger cohorts who were exposed to conflict in their schooling years, compared to their peers who were residing in non-conflict provinces with possible varying trends in educational outcomes.

Furthermore, as well established in evaluation literature on quasi-experiment design, where economic, social, or political disruptions, such economic crisis, disasters, and wars, are used to identify the causal relation between variables, the unbiasedness and consistency of the DD estimates require a non-selective distribution of individuals across provinces over time. That is, when individuals are born or attend school, and where they live during their school-age years, must be determined independently from factors that might influence both their educational success and the onset of conflict. It is possible, however, that selective migration, presumably induced by conflict, may alter children's geographic distribution over time in terms of both their quantity and quality, biasing the DD estimates. Thus, to determine the extent to which biases in the conflict's estimated impacts are caused by conflict-induced migration behavior, I execute a detailed analysis of the consequences of using different sets of individuals' residential information reflecting different degrees of conflict exposure. The negative association between exposure to conflict and children's educational outcomes remains robust after controlling changes in individuals' migration propensities caused by conflict. In addition, the findings show that the negative effects of conflict are far greater for non-mover children than their counterparts, where these effects increase with the length of time in which the non-mover population resides in conflict provinces.[24]

Although these robustness analyses altogether indicate that armed conflict may be negatively related with children's educational outcomes, it must be

[24] It is noteworthy that similar to migration, changes in fertility and mortality behavior led by conflict might also contaminate estimated impacts of exposure to conflict by altering both the size and composition of the population of children who are exposed to conflict. In particular, to the extent that selections into fertility and mortality are determined by children's endowment, their familial resources and preferences, impacts of exposure to conflict may be estimated with a bias such that its direction depends on the nature of such selection mechanisms. Although the investigation of whether and to what extent conflict-induced changes in fertility and mortality behavior may influence the estimated impacts of conflict on educational outcomes is beyond the scope of the paper, mainly because of limitations imposed by the Census data, in the Web Appendix I provide a simple DD analysis of the number of children ages 0-15 as a fertility measure. As reported in Table A9 of the Web Appendix, the estimation results provide suggestive evidence that exposure to conflict may be positively related to fertility behavior and that there may exist a negative selection into fertility in the sense that the presence of conflict may cause households with lower socio-economic resources to have more children than those with higher resources.

acknowledged that comparing both provinces and cohorts with different degrees of exposure to armed conflict yields a lower bound of the conflict's effects on such outcomes. Indeed, if such a comparison is performed at two levels—that is, the province and cohort levels--, there might be several mechanisms that lead to an underestimation of the conflict's impacts on educational outcomes. In particular, two important mechanisms may be operating by diffusing the conflict itself and its negative consequences to other regions that are not exposed to conflict geographically. One such mechanism is that the migrant influx from conflict provinces to the rest of the country, particularly that caused by forced displacement, may alter both the labor market and educational outcomes for local populations in non-conflict provinces, depending on the extent to which conflict-induced migrants compete for local resources with residents of host provinces. More importantly, increases in military expenditures accompanied by reductions in development-oriented expenditures, such as those related to education and health, may influence individuals throughout the country regardless of where they resided during the geographically concentrated conflict. Therefore, given the possibility of the conflict's general equilibrium effects, which may also arise because of its spillover effects, the DD estimates based on the conflict's partial equilibrium analysis may appear to be lower than the conflict's true causal effects on educational outcomes.

Furthermore, whether an individual was exposed to conflict during her schooling period hinges on a dual-dichotomy that enables us to capture spatial and time (cohort) variations in exposure to conflict. The first one is built on where she spent her school-age years (conflict or non-conflict province). The second one, in contrast, is drawn from information regarding when she was expected to complete a given educational degree (before the onset of conflict or 6 or 16 years after the onset of conflict). Within this framework, to the extent that such a crude measure of conflict exposure may measure the actual exposure to conflict with an error, the conflict's negative estimated effects are further biased towards zero.

Another concern regarding the magnitude of the negative DD estimates may arise from the fact that these estimates are based on comparisons of educational outcomes (for both conflict and non-conflict provinces) before and after the onset of conflict. The historical roots of internal armed conflict in Turkey, however, can be traced back to the late Ottoman Empire period and the early-period Republic of Turkey. Indeed, even in periods before the clashes between the Turkish state and the PKK started, conflict provinces' residents, mainly their Kurdish population, had been subject to varying degrees of repression of their political and cultural rights, which might have had negative consequences for their educational outcomes. In this regard, as an estimation

strategy, exploiting changes in exposure to conflict over time in such a socio-political historical setting of Turkey may lead to negative DD estimates that are less than the actual negative impacts of the conflict on educational outcomes. Thus, when evaluating these caveats that point out that the estimation results of DD method may underestimate impacts of the conflict on educational outcomes, the DD estimates should be considered as a lower bound on the magnitude of a statistical association between exposure to conflict and educational outcomes.

Finally, from a public-policy perspective, it is noteworthy that in addition to their lower educational outcomes, I obtain evidence, reported in the Web Appendix, suggesting that children who are exposed to conflict are less likely to work and more likely to be idle.[25] Therefore, by lowering the opportunity cost of being involved in armed conflict, the conflict itself may create an environment in which both its causes and consequences become permanent, leading Turkey to fall into trap of low human capital-conflict, as well as that of poverty-conflict, considering that human capital outcomes are the most important determinant of poverty. In this regard, Turkey must continue its economic and social reforms that have been implemented in the last decade or launch new ones, if necessary, to end this armed conflict, remove the conditions that may trigger its reemergence, and restore damages it may have caused, particularly for human capital outcomes that may have lingering effects through their intergenerational transmission mechanisms.

[25] For a detailed discussion, see Table A10 along with its explanation located in the Web Appendix.

REFERENCES

ALMOND, D. (2006), Is the 1918 Influenza Pandemic over? Long-term effects of in utero influenza exposure in the post-1940 U.S. population. Journal of Political Economy 114 (4), 672-712.

ALMOND, D., EDLUND, L., Li, H., ZHANG, J. (2007), Long-term effects of the 1959-1961 China Famine: Mainland China and Hong Kong. NBER Working Paper No: 13384.

AKBULUT-YÜKSEL, M. (2009), The long-run effects of large scale physical destruction and warfare on children. HiCN Working Paper No. 62

AKRESH, R., WALQUE, D. (2008), Armed conflict and schooling: Evidence from the 1994 Rwandan Genocide. HiCN Working Paper No.47.

BECKER, S. G., LEWIS, G. L. (1973), On the interaction between the quantity and quality of children, Journal of Political Economy 81, S279-S288.

BELLOWS, J., MIGUEL, E. (2008), War and institutions: New evidence from Sierra Leone. American Economic Review 96 (2), 394-399.

BERKER, A. (2009), The impact of internal migration educational outcomes: Evidence from Turkey. Economics of Education Review 28 (6), 739-749.

BİLA, F. (2004), Satranç tahtasındaki yeni hamleler: Hangi PKK? Ümit Yayıncılık, Ankara.

BLATTMAN, C., MIGUEL, E. (2009), Civil war. NBER Working Paper No. 14801

COLLIER, P., HOEFFLER, A. (1998), On economic causes of civil war. Oxford Economic Papers 50 (4), 563-573.

BLEAKLEY, H., LANGE, F. (2009), Chronic disease burden and the interaction of education, fertility and growth. Review of Economics and Statistics 91 (1), 52-65.

BORJAS, J. G. (2003), The labor demand curve is downward sloping: Reexamining the impact of immigration on the labor market. The Quarterly Journal of Economics 118 (4), 135-1374.

BOZARSLAN, H. (2001), Human rights and the Kurdish issue in Turkey: 1984-1999. Human Rights Review 3 (1), 45-54.

BOZARSLAN, M. E. (2002), Dogu'nun sorunları. Avesta. İstanbul.

BUNDERVOET, T.,VERWIMP, P., AKRESH, R. (2009), Health and civil war in rural Burundi. Journal of Human Resources 44 (2), 536-563.

CHAMARBAGWALA, R., MORAN, H. E. (2007), The human capital consequences of civil war: Evidence from Guatemala. HiCN Working Paper No. 59.

COLLIER, P., Elliot, V. L., HEGRE, H., HOEFFLER, A., REYNAL-QUREOL, M. and SAMBANIS, N. (2003), Breaking the conflict trap: Civil war and development policy. World Bank. Washington.

COLLIER, P., HOEFFLER, A. (2004), Greed and grievance in civil war. Oxford Economic Papers 50 (4), 563-573.

COULIBALY, S., DEICHMANN, U., and LALL, S. (2007), Urbanization and productivity: Evidence from Turkish provinces over the period 1980-2000. World Bank, Policy Research Working Paper No: 4327.

CURRIE, J., MORETTI, E. (2003), Mother's education and intergenerational transfer of human capital: Evidence from college openings. The Quarterly Journal of Economics 118 (4), 1495-1532.

DEININGER, K. (2003), Causes and consequences of civil strife: Micro-level evidence from Uganda. Oxford Economic Papers 55 (4), 579-606.

GOULD, D. E., LAVY, V., PASERMAN, M. D. (2009), Sixty years after the magic carpet ride: The long-run effect of the early childhood environment on social and economic outcomes. NBER Working Paper No. 14884.

GUERRERO-SERDAN, G. (2009), The effects of the war in Iraq on nutrition and health: An analysis using anthropometric outcomes of children. HiCN Working Paper No. 55.

HASSANPOUR, A., MOJAB, S. (2005), Kurdish Diaspora. In: M. Ember, C. R. Ember, I. Skoggard (eds.) Encyclopedia of diasporas: Immigrant and refugee cultures around the world. Volume I: Overviews and topics. Springer, New York. pp. 73-89.

Hacettepe Üniversitesi Nüfus Etütleri Enstitüsü (HUNEE). (2006), Türkiye göç ve yerinden olmuş nüfus araştırması (TGYONA). İsmat Matbaacılık ve Yayıncılık, Ankara.

HEPER, M. (2007), State and Kurds in Turkey: The question of assimilation. Palgrave Macmillan, New York.

FİLİZTEKİN, A., GÖKHAN, A. (2008), The determinants of internal migration in Turkey. Working Paper. Sabanci University. İstanbul

IZADY, R. M. (1992), The Kurds: A concise handbook. Crane Russak, Washington.

ICHİNO, A., WİNTER-EBMER, R. (2004), The long-run educational cost of World War II. Journal of Labor Economics 22 (1), 57-85.

JACOBY, T. (2005), Semi-authoritian incorporation and autocratic militarism in Turkey. Development and Change 36 (4), 641-665.

JUSTİNO, P. (2007), On the links between violent conflict and household poverty: How much do we really know? MICROCON Research Working Paper No.1.

KİRİŞCİ, K., WİNROW, M. G. (1997), The Kurdish question and Turkey: An example of a trans-state ethnic conflict. Frank Cass & Co. Ltd, London.

KOC, I., HANCIOĞLU, A., CAVLİN, A. (2008), Demographic differentials and demographic differential of Turkish and Kurdish populations in Turkey. Population Researhc and Policy Review 27(4), 447-457.

KURBAN, D., YÜKSEKER, D., ÇELİK B. A., ÜNALAN T., AKER, A. T. (2006), Zorunlu göç ile yüzleşmek: Türkiye'de yerinden edilme sonrası vatandaşlığın inşası. TESEV Yayınları, İstanbul.

LAİ, B., THYNE, C. (2007), The effect of civil war on education, 1980-1997. Journal of Peace Research 44 (3), 277-292.

MCDOWALL, D. (2007), A modern history of the Kurds. I. B. Tauris, New York.

MERROUCHE, Q. (2006), The long-term educational cost of war: Evidence from landmine contamination in Cambodia. IFS Working Paper Series No. W06/11.

MIGUEL, E., SATYANATH, S., SERGENTI. E. (2004), Economic shock and civil conflict: An instrumental variables approach. Journal of Political Economy 112 (4), 725-753.

MIQUEL, E., GERARD R. (2004), The long run impact of bombing Vietnam. Working Paper, Depertmanet of Economics, Universtiy of California, Berkley.

MUTLU, S. (1996), Ethnic Kurds in Turkey: A demographic study. International Journal of Middle Eastern Studies 28 (4), 517-541.

MUTLU, S. (2002), Dogu sorunun kökenleri: ekonomik açıdan. Ötüken Neşriyat, İstanbul.

SHEMYAKINA, O. (2006), The effect of armed conflict on accumulation of schooling: results from Tajikistan. HiCN Working Paper No. 21.

SWEE, E. L. (2009), On war and schooling attainment: The case of Bosnia and Herzegovina. HiCN Working Paper No. 57.

TBMM. (1998), Doğu ve Güneydoğu Anadolu'da boşaltılan yerleşim birimleri nedeniyle göç eden yurttaşlarımızın sorunlarının araştırlarak alınması gereken tedbirlerin tespit edilmesi amacıyla kurulan meclis araştırma komisyonu raporu. Tutanak Dergisi 53, Dönem 20.

YOUNG, A. (2005), The gift of the dying: The tragedy of AIDS and welfare of future African generation. The Quarterly Journal of Economics 120 (2), 467-515.

WEB APPENDIX

Table A1 Descriptive statistics for some basic characteristics of conflict and non-conflict provinces from the 1985 Turkish Census.

	Percentage of working-age population who are employed	Percentage of employed population in agricultural sector	Percentage of employed population in manufacturing sector	Percentage of urban population	Percentage of population over 25 years old without an upper-secondary school degree	Percentage of population over 25 years old with an upper-secondary school degree or higher
Conflict provinces	0.723 (0.055)	0.743 (0.077)	0.033 (0.021)	0.386 (0.092)	0.946 (0.016)	0.054 (0.016)
Non-conflict provinces	0.648 (0.098)	0.521 (0.021)	0.125 (0.088)	0.533 (0.225)	0.899 (0.052)	0.100 (0.052)

Note: Author's calculation based on the 1985 Turkish Census. The working-age population includes individuals ages 25-65. For the estimates of employment figures in both agricultural and manufacturing sectors, the universe is composed of individuals ages 25-65 who are employed. Standard deviations are given in parantheses.

Table A1 Descriptive statistics for some basic characteristics of conflict and non-conflict provinces from the 1985 Turkish Census.

Provinces	Regrouping of provinces	Province code	Ranking of provinces with respect to their lower-secondary-school-completion rates in 1985 (Among the 16-20 age group)	Ranking of provinces with respect to their upper-secondary-school completion rates in 1985 (Among the 18-20 age group)
A. Conflict-provinces				
Adıyaman		2	Lower Education	Lower Education
Bingol		12	Lower Education	Lower Education
Bitlis		13	Lower Education	Lower Education
Diyarbakir		21	Lower Education	Lower Education
Elazıg		23	Higher Education	Higher Education
Hakkari	Hakkari= Hakkari (30)+Batman (72)+Şırnak(73)+Siirt (56)+Mardin(47)	30	Lower Education	Lower Education
Muş		49	Lower Education	Lower education
Tunceli		62	Medium Education	Medium Education
Van		65	Lower Education	Lower Education
B. Non-conflict provinces				
B1. Eastern region				
Ağrı		4	Lower Education	Lower Education
Erzincan		24	Medium Education	Medium Education
Erzurum		25	Lower Education	Lower Education
Kars	Kars=Kars (36)+Ardahan (75)+Iğdır(76)	36	Medium Education	Medium Education
Malatya		44	Higher Education	Medium Education
B2. Southeastern Region				
Gaziantep	Gaziantep=Gaziantep(27)+Kilis(79)	27	Lower Education	Lower Education
Şanlıurfa		63	Lower Education	Lower Education
B3. Marmara Region				
Balıkesir		10	Higher Education	Higher Education

Bilecik		11	Higher Education	Higher Education
Bursa		16	Higher Education	Higher Education
Çanakkale		17	Medium Education	Medium Education
Edirne		22	Higher Education	Higher Education
İstanbul	İstanbul= İstanbul(34)+Yalova(77)	34	Higher Education	Higher Education
Kırklareli		39	Higher Education	Higher Education
Kocaeli		41	Higher Education	Higher education
Sakarya		54	Medium Education	Medium Education
Tekirdag		59	Medium Education	Higher Education
B4. Aegean Region				
Afyonkarahisar		3	Higher Education	Medium Education
Aydın		9	Medium Education	Higher Education
Denizli		20	Medium Education	Medium Education
İzmir		35	Higher Education	Higher Education
Kütühya		43	Medium Education	Lower Education
Manisa		45	Lower Education	Lower Education
Muğla		48	Medium Education	Medium Education
Uşak		64	Medium Education	Medium Education
B5. Mediterranean region				
Adana	Adana=Adana(1)+Osmaniye(80)	1	Higher Education	Higher Education
Antalya		7	Medium Education	Medium Education
Burdur		15	Higher Education	Higher Education
Hatay		31	Higher Education	Medium Education
Isparta		32	Higher Education	Higher Education
Kahramanmaraş		46	Lower Education	Medium Education
Mersin/İçel		33	Higher Education	Medium Education
B6. Central Anatolia region				
Ankara	Ankara=Ankara(6)+Kırıkkale(71)	6	Higher Education	Higher Education
Çankırı		18	Medium Education	Medium Education
Eskişehir		26	Higher Education	Higher Education
Kayseri		38	Higher Education	Higher Education

Kırşehir		40	Higher Education	Higher Education
Konya	Konya=Konya(42)+Karaman(70)	42	Medium Education	Medium Education
Nevşehir		50	Medium Education	Medium Education
Niğde	Niğde=Nigde(51)+Aksaray(68)	51	Lower Education	Lower Education
Sivas		58	Medium Education	Lower Education
Yozgat		66	Lower Education	Lower Education
B7. Black Sea region				
Amasya		5	Medium Education	Higher Education
Artvin		8	Higher Education	Higher Education
Bolu	Bolu=Bolu(14)+Düzce(81)	14	Medium Education	Medium Education
Çorum		19	Lower Education	Lower Education
Giresun		28	Medium Education	Medium Education
Gümüşhane	Gümüşhane=Gümüşhane(29)+Bayburt(69)	29	Lower Education	Lower Education
Kastamonu		37	Lower Education	Lower Education
Ordu		52	Lower Education	Medium Education
Rize		53	Medium Education	Medium Education
Samsun		55	Lower Education	Medium Education
Sinop		57	Lower Education	Lower Education
Tokat		60	Lower Education	Lower Education
Trabzon		61	Higher Education	Higher Education
Zonguldak	Zonguldak=Zonguldak(67)+Bartın(74)+Karabük (78)	67	Medium Education	Lower Education

Note: For details on the reclassification of provinces, see pages 10-11, particularly footnote 7, in the main text. Furthermore, in this table, each province is marked with respect to its rank on each form of educational success measured in 1985 as follows: lower-education provinces, medium-education provinces, and higher-education provinces. For details of how these three groups are defined, see page 26 in the main text.

Table A3 Estimates of the impacts of conflict on the two educational attainments: Analysis samples include all children regardless of whether they were residing in a household setting.

	(1)	(2)	(3)
	A. Lower-secondary school		
Conflict_provinces*Year1990	-0.022*	-0.025***	-0.027***
	(0.013)	(0.010)	(0.009)
Conflict_provinces*Year 2000	-0.042***	-0.055***	-0.062***
	(0.016)	(0.012)	(0.011)
R-squared	0.085	0.166	0.171
Number of observations	965949	965949	965949
	B. Upper-secondary school		
Conflict_provinces*Year1990	-0.022*	-0.027***	-0.026***
	(0.013)	(0.010)	(0.010)
Conflict_provinces*Year 2000	-0.053***	-0.069***	-0.072***
	(0.016)	(0.013)	(0.013)
R-squared	0.102	0.182	0.182
Number of observations	588319	588139	588319
Control variables			
Province dummies	YES	YES	YES
Year dummies (1990, 2000)	YES	YES	YES
Children's characteristics	NO	YES	YES
Children's interacted with year dummies	NO	NO	YES

Note: Panel A presents the estimation results of lower-secondary school outcomes for the 16-20-years-old children, whereas Panel B presents those of upper-secondary school outcomes for the 18-20-year-old individuals. All analysis samples include the entire population of children regardless of whether they were residing in a household setting during the time at which the Census was conducted. Each column shows the estimation of results of a separate specification whose independent variables are indicated in the respective column. For specifications in columns (2) and (3), children's characteristics include the child's age (dummy variables for each age group), gender, and urban status. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Table A4 Descriptive statistics of variables used in the analysis of lower-secondary- school outcomes for the sample of individuals ages 16-20: Turkish Censuses 1985, 1990, and 2000.

	Conflict provinces			Non-conflict provinces		
Variables	1985	1990	2000	1985	1990	2000
Age ⊗	18.05 (1.45)	17.87 (1.43)	17.98 (1.419	17.92 (1.41)	17.85 (1.39)	17.93 (1.39)
Female	0.555 (0.497)	0.563 (0.495)	0.543 (0.498)	0.550 (0.497)	0.545 (0.498)	0.531 (0.499)
Household head's child or not	0.673 (0.468)	0.707 (0.454)	0.756 (0.428)	0.713 (0.451)	0.718 (0.449)	0.743 (0.437)
Household head's education: missing	0.0001 (0.011)	0.0008 (0.028)	0.0004 (0.020)	0.0002 (0.016)	0.0004 (0.021)	0.0001 (0.012)
Household head's education: No formal schooling	0.591 (0.491)	0.518 (0.499)	0.402 (0.411)	0.344 (0.475)	0.263 (0.440)	0.166 (0.372)
Household head's education: Primary-school graduate	0.333 (0.471)	0.386 (0.487)	0.412 (0.492)	0.539 (0.498)	0.588 (0.492)	0.576 (0.494)
Household head's education: Middle-school graduate	0.032 (0.177)	0.041 (0.199)	0.076 (0.264)	0.040 (0.196)	0.055 (0.228)	0.091 (0.288)
Household head's education: High-school graduate	0.032 (0.177)	0.036 (0.188)	0.083 (0.276)	0.050 (0.219)	0.057 (0.232)	0.107 (0.309)
Household head's education: 2- or 4-year university graduate	0.011 (0.104)	0.016 (0.126)	0.026 (0.159)	0.024 (0.155)	0.035 (0.185)	0.058 (0.234)
Household head's age	47.70 (12.46)	47.82 (12.24)	48.15 (11.80)	47.27 (11.42)	46.90 (11.41)	46.38 (11.01)
Household size	9.11 (4.53)	9.17 (5.15)	9.50 (4.80)	6.74 (3.77)	6.47 (3.01)	6.08 (3.27)
Urban	0.396 (0.489)	0.46 (0.498)	0.567 (0.495)	0.514 (0.499)	0.587 (0.492)	0.657 (0.47)
Number of observations	21725	26507	35209	209321	23522	274795

Note: The analysis sample is composed of individuals ages 16-20. For each Census year, it provides descriptive statistics of children and household variables that are included in estimating the baseline specification in column (3) of Table 1. Standard deviations are given in parentheses.

⊗ In the entire regression analysis executed in the paper, dummy variables are used to capture each individual age group. See Table A7 for details.

Table A5 Descriptive statistics of variables used in the analysis of upper-secondary-school outcomes for the sample of individuals ages 18-20: Turkish Censuses 1985, 1990, and 2000.

	Conflict provinces			Non-conflict provinces		
Variables	1985	1990	2000	1985	1990	2000
Age ⊗	19.02 (0.87)	18.93 (0.86)	18.95 (0.834)	18.97 (0.83)	18.91 (0.83)	18.94 (0.82)
Female	0.587 (0.492)	0.601 (0.48)	0.562 (0.496)	0.578 (0.493)	0.577 (0.493)	0.553 (0.497)
Household head's child or not	0.587 (0.492)	0.625 (0.484)	0.708 (0.454)	0.642 (0.479)	0.653 (0.475)	0.687 (0.463)
Household head's education: missing	0.0001 (0.012)	0.0007 (0.026)	0.0005 (0.023)	0.0002 (0.016)	0.0003 (0.019)	0.0002 (0.013)
Household head's education: No formal schooling	0.599 (0.490)	0.529 (0.499)	0.411 (0.492)	0.348 (0.476)	0.268 (0.443)	0.172 (0.377)
Household head's education: Primary-school graduate	0.324 (0.468)	0.376 (0.484)	0.407 (0.491)	0.531 (0.499)	0.577 (0.493)	0.570 (0.495)
Household head's education: Middle-school graduate	0.031 (0.174)	0.040 (0.197)	0.074 (0.261)	0.041 (0.198)	0.056 (0.230)	0.091 (0.287)
Household head's education: High-school graduate	0.033 (0.181)	0.037 (0.188)	0.081 (0.274)	0.054 (0.225)	0.062 (0.230)	0.109 (0.312)
Household head's education: 2- or 4-year university graduate	0.011 (0.103)	0.016 (0.125)	0.025 (0.156)	0.025 (0.155)	0.034 (0.573)	0.056 (0.23)
Household head's age	47.53 (13.14)	47.96 (12.86)	48.36 (12.26)	47.31 (12.10)	47.11 (12.04)	46.42 (11.68)
Household size	9.11 (4.53)	9.14 (5.66)	9.54 (4.89)	6.70 (3.78)	6.40 (3.07)	6.08 (3.35)
Urban	0.385 (0.486)	0.458 (0.4989	0.561 (0.496)	0.513 (0.499)	0.587 (0.492)	0.657 (0.474)
Number of observations	13491	15189	21476	121048	133036	161685

Note: The analysis sample is composed of individuals ages 18-20. For each Census year, it provides descriptive statistics of children and household variables that are included in estimating the baseline specification in column (3) of Table 1. Standard deviations are given in parentheses.

⊗ In the entire regression analysis executed in the paper, dummy variables are used to capture each individual age group. See Table A7 for details.

Table A6 A descriptive framework of the difference-in-differences (DD) analysis.

Time variation / Spatial Variation	Before the conflict (Census 1985)	During the conflict-1 (Census 1990)	During the conflict-2 (Census 2000)	Differences between before and during the conflict-1 (Differences between 1990 and 1985 Censuses)	(Differences between before and during the conflict 2) Differences between the 2000 and 1985 Censuses
	A. Descriptive framework				
Conflict provinces	Y_{conf}^{1985}	Y_{conf}^{1990}	Y_{conf}^{2000}	$Y_{conf}^{1990} - Y_{conf}^{1985}$	$Y_{conf}^{2000} - Y_{conf}^{1985}$
Non-conflict provinces	$Y_{non\_conf}^{1985}$	$Y_{non\_conf}^{1990}$	$Y_{non\_conf}^{2000}$	$Y_{non\_conf}^{1990} - Y_{non\_conf}^{1985}$	$Y_{non\_conf}^{2000} - Y_{non\_conf}^{1985}$
Differences between conflict and non-conflict provinces	$Y_{conf}^{1985} - Y_{non\_conf}^{1985}$	$Y_{conf}^{1990} - Y_{non\_conf}^{1990}$	$Y_{conf}^{2000} - Y_{non\_conf}^{2000}$	$[(Y_{conf}^{1990} - Y_{non\_conf}^{1990})-(Y_{conf}^{1985} - Y_{non\_conf}^{1985})]$	$[(Y_{conf}^{2000} - Y_{non\_conf}^{2000})-(Y_{conf}^{1985} - Y_{non\_conf}^{1985})]$
	B. Lower-secondary school				
Conflict provinces	0.173 (0.378)	0.193 (0.395)	0.356 (0.479)	*0.020* *(0.003)*	*0. 183* *(0.004)*
Non-conflict provinces	0.294 (0.455)	0.340 (0.474)	0.527 (0.499)	*0.046* *(0.001)*	*0.233* *(0.001)*
Differences between conflict and non-conflict provinces	*-0.121* *(0.003)*	*-0.147* *(0.003)*	*-0.171* *(0.003)*	**-0.026** **(0.004)**	**-0.050** **(0.004)**
	C. Upper-secondary school				
Conflict provinces	0.089 (0.284)	0.107 (0.309)	0.232 (0.423)	*0.018* *(0.003)*	*0.143* *(0.004)*
Non-conflict province	0.169 (0.374)	0.209 (0.407)	0.396 (0.489)	*0.040* *(0.001)*	*0.227* *(0.002)*
Differences between conflict and non-conflict provinces	*-0.080* *(0.003)*	*-0.102* *(0.003)*	*-0.164* *(0.003)*	**-0.022** **(0.005)**	**-0.084** **(0.005)**

Note: In Panel A, the sample is restricted to children ages 16-20 for the analysis of lower-secondary school outcomes, whereas the sample is restricted to those ages 18-20 for the analysis of upper-secondary school outcomes. Simple difference estimates and their standard errors are given in italics. Difference-in-differences estimates with their standard errors are given in bold. Standard deviations reported in other cells are given in parentheses.

A Brief Explanation of Table A6

Before delving into the regression results, it would be insightful to give a descriptive account of what exactly the DD estimation method accomplishes in terms of measuring the conflict's impact on children's educational outcomes. Table A6 consists of three separate panels, where Panel A lays out the notational structure of what exactly the corresponding cells in the following two panels measure. In Panel B, the first three rows present the average completion rate of lower-secondary school among 16-20 year olds differentiated by provinces with respect to whether they were exposed to conflict or not, using individual-level data. Educational outcomes of interest reported in column (1), for example, convey the information for educational outcomes before the conflict; columns (2) and (3) convey the same information measured 6 years and 16 years after the conflict's onset, respectively. Panel C illustrates similar information for the average completion rate of upper-secondary school among 18-20 year olds. For each panel, the last rows of columns (1)-(3) and (5) yield the simple difference estimates of educational outcomes between the conflict and non-conflict provinces in a specified year. Estimates of simple difference provide evidence of a widening gap between the conflict and non-conflict provinces over the 1985 to 2000 period, increasing in absolute terms from -0.121 percentage points in 1985 to -0.171 percentage points in 2000. Additional cells in the same row, marked in bold, provide DD estimates of the conflict's impacts on educational outcomes. Similarly, the column-wise calculations made for the simple differences reported in the first two rows of columns 4 and 5 yield the same DD estimates. All simple difference and difference-in-differences estimates are statistically significant at the 1-percent level, and standard errors are given in parentheses.

Table A7 Full regression results for the sample individuals ages 16-20 and those ages 18-20: Lower-secondary- and upper-secondary-school outcomes.

Variables	Lower-secondary school (Age group 16-20)	Upper-secondary school (Age group 18-20)
Conflict_provinces*Year1985 (Reference interaction term)	-	-
Conflict_provinces*Year1990	-0.021*** (0.008)	-0.018** (0.010)
Conflict_provinces*Year2000	-0.047*** (0.010)	-0.083*** (0.012)
Year 1985 (Reference year)	-	-
Year1990	0.013*** (0.004)	0.013*** (0.0039
Year2000	0.135*** (0.006)	0.145*** (0.004)
Age 16 (Reference group)	-	-
Age 17	0.076*** (0.011)	-
Age 18 (Reference group)	0.072*** (0.011)	-
Age 19	0.075*** (0.011)	0.025*** (0.002)
Age 20	0.084*** (0.011)	0.064*** (0.003)
Female	-0.141*** (0.007)	-0.078*** (0.006)
Household head's child or not	0.098*** (0.007)	0.093*** (0.005)
Household head's education: missing (Reference group)	-	-

Household head's education: No formal schooling	-0.337*** (0.061)	-0.344*** (0.064)
Household head's education: Primary- school graduate	-0.181*** (0.059)	-0.023*** (0.064)
Household head's education: Middle- school graduate	0.035 (0.057)	-0.063 (0.064)
Household head's education: High-school graduate	0.128** (0.056)	0.085 (0.064)
Household head's education: 2- or 4-year university graduate	0.200*** (0.055)	0.220*** (0.064)
Household head's age	0.005*** (0.0002)	0.004*** (0.0002)
Household size	-0.008*** (0.001)	-0.007*** (0.001)
Urban	0.181*** (0.005)	0.134*** (0.004)
Number of observations	802487	465732

Note: For the analysis of both lower- and upper-secondary school outcomes, the table shows the estimates of coefficients for all variables used in estimating baseline specification in column (3) of Table 1. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Table A8 Estimates of the impacts of conflict on two educational attainments: Controlling for differential trends across provinces.

	(1)	(2)
	A. Lower-secondary school	
Conflict_provinces*Year1990	0.022*** (0.009)	-0.016* (0.009)
Conflict_provinces*Year 2000	-0.047*** (0.011)	-0.042*** (0.013)
R-squared	0.2544	0.261
Number of observations	802487	802487
	B. Upper-secondary school	
Conflict_provinces*Year1990	-0.020** (0.010)	-0.011 (0.009)
Conflict_provinces*Year 2000	-0.084*** (0.013)	-0.052*** (0.014)
R-squared	0.240	0.243
Number of observations	465732	465732
Control variables		
Province dummies	YES	YES
Year dummies (1990, 2000)	YES	YES
Children's characteristics	YES	YES
Head of household characteristics	YES	YES
Children's age dummies interacted with province dummies	YES	YES
Children's and head's characteristics interacted with year dummies	NO	YES

Note: Panel A presents the estimation results of lower-secondary school outcomes for the 16-20-year-old children, whereas Panel B presents those of upper-secondary school outcomes for the 18-20-year-old individuals. Each column shows the estimation of results of a separate specification whose independent variables are indicated in the respective column. The specification in column (1) augments the baseline specification in column (3) of Table 1 with the inclusion of interaction terms between children's age dummies and province dummies. The specification in column (2) performs a similar task as the specification in column (4) of Table 1. See note to Table 1 for details. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Table A9 Estimates of the impacts of conflict on the number of children ages 0-15 in a household.

	Lower-secondary school graduate population (Ages 16-20)	Upper-secondary school graduate population (Ages 18-20)
	Number of children ages 0-15 in a household	Number of children ages 0-15 in a household
	A. All heads of household	
Conflict_provinces*Year1990	0.043***	0.037
	(0.025)	(0.025)
Conflict_provinces*Year2000	0.100***	0.094***
	(0.032)	(0.031)
R-squared	0.177	0.177
Number of observations	571700	321024
	B. Heads of household with less than an upper-secondary school degree	
Conflict_province*Year1990	0.045**	0.040*
	(0.024)	(0.023)
Conflict_provinces*Year2000	0.108***	0.105***
	(0.031)	(0.03)
R-squared	0.174	0.175
Number of observations	524775	295503
	C. Heads of household with an upper-secondary school degree or more	
Conflict_provinces*Year1990	-0.010	-0.036
	(0.074)	(0.077)
Conflict_province*Year2000	0.049	0.051
	(0.071)	(0.072)
R-squared	0.148	0.135
Number of observations	46925	25521

Note: For the sample of individuals ages 16-20 for whom I examine their lower-secondary school outcomes and the sample of those ages 18-20 for whom I examine their upper-secondary school outcomes, the logarithmic transformation of the number of children ages 0-15 in a household serves as a dependent variable, whereas independent variables include the interactions terms, a set of province dummy variables, and year dummies for 1990 and 2000 in each specification reported in the columns above. While Panel (A) presents for all heads of household, Panels (B) and (C) break down the estimated effects by the household head's educational level. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

A Brief Explanation of Table A9

In the main text, I provide evidence that even after controlling for conflict-induced migration, exposure to conflict seems to have negative impacts on children's educational outcomes. Similar to changes in migration behavior, it is possible that changes in fertility and mortality behaviors led by conflict may also contaminate estimated impacts of exposure to conflict by altering both the size and composition of the population of children who are exposed to conflict. In particular, to the extent that selections into fertility and mortality are related to the distribution of children's endowments and that of their familial resources and preferences, impacts of exposure to conflict may be estimated with a bias such that its direction depends on the nature of selection. For instance, families with higher levels of resources and/or higher preferences for children with high quality may choose not to have a/n (additional) child because of the insecure and less-education-prone environment caused by the onset of conflict, leading to an overestimation of the conflict's impacts. Such an upward bias is also expected to be found in the case of positive selection of mortality, in which children with a higher ability may decide to join the insurgent group and lose their lives in combat. Furthermore, as was first formally hypothesized by Becker and Lewis (1973) and later empirically tested by Bleakley and Lange (2009) in the context of the eradication of hookworm disease from the American South, by Young (2005) in the context of the spread of AIDS through Africa, and by Currie and Moretti (2003) in the context of new college openings in US, changes in returns to human capital investment resulting from these peculiar economic and social episodes may alter the trade-off between child quantity and quality, thus leading to observed significant changes in both the number of children and the amount of human capital investment made for children. In this regard, it is also possible that by depressing expected returns to human capital investment and increasing its uncertainties, conflict may influence this trade-off in such a way that families may be inclined to have more children and invest less in their education.

While Census data offer many avenues to explore the possible consequences of conflict-induced migration on estimates of the conflict's impacts on children's educational outcomes, it provides a very limited scope to gauge the extent of biases in the estimated impacts because of changes in fertility and mortality that are presumably caused by the onset of conflict. Most importantly, Census data do not make it possible to disentangle the effects of changes in fertility and mortality on the conflict's estimated impacts. Keeping this drawback in mind, I attempt to provide suggestive evidence on net changes in these two demographic behaviors by continuing to apply the DD estimation method where the logarithmic transformation of the number of children ages 0-15 serves as a

dependent variable and interaction terms, province, and year dummies serve as independent variables.

In this empirical setting, DD coefficients for years 1990 and 2000 measure the effect of conflict 6 and 16 year after its onset, respectively. In this regard, given the fact that the focus is on the number of children ages 0-15, reflecting changes in net fertility behavior since the onset of conflict in 1984, the coefficient for year 2000 (16 years after the onset of conflict) is more likely than its counterpart for year 1990 (6 years after the onset of conflict) to reflect the causal effects exposure to conflict on net fertility behavior.

Evidence presented in Panel A of Table A9 suggests that exposure to conflict may be positively related with fertility behavior. When evaluating these estimation results, it is critical to note that although DD estimates for year 2000 are about two times greater that those for 1990, positive and statistically significant estimates for both years' DD coefficient raise concerns about the causal interpretation of DD estimates reported in the first panel of Table A9. Consequently, without a more detailed analysis, it is impossible to rule out the possibility that the DD estimates may also be a simple artifact of differential trends in fertility between conflict and conflict provinces.

Furthermore, to at least partially unravel the selective nature of changes in net fertility behavior with exposure to conflict, I break down the estimates' effects by the head of household's education: i) head of household with less than an upper-secondary school degree (Panel B), and ii) head of household with an upper-secondary school degree or higher (Panel C). As reported in Panels B and C of Table A9, the association between exposure to conflict and the number of children ages 0-15 in a household appears to be more significantly pronounced for households where the head has a lower level of education. To the extent that these suggestive findings indicate a negative selection into fertility, the DD estimates of conflict may overestimate the negative link between exposure to conflict and children's educational outcomes.

Taken together, these results suggest that with the onset of conflict, children residing in conflict provinces are more likely than their peers in non-conflict provinces to compete for resources and thereby have fewer resources, especially the short run, at a household level, a school level, as well as a province level. More importantly, this situation could be worsened dramatically with a negative selection into fertility. Thus, regardless of whether increases in the number of children exposed to conflict through changes in fertility behavior may constitute a channel through which exposure to conflict impacts children's educational outcomes or may be a confounding factor that biases the conflict's estimated impacts, conflict-induced changes in fertility behavior may significantly manifest conflict's adverse impacts on children's educational success, thus causing exposure to conflict to have its intergenerational effects.

Table A10 Estimates of the impacts of conflict on the likelihood of working and the likelihood of being idle.

	Lower-secondary-school graduate population (Age group: 16-20)		Upper-secondary-school graduate population (Age group: 18-20)	
	Work	Idle	Work	Idle
Conflict_provinces*Year1990	-0.026***	0.048***	-0.033***	0.047***
	(0.008)	(0.009)	(0.009)	(0.009)
Conflict_provinces*Year2000	-0.059***	0.068***	-0.074***	0.079***
	(0.009)	(0.008)	(0.009)	(0.008)
R-squared	0.392	0.298	0.405	0.336
Number of observations	748714	748714	432761	432761

Note: See note to Table 1. While the independent variables used in the specification are the same as those used in the baseline specification in column (3) of Table 1, the likelihood of working and that of being idle are examined as the outcomes of interest. Standard errors reported in parentheses are adjusted for the presence of serial correlations within each province-year cell. ***, **, and * indicate that the estimated coefficients are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

A Brief Explanation of Table A10

As suggested by the empirical evidence provided in the main text, if exposure to conflict indeed interrupts children's schooling behavior, the natural place to start for seeking channels that lead to this negative relation is to examine what they do instead. This analysis also serves to test a policy-relevant hypothesis: that is, whether children exposed to conflict may have to deal with a trade-off between child labor and school attendance in the sense that they have to leave the education system to work, and thus bring additional incomes to their families who may have been adversely affected by conflict. Such an analysis requires either cross-sectional or retrospective data that contain information on the school-age populations' schooling and labor market behaviors. While the Census data do not meet this requirement, to have an understanding of how the conflict may alter children's labor market behavior, I examine their likelihood of being employed. Because the Census data do not contain information regarding children's current enrollment status, as a proxy for their schooling behavior, I focus on their likelihood of neither working nor attending school (i.e., being idle).[26] I continue to use the main specification in column (3) of Table 1 located in the main text to estimate the conflict's impact of these two outcomes for individuals ages 16-20 and 18-20. The results reported in Table A10 portray a devastating picture in which, in addition to leaving children without necessary educational credentials, children exposed to conflict are less likely to work and more likely to be idle. Consistent with estimates found for educational attainments, the length of exposure to conflict is positively associated with the magnitude of the conflict's estimated impacts on these two outcomes.

[26] To identify the idle status of individuals who are not seeking a job or not working, I utilized their responses to the question, "Why are you not seeking a job or not working?" Individuals are identified as idle if they indicated reasons other than being a student for why they were not seeking a job or not working.