Short- and Long-Term Impact of Violence on Education: The Case of Timor Leste

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This paper analyzes the impact of the wave of violence that occurred in Timor Leste in 1999 on education outcomes. We examine the short-term impact of the violence on school attendance in 2001 and its longer-term impact on primary school completion of the same cohorts of children observed again in 2007. We compare the educational impact of the 1999 violence with the impact of other periods of high-intensity violence during the 25 years of Indonesian occupation. The short-term effects of the conflict are mixed. In the longer term, we find evidence of a substantial loss of human capital among boys in Timor Leste who were exposed to peaks of violence during the 25-year long conflict. The evidence suggests that this result may be due to household trade offs between education and economic welfare. JEL Codes: I20, J13, O12, O15

The developmental consequences of violence and conflict are far reaching, affecting millions of men, women, and children (World Bank 2011). The objective of this paper is to examine one important channel linking violent conflict and development outcomes: the education of children living in contexts of conflict and violence. The paper focuses on the case of Timor Leste, particularly the last wave of violence in 1999 during the withdrawal of Indonesian troops from the territory. We analyze the short-term impact of the 1999 violence on primary school attendance in 2001 and its longer-term impact on primary school completion in 2007. In addition, we separately examine the impact of early periods of high-intensity violence (HVI) during the 25 years of Indonesian occupation and the effects of the entire conflict on primary school completion in 2007 to compare the average impact of the overall conflict period with the educational impact of singular peaks of violence. This is a

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unique and important feature of this paper because long conflicts are not characterized by constant levels of violence. Although armed conflict has considerable effects on people's lives, there is an important theoretical distinction between the conflict process and the violence that occurs at different times and in different places (Kalyvas 2006).

From a theoretical perspective, the long-term developmental effects of violent conflict are ambiguous. Standard neoclassical growth models predict that the temporary destruction of capital can be overcome in the long run by higher investments in affected areas.[1] However, the long-term destructive effects of violent conflict may remain entrenched in certain regions and among some population groups even if economic growth converges at the aggregate level. Recent research on the microlevel effects of violent conflict has shown that the negative impact of conflict on educational outcomes, labor market participation, and the health status of individuals and households may be observed decades after the conflict.[2]

Children may be particularly affected by conflict because many human capital investments are age specific. The destruction of human capital during childhood is a well-documented mechanism explaining long-term trends in household welfare (Alderman, Hoddinott, and Kinsey 2006; Case and Paxson 2008; Maccini and Young 2009).

The educational effects of violent conflict are particularly substantial. The existing literature shows that violent conflict almost always results in reductions in educational access and attainment (Akresh and de Walque 2011; Alderman, Hoddinott, and Kinsey 2006; Chamarbagwala and Morán 2010; Shemyakina 2011). Relatively minor shocks to educational access during childhood can lead to significant and long-lasting detrimental effects on individual human capital accumulation (Akbulut-Yuksel 2009; Ichino and Winter-Ebner 2004; Leòn 2012).

We analyze the short- and long-term impacts of violence on primary school attendance and completion in Timor Leste using data from two nationally representative household surveys collected in 2001 and 2007. We focus on primary school outcomes because most individuals in Timor Leste (approximately 65 percent) have, at most, only primary school education (TLSS 2007b). Our identification strategy exploits both individual-level violence measures and temporal and geographical variation in the incidence of the conflict using data from the East Timor Human Rights Violations Database (CAVR 2006).

Our results show mixed evidence for the impact of violent conflict on educational outcomes. Mirroring the findings of Bellows and Miguel and others, we find evidence for a rapid recovery in educational outcomes among girls in

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1. See the discussion in Blattman and Miguel (2010) and the evidence in Bellows and Miguel (2006), Davis and Weinstein (2002), and Miguel and Roland (2011).

2. See reviews in Blattman and Miguel (2010) and Justino (2009, 2012a).

Timor Leste. However, we find that the 1999 wave of violence in Timor Leste, as well as the peaks of violence in the 1970s and 1980s, resulted in persistent negative effects on primary school attendance and completion among boys. We present evidence suggesting that boys were less able to benefit from postconflict recovery as a result of household trade offs between education and economic survival that may have led to the removal of boys from school.

The paper is structured as follows. Section I provides a descriptive background of the conflict in Timor Leste and the country's education sector. In section II, we describe the datasets, discuss our identification strategy, and present some descriptive results. Section III discusses our empirical results as well as a range of robustness checks. Section IV concludes the paper.

I. Violent Conflict and the Education Sector in Timor Leste

Timor Leste was under Portuguese colonial rule from 1500 to 1974. After the Portuguese left, Indonesia forcefully annexed the territory, leading to a guerrilla war spurred by the Revolutionary Front for an Independent East Timor and its armed wing (the Armed Forces for the National Liberation of East Timor). Several thousand individuals were forcibly displaced during the Indonesian occupation and forced to live in extreme conditions without adequate food, shelter, or health facilities (Felgueiras and Martins 2006; Gusmão 2004). Approximately 60,000 people lost their lives in the early years of the occupation. The number of deaths reached 200,000 by the end of the occupation (UNDP 2002).

The situation in Timor Leste received little international attention until the Santa Cruz massacre in November 1991, in which Indonesian forces killed 200 protesters. The massacre was broadcast by the international media and raised considerable awareness of human rights violations during the Indonesian occupation. The independence movement received support from the Portuguese government and international organizations, including the UN. These events, in addition to the 1997 financial crisis, resulted in Indonesia agreeing to a referendum on the independence of Timor Leste. On August 30, 1999, 79 percent of the population of Timor Leste voted in favor of independence.

The aftermath of the referendum generated a wave of destruction, violence, and human rights violations by Indonesian forces and militias (Alonso and Brugha 2006). The number of killings during this wave of violence has been estimated at between 1,000 and 2,000 people, approximately 0.2 percent of the Timorese population (Robinson 2003; UNDP 2002). This wave of violence was characterized by massive displacement and the destruction of private dwellings and public infrastructure following the "scorched-earth" tactics employed by the Indonesian troops and pro-Indonesia militia groups (CAVR 2006; UNDP 2002). Approximately 80 percent of the country's infrastructure and buildings were destroyed during the withdrawal of Indonesian troops and

militias (UNDP 2002). In October 1999, a United Nations Transitional Administration was established in Timor Leste.

Variation in the Conflict across Time and Space

The conflict in Timor Leste has evolved in different ways over time and across space. The Timor Leste Commission for Reception, Truth, and Reconciliation, established in 2001, has identified three distinct phases of the conflict during the period between December 1975 and September 1999 (CAVR 2005). The first phase, from 1975 to 1984, was related to the initial Indonesian invasion and occupation of Timor Leste. The first few years, from 1975 to 1979, were the most intense in terms of killings and destruction. The second phase, from 1985 to 1998, was characterized by the consolidation and normalization of the occupation. Although people were killed in this phase (for instance, during the Santa Cruz massacre), the violence during this period was of relatively low intensity. The third phase of the conflict was identified with the 1999 withdrawal of Indonesian troops and the accompanying wave of violence. The main peaks of violence across these three periods were 1975–79, 1983, and 1999, coinciding with more intense fighting between the two factions (CAVR 2005). There were two main types of victims during this last wave of violence. The first was urban households, some (but not all) of which were supporters of the independence movement among or related to the Timorese intelligentsia. Some of these individuals were targeted and killed, whereas others fled from their areas of residence, fearing attacks by the Indonesian troops and militias in Dili and other urban areas (CAVR 2006; Robinson 2003). The second set of victims was mostly poor farmers who fled to safer areas or fell victim to the scorched-earth tactics employed by Indonesian forces withdrawing from Timor Leste (CAVR 2006).

The conflict was also characterized by significant variation at the geographical level, which we exploit in our empirical analysis. The violence was primarily concentrated in specific areas, and its geographic variation generally followed the movement of the Indonesian military forces. The occupation was more intense initially in the western region of Timor Leste because of the proximity to the West Timor border. It then spread to the central and eastern regions. The last wave of violence in 1999 was particularly intense in the western region and the urban areas of the central regions (CAVR 2005). The concentration of violence in 1999 in the western districts was also due to a long-established network of pro-Indonesian groups since before 1999. In contrast, the eastern and central regions were important areas for the resistance forces (Robinson 2003). We will explore this variation in violence across time and space in the empirical analysis below.

The levels of violence experienced in Timor Leste declined considerably after independence. In 2006, Timor Leste faced renewed civil strife as a result of fighting between different factions of the independence movement (Muggah et al. 2010; Scambary 2009). Although fighting and violence have become less

pronounced, some areas of Timor Leste continue to face serious challenges in terms of insecurity, youth unemployment, and violence (Muggah et al. 2010). This paper specifically focuses on the effects of the 1999 wave of violence and the previous years of the Indonesian occupation, but we also discuss the potential implications of the 2006 civil strife on our results in section III.

The Education Sector in Timor Leste

Beginning in 1999, substantial funds from bilateral and multilateral donors flowed into Timor Leste to support the reconstruction and rehabilitation of the country. Although Timor Leste was severely devastated during the 1999 wave of violence, the reconstruction of state institutions, school systems, infrastructure, and markets was relatively successful and rapid (World Bank 2003b). The main development indicators for the country in 2001 were close to the pre-1999 values. However, Timor Leste was (and is) one of the world's least developed countries (UNDP 2002).

Under Portuguese colonial rule, the Catholic Church was the major provider of education, with schooling primarily available for the elite in urban areas. The literacy rate was approximately 5 percent in 1975, and gender disparities were large (UNDP 2002). The Indonesian government expanded educational access to the entire population of Timor Leste, primarily as a means of controlling the population (Nicolai 2004). Enrollment rates increased over those years, and gender gaps began to close (UNDP 2002). Despite this progress, educational performance under the Indonesian occupation was characterized by delayed school entry, high repetition rates, and high dropout rates owing to the low quality of schools and teaching and high fees. Some Timorese were also unwilling to send their children to school because this was perceived as a sign of participation in the repressive Indonesian system (UNPD 2002). In 1995, less than half of individuals aged between 15 and 19 had completed primary school education (UNDP 2002).

The school system was almost totally destroyed in the immediate aftermath of the 1999 violence, and schools did not reopen until October 2000. However, children were still able to attend classes taught in the open air in makeshift camps (Rohland and Cliffe 2002), and substantial effort was applied to the reconstruction of the education system in Timor Leste (World Bank 2003a). In particular, the Trust Fund for East Timor included substantial funding for the renovation of damaged schools and the construction of new ones (USD 27.8 million over three years). Within a few months, many schools had been rebuilt, thousands of books had been replaced, and teachers had been recruited (Rohland and Cliffe 2002; World Bank 2003a).

During this rapid reconstruction process, primary school enrollment rates improved significantly. This increase was aided by the elimination of school fees and the reintroduction of Portuguese as the primary language of instruction. As a result, a large number of over-age students enrolled in primary school for the first time, and net primary school enrollment in Timor Leste

rose from 65 to 74 percent between 1999 and 2001. Gender differentials decreased significantly as a result of a large increase in female literacy rates (World Bank 2003a). However, the reconstruction of the school system in Timor Leste faced numerous challenges owing to the shortage of teachers and schools (UNDP 2006). Makeshift open-air schools were not ideal means of teaching children, and emergency funds were only available for a limited period of time. In 2007, most of the Timorese population continued to have little or no education.

II. DATA DESCRIPTION AND IDENTIFICATION STRATEGY

Our empirical study is based on two cross-sectional household surveys: the Timor Leste Living Standard Measurement Surveys (TLSS), which were jointly conducted by the National Statistics Directorate in Timor Leste and the World Bank in 2001 and 2007, including a broad range of individual- and household-level indicators. The TLSS 2001 surveyed 1,800 households from 100 *sucos* (villages), covering nearly 1 percent of the population (TLSS 2001). The survey included direct questions on the exposure of individuals and households to the violence in 1999. The TLSS 2007 covered a sample of 4,477 households from all 498 *sucos* in Timor Leste (TLSS 2007a). The TLSS 2007 was conducted over a period of 12 months between December 2007 and January 2008.[3] The TLSS 2007 did not contain direct information on exposure to violence. To identify individuals and households affected by violence, we exploit data on the number of killings across time and space collected in the Human Rights Violations Database to identify districts and years that experienced HVI at the beginning of and during the occupation and following the withdrawal of Indonesian troops in 1999.[4]

Identification Strategy I: The Impact of Violence on School Attendance in 2001

We first investigate the short-term impact of the 1999 violence on the school attendance of boys and girls observed in 2001.[5] We consider two different channels of exposure to violence. The first identifies individuals belonging to households that were displaced as a result of the 1999 wave of violence (all

3. The survey was launched in March 2006 but had to be suspended due to the outbreak of internal violence in the country (mostly in Dili). The survey was resumed in January 2007 and conducted over one year. All households interviewed in 2006 (351 households) were revisited and reinterviewed in 2007. Those not found at the time of the new interview (34 households) were replaced with new households (TLSS 2007a).

4. These data were compiled by the Commission for Reception, Truth, and Reconciliation from voluntary statements made by people (victims, perpetrators, and others) affected by violence.

5. We do not analyze primary school completion in 2001 because most children who were of school age in 1999 were still in school in 2001.

members displaced). The second identifies individuals in households that report having their homes completely destroyed by the violent attacks in 1999.[6]

The TLSS 2001 contains useful retrospective information on school attendance in three different school years: 1998/99, 1999/00, and 2000/01. We are interested in the year of recovery (2000/01). Because the 1999 violence primarily occurred in the summer and fall of 1999, we can assume with a high degree of confidence that the 1998/99 school year was not affected by conflict, whereas the 1999/00 school year began during the wave of violence. Note that many children continued to be able to attend school in 1999. However, these were generally makeshift open-air schools in internally displaced person camps established by the international community (Nicolai 2004; Richter 2009).

To employ the retrospective information on school attendance provided in the dataset, we exploit the time variation in school attendance status. We construct a panel dataset in which each individual is observed over three school years, and attendance status is time variant. We focus our analysis on individuals who were of primary school age over the 1998–2001 period, ensuring that all children had a minimum age of seven in 1998/99 and a maximum age of 12 in 2000/01.[7] We estimate the following equation using a linear probability model:

$$E_{it} = \alpha + \beta_1 T_2 + \beta_2 T_3 + \beta_3 V_i^k * T_2 + \beta_4 V_i^k * T_3 + \alpha_i + e_{it} \qquad [1]$$

where E_{it} is a binary variable for school attendance for individual i at time t. T_2 and T_3 are year dummies for the 1999 violence and for the first year of the postviolence period (school year 2000/01), respectively. The reference year is the previolence year, 1998/99. The model includes individual fixed effects, α_i. e_{it} is the random error term. All standard errors are clustered at the village level.

Violence-affected individuals are identified using two different measures, V_i^k, with $k = 1,2$ depending on whether displacement or the destruction of a home is included in the specification. Of the children in the sample, 16 percent and 25 percent live in households that were displaced or had their homes destroyed, respectively. We allow the violence measure to interact with both year

6. The 2001 TLSS also contains self-reported information on the number of household members who have died as a result of violence. In our sample, 148 individuals (living in 27 households) reported the violent death of a household member. Of these individuals, 88 percent were also affected by displacement and/or dwelling destruction, and only 13 of those 148 individuals were children between the ages of 7 and 12 during the violence. We have reestimated our model in table 5 excluding these 13 children. The results remain unchanged. These estimates are not reported because of space constraints but are available upon request.

7. We have analyzed a larger sample that includes children of primary school age in the year of the violence (i.e., between 7 and 12 years old in 1999). This includes individuals aged 6 in 1998 and aged 13 in 2000. The inclusion of these individuals may generate "spurious" results because they are not all of primary school age. We have estimated the model using both samples. The results (not shown) are very similar; therefore, we opted to concentrate on the most restrictive sample.

dummies. The estimation of our specification employs a difference-in-difference methodology. $V_i^k * T_2$ represents the difference-in-difference term between the prewar year and the year of conflict, whereas $V_i^k * T_3$ represents the difference-in-difference term between the prewar year and the postwar year. We focus our attention on the coefficient β_4 because we are primarily interested in understanding the effects of violence on postwar outcomes. We also explore both the separate and joint impacts of each channel of violence by adding a triple interaction between the two violence dummies and the time dummies. This specification allows us to isolate the impact of only being displaced, only having the home destroyed and being affected by both shocks.[8] This specification ensures that the control group does not include individuals affected by violence.

In table 1, we present average school attendance rates, disaggregated by gender and age groups, for the same cohort of children aged 7–10 years in 1998, 8–11 years in 1999, and 9–12 years in 2000. In general, the attendance rates for the whole sample increase over time and are higher for girls. There are, however, considerable differences in attendance rates between children affected by violence and those who do not report victimization. These differences are reported in figure 1, where we disaggregate school attendance averages between violence-affected and unaffected individuals. As expected, we observe a decline in school attendance in 1999 for children affected by violence.

We present the individual and household characteristics of children affected by the 1999 violence in table 2. The table demonstrates that children from displaced households are better off overall than those from households that were not displaced. Many of these were urban households that fled their areas of residence because they feared being targeted by the Indonesian troops stationed in Dili and other urban areas in the central regions (CAVR 2005; Robinson 2003). Households that report having their homes destroyed by violent attacks or affected by both shocks are generally poor farmers living in rural areas. These households are likely to be indiscriminate victims of the scorched-earth tactics employed by the Indonesian troops withdrawing to West Timor (CAVR 2005). Interestingly, we find that boys (aged 10–12) affected by displacement work more hours than unaffected individuals, whereas the opposite is true for girls.

We exploit the panel nature of the data to estimate the causal effects of the 1999 wave of violence on education outcomes. We estimated a fixed effects model,[9] which allows us to eliminate time-invariant unobserved individual characteristics that may be correlated with the conflict measure and our

8. Of the children in our sample, 67.1 percent were not affected by any shock. Moreover, 7.5 percent of all children were only displaced, and 17.4 percent only had their homes destroyed. Finally, 8 percent of the sample was affected by both shocks.

9. The fixed effects model is more appropriate than a random effects model because we would have to assume that the unobserved component of the individual fixed effects and the other covariates specified in the equation are uncorrelated. This assumption is likely to be violated in our case. This choice is also supported by Hausman test results.

TABLE 1. Attendance Rates of Children Aged 7–12 Years Between 1998 and 2001

	All: 8–11 years old				Younger cohort: 8–9 years old				Older cohort: 10–11 years old			
	All	Boys	Girls	t test	All	Boys	Girls	t test	All	Boys	Girls	t test
1998/99	0.634	0.611	0.659		0.509	0.498	0.521		0.750	0.720	0.782	
	(0.012)	(0.018)	(0.017)	n.s.	(0.020)	(0.027)	(0.028)	n.s.	(0.018)	(0.026)	(0.025)	n.s.
1999/00	0.676	0.654	0.700		0.622	0.602	0.647		0.726	0.705	0.749	
	(0.012)	(0.017)	(0.017)	n.s.	(0.019)	(0.027)	(0.028)	n.s.	(0.018)	(0.026)	(0.026)	n.s.
2000/01	0.854	0.836	0.874		0.822	0.789	0.860		0.884	0.881	0.887	
	(0.014)	(0.019)	(0.019)	*	(0.022)	(0.030)	(0.031)	**	(0.021)	(0.029)	(0.029)	n.s.
N	966	512	454		466	251	215		500	261	239	

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. n.s. = not statistically significant. We consider the same cohort over time: the sample is aged 7–10 years in 1998, 8–11 years in 1999, and 9–12 years in 2000.

Source: Authors' computations using TLSS 2001.

FIGURE 1. School Attendance Rates by Channel of Violence Exposure
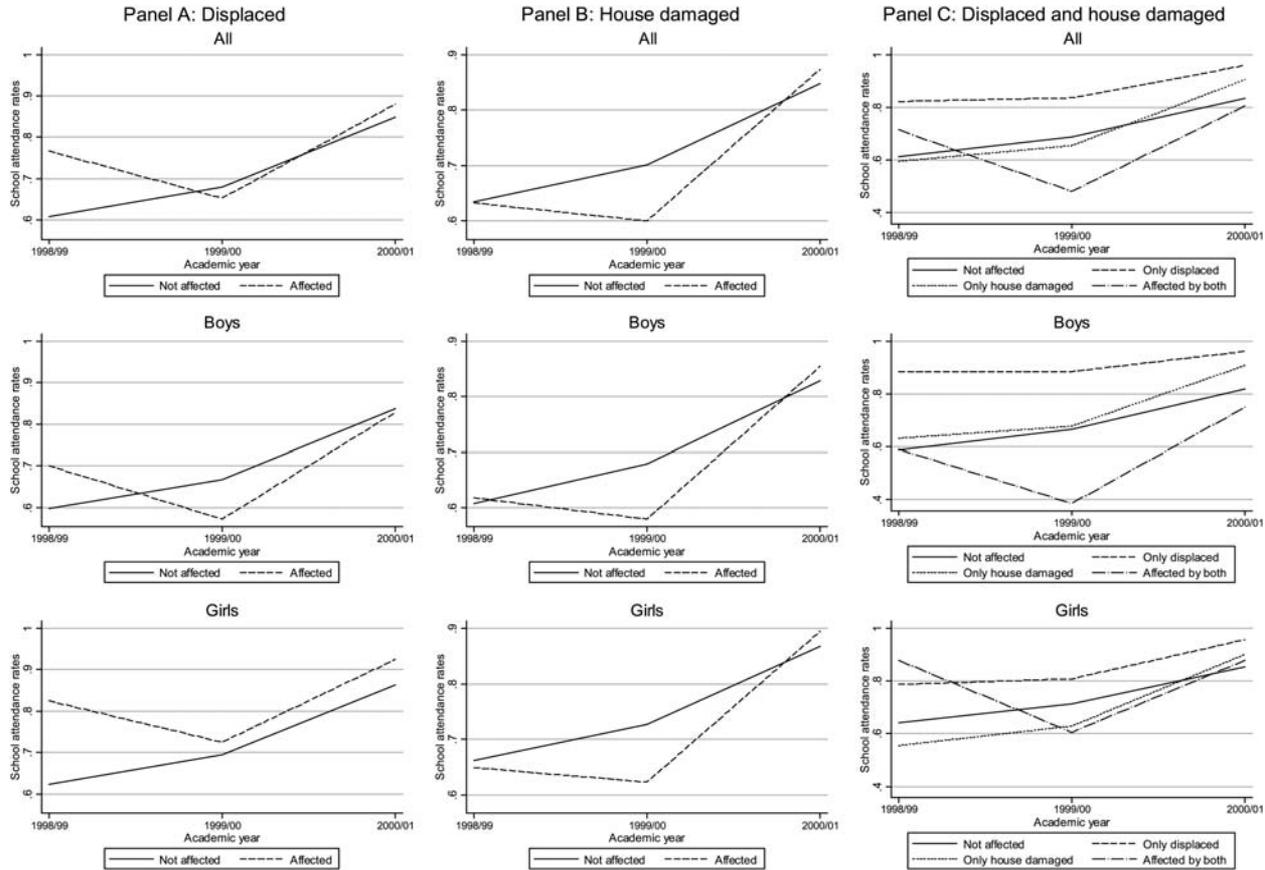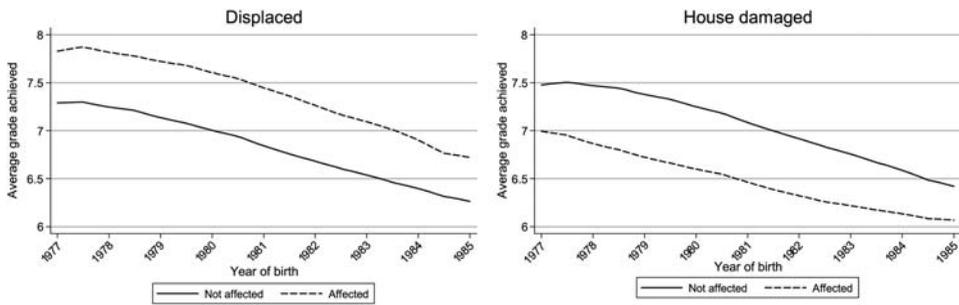

Source: Authors' own computations using TLSS 2001.

TABLE 2. Individual and Household Characteristics by Channel of Violence Exposure in 2001

	All children 7–12						Boys 7–12						Girls 7–12					
	Displaced			House damaged			Displaced			House damaged			Displaced			House damaged		
	0	1	t test	0	1	t test	0	1	t test	0	1	t test	0	1	t test	0	1	t test
Panel A – All children (7–12 years old)																		
Being female	0.472	0.500		0.484	0.454	n.s.												
Speaking Indonesian	0.575	0.720	***	0.584	0.632	n.s.	0.577	0.701	**	0.580	0.634	n.s.	0.574	0.738	***	0.589	0.631	
Speaking Portuguese	0.028	0.033	n.s.	0.030	0.027	n.s.	0.032	0.028	n.s.	0.031	0.035	n.s.	0.024	0.037	n.s.	0.029	0.018	
HH head is a farmer	0.646	0.556	**	0.623	0.659	n.s.	0.649	0.570	n.s.	0.639	0.634	n.s.	0.642	0.542	*	0.606	0.690	**
Education grade of HH head	3.114	3.651	n.s.	3.332	2.783	**	2.954	3.210	n.s.	3.099	2.685	n.s.	3.293	4.084	n.s.	3.581	2.899	*
Education grade of the mother	1.870	2.785	***	2.200	1.435	***	1.827	2.252	n.s.	2.061	1.406	**	1.918	3.318	***	2.348	1.470	***
Education grade of the father	2.824	3.495	*	3.037	2.589	**	2.802	2.869	n.s.	2.944	2.446	n.s.	2.848	4.121	**	3.136	2.762	
Living in urban areas	0.402	0.533	***	0.419	0.427	n.s.	0.396	0.551	***	0.413	0.431	n.s.	0.409	0.514	**	0.426	0.423	
Per capita monthly HH expenditure	238,963	262,113	n.s.	244,940	234,904	n.s.	250,901	249,806	n.s.	245,971	263,914	n.s.	225,635	274,421	*	243,843	200,024	***
N	1236	214		1080	370		652	107		557	202		584	107		523	168	
Panel B – Children aged 10–12 (labor market characteristics)																		
Has worked in the past seven days	0.063	0.088	n.s.	0.070	0.056	n.s.	0.054	0.167	**	0.071	0.063	n.s.	0.073	0.019	**	0.070	0.048	n.s.
Working hours	1.468	2.098	n.s.	1.722	1.084	n.s.	1.117	4.292	**	1.582	1.406	n.s.	1.857	0.148	***	1.868	0.711	n.s.
Has performed domestic chores	0.902	0.912	n.s.	0.897	0.922	n.s.	0.889	0.854	n.s.	0.888	0.875	n.s.	0.916	0.963	n.s.	0.907	0.976	n.s.
N	602	102		525	179		316	48		268	96		286	54		257	83	

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. n.s. = not statistically significant. In the title, 0 refers to not affected individuals while 1 refers to affected individuals. HH indicates household. Per capita monthly HH expenditure is expressed in Rupiah in real values using CPI of 2001.

Source: Authors' computations using TLSS 2001.

FIGURE 2. Preconflict Trends in Education Levels

Note: The plots are based on the estimation of separate kernel-weighted local polynomial regressions of the last completed grade of schooling against age using an Epanechnikov kernel.
  Source: Authors' own computations using TLSS 2001.

dependent variable. Our specification also includes year dummies that allow us to control for unobserved time-variant heterogeneity.

To ensure that our key identifying assumption is not violated, we checked whether trends in education before the 1999 violence were parallel between groups affected by the violence and those unaffected by the violent events. We examined the average school grades of affected individuals and unaffected individuals who were not exposed to the 1999 violence during their primary school years and who were old enough in 1999 to have at least completed primary school.[10] The results indicate that it is unlikely that preexisting differences in education trends drive our postconflict outcomes (see figure 2). This evidence, combined with the association of the violence with the Indonesian troop movements described in section I, strongly indicates that the effects of the violence that occurred in Timor Leste in 1999 on individual educational outcomes are unlikely to be driven by a systematic correlation between the determinants of individual educational attainment levels before 1999 and the incidence of the 1999 violence at the individual level.

Despite the evidence discussed above, there is a small possibility that this strategy may be unable to control for all of the unobservable individual characteristics that may be correlated with both conflict incidence and educational outcomes. In particular, there are two common omitted variables in empirical analyses of conflict that may affect our results (see Kalyvas 2006). The first variable is a household's level of support for armed groups. Supporters of proindependence groups in Timor Leste were likely to be targets of violent attacks by Indonesian forces. In that case, the correlation between the conflict variables and this potentially omitted variable would be positive. If supporters were also more educated and hence more likely to send their children to school, our estimated effect would be

10. We do not include cohorts born after 1985 because the educational attainment of these individuals might be censored.

biased upward. The use of a fixed effects model controls for these effects as a component of time-invariant individual heterogeneity. The results of this paper may nevertheless indicate a lower negative impact of the conflict on education than if we were able to account for this potential unobservable in the case that levels of support changed during the conflict. This is unlikely to have been the case in Timor Leste in light of the discussion in section I. Another common omitted variable is the level of control of different armed factions. In the case of Timor Leste, the level of control of the Revolutionary Front for an Independent East Timor and the Indonesian troops is likely to vary with the geographical characteristics of each area as well as their proximity to West Timor. We control for this by including individual fixed effects in our specifications.

Identification Strategy II: The Impact of Violence on Primary School Completion in 2007

In this section, we investigate the longer-term consequences of the violence experienced in 1999 in Timor Leste on primary school completion in 2007 among the cohorts of children analyzed above. We then compare these results to the educational impact of the peaks of violence that occurred in earlier years of the conflict.

To construct a measure of exposure to violence, we matched information on the number of killings (provided in the Human Rights Violations Database dataset)— which varies over time and across districts—to information on the year and district of birth of each individual (provided in the TLSS 2007a dataset). We focus on the number of killings as our main conflict variable because we find that it serves as a good proxy for the intensity of the conflict across time and space. The occurrence of killings largely tracked the movements of the Indonesian military operations (Silva and Ball 2006). The number of killings also proxies for the destruction of homes and infrastructure and the displacement of people during the 1999 wave of violence, given the manner in which it occurred (i.e., the scorched-earth technique employed by Indonesian troops as they moved toward West Timor).

Matching this measure of violence to the year and district of birth of each individual allows us to identify whether and for how long each individual was exposed to the conflict during his or her primary school years. Our violence measure is defined as $V_{jt} = \sum_{a=7}^{12} v_{t+a}^j$, where each v_{t+a}^j takes a value of one if the individual was of primary school age in districts and years affected by the conflict. Specifically, j is the district of birth, t is the year of birth, and a is the primary school age (from 7 to 12). This measure ranges from zero to six if, from none to all six of a child's primary school years, respectively, were classified as exhibiting HVI. Because we only have information on the years in which individuals were supposed to have attended primary school,[11] we

11. These are not the years in which the individuals actually attended school because we do not have access to this information. The existence of a delay in school means that the "supposed" years of attendance might not coincide with the "actual" years of school attendance. However, given the way in which we identify our control and treatment groups, we do not expect this difference to affect our results.

assume that the district of birth is the district where the child attended school at the time of the violent events.

We define districts and years of HVI as those in which the number of killings in that year and district are above a given threshold, defined as the mean of the number of killings plus one standard deviation. The years in which the conflict was the most intense, as defined by our threshold, are 1975–1979, 1983, and 1999. This observation coincides with the history of the conflict discussed in section I (see CAVR 2005).[12]

The definition of HVI districts and years as a binary variable instead of a continuous one is primarily justified by our interest in capturing the incidence of violent conflict rather than its scale and magnitude.[13] In addition, the distribution of killings is highly right skewed, further justifying the use of a binary variable. A Kernel density plot of the number of killings (not shown) demonstrates that where and when the conflict events occurred, we observe a considerably higher number of violent events; otherwise, we observe a low to negligible number of events. Finally, and more important, the use of a discrete variable allows us to minimize potential biases deriving from the potential underreporting of violent events. The Human Rights Violations Database dataset was compiled from voluntary statements, which may have resulted in biased reports. For instance, individuals living in remote areas or sick and disabled people may have not been able to report abuses, whereas victims of sexual abuse or traumatized people may not have reported their true levels of exposure to violence. In contrast, socially active individuals may have been more likely to volunteer information (Silva and Ball 2006). Under these circumstances, the use of a continuous measure may lead to biased estimates reflecting potential self-selection into reporting violence (Leòn 2012). The direction of this bias is impossible to predict a priori and depends on how unobservable characteristics related to underreporting may be correlated with conflict exposure and the dependent variable.[14]

To estimate the effect of the 1999 violence on school completion in 2007, we include individuals born between 1977 and 1992 in our sample. The treatment group includes individuals who were between 7 and 12 years old in 1999 in HVI districts (born between 1987 and 1992). We do not include individuals born after 1992 because they may have not completed primary school by 2007. The control group includes individuals who were not of primary school age in 1999 (born between 1977 and 1986).

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12. The districts most affected by violence in the earlier years of the conflict are Baucau, Lautem, Viqueque, Ainaro, Manufahi, Manatuto, Aileu, Dili, Ermera, and Bobonaro. Those most affected by the 1999 violence are Dili, Ermera, Bobonaro, Covalima, Liquica, and Oecussi.

13. We have checked the robustness of all results to the use of a continuous variable and two different thresholds of violence intensity defined as the number of killings in each district and year (i) above the mean plus half of a standard deviation and (ii) above the mean plus two standard deviations. The results obtained are largely similar to those reported in the paper and are available from the authors upon request.

14. We thank an anonymous referee for noting this issue.

To analyze the impact of earlier peaks of violence, we focus our analysis on a sample of individuals born between 1968 and 1984. The treatment group includes individuals who were of primary school age between 1975 and 1979 and in 1983 (born between 1968 and 1976) in HVI districts. We exclude those born before 1968 because the schooling system was very different before the Indonesian troops invaded Dili in 1975. We also do not include individuals born between 1985 and 1986 as they may have been affected by the 1999 violence although placebo tests presented later indicate that they have not been affected. One interesting aspect of this analysis is that the treatment term informs us not only about the effects of exposure to HVI but also about the number of years of primary school affected by this exposure to violence.

Finally, we analyze the effect of the whole conflict on school attainment in 2007. For this purpose, we consider the full sample of individuals born between 1968 and 1992, where the treatment groups are those identified above and the control group includes individuals born between 1977 and 1986. This allows us to calculate the average educational effect of exposure to any period of the conflict for boys and girls in different age groups.

To analyze the effect of the conflict on primary school completion in 2007, we estimate the following equation:

$$G_{ihjt} = \beta V_{jt} + \alpha_j + \alpha_t + \alpha_j t + X_h' \gamma + \varepsilon_{ijt} \qquad [2]$$

where G_{ihjt} refers to primary school completion for individual i of household h born in district j in year t, defined as a binary variable equal to one if the individual has completed at least primary school and zero otherwise. The adoption of a binary variable as the dependent variable in place of a continuous one is motivated by our interest in primary school completion rather than school attainment in general. The education sector in Timor Leste is extremely underdeveloped, and most of the population is illiterate. Primary education is therefore a major concern in the country.[15]

In the regression above, all standard errors are clustered at the year and district of birth levels. The term X_h is a vector of household characteristics (education of the household head and whether the household head is a farmer). The term V_{jt} is defined as above and identifies individuals exposed to HVI. β is our parameter of interest, indicating whether an additional year of primary school

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15. We examined whether our results are robust to the use of alternative definitions of the educational outcome measure. To investigate the robustness of the results to the use of a continuous rather than a binary variable, we used a maximum likelihood estimated ordered probit model for school grade attainment allowing for the censorship of those still in school. This estimation follows the methods proposed in Glewwe and Jacoby (1994), Holmes (2003), and Zhao and Glewwe (2010). None of the key findings on the impact of the intensity of violence on educational outcomes reported in this paper are materially altered under this alternative approach. The results of these exercises are available on request from the authors. We are grateful to an anonymous referee for encouraging us to investigate this issue further.

exposure to the conflict affects the probability of primary school completion after the conflict ended compared to an individual who was not affected by HVI during her primary school years. The two parameters α_j and α_t are fixed effects for the districts of birth and the years of birth, respectively, and the term $\alpha_j t$ represents district-specific linear trends.[16]

In table 3, we report the differences in average primary school completion in 2007 between individuals exposed to high- or low-intensity violence in each of the three samples analyzed. These descriptive statistics show that boys exposed to HVI in 1999 (1977–1992 sample) exhibit a lower attainment rate than those who are less exposed to violence. The opposite is true for girls. Children exposed to earlier peaks of high intensity violence (1968–1984 sample) exhibit a lower completion rate than those living in districts and years in which the violence was not as intense.

The empirical strategy discussed above assumes that no systematic relationship exists between the intensity of the violence across districts and preconflict education levels at the district level. The existence of time-varying unobservables that are correlated with both the outcome and the conflict variables would bias our results. We have discussed this issue in the section above. We show here that the assumption also holds for the medium- and long-term analysis. The inclusion of district fixed effects in equation [2] allows us to account for time-invariant differences in education levels across districts. By including district-specific time trends, we account for any difference in trends across districts and hence for any time-varying characteristics in a given district. However, this identification strategy still relies on the assumption that there is no correlation between preconflict trends in education and violence in specific districts. To test for this, we conducted placebo tests on cohorts that supposedly were not exposed to the conflict during their primary school years (table 4).

Because the geographical variation of the conflict differs between the early years and 1999, we estimate two separate models by defining different violence-affected districts and "placebo" cohorts. We construct two violence-affected district dummies equal to one if the individual's district of birth is located in one of the HVI districts as defined above, during the early years of the conflict or during the 1999 violence, and zero otherwise.

The first placebo test concentrates on the early years of the conflict. We are unable to analyze preconflict cohorts because, as explained above, the cohort born before 1968 would have attended a different school system. Therefore, we define those born between 1977 and 1980 as exposed "placebo" cohorts and

16. We reestimated the equation including a cubic district trend and a square root district trend to account for possible nonlinear trends across districts. We do not find any difference in the estimates, and we therefore only show the results that include a linear district trend. Results are available upon request.

TABLE 3. Average Primary School Completion in 2007

	All			Boys			Girls		
	Low-intensity violence	High-intensity violence	t test	Low-intensity violence	High-intensity violence	t test	Low-intensity violence	High-intensity violence	t test
Panel A – All primary school age children									
1977–1992 sample	0.725	0.724	n.s	0.752	0.709	**	0.698	0.739	**
	(0.006)	(0.013)		(0.008)	(0.020)		(0.008)	(0.017)	
1968–1984 sample	0.624	0.572	***	0.680	0.658	n.s.	0.569	0.472	***
	(0.007)	(0.023)		(0.010)	(0.032)		(0.009)	(0.034)	
1968–1992 sample	0.679	0.674	n.s	0.720	0.692	**	0.636	0.654	n.s.
	(0.005)	(0.009)		(0.007)	(0.013)		(0.006)	(0.013)	
Panel B – Children of grade 1–3 age									
1977–1992 sample	0.731	0.692	**	0.751	0.673	***	0.709	0.711	n.s.
	(0.005)	(0.019)		(0.008)	(0.029)		(0.007)	(0.025)	
1968–1984 sample	0.623	0.571	***	0.679	0.659	n.s.	0.567	0.471	***
	(0.007)	(0.024)		(0.010)	(0.032)		(0.010)	(0.035)	
1968–1992 sample	0.687	0.637	***	0.723	0.666	***	0.650	0.605	**
	(0.004)	(0.012)		(0.006)	(0.016)		(0.006)	(0.016)	
Panel C – Children of grade 4–6 age									
1977–1992 sample	0.719	0.759	**	0.738	0.749	n.s.	0.700	0.769	***
	(0.005)	(0.018)		(0.008)	(0.028)		(0.007)	(0.023)	
1968–1984 sample	0.622	0.524	***	0.680	0.634	n.s.	0.564	0.385	***
	(0.007)	(0.037)		(0.010)	(0.051)		(0.009)	(0.054)	
1968–1992 sample	0.675	0.690	n.s.	0.711	0.713	n.s.	0.638	0.664	n.s.
	(0.004)	(0.014)		(0.006)	(0.021)		(0.006)	(0.019)	

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. n.s. = not statistically significant.

Source: Authors' computations using TLSS 2007a.

compare them to those born between 1981 and 1984.[17] As a further check, we also analyze violence exposure for cohorts born between 1977 and 1981 and compare them to those born between 1982 and 1986. The treatment term is the interaction between the placebo cohort and the HVI dummies. We expect to find no effect of "exposure" for cohorts who were not of primary school age but were born in districts with HVI. We repeat the analysis with a focus on the 1999 violence. Individuals born between 1982 and 1986 were not of primary school age in 1999. We define this latter cohort as the "placebo" cohort and compare their exposure to that of those born between 1977 and 1981 in districts with high- and low-intensity violence. The results in table 4 show that cohorts who were not supposed to be of primary school age during the most violent years, but who were born in HVI districts, do not show significant differences in primary school completion rates relative to the same cohorts born in districts of low-intensity violence. This result supports our identification assumptions.

III. EMPIRICAL RESULTS

In this section, we discuss the results of the short- and long-term analyses.

School Attendance in 2001

The results in table 5 report the impact of the two channels of exposure to violence in 1999 on school attendance in the 1999/00 and 2000/01 school years. We are primarily interested in the differential effects of the violence on school attendance in the postviolence period, T_3 (2000/01), relative to the previolence year, T_1 (1998/99).

The results show a negative and significant impact of displacement on school attendance in 2000/01 for the overall sample. We find that being affected by displacement alone (panel C, table 5) decreases school attendance by 8.5 percentage points on average, with stronger effects for boys. Individuals affected by both shocks experience a reduction in school attendance of 13.3 percentage points on average, with girls being more severely affected.[18] The effects are stronger for younger children.

These results suggest that different violence channels affect school attendance in heterogeneous ways. School attendance is most severely disrupted for children, particularly girls, who are affected by both types of violence. Considering the channels separately, we observe that displacement is the most disruptive channel in terms of consequences on children's school attendance because all household assets are likely to have been lost. Ibáñez and Moya

17. The cohorts truly exposed to the early years of the conflict are those born between 1968 and 1976. In our placebo test, we examine the cohorts immediately following these.

18. We estimated a pooled model with interactions of the violence measures with the female dummy. The results reported in panel C are statistically different between girls and boys, as in table 5.

TABLE 4. Placebo Test for Differences in Trends in Education Levels

	1977–1984 sample			1977–1986 sample			1977–1986 sample		
	(1) All	(2) Boys	(3) Girls	(4) All	(5) Boys	(6) Girls	(7) All	(8) Boys	(9) Girls
HVI district*Cohort 1977–80	0.062 (0.047)	0.070 (0.054)	0.052 (0.061)						
HVI district*Cohort 1977–81				0.021 (0.044)	0.059 (0.048)	−0.018 (0.058)			
HVI district(a)	0.014 (0.031)	0.031 (0.032)	−0.003 (0.043)	0.040 (0.030)	0.014 (0.029)	0.063 (0.043)			
HVI district*Cohort 1982–86							−0.015 (0.043)	0.044 (0.050)	−0.078 (0.054)
HVI district(b)							−0.051 (0.031)	−0.037 (0.040)	−0.060 (0.038)
N	2,542	1,255	1,287	3,402	1,699	1,703	3,402	1,699	1,703
R-squared	0.158	0.156	0.153	0.140	0.131	0.141	0.141	0.130	0.151

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses clustered at the year of birth * district level. Regressions include year and district fixed effects and controls (whether the household head is a farmer and the household head's level of education). HVI district(a) equals one if the individual's district of birth is found to be a conflict-affected district during the early years of conflict (1975–1979 and 1983), as defined by our violence measure. HVI district(b) equals one if the individual's district of birth is found to be a conflict-affected district during the 1999 violence as defined by our violence measure.

Source: Authors' computations using TLSS 2007a.

TABLE 5. Impact of 1999 Violence on School Attendance in 2001

	(1) All 8−11	(2) Boys 8−11	(3) Girls 8−11	(4) All 8−9	(5) Boys 8−9	(6) Girls 8−9	(7) All 10−11	(8) Boys 10−11	(9) Girls 10−11
Panel A − Impact of displacement									
$D*T_2$	−0.184***	−0.199***	−0.172***	−0.198***	−0.212***	−0.185**	−0.188***	−0.209**	−0.170**
	(0.045)	(0.056)	(0.060)	(0.047)	(0.065)	(0.074)	(0.061)	(0.088)	(0.077)
$D*T_3$	−0.127***	−0.111**	−0.141***	−0.182***	−0.138*	−0.233***	−0.089**	−0.106*	−0.066
	(0.037)	(0.049)	(0.048)	(0.053)	(0.077)	(0.071)	(0.038)	(0.055)	(0.055)
N	2,898	1,536	1,362	1,398	753	645	1,500	783	717
R-squared	0.151	0.155	0.146	0.217	0.199	0.241	0.110	0.130	0.091
Panel B − Impact of house damage									
$H*T_2$	−0.101**	−0.109**	−0.091*	−0.075	−0.137*	−0.012	−0.129***	−0.077	−0.196***
	(0.042)	(0.054)	(0.054)	(0.051)	(0.070)	(0.070)	(0.048)	(0.067)	(0.059)
$H*T_3$	0.027	0.016	0.040	0.046	0.027	0.061	0.004	0.014	−0.016
	(0.041)	(0.051)	(0.052)	(0.059)	(0.077)	(0.078)	(0.037)	(0.052)	(0.056)
N	2,898	1,536	1,362	1,398	753	645	1,500	783	717
R-squared	0.147	0.153	0.142	0.209	0.198	0.226	0.110	0.122	0.106

Panel C – Impact of displacement and house damage

$D*T_2$	−0.060	−0.076	−0.050	−0.070	−0.086	−0.058	−0.079	−0.122	−0.064
	(0.049)	(0.060)	(0.062)	(0.057)	(0.068)	(0.087)	(0.069)	(0.109)	(0.085)
$D*T_3$	−0.085*	−0.154***	−0.041	−0.153**	−0.248***	−0.098	−0.039	−0.048	−0.020
	(0.047)	(0.059)	(0.065)	(0.064)	(0.070)	(0.100)	(0.055)	(0.110)	(0.065)
$H*T_2$	−0.015	−0.030	0.002	0.027	−0.039	0.084	−0.062	−0.011	−0.130**
	(0.050)	(0.068)	(0.057)	(0.063)	(0.091)	(0.077)	(0.053)	(0.076)	(0.058)
$H*T_3$	0.087*	0.045	0.134**	0.114	0.036	0.173**	0.052	0.065	0.028
	(0.050)	(0.064)	(0.058)	(0.072)	(0.096)	(0.086)	(0.044)	(0.063)	(0.057)
$D*H*T_2$	−0.233**	−0.174	−0.296**	−0.282***	−0.193	−0.364**	−0.156	−0.116	−0.158
	(0.094)	(0.114)	(0.123)	(0.104)	(0.136)	(0.152)	(0.122)	(0.171)	(0.151)
$D*H*T_3$	−0.133*	0.037	−0.304***	−0.122	0.166	−0.413***	−0.123	−0.129	−0.119
	(0.077)	(0.103)	(0.087)	(0.114)	(0.158)	(0.121)	(0.075)	(0.128)	(0.098)
N	2,898	1,536	1,362	1,398	753	645	1,500	783	717
R-squared	0.162	0.164	0.163	0.228	0.217	0.257	0.122	0.134	0.117

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports fixed effect estimates. Robust standard errors in parentheses are clustered at the village level. Regressions include time effects (T_2 refers to 1999/00; T_3 refers to 2000/01). D, H, and D*H are dummies, respectively defined as one if individual was displaced with the whole household, whether the house was completely damaged, and whether the individual was affected by both violent shocks.

Source: Authors' computations using TLSS 2001.

(2010) show similar evidence for Colombia. The destruction of a home affects household wealth, but perhaps less so if the household was able to retain other assets or to live with friends, neighbors, or relatives.

School Completion in 2007

In table 6, panel A, we report the estimates of our analysis of the effect of the 1999 violence on primary school completion in 2007.

The coefficient for the violence measure is negative but not statistically significant. However, once we split the sample into boys and girls (columns 2 and 3),[19] the results show that boys exposed to violence during their primary school years are 18.3 percentage points less likely to have completed primary school eight years after the 1999 violence relative to boys not exposed to violence. This represents a 25 percent decrease in the probability of primary school completion. In contrast, we observe that girls exposed to the 1999 violence are 10.4 percentage points more likely to have completed primary school in 2007. This represents a 14 percent increase in the probability of girls completing primary school. We do not find any statistically significant differences across age groups.

We now turn to the effect of the peaks of violence in the earlier years of the conflict on primary school completion in 2007. In table 6, panel B, we report the results for the sample of individuals born between 1968 and 1984. We find that an additional year of exposure decreases school completion in 2007 for all individuals by 2.6 percentage points and by 3 percentage points for boys. Therefore, the likelihood of primary school completion for boys was reduced (for an average exposure of one year and 10 months) by 5.6 percentage points. The effect is particularly strong for boys attending the last three years of primary school (grades four to six) (column 5). We do not find a significant effect for girls.

The results in table 6, panel C, report the effect of the overall conflict on primary school completion in 2007. The sample includes individuals born between 1968 and 1992. The results indicate the average effect of exposure to both the first years of the conflict and the 1999 violence. Because we examine the effects of both periods of high intensity violence and because only one year of primary school could have been affected during the 1999 violence, we have transformed our treatment term into a binary variable (exposed or not exposed during primary school) to ensure that we do not confound the results. These results indicate that boys exposed to the conflict in any period are, on average, 7.4 percentage points less likely to complete primary school in 2007 than those less exposed to violence. This effect represents a 10 percent decrease in the probability of primary school completion for boys. This effect is stronger

19. Similar to 2001, we estimated a pooled model for 2007 including an interaction term with the female dummy. The results show that the effects are statistically different between boys and girls in panels A and C. We report separate estimates for clarity in the exposition.

TABLE 6. Impact of Conflict on Primary School Completion in 2007

	(1) All	(2) Boys	(3) Girls	(4) All	(5) Boys	(6) Girls
Panel A – Impact of the 1999 violence (1977–1992 sample)						
Years of prim. school in HVI	−0.041	−0.183***	0.104**			
	(0.029)	(0.044)	(0.047)			
Years of grade 1–3 in HVI				−0.069*	−0.210***	0.080
				(0.038)	(0.056)	(0.064)
Years of grade 4–6 in HVI				−0.040	−0.183***	0.105**
				(0.029)	(0.044)	(0.048)
N	6,676	3,383	3,293	6,676	3,383	3,293
R-squared	0.150	0.144	0.180	0.150	0.144	0.180
Panel B – Impact of early years of conflict (1968–1984 sample)						
Years of prim. school in HVI	−0.026**	−0.030**	−0.021			
	(0.011)	(0.014)	(0.017)			
Years of grade 1–3 in HVI				−0.021*	−0.022	−0.018
				(0.012)	(0.016)	(0.020)
Years of grade 4–6 in HVI				−0.040**	−0.054**	−0.031
				(0.018)	(0.021)	(0.028)
N	5,195	2,625	2,570	5,195	2,625	2,570
R-squared	0.338	0.358	0.318	0.338	0.358	0.318

(Continued)

TABLE 6. Continued

	(1) All	(2) Boys	(3) Girls	(4) All	(5) Boys	(6) Girls
Panel C – Impact of entire conflict (1968–1992 sample)						
HVI in primary school	−0.012	−0.074***	0.055*			
	(0.020)	(0.027)	(0.030)			
HVI in grade 1–3				−0.006	−0.035	0.031
				(0.019)	(0.023)	(0.029)
HVI in grade 4–6				−0.019	−0.075***	0.044
				(0.019)	(0.027)	(0.029)
N	9,329	4,753	4,576	9,329	4,753	4,576
R-squared	0.241	0.231	0.266	0.241	0.231	0.266

Note: *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses clustered at the year of birth * district level. Regressions include year and district fixed effects, district time trend (defined as the interaction between the district category and birth year), and controls (whether the household head is a farmer and the household head's level of education).

Source: Authors' computations using TLSS 2007a.

among boys attending the last three years of primary school. The overall effect on girls is positive (most likely driven by the 1999 effects), corresponding to an 8.5 percent increase in the likelihood of primary school completion.

Discussion

The results above indicate that violent conflict in Timor Leste had mixed effects on education. On average, the wave of violence in 1999 resulted in immediate hardships for the education of boys and girls. Girls, however, recovered from the negative consequences of the 1999 violence in the medium term. When the same cohort was observed in 2007, girls affected by the conflict had a higher and statistically significant positive chance of completing primary school than girls who were not exposed to the violence. We find no effect of the earlier peaks of violence on girls' primary school completion, but we find a positive and statistically significant effect (at 10 percent) of the entire conflict on girls' primary school completion.

In contrast, boys exposed to the wave of violence in 1999 had a much lower probability of having completed primary school by 2007 relative to boys unaffected by the violent events. Earlier peaks of violence as well as the entire conflict have similar negative effects on the education of boys in Timor Leste, particularly among boys attending the last grades of primary school. The effect of earlier peaks of violence is smaller than the impact of the 1999 violence, although we observe the persistence of significant negative educational effects of the earlier years of the conflict in the longer term. The difference in the magnitudes of the impacts of different peaks of violence may be because individuals affected by the earlier violence may have had the opportunity to complete primary school or may reflect the particularly violent nature of the 1999 events. In all cases, boys were rather severely affected by violence over the 25 years of the Timor Leste conflict.

The 1999 wave of violence in Timor Leste was brutal, but the recovery was rapid, as discussed in section I. Although problems remain, violence-affected areas have clearly benefited from this reconstruction effort. This finding is in line with results reported for other conflict-affected countries in Bellows and Miguel (2006) and elsewhere. Our results show, however, that only girls affected by the violence seem to have been able to recover (and even improve) their educational outcomes. The results for boys are persistently negative.[20]

Given the discussion in section I, it is unlikely that this result is explained by supply-side factors, such as the destruction of schools or the absence of teachers. The postconflict reconstruction process had clear, positive impacts on the educational outcomes of girls exposed to violence, possibly because of a strong consideration of gender concerns in the UN interventions in Timor Leste

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20. An uneven negative impact of violent conflict on boys' educational outcomes is also reported in Akresh and de Walque (2011) for Rwanda and Verwimp and van Bavel (2011) for Burundi. For a review of the literature on the impact of violent conflict on education, see Justino (2012b).

(Olsson 2009). However, it is highly unlikely that these programs would have been biased against educating boys. A more likely explanation is that the negative impact of the conflict on boys' education in Timor Leste is related to the different roles that boys and girls play within the household.

As mentioned in section II, boys who were affected by the violence in 1999, on average, tended to work more and longer hours in 2001 (table 2). We estimated a reduced form regression of the incidence of conflict on child labor in the aftermath of the 1999 violence. The results (not shown) indicate a positive correlation between conflict exposure (displacement) and the probability of boys working: boys affected by displacement are 11 percentage points more likely to work than boys unaffected by violence. Affected girls, however, are 3.6 percentage points less likely to work.[21] Other studies have shown that child labor is a key factor in explaining low school enrolment rates in Timor Leste, particularly among boys. For instance, as Pedersen and Arneberg (1999) report, "Poverty is the main reason why some 20 percent of children never get the chance to go to school. [. . .] Children, especially boys, work when their parents do not have jobs or their families are headed by single mothers" (p. 83). This argument is in line with findings in the literature regarding household coping strategies in the face of adverse shocks, which have widely documented the use of children as an economic security mechanism (see Dasgupta 1993; Duryea, Lam, and Levinson 2007; Nugent and Gillaspy 1983). In areas experiencing violent conflicts, households may decide to replace dead, injured, absent, or disabled adult workers with children (if they have not also become fighters). Rodriguez and Sanchez (2009) analyze the effect of war on child labor and find that violent attacks by armed groups in Columbian municipalities significantly increased the probability of school dropout and the presence of children, particularly boys, in the labor market.

The above results suggest that household economic needs in Timor Leste may also have resulted in boys dropping out of school, a mechanism that may, in turn, explain the negative impact of the conflict on boys' education. This mechanism is not conclusive, and it is possible that school dropout may have occurred if boys joined armed groups as fighters or occupied other supporting roles. Data to test this alternative hypothesis are unavailable, but there are some indications that children joined both the proindependence troops and paramilitary groups and militias. UNICEF (2001) states that "[b]oth the proindependence and prointegration forces in East Timor used children as armed combatants during period of the Indonesian occupation and its violent resolution after the 1999 referendum. On both sides of the conflict the age of child

21. These results were obtained from a probit regression estimated for a sample of children aged 10–14 years where the dependent variable was whether the individual worked in the week prior to the 2001 survey. Controls include household per capita expenditures, whether they speak Indonesian, grades completed by the mother and father, the occupation of the head of household, gender of the head of household, household size, and region of residence. These estimates are not shown in the paper because of space constraints but are available upon request from the authors.

soldiers ranged from 10 to 18 years old, although most were between the age of 15 and 18 years old" (p. 18). Given this age range, it is unlikely that our results are strongly driven by increases in the number of child soldiers. However, we cannot completely exclude this channel given the lack of sufficiently rigorous empirical evidence.

Taken together, the various pieces of evidence discussed above point to school dropout—most likely owing to economic necessity, but potentially for other reasons—as an important channel through which the conflict may have negatively affected educational outcomes among boys in Timor Leste. These effects may have considerable consequences for the country's future economic and political stability given the accumulation of negative education shocks among boys over the 25-year conflict, which may have trapped a significant number of individuals in cycles of low human capital and low productivity. In particular, recent studies have reported that large numbers of young men who dropped out of school during the conflict in Timor Leste are currently members of gangs and martial arts groups in Dili, which are responsible for increases in insecurity and violence in Timor Leste (Kostner and Clark 2007; Muggah et al. 2010; Scambary 2009).

Robustness Checks

We performed several robustness checks to address some important issues that may affect the results discussed above. In addition to the various validity checks reported in previous sections, we separately address the possible exposure of the 2007 sample to the civil violence that erupted in Timor Leste in 2006 and potential biases in the 2001 and 2007 results due to nonrandom migration patterns. Supporting tables are presented in the appendix.

As mentioned in section I, in 2006, Timor Leste experienced substantial internal civil strife owing to fighting between different factions of the independence forces. The violence in 2006 resulted in 37 killings, 2,000 severely damaged houses, 3,000 completely destroyed houses, and 150,000 displaced people (Muggah et al. 2010; Scambary 2009). Most displaced people were located in the vicinity of Dili (where 65 internally displaced person camps were located) and were still displaced in 2007. Despite the decision to restart the 2007 survey once the violence had subsided (see footnote 3), it is possible that some of the results discussed in the section above are not due to exposure to the 1999 violence but due to exposure to the civil upheaval in 2006. To control for this potential exposure to the violence in 2006, we explore a variable in the 2007 dataset that captures whether an individual was absent from home in the past 12 months for security reasons (2.7 percent of the sample). Our calculations show that individuals who were absent from home for security reasons in 2006 all resided in Dili. Therefore, we believe that this dummy reliably captures the level of exposure to the 2006 violence. The results in table A.1 are nearly identical to those in table 6, indicating that our main conclusions are unlikely to be biased by the effects of the civil violence in 2006.

Another important concern is that some individuals migrated at some point in their lives.[22] The 2001 and 2007 datasets provide information on their places of birth and their current places of residence. The data do not allow us to establish when this migration occurred or whether these individuals migrated for conflict-related reasons. Thus, the migration variable is potentially noisy and prone to misclassification error. The direction of the endogeneity bias is difficult to predict a priori.[23] If, for instance, individuals did not choose their new place of residence randomly (Kondylis 2010) and those who migrated went to areas in which economic conditions are typically better (for instance, urban areas), our results would likely be underestimated. Conversely, the effect of the violence would be overestimated if migrants relocated to places where they received inferior education. In addition, individuals who decided to migrate may differ from those who did not migrate. If this is the case and, for example, only wealthier and more educated households were able to migrate, then including these individuals in our estimates would underestimate the overall effect of the violence.

To assess whether the bias deriving from migration is a serious concern in our analysis, in tables A.2 and A.3, we present estimates from regressions that include a sample of individuals who never moved from their places of birth. These estimates test whether the results in section III hold when we restrict the sample to nonmigrants. The results are broadly comparable in terms of magnitude, signs, and significance to those obtained using the full sample. In addition, the proportions of individuals who migrated to a different place are 13 percent of the 2001 sample and 19 percent and 24 percent of the 2007 samples (the 1977–92 and 1968–84 cohorts, respectively). This finding suggests that even in the extreme scenario where the estimated effect was zero for migrants, the overall estimated effect would only be attenuated by approximately one-quarter of its value. We are therefore quite confident that our results are not biased as a result of migration choices.

IV. FINAL REMARKS

The aim of this paper was to examine the effects of the 25 years of conflict in Timor Leste on educational outcomes among boys and girls exposed to

22. The migration decision should be interpreted as distinct from the occurrence of displacement in 1999 in Timor Leste. Although it is relatively common in the conflict literature to treat displacement as a migration decision (see, for instance, Chamarbagwala and Morán [2010] for Guatemala, Kondylis [2010] for Bosnia-Herzegovina, and Ibañez and Moya [2010] for Colombia), this is not an appropriate means of addressing displacement in the case of Timor Leste because our displacement variable is based on the respondents' reported displacement experience rather than a migration outcome. We have also estimated the determinants of migration and found that displacement does not play a significant role in migration decisions.

23. It is important to note that issues regarding the potential endogeneity of the migration decision need to be considered as distinct in this context from potential endogeneity concerns regarding the displacement measure, which have been discussed in section II.

violence. We began by analyzing the impact of the wave of violence that oc-
curred during the withdrawal of Indonesian troops in 1999. We first analyzed
the short-term impact of the 1999 violence on primary school attendance in
2001 and its longer-term impact on school completion for the same cohorts of
children observed again in 2007. We compared these latter results to the
impacts of the peaks of violence in the 1970s and 1980s on schooling out-
comes observed in 2007 (among those who were of primary school age at the
time of the various violent events) and to the overall average educational
impact of the conflict. This approach enabled us to compare the impact of a
long-duration conflict on educational outcomes during the overall conflict and
during peaks of violence.

In line with the existing literature on the effects of violent conflict on educa-
tional outcomes, we find that the conflict in Timor Leste led to considerable
adverse impacts on educational outcomes, particularly among boys exposed to
the violence. We find, however, that the impact of the conflict on girls' educa-
tion, although negative in the short term in terms of school attendance, did not
hinder their school attainment in the longer term because they were able to
benefit from the rapid reconstruction of the education system in violence-
affected areas. In contrast, the 25 years of violent conflict had a clearly negative
impact on the education of boys in Timor Leste that persisted across genera-
tions. This result is consistent for different peaks of violence throughout the
conflict in Timor Leste. Generations of young Timorese boys have experienced
considerable reductions in their accumulation of human capital, which may
now be reflected in increases in insecurity, unemployment, and violence in the
country since 2006.

We have discussed evidence suggesting that the negative impact of violence
on boys' education is due to boys dropping out of school. This is likely to be
caused by household investment trade offs between education and economic
survival, where boys would have been removed from school to participate in
household economic activities. It is also possible that a small number of young
boys may have dropped out of school to join armed groups.

These results have important policy implications. One implication is the im-
portance of educational recovery in areas affected by violent conflict. The
Timor Leste case suggests that early recovery may have positive results for the
lives of children (girls, in this case). Another key implication is that reconstruc-
tion policies must pay greater attention to their redistributive impacts across
genders and different population characteristics. Although girls recovered
quickly from the conflict, boys did not, despite the large investment in the
early recovery of the education system in Timor Leste. The evidence for Timor
Leste suggests that boys were very vulnerable to both the direct effects of vio-
lence on education outcomes and indirect effects through household welfare
mechanisms. This result implies that much more attention must be paid to un-
derstanding how children are affected by violent conflict and the different roles
girls and boys assume during and after the conflict because these are likely to

perpetuate the risks associated with renewed conflict and persistent vulnerabilities across generations.

APPENDIX – ROBUSTNESS CHECKS

TABLE A1. Robustness Check: Impact of Conflict on Primary School Completion in 2007, Controlling for 2007 Civil Violence

	Impact of 1999 violence (1977–1992 sample)			Impact of early years of conflict (1968–1984 sample)		
	All	Boys	Girls	All	Boys	Girls
Years of prim. school in HVI	−0.042	−0.186***	0.105**	−0.026**	−0.032**	−0.021
	(0.029)	(0.044)	(0.047)	(0.011)	(0.014)	(0.017)
Absent home past 12 months	0.083***	0.066	0.104***	0.086***	0.095**	0.079*
	(0.023)	(0.043)	(0.032)	(0.030)	(0.040)	(0.045)
N	6,676	3,383	3,293	5,195	2,625	2,570
R-squared	0.151	0.144	0.181	0.339	0.359	0.318

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses clustered at the year of birth * district level. Regressions include year and district fixed effects, district time trend (defined as the interaction between the district category and birth year), and controls (whether the household head is a farmer and the household head's level of education).

Source: Authors' computations using TLSS 2007a.

TABLE A2. Robustness Check: Impact of 1999 Violence on School Attendance in 2001, Nonmigrant Sample

	(1) All	(2) Boys	(3) Girls	(4) All	(5) Boys	(6) Girls	(7) All	(8) Boys	(9) Girls
$D*T_2$	−0.200***	−0.213***	−0.188**				−0.053	−0.071	−0.042
	(0.052)	(0.063)	(0.072)				(0.058)	(0.067)	(0.076)
$D*T_3$	−0.144***	−0.130***	−0.157***				−0.082	−0.150**	−0.037
	(0.038)	(0.050)	(0.053)				(0.053)	(0.066)	(0.074)
$H*T_2$				−0.124***	−0.119*	−0.129**	−0.027	−0.031	−0.022
				(0.047)	(0.061)	(0.062)	(0.057)	(0.077)	(0.067)
$H*T_3$				0.005	0.003	0.007	0.077	0.043	0.118*
				(0.044)	(0.054)	(0.056)	(0.055)	(0.070)	(0.065)
$D*H*T_2$							−0.263**	−0.206	−0.326**
							(0.108)	(0.129)	(0.143)
$D*H*T_3$							−0.167*	0.002	−0.343***
							(0.088)	(0.113)	(0.096)
N	2,553	1,383	1,170	2,553	1,383	1,170	2,553	1,383	1,170
R-squared	0.157	0.161	0.154	0.154	0.157	0.150	0.169	0.169	0.173

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table reports fixed effect estimates. Robust standard errors in parentheses clustered at the village level. Regressions include time effects (T_2 refers to the 1999/00 school year, and T_3 refers to the 2000/01 school year); D, H, and D*H are dummies, respectively, defined as 1 if the individual was displaced with the whole household, whether the house was completely damaged, and whether the individual was affected by both violent shocks.

Source: Authors' computations using TLSS 2001.

TABLE A3. Robustness Check: Impact of Conflict on Primary School Completion in 2007, Nonmigrant Sample

	Impact of 1999 violence (1977–1992 sample)			Impact of early years of conflict (1968–1984 sample)		
	All	Boys	Girls	All	Boys	Girls
Years of prim. school in HVI	−0.033	−0.166***	0.103*	−0.015	−0.018	−0.014
	(0.036)	(0.054)	(0.057)	(0.013)	(0.016)	(0.021)
N	5,446	2,803	2,643	3,963	2,041	1,922
R-squared	0.151	0.150	0.178	0.330	0.374	0.292

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses clustered at the year of birth * district level. Regressions include year and district fixed effects, district time trend (defined as the interaction between the district category and birth year), and controls (whether the household head is a farmer and the household head's level of education).

Source: Authors' computations using TLSS 2007a.

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