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The long-term effects of civil conflicts on education, earnings, and fertility: Evidence from Cambodia

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ABSTRACT

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This paper examines the long-term effects of exposure to civil war and genocide on the educational attainment, earnings, and fertility of individuals in Cambodia. Given the well-documented causal links between schooling and labor productivity, it is surprising that past studies show that civil conflicts reduce educational attainment, but generally not earnings of individuals. Using variation in the degree of Cambodians' exposure to civil conflicts during primary school age, we find that disruption to primary education during civil conflicts decreases educational attainment and earnings, increases fertility, and has negligible effects on health of individuals several decades later. Our findings suggest that the effect of conflict on schooling disruption has adverse consequences on long-term labor productivity and economic development. *Journal of Comparative Economics* **000** () (2015) 1–21. Monash Business School, Monash University, Wellington Road, Clayton, Victoria 3800, Australia.

1. Introduction

Civil armed conflicts have become far more common than wars between states and the average duration, and frequency, of civil wars have increased substantially over the past 50 years (Human Security Report, 2012). An increasing number of studies have shown that civil conflicts reduce the educational attainment of individuals. For example, Akresh and de Walque (2008) found that children exposed to the Rwandan genocide experienced an 18.3% decline in average years of education. However, few studies have shown a significant negative effect of exposure to conflicts on labor productivity, despite the well-documented causal links between schooling and earnings.

The main objective of this paper is to provide evidence on the long-term effects of exposure to civil conflicts during primary school age on the educational attainment, earnings, and fertility of individuals in Cambodia. During the 1970s, Cambodia experienced arguably the most intensive civil conflicts in human history. Almost 5 years of civil war under the Lon Nol (LN) regime (1970–1975) was followed by another 4 years of genocide under the Khmer Rouge (KR) regime (1975–1979). These violent periods, especially during the KR regime, impeded economic development, disrupted education and resulted in the deaths of between 1.2 million and 3.4 million across Cambodia, depending on the precise estimate used (Heuveline, 1998). We take advantage of variation in the extent to which schooling of individuals of primary school age overlapped with the civil conflicts

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during the period 1970–1979 to examine the long-term effects of civil conflict on education, earnings, fertility and health of individuals.

We find that for each year of disruption in primary education from conflict exposure, completed years of schooling fall by 2.9–3.9 months for men and by 2.2–3.5 months for women. On average, the civil conflicts resulted in 0.9–1.1 years of education loss for men and 0.6–0.9 years of education loss for women. We also find that for each year of civil conflict exposure during primary school age, average earnings fall by 6.6%–8.6% for men. However, the effect of civil conflict exposure on earnings of women is not statistically different from zero. These estimates suggest that the rate of returns to schooling is roughly 15%–20% per annum for Cambodian males.

Since the labor force participation rate is low for Cambodian women, the effect of civil conflict exposure on female labor productivity and shadow price of time may be better captured by their fertility response. Our emphasis on fertility behavior is particularly important in the context of developing countries, as these countries tend to have low female labor force participation. We find that for each year of civil conflict exposure during primary school age, fertility increases by 0.04 births for women. This number translates to a reduction of fertility by 0.23 births for an additional year of completed schooling. We show that these results are not driven by differences in the quality of schooling that different cohorts received, the intensity of KR and LN related deaths across different regions or the effects of civil conflict exposure on health.

This paper extends the growing evidence of the negative impact of conflicts on human capital accumulation. It presents evidence that civil conflicts have a long-term negative impact on the labor productivity and fertility of individuals. Existing studies, such as Ichino and Winter-Ebmer (2004), Akresh and de Walque (2008), de Walque (2006), Akresh et al, (2011), Leon (2012), Shemyakina (2011), Merrouche, (2011), Chamarbagwala and Morán (2011), Dabalen and Paul (2012), Galdo (2013) and Verwimp and van Bavel (2014) have shown that armed conflict has a significant negative effect on the educational attainment of individuals in several countries. However, evidence related to the long-term impact of conflicts on earnings and fertility is sparse. For example, Miguel and Roland (2011) find no negative effect of United States bombing in Vietnam in the Vietnam War on local poverty rates and consumption levels several decades later. Similarly, Merrouche (2011) finds that exposure to landmines during the Cambodian civil conflicts has had no effect on the earnings of Cambodian individuals.

Ichino and Winter-Ebmer (2004) and Galdo (2013) are among a limited number of studies that show that armed conflicts negatively affect the earnings of individuals decades later,[1] while few studies show long-term effects of armed conflict on fertility. Galdo (2013) shows that conflict affects earnings of individuals in Peru through its adverse health impact on individuals during their early lives, while Ichino and Winter-Ebmer (2004) show that World War II affected earnings of Austrians and Germans through its effect on schooling. Studies about the impact of conflict on fertility tend to focus on the short-term effects of war on fertility. For example, Agadjanian and Prata (2002) find that fertility dropped at the height of Angolan civil war but rebounded after the war ended. Verwimp and van Bavel (2005) find that the women subjected to forced migration during the Rwandan genocide had higher fertility than women who never migrated. In contrast, Lindstrom and Berhanu (1999) show that marital fertility in Ethiopia continued to fall in the decade after famine and war ended in the 1980s.[2]

While it is not possible to identify precisely all the channels through which exposure to civil conflicts during primary school age affect earnings and fertility, we provide evidence that the effects on earnings and fertility primarily channel through its effect on education. First, our results indicate that disability, health status, and height, some of the most obvious correlates of productivity, are uncorrelated with the years of conflict exposure during primary school age. These findings do not mean that conflict has no effect on individuals' health, but rather that the long-term effect of conflict-driven disruption to education on labor productivity does not channel through health. Second, because we exploit exogenous variation in civil conflict exposure during primary school age and focus on individuals who were much older during the civil conflicts, our results rule out early-life exposure as a likely candidate for the impact of civil conflicts on education, earnings, and fertility. Third, because we show that the variation in exposure to conflict during primary school age is unrelated to the probability of mortality and that educational attainment of surviving individuals is unrelated to the geographical intensity of mortality under the LN and KR regimes, our results are not susceptible to selective mortality.

2. Civil conflicts and education disruption in Cambodia

2.1. Background to Cambodia's civil conflicts and disruption of education

We examine the long-term effects of exposure to Cambodia's civil war in 1970–1975 (LN regime) and the genocide in 1975–1979 (KR regime) on the education, earnings and fertility outcomes of individuals. Since these two regimes directly followed one another in time, we focus on their joint effects and describe them jointly as civil conflicts.

After gaining independence from the French in 1953, Cambodia enjoyed relative economic prosperity and political stability under Norodom Sihanouk. Civil war broke out in March 1970 when Lon Nol seized power through a military coup d'état, deposing Norodom Sihanouk and declaring the creation of a new Republic. While Lon Nol's new regime (which we call the LN regime) was

[1] See also Cassar et al. (2013) who show that civil conflict in Tajikistan reduces intra-community trust and impersonal exchanges, implying that civil conflict has long lasting effect on economic development.

[2] Caldwell (2004) shows that in many historical cases of political and social upheavals, such as Germany and Austria before World War I and Japan before World War II, fertility transition was already under way, but was accelerated by the war and crisis, reflecting an increase in uncertainty that led to delays in marriage and having families.

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embraced in Cambodia's urban centers, rural support for Sihanouk was strong and there were protests against his deposition (Ayres, 2000). In response, Lon Nol sent military forces to violently crack down on protests. The protesters against Lon Nol joined the Khmer Rouge (KR). The civil war in the first half of the 1970s resulted in the death of between 30 thousand[3] and about half a million Cambodians (Becker, 1998).

The KR took power in 1975 and ruled Cambodia from 1975 to 1979. This period is commonly known as Cambodia's genocide. The aim of the KR was to create a "new" Cambodia based on the Maoist-Communist model, wherein all citizens would participate in rural work projects, often without adequate food. Markets and currency were abolished, and schools, libraries, western medicine, religion and anything associated with the previous regime was discarded (UNESCO, 2011). Between 1.7 million people, based on an estimate by the Cambodian Genocide Program at Yale University, and 3.3 million people, based on an estimate by Clayton (1998), died under the KR regime.

On 7 January 1979, the Vietnamese drove the KR to the Cambodian–Thai border and established the People's Republic of Kampuchea (PRK). The Vietnamese occupied Cambodia until 1989. When the KR regime fell in 1979, Cambodia was left with no institutions and infrastructure. There was no currency, no markets, no financial institutions, virtually no industry and most roads were in a state of disrepair (Ayres, 2000). Armed conflicts between the PKR and the remnants of the KR continued in provinces along the Cambodian–Thai border. Other parts of Cambodia were also occasionally attacked by small units of KR operating inside the country (Gottesman, 2002).

2.2. Disruption to education in the conflicts

The education system in Cambodia was mainly structured according to the French system. Under French colonization, education was primarily used to maintain political legitimacy and not well placed to respond to the human capital needs of the country (UNESCO, 2011). Under Sihanouk, educational expansion was a policy priority. The national budget for education increased dramatically and schools were built across the country, resulting in an increasing enrolment rate in primary and secondary schools (Chandler, 1996). However, most individuals residing in rural areas remained illiterate as educational progress moved at a slower pace in the more remote provinces (Desbarats, 1995).

A breakdown in the education system occurred in the 1970s as the result of its neglect by the Lon Nol government and the civil war (Ayres, 2000). School closures occurred frequently due to security risks and the industrial action of teachers, who often went on strike against the declining purchasing power of their salaries. Teachers were not alone in expressing their resentment against the LN regime. Students also protested against what they perceived to be an unjust and corrupt regime (UNESCO, 2011). Clayton (1998) noted that many schools were closed as early as 1971 in areas controlled by the KR.

When the KR gained control of Cambodia in 1975, the education system ceased at all levels and locales. The KR destroyed nearly all school buildings, equipment for educational use and library materials (Clayton, 1998). Mortality rates among educated Cambodians were highest under the KR (Chandler, 1996). Clayton (1998) cites statistics from the Ministry of Education that 75% of all teachers, 96% of tertiary students and 67% of all elementary and secondary pupils died during the 4 years of the KR regime.

The KR regime devised a new basic educational system, in which children were expected to engage in 3 years of half-time primary education. However, in practice, children attended school at most 1 or 2 h/day and in most areas there was no schooling at all (Clayton, 1998). Classes were mostly run by illiterate peasants and were organized during the lunch breaks of 14-h workdays (Chandler, 1996).

After the KR regime ended, the Vietnamese occupation of Cambodia itself did not disrupt education. Indeed, the PRK, with Vietnamese assistance, aimed to restore educational infrastructure, but, for some time, there was no educational administration in place, no curricula, no adequate learning materials and hardly any qualified teaching personnel. This was a lingering effect of the earlier conflicts. Only some 87 of the 1009 teachers in higher education prior to the KR period had survived (Pich, 1997). Many of these teachers had also fled to neighboring Thailand for re-settlement. Fighting continued in provinces along the Cambodian–Thai border in the early 1980s and schooling continued to be disrupted. It took the PRK several years to rebuild the educational infrastructure (Ayres, 2000). The educational situation during the 1980s was generally poor and the opportunity cost of schooling was extremely high. The genocide that disproportionately killed relatively young and prime-aged males led to imbalances in age and sex structures and carried significant implications for the decision to return to school. Children engaged in agricultural labor to help support their families (Desbarats, 1995).

3. Data

We derive data from several sources: the 10% micro sample of the General Population Census of Cambodia in 1998 (Census 1998) and in 2008 (Census 2008), the Cambodia-Socio-Economic Survey (CSES) from 2007 to 2010, the Cambodian Genocide Database and the 2000, 2005, and 2010 Demography and Health Surveys (DHS 2000, 2005, 2010). Most of the results presented in this paper are based on Census 2008 and CSES 2007–2010.[4]

Census 2008 provides a large sample size and rich information on education, fertility and other socio-demographic characteristics of individuals and allows us to precisely estimate the effects of exposure to civil conflict on educational attainment and fertility. We limit the Census 2008 sample to individuals born in Cambodia, who represent 99.4% of the total sample, excluding those not in Cambodia during the conflicts.

[3] According to GlobalSecurity.org. Retrieved from http://www.globalsecurity.org/military/world/cambodia/history-lon-nol.htm

[4] The census data were sourced from the Integrated Public Use Microdata Series, International (IPUMS-I), by the Minnesota Population Center (2014).

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As CSES also contains information related to the health, income, work activities and socio-economic background of individuals and other household members, much of which is not available in the Census, we use CSES to examine the effects of civil conflict exposure on the earnings and health status of individuals. Because CSES has a smaller sample size than Census 2008, we pool data from CSES for 2007–2010. Besides the CSES full sample, we also use a CSES employee subsample and CSES earnings subsample. The full sample contains all individuals of the relevant cohorts. The employee subsample contains only individuals of the relevant cohorts who are employees. The earnings subsample contains individuals of relevant cohorts who are employees or self-employees.

DHS data allow us to examine the health and mortality outcomes of individuals. DHS 2000 provides us with information about the height of childbearing age women (15–49 years old). DHS 2010 provides us with information about all the living and deceased siblings of childbearing age women in 2010. This information enables us to conduct robustness checks to examine whether selective mortality may be biasing our estimates.

3.1. Cohort selection and measurement of earnings

As we aim to examine the effects of civil conflict exposure during primary school age on the long-term outcomes of individuals, we restrict the estimation sample to individuals born between 1950 and 1965. These birth cohorts were economically active by the time of the 2010 survey and were of school age during the LN and KR regimes between 1970 and 1979. We allow for variation in years of civil conflict exposure by including those who had most likely completed their primary education before the civil war started in 1970 (e.g., cohorts born in the early 1950s) and those who were yet to complete primary school at the end of the civil conflicts. We exclude individuals born after 1965 for several reasons. First, births in these cohorts are potentially affected by the civil conflicts. Second, they were still in primary school age in the post-KR period. As we discussed above, during this post-KR period, the education system was still in a state of flux; teachers and educational institutions were non-existent for some years and fighting between the KPR and remnants of the KR continued. Third, because they are younger, their wage profiles are likely to be fairly steep compared to the older cohorts, making it more difficult to use the same age function to control for cohort differences in education and wage trends.[5]

For individuals who were employees we have data on wages and hours worked per week. For individuals who were self-employed or work in agriculture there is no data available on hours worked in the past month and information on income earned is reported at the household level. We aggregate monthly wages and diary household income for those who reported that they worked during the past 7 days and divide the total household income by the number of adults in the household who reported that they worked during the past 7 days to construct an earnings sample that pools employees and self-employees. Following this, we deflate monthly earnings per working household member to 2005 prices using the Consumer Price Index. We report results based on both monthly earnings (for the earnings subsample) and hourly wages (for the employee subsample).[6]

3.2. Khmer Rouge mortality rates

Because violent incidents occur at different points in time in specific locales over the course of a civil war, some previous studies have exploited the geographic variation in the intensity and timing of violent incidents across different regions to estimate the effects of civil conflict on health (see, e.g., Bundervoet et al., 2009; Akresh et al., 2012). Data on mortality rates across districts would provide us a means to assess if the intensity of conflict across districts has any impact on educational outcomes.

Information on the geographic intensity of the genocide between 1975 and 1979 is available in the Cambodian Genocide Database (CGD), although there is no direct information on the geographic intensity of the civil war during the LN regime. The CGD includes the district identifier of each KR mass gravesite and the estimated number of bodies in each mass grave.[7] Some graves have minimum and maximum estimates of bodies and we use the average of the two estimates in constructing district-level KR mortality rates. To do so, we divide our estimated number of deaths in a district, based on information in the CGD, by the sum of the estimated deaths under the KR and the number of individuals born in each district before 1980 who were still living in 1998, based on data in Census 1998. As we do not have information on the number of individuals who survived the KR regime, but died between 1980 and 1998 at the district level, the estimated KR mortality rates are likely higher than the true rates.[8]

We have indirect information on mortality under the LN and KR regimes in DHS as childbearing aged women reported the mortality information of their siblings. However, because the DHS sibling mortality module provides estimated total numbers of deaths under the LN and KR regimes that are many times below the lowest estimates reported in the prior literature, we only use the DHS data (DHS 2000 and DHS 2005) to investigate the spatial variation of LN war intensity and its effects on education, earnings, and fertility in a robustness section. We detail our approach in the robustness section below.

Fig. 1 illustrates the geographical variation in the estimated KR mortality rates across districts in Cambodia. The mean KR mortality rate is roughly 0.14 and the standard deviation is around 0.16. Between 70% and 90% of the population died under the

[5] Our results are robust to the inclusion of individuals born in 1966-1971 and assuming that their schooling was disrupted by post-KR conflicts.

[6] Our results are robust to outliers in wages and fertility, defined as observations plus and minus 3.3 standard deviations from the mean. The results are available upon request.

[7] The database was developed by Yale University and has been updated by the Documentation Center of Cambodia (DC-Cam). We have used both information from the original Yale database and data on additional mass grave sites and estimates of number of deaths using the DC-Cam updates. For details on the original Yale database and the Cambodian Genocide Program, see http://www.yale.edu/cgp/ and http://www.d.dccam.org/Database/Index1.htm for data kept by DC-Cam.

[8] Our results are robust to using only the minimum or the maximum estimates of bodies in the calculation of mortality rates, as well as to using absolute average estimates of bodies as the measure of intensity.

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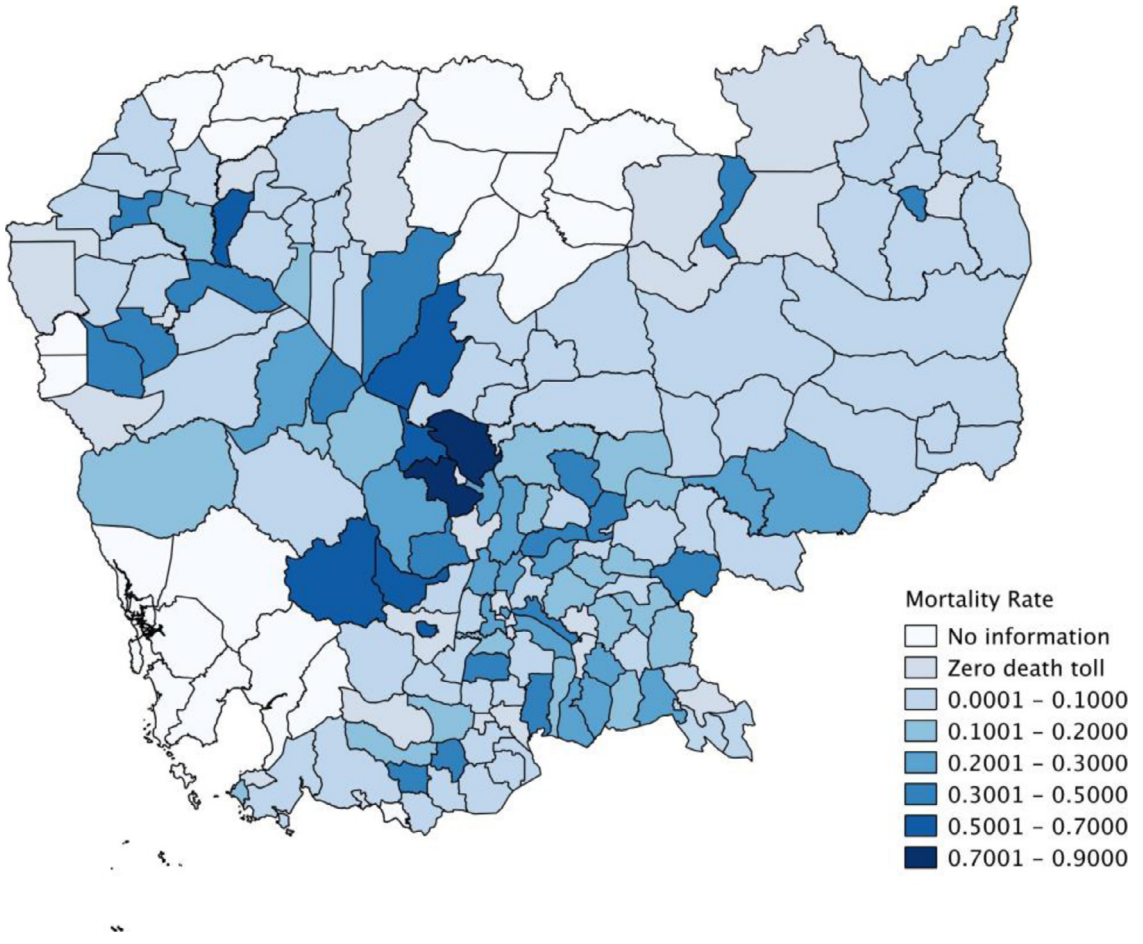

Fig. 1. Geographical distribution and intensity of mortality under the Khmer Rouge regime.

KR regime in districts in the central province of Kampong Chhnang, reflecting the high proportion of Muslims in this province. The majority of districts in provinces neighboring Vietnam, such as Kracheh and Kampong Cham, have mortality rates below 1%. Five provinces (Kaoh Kong, Preah Vihear, Otdar Mean Chey, Krong Kaeb, and Krong Pailin), which lie at Cambodia's borders with Laos and Thailand, have no information on the estimated number of deaths under the KR regime in the Cambodia Genocide Database.

3.3. Descriptive statistics

Table 1 provides summary statistics of the main variables by sample. Census 2008 and CSES 2007–2010 samples have 44% of males. The earnings subsample includes both employees and self-employees and represents 75% of the CSES full sample. Males have much higher average years of schooling than females in all samples. However, the mean years of educational attainment in CSES for males is slightly higher than in the Census sample (5.3 years vs. 4.9 years), whereas the average number of years of exposure to civil conflict during primary school age is similar across samples. The average number of children ever born in our estimation sample, which consists of 97,879 women, is 4.4. For the CSES employee sample, real wages are higher for males than females. Male employees, on average, earned nearly twice as much as their female counterparts, which might reflect the shortage of males in the cohorts born in 1950–1965.

4. The long-term impact of civil conflict

4.1. The long-term effects of conflict on educational attainment

There are two potential sources of variation in the exposure to civil conflicts that may influence educational attainment. The first is cohort variation in the years of exposure to civil conflict during primary school age. Since individuals tend to invest in education when young, the longer the duration of a conflict, the higher is the likelihood for an individual to shorten her educational attainment through delaying schooling or dropping out early. As the births of our sample cohorts occurred before

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Table 1
Descriptive statistics.

Main variables	Census			CSES								
	Full sample			Full sample			Employee sample			Earnings sample[a]		
	All	Men	Women	All	Men	Women	All	Men	Women	All	Men	Women
Years of schooling	3.88	4.92	3.09	4.05	5.29	3.09	6.15	7.09	4.39	4.25	5.32	3.08
	(3.55)	(3.60)	(3.30)	(3.68)	(3.90)	(3.19)	(4.78)	(4.64)	(4.52)	(3.80)	(3.92)	(3.27)
Years of exposure	3.35	3.52	3.21	3.24	3.40	3.12	3.52	3.54	3.48	3.34	3.46	3.22
	(2.59)	(2.57)	(2.59)	(2.60)	(2.59)	(2.60)	(2.56)	(2.57)	(2.55)	(2.59)	(2.58)	(2.59)
Number of children ever born[b]			4.35									
			(2.70)									
Real hourly wages (Riels)							1343	1565	925			
							(3301)	(3964)	(1244)			
Log of real hourly wages (Riels)							6.66	6.81	6.38			
							(1.01)	(0.99)	(0.98)			
Real monthly wages (Riels)							273,626	322,670	181,141			
							(663,456)	(797,418)	(241,417)			
Log of real monthly wages (Riels)							12.00	12.17	11.66			
							(1.01)	(0.98)	(1.00)			
Real monthly earnings (Riels)										618,475	594,872	644,319
										(3,054,393)	(2,722,769)	(3,380,460)
Log of real monthly earnings (Riels)										11.99	12.03	11.94
										(1.45)	(1.41)	(1.50)
Age	49.55	49.28	49.76	49.98	49.73	50.17	49.41	49.38	49.48	49.88	49.63	50.14
	(4.54)	(4.54)	(4.53)	(4.63)	(4.64)	(4.61)	(4.52)	(4.53)	(4.50)	(4.59)	(4.60)	(4.56)
Male	0.43			0.44			0.65			0.52		
	(0.50)			(0.50)			(0.48)			(0.50)		
Observations	172,927	75,018	97,909	15,136	6587	8549	3056	1997	1059	11,409	5963	5446

Note: The standard deviations are in parentheses. The total observations are restricted to individuals born between 1950 and 1965.
[a] Earning sample includes employees and self-employed individuals.
[b] The total observation of women who reported the number of children ever born is 97,879.

the civil conflicts and it was virtually impossible to predict *ex ante* where the civil conflicts in Cambodia would occur and how long they would last, the overlap between an individual's primary school age and the duration of the conflicts is exogenous to when the individual was born. The second source of variation in the exposure to civil conflicts is the geographical variation in the intensity of the KR and LN regimes. It is plausible that in locales in which the KR or LN regime was more active, KR or LN related deaths and the extent of school disruption were higher.

Given these two sources of variation in conflict exposure, we examine the impact of disruption to primary school education during the conflicts on the educational attainment of an individual employing following empirical specification:

$$S_{ikjt} = \beta_0 + \beta_1 \text{Exposure}_i + \beta_2 \text{DR}_j + \beta_3 \text{Exposure}_i{}^{*}\text{DR}_j + \beta_4 \mathbf{X}_{ijt} + \gamma_k + \gamma_t + u_{ikjt} \quad (1a)$$

where S_{ijkt} denotes the years of schooling of individual i in district j within province k surveyed in year t. Expsoure_i represents the number of years of exposure to conflict during the individual's primary school age years. Our measure of exposure to civil conflict is similar to those adopted in the literature. For example, Verwimp and van Bavel (2014) use years of violent conflict exposure during a child's primary school years based on the combination of birth year and province of residence at the onset of conflict.[9] DR_j denotes the KR mortality rate in district j. We also consider geographical variation in LN mortality rates in a variant of specification (1a), in which the LN mortality rate and its interaction with years of exposure to conflict are included as additional explanatory variables, in the robustness section because our measure of the LN mortality rate is based on lower-quality data. $\mathbf{X}_{ijt}$ is a set of individual characteristics including polynomials for age, which capture, *inter alia,* differences in cohort trends and the educational environment. γ_t is the survey-year fixed effects. γ_k is a set of province fixed effects to control for idiosyncratic geographical differences.[10] u_{ijkt} is the error term. We expect β_1 and β_2 to be negative if exposure to civil conflict during primary school age and the intensity of the KR policies led to lower educational attainment. We also expect β_3 to be negative if the adverse effect of KR policies on educational attainment is stronger for individuals, for whom the ages at which they would have attended primary school had greater overlap with the civil conflicts.

We measure $Expsoure_i$ by years of civil conflict exposure during one's primary school age, as most Cambodians born in the 1940s and 1950s have no more than primary education and the mean years of schooling is less than 6. We assume that primary school age ranges between the ages of 9 and 14 years old for the sampled cohorts. We are confident that this represents a realistic approximation based on the earliest Census data currently available, which is the 1998 Census. The average age was 8.8 years old among all first graders in 1998. Given the tendency for most developing countries to lower their primary school start ages

[9] Our measure is slightly different in that in Verwimp and van Bavel's (2014) study, years of exposure differ across individuals who were born in the same year, while in our study all individuals born in the same year have the same number of years of exposure.
[10] We also estimated a specification that includes district fixed effects. The results are similar and available upon request.

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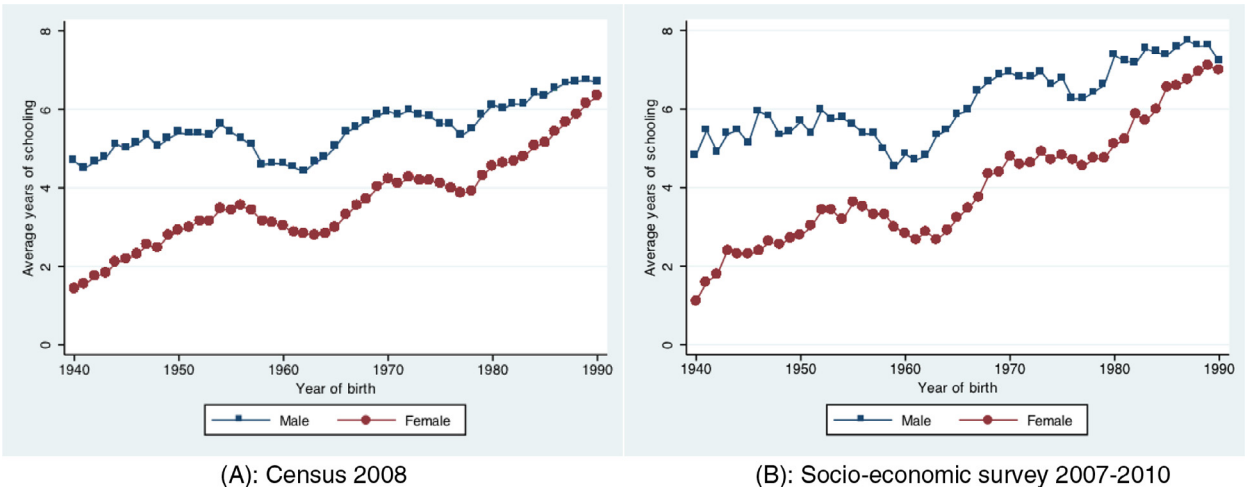

Fig. 2. Mean years of schooling, year of birth and gender.

Table 2
Years of school age exposed to civil wars by birth year.

Birth year	Year turned age 9 (primary school start age)	Year turned age 14 (primary school completion age)	Years of schooling exposed to civil conflicts
1950	1959	1964	0
1951	1960	1965	0
1952	1961	1966	0
1953	1962	1967	0
1954	1963	1968	0
1955	1964	1969	0
1956	1965	1970	1
1957	1966	1971	2
1958	1967	1972	3
1959	1968	1973	4
1960	1969	1974	5
1961	1970	1975	6
1962	1971	1976	6
1963	1972	1977	6
1964	1973	1978	6
1965	1974	1979	6

Note: The main sample includes cohorts born between 1950 and 1965. The cohorts born between 1966 and 1971 are added to the sample in some robustness checks.

over time, it is likely that the typical school commencing age was older in Cambodia in the period spanning the 1950s and 1960s. Thus, we conservatively assume that average school start age was 9 during the 1950s and 1960s.

Fig. 2 illustrates the difference in mean years of schooling across birth cohorts. Fig. 2A exhibits a smoother trend than Fig. 2B, as the Census data have more observations and thus smaller sampling error than the CSES data. The mean years of schooling was gradually rising for cohorts born before the mid-1950s. However, it started to fall quite significantly for cohorts born in 1955 onwards. Mean years of schooling bottomed out for cohorts born between 1958 and 1962 and then began to rise again.

Differences in the number of years of exposure to civil conflict when the various cohorts were of primary school age can explain the patterns in Fig. 2. If Cambodians born in the 1950s and 1960s mostly started school at 9, then the civil conflicts between 1970 and 1979 would likely cut short the total years of schooling of those born in the early 1950s and delay the commencement of school of those born in the late 1960s. For example, an individual born in 1960 would only have completed 1 year of education when the civil war broke out in 1970. This person might not continue schooling at all during the decade of civil conflicts in Cambodia. By the time the civil conflicts ended in 1979, this person was already 19 years old and might find that the opportunity cost of returning to school was too high. On the other hand, an individual born in 1965 might delay starting school until the conflicts between the KPR and remnants of the KR ended in the 1980s and start primary school education in his or her late teenage years. However, as the cost of not working or getting married in a post-war environment was high and the school system was in disarray for several years, such a person might not have returned to school after 1979.

Table 2 illustrates how years of exposure to civil conflicts during primary school age vary across birth cohorts under the assumption that primary school age falls between 9 and 14 years old and that both the LN and KR regimes disrupted schooling.

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Table 3
Estimates of civil conflict exposure and mortality intensity at district level on educational attainment of 1950–1965 birth cohorts.

	Census			CSES					
Dependent variable:	Full sample			Full sample			Earnings sample		
years of schooling	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)	All (7)	Men (8)	Women (9)
Years of exposure	−0.221***	−0.257***	−0.187***	−0.301***	−0.324***	−0.282***	−0.319***	−0.324***	−0.307***
	(0.014)	(0.018)	(0.016)	(0.049)	(0.071)	(0.056)	(0.055)	(0.076)	(0.064)
Years of exposure* KR mortality rate	0.003	0.012	0.004	−0.039	−0.090	0.019	−0.047	−0.111	0.033
	(0.039)	(0.060)	(0.034)	(0.082)	(0.131)	(0.088)	(0.104)	(0.131)	(0.128)
KR mortality rate	0.224	−0.048	0.403	0.419	0.395	0.385	0.325	0.410	0.228
	(0.391)	(0.422)	(0.395)	(0.642)	(0.801)	(0.629)	(0.753)	(0.814)	(0.835)
Age	−0.157***	−0.876***	0.394***	−0.564***	−1.463***	0.140	−0.758***	−1.627***	0.254
	(0.055)	(0.082)	(0.067)	(0.162)	(0.257)	(0.176)	(0.202)	(0.276)	(0.264)
Age squared	0.001	0.008***	−0.005***	0.004***	0.013***	−0.003	0.006***	0.014***	−0.004
	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Male	1.851***			2.106***			2.045***		
	(0.034)			(0.083)			(0.073)		
Provincial level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.140	0.094	0.074	0.221	0.177	0.123	0.232	0.183	0.139
Observations	165,806	71,592	94,214	14,525	6300	8225	10,950	5698	5252

Note: Robust standard errors clustered by district in parentheses. Regressions include sampling weights. KR-mortality rates measure the mortality rates under Khmer Rouge regime based on the estimated deaths by district in the Cambodian Genocide Database and the number of individuals born before 1980 based on their district of birth. Statistical significance of estimates remains unchanged when we include a set of district fixed effects and drop the KR mortality rate variable (results available upon request).

*** Significant at 1%.

** Significant at 5%.

* Significant at 10%.

Specifically, individuals born between 1961 and 1965 were most significantly affected by the civil conflicts, which is consistent with the trough in Fig. 2.

Table 3 presents estimates based on regression specification (1a). It shows that neither the coefficients on the KR mortality rates, nor the interaction term between the KR mortality rates with years of exposure, is statistically significant. This result suggests that geographical variation in the intensity of KR policies did not have differential effects on the schooling of individuals exposed to the civil conflicts during primary school age. Similarly, we also find that geographical variation in KR mortality rates did not affect earnings and fertility (see Table A1). The lack of effect is plausible because the KR gained control of the whole country within a short period of time and implemented a national plan that closed down the educational system. Thus, despite the geographical variation in KR related deaths evident in Fig. 1, the evidence suggests that the disruption to education under KR was fairly uniform across the country. Given these findings, we drop the district level variation in KR mortality rates and estimate the effect of conflict exposure during primary school age on educational attainment using a specification that includes a set of district fixed effects:

$$S_{ijt} = \pi_0 + \pi_1 \text{Exposure}_i + \pi_2 \mathbf{X}_{ijt} + \gamma_j + \gamma_t + \varepsilon_{ijt} \quad (1b)$$

Table 4 reports estimates using Census and CSES's full and earning samples. The estimated coefficients on years of exposure to civil conflict are all statistically significant at conventional levels (columns 1–9 in Table 4). Although estimates based on Census data tend to be smaller in magnitude compared to those based on CSES data, the CSES data tend to generate wider standard errors and the 95% confident intervals of the two sets of estimates overlap. For each additional year of exposure to civil conflict during primary school age, educational attainment is reduced by 2.5 months in the Census full sample (column 1) and 3.6 months in the CSES full sample (column 4). The mean years of conflict exposure during primary school age is 3.4 years in the Census data, compared to 3.2 years in the CSES data. These estimates imply that the conflicts result in an average loss of 0.7–1.0 years of schooling. Both the CSES full sample (column 4) and earnings sample (column 7) suggest that there is roughly a 4-month reduction in schooling for each year for which the individual is exposed to the conflicts. These estimates translate to a loss, on average, of 1.1 years of education due to the conflicts.

Table 4 also shows that conflict has a stronger effect on education loss for men than for women. For a man, an additional year of exposure to civil conflict during the age range in which he should attend primary school results in a reduction of completed schooling of 2.9 months in the Census sample and 3.9 months in the CSES full sample. For a woman, the estimated effect is 2.2 months and 3.5 months, respectively. In the CSES earnings sample, men and women experienced an educational loss of 3.9 months and 3.5 months, respectively, for each additional year of exposure to civil conflict (columns 8 and 9). On average, the results demonstrate that men exposed to conflict suffered a reduction in years of schooling of 0.9 years in the Census sample, 1.1 years in the CSES full sample, and 1.2 years in the CSES earnings sample, while for women exposed to conflict, there was a comparable reduction in years of schooling of 0.6 years in the Census sample, 0.9 years in the CSES full sample and 1 year in the CSES earnings sample.

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Table 4
Estimates of the effect of the civil conflicts on educational attainment of 1950–1965 birth cohorts.

	Census			CSES					
Dependent variable:	Full sample			Full sample			Earnings sample		
years of schooling	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)	All (7)	Men (8)	Women (9)
Years of exposure	−0.212***	−0.243***	−0.180***	−0.303***	−0.326***	−0.289***	−0.322***	−0.328***	−0.289***
	(0.013)	(0.017)	(0.015)	(0.042)	(0.063)	(0.049)	(0.048)	(0.070)	(0.060)
Age	−0.139**	−0.866***	0.418***	−0.551***	−1.479***	0.218	−0.794***	−1.675***	0.278
	(0.055)	(0.079)	(0.067)	(0.147)	(0.237)	(0.175)	(0.163)	(0.257)	(0.228)
Age squared	0.001	0.008***	−0.005***	0.004***	0.013***	−0.004**	0.006***	0.015***	−0.004*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Male	1.855***			2.048***			2.003***		
	(0.032)			(0.084)			(0.076)		
District level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.175	0.134	0.112	0.280	0.247	0.208	0.292	0.255	0.233
Observations	172,927	75,018	97,909	15,136	6587	8549	11,409	5963	5446

Note: Robust standard errors clustered by district in parentheses. Regressions include sampling weights.
*** Significant at 1%.
** Significant at 5%.
* Significant at 10%.

Male cohorts who were older than primary school age at the onset of the civil war have roughly an average 6 years of completed schooling, while equivalent female cohorts have only average 3 years of schooling (Fig. 2A and B). Thus, the negative effect of conflict exposure on education attainment is relatively stronger for females. The gender differences likely reflect the fact that Cambodian girls have less schooling to start with and were more likely to leave school after primary school in the 1960s (de Walque, 2006).

4.2. The long-term effects of civil conflict exposure on earnings and fertility

First, we estimate the reduced-form effect of exposure to civil conflict during primary school age on earnings, using the following specification:

$$\log W_{ijt} = \alpha_0 + \alpha_1 Exposure_i + \alpha_2 \mathbf{X}_i + \gamma_j + \gamma_t + \upsilon_{ijt} \quad (2)$$

where $\log W_{ijt}$ denotes the logarithm of weekly/monthly earnings of individual i in district j for survey-year t. Because most females in the relevant birth cohorts are not in the labor force, Eq. (2) will not fully reflect the reduced-form effect of civil conflict exposure on female labor productivity. Since the fertility decision is likely influenced by the shadow price of female labor productivity, we also estimate the reduced-form effect of civil conflict exposure during primary school age on female fertility later in life:

$$F_{ij} = \tau_0 + \tau_1 Exposure_i + \tau_2 \mathbf{X}_i + \gamma_j + \varepsilon_{ij} \quad (3)$$

where F_{ij} denotes the number of children ever born to female i in district j reported in Census 2008. Since the mean age of the sampled females was 50 in 2008, the dependent variable should reflect the completed fertility of women in the sample.

We report the reduced-form estimates of the effects of exposure to civil conflict during primary school age on the log of hourly wages, log of monthly earnings and fertility of individuals in Table 5. Columns 1 and 4 show that the effects of exposure to civil conflict during primary school age on the log of hourly wages and log of monthly wages of employees are almost identical. Hourly and monthly wages are reduced by 5.3% and 3.8%, respectively, for an additional year of exposure to civil conflict during primary school age. Since the average years of civil conflict exposure during primary school age are 3.5 years for employees, their hourly and monthly wages decrease by 18.6% and 13.3% on average. For the earnings subsample in which we use the log of monthly earnings as the dependent variable, the impact of conflicts on earnings is similar to that in the employee sample. For every year of civil conflict exposure during primary school age, monthly earnings fall by 5.6%. This effect is equivalent to a reduction of 18.5% of monthly earnings, as average years of exposure are 3.3 years for this sample.

When splitting the samples by gender, we find that for each year of exposure to civil conflict during primary school age, hourly wages fall by 8.6% for males, monthly wages fall by 6.9% for males in the employee sample and monthly earnings fall by 6.6% for males in the earnings sample. The effects of exposure to conflict on hourly wages and monthly earnings are not statistically significant for women. Thus, disruption to education during civil conflicts reduces the earnings of males, but not the earnings of females.

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Table 5
Estimates of the effects of civil conflict on earnings and fertility of 1950–1965 birth cohorts.

	CSES									Census
Dependent variable	Employee sample (log of hourly wages)			Employee sample (log of monthly wages)			Earnings sample (log of monthly earnings)			Full sample fertility
	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)	All (7)	Men (8)	Women (9)	Women (10)
Years of exposure	−0.053**	−0.086**	0.016	−0.038	−0.069*	0.041	−0.056***	−0.066**	−0.041	0.041***
	(0.025)	(0.042)	(0.045)	(0.025)	(0.040)	(0.045)	(0.019)	(0.026)	(0.030)	(0.010)
Age	0.058	0.127	−0.081	0.069	0.116	−0.088	−0.027	−0.015	0.009	0.415***
	(0.110)	(0.138)	(0.184)	(0.112)	(0.132)	(0.203)	(0.079)	(0.101)	(0.118)	(0.056)
Age squared	−0.001	−0.002	0.001	−0.001	−0.002	0.001	−0.000	−0.000	−0.000	−0.004***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.347***			0.465***			0.060**			
	(0.050)			(0.052)			(0.029)			
District level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
R-squared	0.271	0.273	0.340	0.268	0.261	0.309	0.221	0.236	0.229	0.044
Observations	3056	1997	1059	3056	1997	1059	11,409	5963	5446	97,879

Note: Robust standard errors clustered by district in parentheses. Regressions include sampling weights.
*** Significant at 1%.
** Significant at 5%.
* Significant at 10%.

We now turn to the effect of conflict exposure during primary school age on fertility later in life. For each year of exposure to conflict during primary school age, female fertility increases by 0.04 births (column 10 in Table 5). This estimate implies that, on average, the civil conflicts resulted in about a 1% increase in female fertility.[11]

Overall, the results presented in this section indicate that conflict exposure during primary school age significantly impedes labor productivity later in life. The decrease in labor productivity reflects in the loss of earnings for men and the increase in fertility for women. Thus, civil conflict has a long-term adverse impact on labor productivity.

4.3. The causal effects of schooling

If the effects of exposure to civil conflicts on earnings and fertility solely channel through its effect on educational attainment, then we can estimate the causal effects of education on earnings and fertility in an instrumental variable (IV) framework. Specifically, we can use Eq. (1b) as the first-stage of the IV regression and estimate the returns to schooling and the effect of education on fertility in the following second-stage regressions:

$$\log W_{ijt} = \delta_0 + \delta_1 \widetilde{S}_i + \delta_2 \mathbf{X}_i + \gamma_j + \gamma_t + \varepsilon_{ijt} \tag{4}$$

$$F_{ij} = \gamma_0 + \gamma_1 \widetilde{S}_i + \gamma_2 \mathbf{X}_i + \gamma_j + \varepsilon_{ij} \tag{5}$$

The instrumental variable is the years of exposure to civil conflicts during primary school ages ($Exposure_i$). This IV must satisfy two conditions to identify the causal effects of schooling. First, as we show in Table 4, it strongly predicts the completed years of schooling. Second, as we discuss in Section 4.1, the year in which the civil war broke out in Cambodia and the year in which the Khmer Rouge regime ended are likely exogenous to the birth years of the 1950–1965 cohorts. To interpret the IV estimates as the causal effects of schooling, the channel through which the IV influences earnings and fertility must solely mediate through education. We further assess if this exclusion restriction is violated by checking whether exposure to civil conflict has an effect on other correlates of earnings and fertility in the next subsection. Since it is not possible to completely rule out that exposure to conflict during one's primary school education influenced earnings and fertility through channels other than education, we discuss other possible channels in details in Section 4.4, and how one should interpret our findings if the exclusion restriction fails to hold.

Table 6 presents the IV estimates, as well as the Ordinary Least Squares (OLS) estimates for comparison. The OLS estimates show statistically significant returns to schooling. For employees, the rates of return to schooling based on the log of hourly wages and log of monthly wages (columns 1 and 4) is about 4% per year. The estimated rate of return to schooling in the earnings sample is 4.5% per year (column 7). These estimates are generally low and much smaller than the OLS estimates that Psacharopoulos and Patrinos (2004) find for Asia (9.9%) and other low-income countries (10.9%).

The IV estimates in Table 6 show that the first-stage *F*-statistics are strongly predictive of educational attainment, except for the female employee sample. The IV estimates are generally greater than the OLS estimates. This result is consistent with most previous evidence that has used IVs to address measurement errors and omitted ability bias. When pooling genders, the estimated rate of return to schooling for employees is 11.6% based on hourly wages (column 1) and 8.3% based on monthly wages (column 4). For the pooled sample, the IV estimate rate of the return to schooling is 17.5% based on monthly earnings of employees and individuals who are self-employed (column 7).

The IV results in Table 6 show that the rate of return to schooling is statistically significant for men, but not women, across different samples. For male employees, the rate of return to an additional year of schooling is roughly 19.1% based on hourly wages and 15.1% based on monthly wages. The rate of return to each year of schooling is 20% for males in the earnings sample. Although these estimates seem high compared to the OLS estimates, they are similar to IV estimates in neighboring Asian countries, such as 14–16% in Thailand (Warunsiri and McNown, 2010) and 14.5% in the Philippines (Maluccio, 1998).

One explanation for the relatively high rate of return to schooling for Cambodian men is that the high death toll of men, especially more educated men, under the KR regime lowered the supply of high skilled men. The shortage of skills in a relatively gender-segmented labor market drives up the returns to schooling. A second potential explanation is that as average educational attainment is low in Cambodia, the marginal return to an additional year of schooling is likely to be high if returns to schooling exhibit diminishing marginal returns. The existence of diminishing returns to education is well documented at a cross-country level (Psacharopoulos and Patrinos, 2004).

Table 6 also shows that the estimated return to schooling for women is not significantly different from zero. Compared to the significant OLS estimates, the insignificant IV estimates imply that there is positive selection into schooling on the basis of labor productivity among women. The lack of returns to schooling is consistent with two observations on female employment in Cambodia. The first is that female labor force participation in the formal sector in Cambodia is very low. The second is the types of low-skilled jobs, in which Cambodian females are engaged. This is reflected in Fig. 3, which shows that women tend to work in elementary occupations.

We now turn to the estimated effect of schooling on female fertility, measured using the number of children ever born in Census 2008. The OLS estimate in column 10 in Table 6 indicates that a 1-year increase in years of schooling is associated with a

[11] We also examine if conflict exposure during primary school age affects the probability of being ever married and being currently married. We find no effects. Thus, the increase in fertility is not driven by changes in the probability of marriage.

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Table 6
Estimates of returns to schooling of 1950–1965 birth cohorts.

Dependent variable	CSES Employee sample (log of hourly wages)			Employee sample (log of monthly wages)			Earnings sample (log of monthly earnings)			Census Full sample fertility
	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)	All (7)	Men (8)	Women (9)	Women (10)
Years of schooling										
OLS	0.037***	0.031***	0.052***	0.035***	0.028***	0.052***	0.045***	0.040***	0.057***	−0.042***
	(0.006)	(0.007)	(0.010)	(0.006)	(0.008)	(0.008)	(0.009)	(0.009)	(0.012)	(0.004)
TSLS	0.116**	0.191**	−0.038	0.083*	0.151*	−0.099	0.175***	0.200***	0.143	−0.226***
	(0.051)	(0.093)	(0.100)	(0.050)	(0.090)	(0.109)	(0.062)	(0.074)	(0.106)	(0.056)
Control variables:										
Age and age squared	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Male	Yes	–	–	Yes	–	–	Yes	–	–	–
District level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
First-stage *F-statistic*	16.66***	11.89***	4.36**	16.66***	11.89***	4.36**	44.55***	22.13***	23.39***	150.47***
Observations	3056	1997	1059	3056	1997	1059	11,409	5963	5446	97,879

Note: Robust standard errors clustered by district in parentheses. Regressions include sampling weights.
*** Significant at 1%.
** Significant at 5%.
* Significant at 10%.

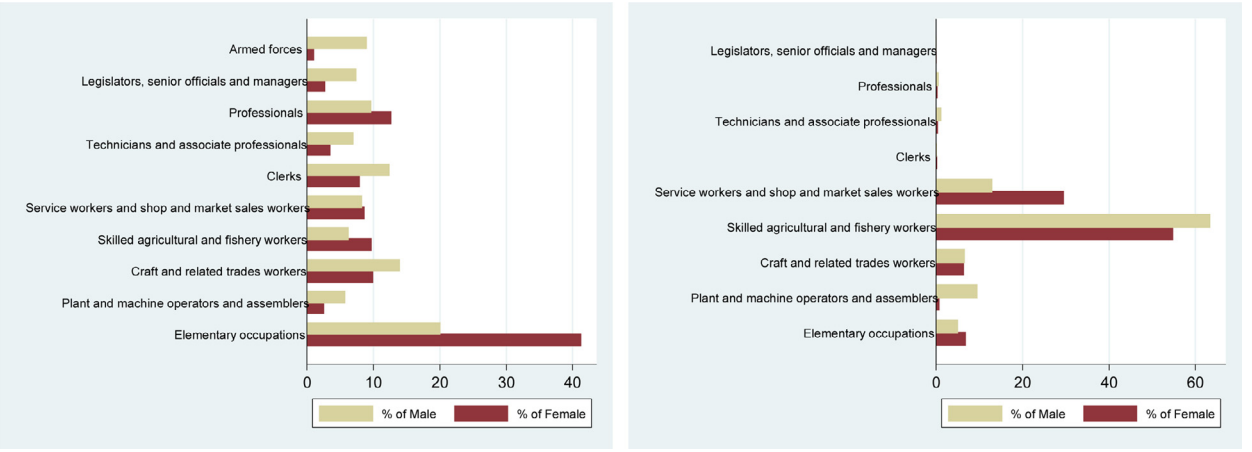

Fig. 3. Main occupations by gender. Notes: Major groups of occupations based on ILO, ISCO-88. The figure on the left is for employees and the figure on the right is for individuals who are self employed. Total observations are restricted to people born between 1950 and 1965.

0.04 reduction in the number of lifetime births. The IV results show a much larger effect: a 1-year increase in female schooling reduces fertility by 0.23 births. This implies that the difference in fertility between a woman without any formal education and one who completed primary education is more than one child. The estimate for Cambodia is slightly smaller than Osili and Long's (2008) estimate of 0.26 for Nigeria. The difference most likely reflects the fact that Osili and Long (2008) focus on the fertility of women in their prime childbearing years, while most women in our sample were older than 45 in 2008.

4.4. Other possible channels of conflict exposure on earnings and fertility

We show that the effects of schooling on male earnings and female fertility that are driven by civil conflict exposure during primary school age are fairly similar to previous estimates for countries with similar level of economic development to Cambodia. Never the less, it is possible that the negative effects of conflict exposure on labor productivity may also channel through its adverse effects on other correlates of labor productivity, such as health and school quality. If the channel via health is particularly strong, our estimates may also suffer from selection bias as a result of selective mortality.

4.4.1. Health and mortality channels

It is possible that exposure to civil conflicts during primary school age may also affect the health and mortality of individuals. First, although all cohorts in our sample experienced identical years of civil conflicts and were at least 5 years old at the onset of the civil war, one could argue the long-term health of an individual may depend on the extent to which the person was exposed to conflicts during adolescence. In particular, we would expect that the effect of being exposed to conflict during the adolescent growth spurt to be negative as a result of lack of food and nutrition. To assess this possibility, we test whether indicators of health problems available in CSES and Census 2008, such as being sick during the last 30 days, having a disability and experiencing various forms of physical and psychological difficulties, are correlated with years of civil conflict exposure during primary school age. We also use the DHS (2000) data to examine whether the height and likelihood of stunting (height $<$ 150 cm) of women born between 1950 and 1965 are correlated with years of civil conflict exposure during primary school age, as de Walque (2006) shows that girls experiencing adolescence during the KR regime period were shorter and more likely to be stunted than girls born in later cohorts.

Table 7 reports results for a range of potential health problems, plus height and the likelihood of stunting of individuals exposed to the conflicts. Most of the estimates are close to zero and statistically insignificant. There is also no systematic pattern in the signs of the estimated coefficients. The fraction of significant estimates (7 out of 55) is roughly consistent with what the null hypothesis of zero effect would suggest at conventional levels of significance. Moreover, those coefficients that are statistically significant are small and have a sign opposite to what we expected, indicating that, if anything, more years of exposure to conflict decreases health problems later in life.

Second, it is possible that the adverse health effects of the conflicts were so severe that they resulted in the premature death of individuals who are consequently not being sampled in our study. To examine this possibility, we use the DHS (2010) data to examine whether the likelihood of surviving until 2010 is correlated with years of civil conflict exposure during primary school age. Specifically, DHS (2010) asks women of childbearing age (15–49 years old) information related to the survival status and date of birth of all siblings. We use the sibling information to reconstruct a sample of individuals who were ever born between 1950 and 1965 and test whether the likelihood that they had died by 2010 is correlated with years of exposure to conflict during primary school age.[12] The results, which are reported in column 9 in Table 7, indicate that years of exposure to conflict during primary school age and mortality are not correlated.

[12] Our approach of using sibling information to construct survival status is similar to that in De Walque (2005).

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Table 7
Estimates of effect of civil conflicts on health related outcomes of 1950–1965 birth cohorts.

	CSES: Employee sample			CSES: Earnings sample			Census: Fertility	DHS 2000	DHS 2010
	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)	Women (7)	Women (8)	All (9)
Dependent variables									
Ill/injured during the last 30 days	0.0004	0.0090	−0.0152	−0.0029	0.0049	−0.0136*			
	(0.0125)	(0.0159)	(0.0209)	(0.0059)	(0.0083)	(0.0082)			
Disabled	−0.0044	−0.0026	−0.0017	−0.0092**	−0.0103*	−0.0091			
	(0.0082)	(0.0110)	(0.0130)	(0.0044)	(0.0060)	(0.0059)			
Difficulty in seeing	0.0038	0.0036	0.0073	−0.0023	−0.0022	−0.0028	0.0003		
	(0.0063)	(0.0085)	(0.0079)	(0.0035)	(0.0048)	(0.0045)	(0.0003)		
Difficulty in hearing	−0.0044**	−0.0048	−0.0033	−0.0005	−0.0005	−0.0004	0.0000		
	(0.0020)	(0.0032)	(0.0024)	(0.0015)	(0.0020)	(0.0023)	(0.0002)		
Difficulty in speaking	0.0002	0.0005	0.0000	−0.0008	−0.0011	−0.0003*	−0.0001		
	(0.0005)	(0.0008)	(0.0000)	(0.0008)	(0.0016)	(0.0002)	(0.0001)		
Difficulty in moving	−0.0025	−0.0020	0.0011	−0.0033**	−0.0061**	−0.0012			
	(0.0036)	(0.0041)	(0.0059)	(0.0017)	(0.0027)	(0.0024)			
Difficulty in sensing	−0.0009	−0.0010	−0.0004	−0.0015	−0.0011	−0.0022			
	(0.0018)	(0.0034)	(0.0005)	(0.0010)	(0.0010)	(0.0016)			
Psychological or behavioral difficulties	−0.0016	0.0000	−0.0064	−0.0013	0.0001	−0.0025	0.0003		
	(0.0016)	(0.0000)	(0.0066)	(0.0010)	(0.0003)	(0.0021)	(0.0002)		
Height (centimetres)								−0.173	
								(0.114)	
Stunted (height <150 cm)								0.013	
								(0.012)	
Dead by 2010									−0.0099
									(0.0073)
Observations	3056	1997	1059	11,409	5963	5446	97,879	2771	9391

Note: We report the estimated coefficient for the years of civil conflict exposure during primary school age. Regressions control for age, age squared, gender, district level fixed effects and survey-year fixed effects whenever possible. Illness/injured during the last 30 days equals 1 if individual is sick or injured during the last 30 days and 0 if otherwise. Disabled equals 1 if individual is disabled and 0 if otherwise. Having seeing, hearing, speaking, moving, sensing, as well as having psychological or behavioral difficulties are coded similarly. Regressions adjusted for sampling weights. Robust standard errors clustered by district in parentheses.

*** Significant at 1%.
** Significant at 5%.
* Significant at 10%.

One may make the opposite argument that children who survived the civil conflicts, especially under the KR regime, are likely to be the fittest. In particular, cohorts that experienced more years of conflict during primary school age have a higher proportion of healthy survivors than those that experienced fewer years of conflict during primary school age. Past studies indicate that healthier individuals should have higher labor productivity. However, we find that years of exposure to conflict decreases educational attainment and, in turn, labor productivity. As this form of selection channel inflates earnings, if it is present, we have likely estimated the lower bound of the negative effects of exposure to civil conflict on labor productivity that channels through education.

One may also argue that because the KR targeted intellectuals, only individuals with high marginal cost of schooling and low labor market productivity survived. Then, in locales in which the KR killed more individuals, fewer individuals with high labor productivity and schooling survived. This may be the case, but we show in Table 3 that the geographical variation in KR mortality rates does not influence educational attainment of the sampled cohorts. More crucially, our estimates are not sensitive to this form of selection bias as our regression specifications include a set of district fixed effects.

4.4.2. School quality channel

Another possible channel is that earnings could be affected by the quality of education received by the cohorts born between 1950 and 1965. We explore whether exposure to civil conflicts during primary school age has a significant effect on the quality of education as measured by the ability to read or write a simple message and to speak a foreign language after controlling for educational attainment. The results in the top panel of Table 8 indicate that after controlling for the years of schooling, exposure to conflicts during primary school age does not affect the ability to read or write a simple message. However, conflict exposure has a significant negative effect on the ability of males in the earnings sample to speak a foreign language, though the magnitude is only 1.6 percentage points. To further check whether the estimated returns to schooling are sensitive to the difference in foreign language skill, we add the foreign language indicator as an additional control variable and report the results in the bottom panel of Table 8. The estimated returns to schooling for the earnings sample are almost identical to those in Table 6. The estimated returns to schooling for the employee sample become larger and noisier. Overall, the estimated returns to schooling, especially for the earnings sample, are not likely influenced by differences in school quality.

While we have ruled out various health and school quality channels, it is not possible to fully ascertain whether exposure to conflicts during primary school years has other direct effects on earnings and fertility later in life (not mediated through

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Table 8
The effects of civil conflict exposure on school quality indicators and returns to schooling after controlling for school quality indicators of 1950–1965 birth cohorts.

	Effects of civil conflict exposure on school quality indicators					
	CSES employee sample			CSES earnings sample		
	All (1)	Men (2)	Women (3)	All (4)	Men (5)	Women (6)
Dependent variables						
Can read a simple message	−0.0086	−0.0185	0.0065	0.0009	−0.0001	0.0025
	(0.0099)	(0.0118)	(0.0200)	(0.0049)	(0.0067)	(0.0075)
Can write a simple message	−0.0092	−0.0152	−0.0024	0.0004	−0.0004	0.0007
	(0.0103)	(0.0113)	(0.0205)	(0.0048)	(0.0065)	(0.0071)
Speak any foreign language	−0.0139	−0.0182	−0.0035	−0.0058	−0.0164**	0.0036
	(0.0149)	(0.0168)	(0.0157)	(0.0054)	(0.0077)	(0.0053)
	IV estimates of returns to schooling after controlling for school quality					
Explanatory variables						
Years of schooling	0.112*	0.204	−0.064	0.175***	0.200**	0.143
	(0.063)	(0.124)	(0.130)	(0.066)	(0.084)	(0.105)
Speak any foreign language	0.068	−0.178	0.624	0.010	0.009	−0.024
	(0.228)	(0.398)	(0.571)	(0.145)	(0.181)	(0.192)
Observations	3056	1997	1059	11,409	5963	5446

Note: In the top panel, the coefficients reported are for the years of conflict exposure. All specifications in the top panel control for age, age squared, years of schooling, district level fixed effects, and survey-year fixed effects. In the bottom panel, the coefficients reported are for the years of schooling. All specifications in the bottom panel control for whether the person speaks any foreign language, age, age squared, district level fixed effects, and survey-year fixed effects. Regressions adjusted for sampling weights. Robust standard errors clustered by district in parentheses. Can read a simple message and can write a simple message are binary variables equal 1 if yes and 0 if otherwise.

*** Significant at 1%.
** Significant at 5%.
* Significant at 10%.

education). For example, the exposure to war during adolescence may have other direct, but unobserved psychological and physical effects on labor productivity and fertility than exposure to war at an older age may have. As these factors most likely reduce labor productivity, if they are present, we potentially over-estimate the returns to schooling and the effects of education on fertility using the IV specifications in the previous section.

5. Robustness checks

5.1. Spatial variation in the intensity of the LN war

We show earlier that the outcomes of interest of individuals whose primary school years were more exposed to civil conflicts do not vary with the intensity of the KR conflict. We report similar results in this robustness section that the outcomes of interest of individuals whose primary school years were more exposed to civil conflicts also do not vary with the intensity of the LN war as measured by excess mortality under the LN regime.

Because mortality information of childbearing aged women's siblings in DHS is the only source of data available for us to construct spatial variation of war intensity during the LN period, we use it to investigate whether the intensity of war during the LN period influences education, earnings, and fertility. The specification we use is essentially specification (1a) plus two additional variables: LN war intensity (i.e., mortality rate under LN) and its interaction with years of exposure during primary school years.

However, using DHS data to estimate mortality rates across different regions suffer from a number of problems. The major concern is the sample used to calculate the mortality rates. The DHS sample includes only childbearing-age women who survived the conflicts or who were born to surviving parents of their dead siblings after the conflicts. It turns out that the DHS data are unreliable and the estimated mortality figures are significantly below the estimates in other documented sources. We consider a number of ways to improve the reliability of the estimated intensity of the LN war based on the DHS data in view of the fact that this is the only source of data available for us to construct spatial variation of war intensity during the LN period. First, we calculated the distribution of excess mortality rates during the LN period (1970–1975) relative to the prior period (1965–1969) across districts using DHS 2000 data and DHS 2005 data. Second, we averaged the distributions of excess mortality rates across districts between DHS 2000 and DHS 2005 to reduce the amount of noise. Third, we use the distribution of average excess mortality rates to allocate the total number of estimated deaths across districts. We use two total numbers of estimated

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Table 9
Robustness: estimates of civil conflict exposure and LN and KR mortality intensity at district level on educational attainment, earnings and fertility of 1950–1965 birth cohorts.

Dependent variable	Census			CSES						Census
	Full sample years of schooling			Earnings sample years of schooling			Earnings sample log of monthly earnings			Fertility
	All	Men	Women	All	Men	Women	All	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Assuming total number of deaths under Lon Nol = 30,000										
Years of exposure	−0.221***	−0.253***	−0.189***	−0.311***	−0.321***	−0.290***	−0.053**	−0.055*	−0.050	0.037***
	(0.014)	(0.019)	(0.016)	(0.056)	(0.077)	(0.064)	(0.021)	(0.029)	(0.032)	(0.011)
Years of exposure* mortality rate under LN	0.087	−0.922	0.635	−1.724	−0.319	−3.733	0.496	0.044	1.180	0.621
	(0.991)	(1.313)	(0.834)	(2.693)	(3.471)	(2.684)	(1.229)	(1.418)	(1.426)	(0.680)
Mortality rate under LN	−9.575	−3.270	−13.304*	13.277	9.994	17.041	9.267	11.328	6.676	3.774
	(7.577)	(8.089)	(7.664)	(19.733)	(25.041)	(15.911)	(8.707)	(9.769)	(8.444)	(3.615)
Years of exposure* KR mortality rate	0.002	0.011	0.004	−0.051	−0.113	0.024	0.014	0.016	0.020	−0.007
	(0.039)	(0.060)	(0.034)	(0.104)	(0.130)	(0.128)	(0.042)	(0.052)	(0.059)	(0.032)
KR mortality rate	0.228	−0.042	0.402	0.358	0.432	0.274	−0.046	−0.019	−0.087	−0.112
	(0.385)	(0.416)	(0.389)	(0.760)	(0.817)	(0.845)	(0.295)	(0.320)	(0.337)	(0.226)
R-squared	0.141	0.095	0.074	0.232	0.183	0.140	0.131	0.132	0.135	0.028
Panel B: Assuming total number of deaths under Lon Nol = 500,000										
Years of exposure	−0.223***	−0.253***	−0.192***	−0.312***	−0.323***	−0.290***	−0.055**	−0.058**	−0.051	0.038***
	(0.015)	(0.019)	(0.017)	(0.056)	(0.077)	(0.065)	(0.021)	(0.029)	(0.032)	(0.012)
Years of exposure* mortality rate under LN	0.036	−0.070	0.094	−0.128	−0.010	−0.290	0.063	0.045	0.096	0.026
	(0.092)	(0.124)	(0.084)	(0.238)	(0.287)	(0.267)	(0.100)	(0.111)	(0.128)	(0.066)
Mortality rate under LN	−1.016	−0.310	−1.434*	0.321	0.098	0.608	0.414	0.590	0.171	0.509
	(0.836)	(0.883)	(0.846)	(1.611)	(1.933)	(1.541)	(0.795)	(0.847)	(0.817)	(0.379)
Years of exposure* KR mortality rate	0.002	0.012	0.003	−0.048	−0.111	0.029	0.015	0.017	0.020	−0.008
	(0.039)	(0.060)	(0.033)	(0.104)	(0.131)	(0.128)	(0.042)	(0.052)	(0.059)	(0.032)
KR mortality rate	0.241	−0.037	0.418	0.327	0.410	0.233	−0.065	−0.038	−0.108	−0.118
	(0.380)	(0.411)	(0.384)	(0.759)	(0.816)	(0.845)	(0.295)	(0.322)	(0.334)	(0.224)
R-squared	0.141	0.094	0.074	0.232	0.183	0.139	0.129	0.130	0.134	0.028
Provincial level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Observations	165,806	71,592	94,214	10,950	5698	5252	10,950	5698	5252	94,193

Note: Robust standard errors clustered by district in parentheses. Regressions include sampling weights. All specifications control for age, age squared, and gender for pooled sample. Mortality rate under LN is measured using DHS data together with estimated total deaths sourced from other studies. The lower bound figure of total deaths under the LN regime came from http://www.globalsecurity.org/military/world/cambodia/history-lon-nol.htm, while the upper bound figure came from Becker (2008).

*** Significant at 1%.

** Significant at 5%.

* Significant at 10%.

Table 10
Robustness: estimates of civil conflict exposure on educational attainment, earnings and fertility of various age windows of birth cohorts.

Dependent variable	Census			CSES						Census
	Full sample years of schooling			Earnings sample years of schooling			Earnings sample log of monthly earnings			Fertility
	All	Men	Women	Women	Men	Women	All	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Reduced-form estimates for 1954–1965 birth cohorts										
Years of exposure	−0.157***	−0.111***	−0.175***	−0.412***	−0.383***	−0.430***	−0.071**	−0.075*	−0.066	0.041*
	(0.024)	(0.035)	(0.030)	(0.068)	(0.094)	(0.100)	(0.030)	(0.038)	(0.046)	(0.023)
R-squared	0.175	0.139	0.116	0.301	0.278	0.241	0.216	0.232	0.228	0.047
Observations	139,857	61,452	78,405	9332	4906	4426	9332	4906	4426	78,381
Panel B: Reduced-form estimates for 1950–1969 birth cohorts										
Years of exposure	−0.263***	−0.241***	−0.273***	−0.320***	−0.256***	−0.386***	−0.036**	−0.043**	−0.027	0.048***
	(0.012)	(0.015)	(0.014)	(0.039)	(0.054)	(0.058)	(0.017)	(0.021)	(0.026)	(0.009)
R-squared	0.194	0.153	0.124	0.306	0.269	0.242	0.215	0.221	0.226	0.062
Observations	233,766	103,563	130,203	15,122	7978	7144	15,122	7978	7144	130,159
Panel C: Reduced-form estimates for 1950–1971 birth cohorts										
Years of exposure	−0.259***	−0.229***	−0.278***	−0.312***	−0.236***	−0.389***	−0.037**	−0.045**	−0.031	0.046***
	(0.011)	(0.015)	(0.013)	(0.037)	(0.048)	(0.059)	(0.016)	(0.019)	(0.025)	(0.009)
R-squared	0.202	0.160	0.135	0.312	0.274	0.255	0.210	0.214	0.223	0.075
Observations	268,663	120,454	148,209	17,315	9185	8130	17,315	9185	8130	148,163
Panel D: IV estimates for 1954–1965 birth cohorts										
Years of schooling							0.173**	0.196*	0.153	−0.233*
							(0.075)	(0.101)	(0.105)	(0.135)
First-stage *F-statistic*							36.47***	16.57***	18.57***	33.72***
Observations							9332	4906	4426	78,381
Panel E: IV estimates for 1950–1969 birth cohorts										
Years of schooling							0.111**	0.167**	0.069	−0.177***
							(0.053)	(0.082)	(0.065)	(0.033)
First-stage *F-statistic*							68.52***	22.36***	44.64***	377.53***
Observations							15,122	7978	7144	130,159
Panel F: IV estimates for 1950–1971 birth cohorts										
Years of schooling							0.118**	0.189**	0.079	−0.167***
							(0.052)	(0.085)	(0.063)	(0.032)
First-stage *F-statistic*							72.49***	24.01***	43.84***	424.12***
Observations							17,315	9185	8130	148,163

Note: Robust standard errors clustered by district in parentheses. All specifications control for age, age squared, gender (for pooled sample), district fixed effects and survey-year fixed effects (for CSES). Regressions include sampling weights. We assume that cohorts born after 1965 also experienced school disruptions during Vietnamese occupation period. The statistical significance of estimates is robust to alternative assumptions on when schooling was no longer disrupted during this period.

*** Significant at 1%.

** Significant at 5%.

* Significant at 10%.

deaths to do the allocation. The first is a low estimate of 30,000, while the second is a high estimate of 500,000.[13] The lower and upper bounds provide a sensitivity test. Fourth, to compute the measure of LN war intensity in each district, we divided the estimated deaths due to the LN conflict by the sum of the estimated deaths under LN, estimated deaths under KR (based on Cambodia Genocide data) and surviving population born before 1975 captured in the 1998 Census in each district. The results are reported in Table 9. Overall, the variation in the interaction of LN war intensity and exposure to conflict does not explain our outcomes of interest.

5.2. Sensitivity to age and experience differences

The years of conflict exposure variable is a function of age and may capture the direct effect of age and experience on earnings or fertility not mediating through education. This problem is, to a large extent, dealt with above. First, we included controls for age and age squared, to absorb these differences. After controlling for age/experience differences, our estimates show that conflict exposure of younger cohorts led them to earn less and have more children. These results suggest that the age function is doing a fairly good job in absorbing differences in earnings and fertility due to differences in age and experience that are unrelated to war. Second, our estimates on the returns to schooling and effect of education on fertility are not too different to previous studies estimating causal effects of education in the context of other developing countries. This gives us confidence to argue that there are no direct effects of war on earnings and fertility (not mediating through education), and any effects of war on earnings and fertility are mediated through its effects on education.

However, we cannot completely rule out the possibility that our age control function fails to fully capture the effects of age/experience and the possibility of direct effects of war on earnings and fertility. Hence, we examine the sensitivity of our reduced-form estimates of conflict exposure during primary school years on educational attainment, log monthly earnings and fertility, as well as the IV estimates on effects of schooling on earnings and fertility, to changing the age windows of the sampled birth cohorts. Specifically, we include younger cohorts whose primary school years were least likely to be directly affected by the conflicts, as well as dropping older cohorts whose fertility decisions were most likely to be directly affected by the conflicts. It is important to note that by widening the age window, we are stretching the ability of the age function to fully capture the direct effects of age and experience. Including young people in the sample would mean including cohorts who also experienced other political turmoil during the post-KR period and whose births occurred during the period of civil conflicts.

Table 10 reports the estimates based on various age windows of birth cohorts. Panel A reports reduced-form estimates for birth cohorts 1954–1965; panel B reports reduced-form estimates for birth cohorts 1950–1969; and panel C reports reduced-form estimates for birth cohorts 1950–1971. Panel D reports IV estimates for birth cohorts 1954–1965; panel E reports IV estimates for birth cohorts 1950–1969; and panel F reports IV estimates for birth cohorts 1950–1971. Although the magnitudes of the estimated effects vary somewhat as we change the age windows, the estimated coefficients remain statistically significant, while the signs on the coefficients for age and age squared remain unchanged. [14] Thus, our results are unlikely to be confounded by the direct effects of age and experience on earnings and fertility.

6. Conclusion

This study investigates the long-term impacts of exposure to civil conflicts in Cambodia between 1970 and 1979 on the educational attainment and labor productivity of individuals. We use the variation in years of civil conflict exposure during primary school age to estimate the effects of civil conflict exposure on the educational attainment, earnings, fertility and health outcomes of individuals several decades after the civil conflicts ended. We find that exposure to civil conflicts during primary school age, on average, reduced the educational attainment of men by 0.9–1.1 years and the educational attainment of women by 0.6–0.9 years. We find that exposure to civil conflicts during primary school age lowers the earnings of men between 6.6% and 8.6%, but not the earnings of women. Finally, we find that exposure to civil conflicts during primary school age increases female completed fertility by 0.04 births per women, which translates to reduced fertility of 0.23 births for each additional year of completed schooling.

We have used data from the Cambodian Genocide Database to estimate variation in the geographical intensity of the mortality rate during the KR regime. A limitation of the database is that for many gravesites there are only minimum and maximum estimates of the number of deaths, potentially impeding the accuracy of the calculation of regional differences in mortality rates under the KR. A further limitation is that we do not have information on the number of individuals at the district level who survived the genocide, but died before the 1998 Census. Nonetheless, our results are not sensitive to whether we use the minimum, maximum, or average estimates of deaths to construct mortality rates, or to the use of absolute number of deaths (without dividing by population). We also combine information in the DHS sibling mortality module with upper and lower bounds of the estimated total deaths under the LN regime on past studies to estimate the geographical variation in the intensity of LN war. Although we find no evidence that the geographical variation in LN war intensity affects outcomes of conflict exposed individuals, the data are based on sibling information of childbearing aged women and obtained several decades after the LN war. These limitations impede our attempt to exploit geographic variation in the intensity of the conflicts and clearly separate

[13] The low estimate of 30,000 came from http://www.globalsecurity.org/military/world/cambodia/history-lon-nol.htm, while the high estimate of 500,000 came from Becker (1998).

[14] We also performed placebo tests by changing the timing of the war and estimating the effects of conflict exposure using birth cohorts 1950-1965. As we move the start and end years of the war earlier, the estimated effects reduce and eventually change signs and lose statistical significance.

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Table A1
Estimates of civil conflict exposure and mortality intensity at district level on earnings and fertility of 1950–1965 birth cohorts.

Dependent variable	CSES									Census
	Employee sample (log of hourly wages)			Employee sample (log of monthly wages)			Earnings sample (log of monthly earnings)			Full sample fertility
	All	Men	Women	All	Men	Women	All	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of exposure	−0.035	−0.078*	0.030	−0.014	−0.047	0.036	−0.052**	−0.057*	−0.045	0.040***
	(0.030)	(0.045)	(0.047)	(0.029)	(0.042)	(0.047)	(0.021)	(0.029)	(0.031)	(0.011)
Years of exposure* KR mortality rate	−0.075	−0.024	−0.158*	−0.071	−0.047	−0.099	0.016	0.017	0.019	−0.008
	(0.063)	(0.083)	(0.084)	(0.056)	(0.074)	(0.079)	(0.042)	(0.052)	(0.059)	(0.032)
KR mortality rate	0.414	0.312	0.570	0.513	0.500	0.471	−0.076	−0.046	−0.118	−0.111
	(0.362)	(0.422)	(0.426)	(0.332)	(0.370)	(0.440)	(0.292)	(0.323)	(0.330)	(0.228)
Age	0.097	0.180	−0.037	0.120	0.195	−0.024	0.026	0.030	0.036	0.410***
	(0.116)	(0.132)	(0.176)	(0.118)	(0.125)	(0.198)	(0.087)	(0.109)	(0.116)	(0.057)
Age squared	−0.001	−0.002*	0.001	−0.001	−0.002*	0.000	−0.001	−0.001	−0.001	−0.004***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.380***			0.476***			0.063**			
	(0.053)			(0.052)			(0.030)			
Provincial level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
R-squared	0.167	0.161	0.131	0.172	0.145	0.119	0.128	0.129	0.134	0.028
Observations	2956	1923	1033	2956	1923	1033	10,950	5698	5252	94,193

Note: Robust standard errors clustered by district in parentheses. Total samples are individuals born between 1950 and 1965. Regressions include sampling weights. KR-mortality rates measure the mortality rates under Khmer Rouge regime based on the estimated deaths by district in the Cambodian Genocide Database and the number of individuals born before 1980 based on their districts of birth. Statistical significance of estimates remain unchanged when we include a set of district fixed effects and drop the KR mortality rate variable (results available upon request).

*** Significant at 1%.
** Significant at 5%.
* Significant at 10%.

the effects of conflicts under the LN and KR regimes. The results for that exercise need to be viewed in this context. This said, it is always going to be difficult to obtain accurate mortality figures for a violent conflict as large as the Cambodian genocide, particularly at the regional level.

The evidence that we have presented suggests that the channel through which the civil conflicts affected earnings and fertility is most probably the educational loss induced by the conflicts. While it is not possible to fully attribute the effects of conflict exposure during primary school years on earnings and fertility to educational loss alone, we rule out several plausible channels through which the conflicts could have affected earnings and fertility later in life. Specifically, we find that variation in years of conflict exposure during primary school age does not systematically explain individuals' health indicators and quality of schooling indicators later in life. We also demonstrate that the estimates are unlikely to be affected by selective survival. If our results for earnings and fertility can be solely attributed to the educational channel, our IV estimates can be interpreted as average earnings and fertility changes by those individuals who received less education just because of the civil conflicts in Cambodia throughout the 1970s.

Our main findings about the long-term labor market and fertility effects of conflict-driven disruption to education have several policy implications. The first is that it may be particularly useful to focus on children of primary school age living in regions affected by civil conflict. Their long run productivity may be significantly improved by policies designed to keep them in school. If it is not feasible to implement such policies during the period of actual conflict, long run negative earnings and fertility outcomes may be avoided or reduced by paying particular attention to improving their human capital in the aftermath of civil conflict. The second implication, which may be relevant in the reconstruction period, is to recognize that the effects of civil conflict are not homogenous. While we find that regional variation in the mortality rate during the KR regime did not translate into spatial differences in educational outcomes, we do find gender differences in the effect of educational disruption on loss of earnings. The existence of heterogeneous effects due to conflict point to the need to target particular groups that might be more adversely affected by the long-run effects of conflict or adversely affected by the long run effect of conflict in different ways. Finally, our findings have implications for events other than civil conflict, such as natural disasters, which can result in similar losses of human capital with potential long run effects for earnings and fertility. Policies aimed at improving the human capital of individuals displaced by natural disaster in the recovery period can potentially avoid productivity losses later in life.

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Appendix.

Table A1.

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