Three Essays on the Long-term Effects of Civil Conflicts in Cambodia

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Abstract

This dissertation presents three self-contained, but related, essays on the long-term effects of civil conflicts on individuals in Cambodia.

The first essay examines the long-term effects of exposure to civil war from 1970 to 1975 and genocide under the Khmer Rouge regime from 1975 to 1979 on the educational attainment and labor productivity of individuals in Cambodia. Given the well-documented causal links between schooling and labor productivity, it is surprising that past studies have shown that civil conflicts generally reduce the educational attainment but not the 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 the 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.

The second essay uses geographical variation in gender-differentiated mortality during the genocide under the Khmer Rouge regime from 1975 to 1979 to study the effect of violent conflict on the educational and health outcomes of children born years after the conflict ended. We show that the adverse effects of violent conflict are transmitted from one generation to the next through its effect on the sex ratio and marriage outcomes of those who survived the conflict. We find that mortality rates under the Khmer Rouge regime predict a lower likelihood of normal grade progression and lower height-for-age Z-scores for children born to parents who were of prime marriage age (14–29) during the time that the Khmer Rouge was in power. Using mortality rates during the Khmer Rouge regime as an instrumental variable for the sex ratio, we find that the lower sex ratio in the parents'

generation also reduces the likelihood of children exhibiting normal grade progression and decreases the height-for-age Z-scores.

The third essay uses an artefactual field experiment to examine the long-term effects of exposure to the Cambodian genocide from 1975 to 1979 on individuals' pro-social and anti-social behavior and risk preferences. Our results show that individuals who were exposed to the genocide during childhood and early adolescence are less trusting, less altruistic, and more risk averse than those who were not exposed. We find little evidence that exposure to genocide leads to dishonest and vindictive behavior. Our results are corroborated by survey data and questionnaires on personality traits. The findings suggest that direct exposure to genocide during childhood and early adolescence has a lasting impact on social capital and attitude toward risk. It can also make individuals less extraverted and agreeable.

The main findings from these three essays suggest that the civil conflicts in Cambodia have had long-lasting impacts on survivors and their children.

Declaration

This dissertation contains no material that has been accepted for the award of any other degree or diploma at any university or equivalent institution and that, to the best of my knowledge and belief, this dissertation contains no material previously published or written by another person, except where due reference is made in the text of this dissertation.

[redacted]

Chandarany Ouch
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Chapter 1: Introduction

1.1 Objectives of the Dissertation

A growing body of research shows that the experience of violent conflict has lasting effects on individuals' later-life outcomes which are difficult to reverse. The objective of this dissertation is to examine the long-term impacts of the civil conflicts that occurred in Cambodia during the 1970s. This dissertation focuses on the effects of the civil conflict on individuals through: (1) disruption to education; (2) gender-differentiated mortality; and (3) disruption to social capital formation, and presents the findings in a collection of three independent essays.

The first and second essays use observational data, while the third essay uses an artefactual field experiment. The first essay exploits variation in the disruption to primary schooling across birth cohorts during the civil war under the Lon Nol (LN) regime from 1970–1975 and the genocide under the Khmer Rouge (KR) regime from 1975–1979 to provide evidence on the long-term effects of exposure to civil conflicts during primary school age on individuals' educational attainment, earnings, and fertility. We show that conflict exposure during primary school age has a long-term effect on labor productivity through the disruption to schooling.

The second essay uses variation in gender-differentiated mortality during the KR regime to examine the negative intergenerational effects of civil conflict. We show that the genocide had negative impacts on the educational and health outcomes of children born years after the civil conflict ended, and we provide evidence that the impacts were transmitted through the imbalanced sex ratio that existed in the parents' generation.

The third essay uses an artefactual field experiment and exploits cohort differences to examine the effects of direct exposure to genocide under the KR regime during childhood and early adolescence on social capital and attitudes toward risk. We show that individuals exposed are less trusting, less altruistic, and more risk averse than those who were not exposed.

1.2 Background to Cambodia's 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 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[1] and about half a million Cambodians (Becker, 1998).

The KR took power in 1975 and ruled Cambodia from April 1975 to January 1979. This period is commonly known as Cambodia's genocide characterized by massive destruction, violence, torture, and death. 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. The urban population was evacuated from cities throughout the country and forced to engage in agricultural labor in the countryside. Markets and currency were abolished, and schools, libraries, western medicine, religion and

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

anything associated with the previous regime were discarded (UNESCO, 2011). The KR also destroyed traditional Cambodian social norms, culture, religion, organizations, networks, and even the family structure (Collier et al., 2003). The regime forced people to live in communal work camps under the control of the "*Angka*" (organization) with extremely strict rules and policies. Children, mostly from the age of 8, were sent to live with other children and were supervised by two or three senior KR officials. Community and family members were encouraged to spy and report on each other, which destroyed trust and established deeply rooted fear (Collier et al., 2003). In addition, the KR targeted and killed those who were educated and had a high social or professional status. To avoid being targeted, people hid their identities and tried to be as inconspicuous as possible. The KR killed suspected political opponents, educated individuals, those from high social classes and those who did not share their vision for a new Cambodia. Indentured labor, food shortages, and the absence of modern medicine were responsible for large numbers of deaths.

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), were killed or died from starvation or exhaustion during the KR regime. The intensity of the killing and death under the KR regime differed across regions of Cambodia. Many Cambodians who survived this period are either direct victims of the regime or witnessed violence during the KR regime and experienced threats to and loss of loved ones and prolonged parental absence.

Adult males were the demographic group most likely to die under the KR regime (de Walque, 2006). A simulation study by Neupert and Prum (2005) found that mortality rates during the KR regime exhibited a high gender imbalance, with males accounting for approximately two-thirds of all deaths.

Marriage rates were very low under the regime but rebounded immediately after its collapse (de Walque, 2006; Heuveline & Poch, 2007). The marriage boom after the KR was likely due to the delay in marriage by women who, based on their age, would otherwise have married for the first time during the KR regime (de Walque, 2006).

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).

1.3 Contributions of the Dissertation

The first essay is among the few studies to show that exposure to violent conflicts during primary school years reduces earnings of men and increases fertility of women in later life. Most existing studies find no effect of war on earnings (e.g., Miguel & Roland, 2011; Merrouche, 2011). This essay also contributes to the literature on schooling and earnings by showing that conflict-driven disruption to schooling can reduce labor productivity through its negative impact on educational attainment.

The second essay is the first study to provide evidence on the intergenerational impacts of civil conflicts on health and educational outcomes of children born years after the civil conflict ended, while previous studies have typically examined the effects of civil conflict exposure in early life, either *in utero* or infancy, and in adolescence on health status and educational attainment. The study suggests that the gender-differentiated mortality effect of violent conflict can affect children through its effect on the imbalanced sex ratio and on the marriage market.

Our findings in the third essay contribute to the growing literature on the effects of civil conflicts on social capital and risk behavior. While existing studies have focused on recent civil conflicts, this essay captures the long-term impacts of civil conflict on social behavior and risk. It also adds to the existing literature by examining the effects of direct exposure to civil conflict on anti-social behavior. Finally, it also sheds light on the relatively unknown effects of conflict exposure on personality by establishing that exposure to civil conflict during childhood and early adolescence can influence personality traits.

1.4 Structure of the Dissertation

This dissertation is organized as follows. Chapter 2 explores the long-term effects of exposure to civil conflicts during primary school age on the educational attainment, earnings, and fertility of individuals. Chapter 3 studies the intergenerational effects of the genocide. Chapter 4 examines the long-term effects of direct exposure to genocide during childhood and early adolescence on social capital and risk behavior. Chapter 5 concludes the dissertation, presents policy implications, and discusses potential areas for future research.

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Chapter 2: The Long-term Effects of Civil Conflicts on Education, Earnings, and Fertility

2.1 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 chapter 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 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 study 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, Verwimp, and Bundervoet (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, [2] 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)

[2] See also Cassar, Grosjean, and Whitt (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.

show that marital fertility in Ethiopia continued to fall in the decade after famine and war ended in the 1980s.[3]

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 conflicts 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.

[3] 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.

2.2 Civil Conflicts and Education Disruption in Cambodia

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.

2.2.1 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 hours per day and in most areas there was no schooling at all (Clayton, 1998). Classes were mostly organized during the lunch breaks of 10 to 12-hour 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 1,009 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).

2.3 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 chapter 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 conflicts 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.

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

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

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.

2.3.1 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

[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.

who reported that they worked during the past seven days and divide the total household income by the number of adults in the household who reported that they worked during the past seven days to construct an earnings sample that pools employees and self-employees.[6] 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).[7]

2.3.2 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, Verwimp, & Akresh, 2009; Akresh, Bhalotra, Leone, & Osili, 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 from 1975 to 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.[8] 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 the estimated number of deaths in a district, based on information in the CGD,

[6] This approach may inflate the earnings for women.
[7] 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.
[8] The database was developed by Yale University and has been updated by the Documentation Center of Cambodia (DC-Cam). We use both information from the original Yale database and data on additional mass gravesites and estimates of the number of deaths from 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.

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. 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 (see Table A2.1 in appendix).

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.

Figure 2.1 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 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.

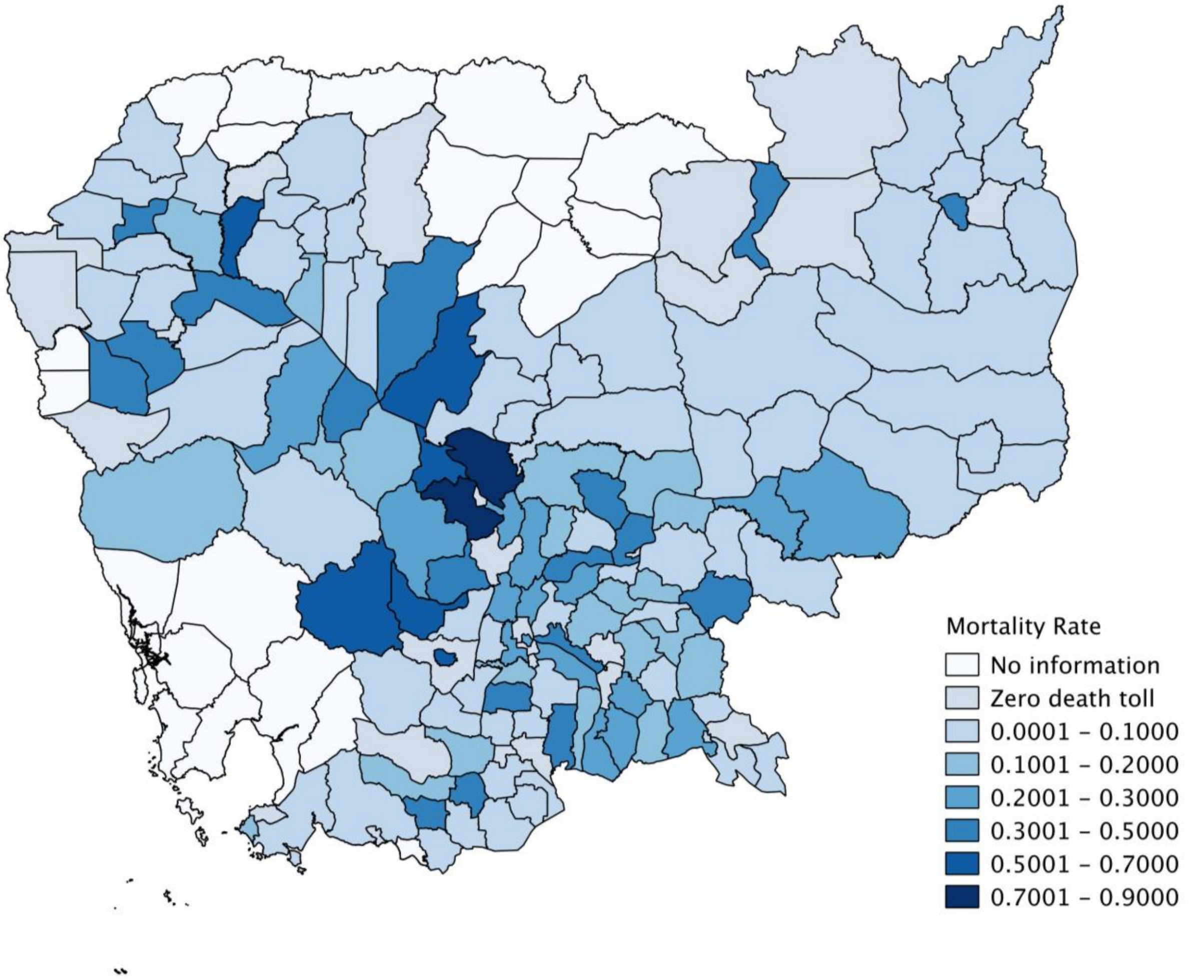


Figure 2.1: Geographical distribution and intensity of mortality under the Khmer Rouge regime

2.3.3 Descriptive Statistics

Table 2.1 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 conflicts 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.

Table 2.1: Descriptive statistics

Main Variables	Census			CSES								
	Full sample			Full sample			Employee sample			Earnings sample*		
	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**			4.35									
			(2.70)									
Real hourly wages (Riels)							1,343	1,565	925			
							(3,301)	(3,964)	(1,244)			
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	6,587	8,549	3,056	1,997	1,059	11,409	5,963	5,446

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

2.4 The Long-term Impact of Civil Conflicts

2.4.1 The Long-term Effects of Civil Conflict Exposure 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 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:

$$S_{ikjt} = \beta_0 + \beta_1 Exposure_i + \beta_2 DR_j + \beta_3 Exposure_i * DR_j + \beta_4 \mathbf{X}_{ijt} + \gamma_k + \gamma_t + u_{ikjt}$$

(2.1a)

where S_{ijkt} denotes the years of schooling of individual i in district j within province k surveyed in year *t*. $Exposure_i$ represents the number of years of exposure to conflicts during the individual's primary school age years. Our measure of exposure to civil conflicts 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 (2.1a), in which the LN mortality rate and its interaction with years of exposure to conflicts 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 conflicts 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.

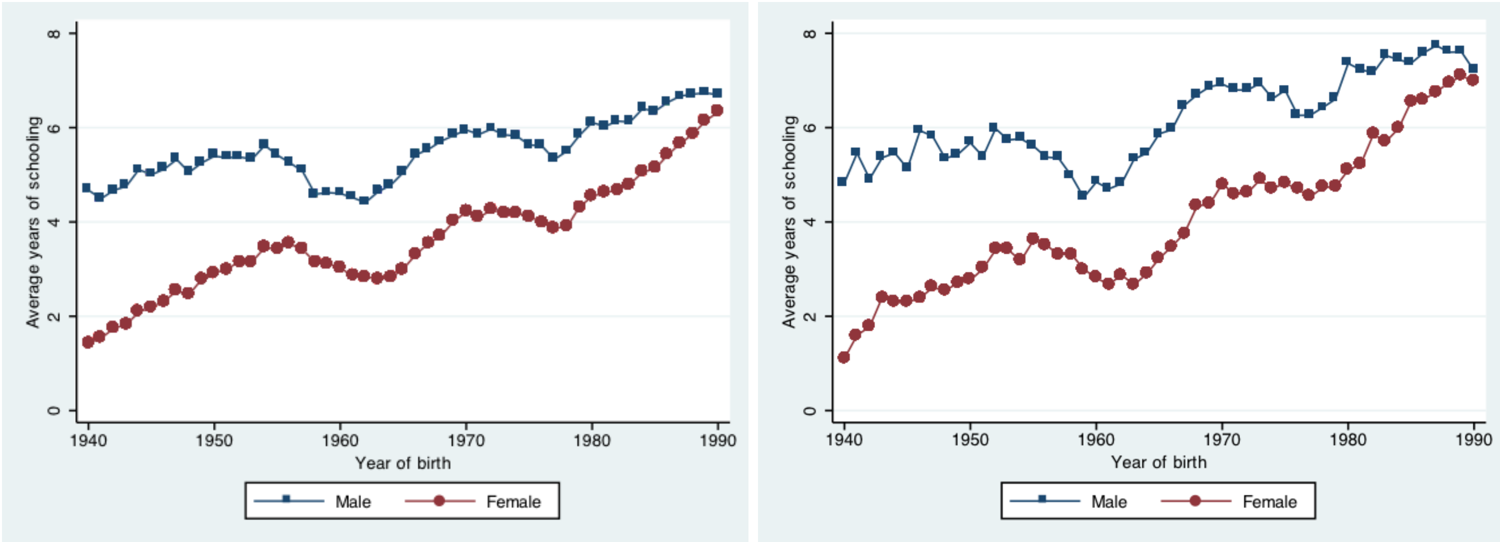
We measure $Exposure_i$ by years of civil conflict exposure during one's primary school age, as most Cambodians born in the 1940s and 1950s have no more than primary education and the mean years of schooling is less than 6. We assume that primary school age ranges between the ages of 9 and 14 years old 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 over time, it is likely that the typical school commencing age was older in

[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.

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.

Figure 2.2 illustrates the difference in mean years of schooling across birth cohorts. Figure 2.2A exhibits a smoother trend than Figure 2.2B, 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 conflicts when the various cohorts were of primary school age can explain the patterns in Figure 2.2. If Cambodians born in the 1950s and 1960s mostly started school at nine, 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 one 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.



(A): Census 2008

(B): Socio-Economic Survey 2007–2010

Figure 2.2: Mean years of schooling, year of birth and gender

Table 2.2: Years of school age exposed to civil conflicts 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.

Table 2.2 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. Specifically, individuals born between 1961 and 1965 were most significantly affected by the civil conflicts, which is consistent with the trough in Figure 2.2.

Table 2.3 presents estimates based on regression specification (2.1a). 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 Table 2.4. 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 Figure 2.1, 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:

$$S_{ijt} = \pi_0 + \pi_1 Exposure_i + \pi_2 \mathbf{X}_{ijt} + \gamma_j + \gamma_t + \varepsilon_{ijt} \qquad (2.1b)$$

Table 2.5 reports estimates using Census and CSES's full and earning samples. The estimated coefficients on years of exposure to civil conflicts are all statistically significant at conventional levels (columns 1–9 in Table 2.5). 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 conflicts 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 2.5 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 conflicts 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 conflicts (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 one year in the CSES earnings sample.

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 (Figures 2.2A and 2.2B). 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).

Table 2.3: 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	Men	Women	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(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 rates	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 rates	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	6,300	8,225	10,950	5,698	5,252

Note: Robust standard errors clustered by district are in parentheses. All the regressions include sampling weights. KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD 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%.

Table 2.4: Estimates of civil conflict exposure and mortality intensity at district level 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	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 rates	-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 rates	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	2,956	1,923	1,033	2,956	1,923	1,033	10,950	5,698	5,252	94,193

Note: Robust standard errors clustered by district are in parentheses. Total samples are individuals born between 1950 and 1965. All the regressions include sampling weights. KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD 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%.

Table 2.5: Estimates of effects of civil conflicts on educational attainment of 1950–1965 birth cohorts

	Census			**CSES**					
Dependent variable: Years of schooling	Full sample			Full sample			Earnings sample		
	All	Men	Women	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(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	6,587	8,549	11,409	5,963	5,446

Note: Robust standard errors clustered by district are in parentheses. All the regressions include sampling weights.
*** significant at 1%, ** significant at 5%, * significant at 10%.

2.4.2 The Long-term Effects of Civil Conflict Exposure on Earnings and Fertility

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

$$\log W_{ijt} = \alpha_0 + \alpha_1 Exposure_i + \alpha_2 \mathbf{X}_i + \gamma_j + \gamma_t + \upsilon_{ijt} \qquad (2.2)$$

where $logW_{ijt}$ 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, equation (2.2) 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:

$$F_{ij} = \tau_0 + \tau_1 Exposure_{\mathrm{i}} + \tau_2 \mathbf{X}_i + \gamma_j + \varepsilon_{ij} \qquad (2.3)$$

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 2.6. 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.

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 2.6). 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.

[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.

Table 2.6: Estimates of 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	Men	Women	All	Men	Women	All	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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	-0.0001	-0.0003	-0.0004	-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	3,056	1,997	1,059	3,056	1,997	1,059	11,409	5,963	5,446	97,879

Note: Robust standard errors clustered by district are in parentheses. All the regressions include sampling weights.
*** significant at 1%, ** significant at 5%, * significant at 10%.

2.4.3 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 equation (2.1b) 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:

$$\log W_{ijt} = \delta_0 + \delta_1 \widetilde{S_i} + \delta_2 \mathrm{X}_i + \gamma_j + \gamma_t + \varepsilon_{ijt} \tag{2.4}$$

$$F_{ij} = \gamma_0 + \gamma_1 \widetilde{S_i} + \gamma_2 \mathrm{X}_i + \gamma_j + \varepsilon_{ij} \tag{2.5}$$

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 2.5, it strongly predicts the completed years of schooling. Second, as we discuss in Section 2.4.1, 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 2.4.4, and how one should interpret our findings if the exclusion restriction fails to hold.

Table 2.7 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 2.7 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 2.7 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 & 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 & Patrinos, 2004).

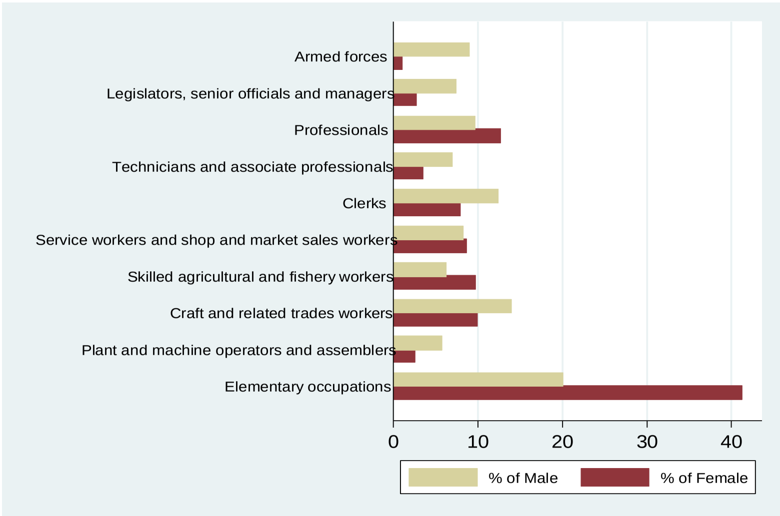
Table 2.7 also shows that the estimated return to schooling for women is not significantly different from zero. Compared to the significant OLS estimates, the insignificant IV estimates imply that there is positive selection into schooling on the basis of labor productivity among women. The lack of returns 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 Figure 2.3, 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 of Table 2.7 indicates that a one-year increase in years of schooling is associated with a 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.

Table 2.7: Estimates of returns to schooling 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	Men	Women	All	Men	Women	All	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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 & 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	3,056	1,997	1,059	3,056	1,997	1,059	11,409	5,963	5,446	97,879

Note: Robust standard errors clustered by district are in parentheses. All the regressions include sampling weights.
*** significant at 1%, ** significant at 5%, * significant at 10%.



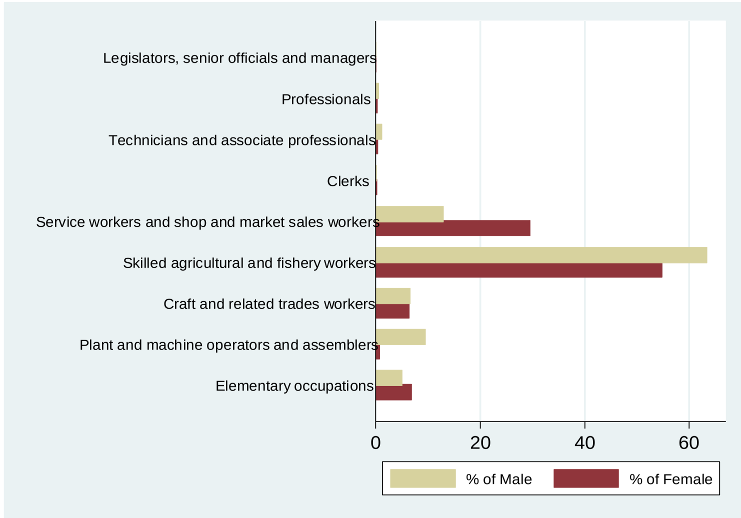


Figure 2.3: Main occupations by gender

Note: 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.

2.4.4 Other Possible Channels of Civil 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. Nevertheless, 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.

2.4.4.1 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<150cm) 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 2.8 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 2.8, indicate that years of exposure to conflict during primary school age and mortality are not correlated.

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.

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

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 2.3 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.

2.4.4.2 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 2.9 indicates 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 2.9. The estimated returns to schooling for the earnings sample are almost identical to those in Table 2.7. 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 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.

Table 2.8: Estimates of effects 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	Men	Women	All	Men	Women	Women	Women	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variables									
Illness/injury during the past 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 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 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 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 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 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 (centimeters)								-0.173	
								(0.114)	
Stunted (height < 150cm)								0.013	
								(0.012)	
Dead by 2010									-0.0099
									(0.0073)
Observations	3,056	1,997	1,059	11,409	5,963	5,446	97,879	2,771	9,391

Note: We report the estimated coefficients for the years of civil conflict exposure during primary school age. All the regressions control for age, age squared, gender, district level fixed effects, and survey-year fixed effects whenever possible. Illness/injury during the past 30 days equals 1 if an individual was sick or injured during the last 30 days and 0 otherwise. Disabled equals 1 if an individual is disabled and 0 otherwise. Difficulty seeing, hearing, speaking, moving, and sensing and psychological or behavioral difficulties are coded in the same way. Regressions adjusted for sampling weights. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 2.9: Estimates of 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	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(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	3,056	1,997	1,059	11,409	5,963	5,446

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. All the regressions include for sampling weights. Robust standard errors clustered by district are in parentheses. Can read a simple message and can write a simple message are binary variables equal 1 if yes and 0 otherwise.
*** significant at 1%, ** significant at 5%, * significant at 10%.

2.5 Robustness Checks

2.5.1 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 (2.1a) plus two additional variables: LN war intensity (i.e., mortality rates 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 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 2.10. Overall, the variation in the interaction of LN war intensity and exposure to conflict does not explain our outcomes of interest.

2.5.2 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

[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).

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 2.11 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. Thus, our results are unlikely to be confounded by the direct effects of age and experience on earnings and fertility.

2.5.3 Placebo Test

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 (Table 2.12).

Table 2.10: 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

	Census			CSES						Census
	Full sample Years of schooling			Earnings sample Years of schooling			Earnings sample Log of monthly earnings			
Dependent variable:	All	Men	Women	All	Men	Women	All	Men	Women	Fertility 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 rates 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 rates 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 rates	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 rates	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 rates 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 rates 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 rates	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 rates	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	5,698	5,252	10,950	5,698	5,252	94,193

Note: Robust standard errors clustered by district are in parentheses. All the regressions include sampling weights. All specifications control for age, age squared, and gender for pooled sample. Mortality rates under LN are 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 (1998). *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 2.11: Robustness: Estimates of civil conflict exposure on educational attainment, earnings, and fertility of various age windows of birth cohorts

	Census			CSES						Census
Dependent variable:	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	9,332	4,906	4,426	9,332	4,906	4,426	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	7,978	7,144	15,122	7,978	7,144	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	9,185	8,130	17,315	9,185	8,130	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							9,332	4,906	4,426	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	7,978	7,144	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	9,185	8,130	148,163

Note: Robust standard errors clustered by district are in parentheses. All specifications control for age, age squared, gender (for pooled sample), district fixed effects, and survey-year fixed effects (for CSES). All the 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%.

Table 2.12: Placebo tests of civil conflict exposure on educational attainment, earnings, and fertility of 1950–1965 birth cohorts

	Census			CSES						Census
	Full sample Years of schooling			Earnings sample Years of schooling			Earnings sample Log of monthly earnings			Fertility
Dependent variable:	All	Men	Women	All	Men	Women	All	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Placebo period: 1966–1975										
Years of exposure	-0.204***	-0.279***	-0.146***	-0.178***	-0.222***	-0.110**	-0.0003	-0.001	0.005	0.055***
	(0.019)	(0.030)	(0.023)	(0.049)	(0.067)	(0.051)	(0.017)	(0.022)	(0.026)	(0.018)
R-squared	0.174	0.132	0.111	0.289	0.253	0.229	0.220	0.235	0.229	0.044
Placebo period: 1963–1972										
Years of exposure	0.259***	0.267***	0.239***	0.025	0.005	0.050	0.029**	0.033*	0.025	-0.055***
	(0.018)	(0.023)	(0.021)	(0.040)	(0.057)	(0.047)	(0.015)	(0.020)	(0.023)	(0.014)
R-squared	0.175	0.133	0.112	0.287	0.250	0.228	0.220	0.235	0.229	0.044
Placebo period: 1960–1969										
Years of exposure	0.190***	0.220***	0.159***	0.290***	0.299***	0.265***	0.049***	0.058**	0.036	-0.037***
	(0.012)	(0.016)	(0.014)	(0.044)	(0.065)	(0.053)	(0.017)	(0.024)	(0.028)	(0.009)
R-squared	0.175	0.134	0.112	0.292	0.255	0.233	0.221	0.236	0.229	0.044
District 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	172,927	75,018	97,909	11,409	5,963	5,446	11,409	5,963	5,446	97,879

Note: Robust standard errors clustered by district are in parentheses. All the regressions include sampling weights.
*** significant at 1%, ** significant at 5%, * significant at 10%.

2.6 Conclusion

This chapter investigates the long-term impact 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 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 to Chapter 2

Table A2.1: Alternative measure of KR intensity

Estimates of civil conflict exposure and KR intensity on the educational attainment, earnings and fertility of 1950–1965 birth cohorts

	Census			CSES						Census
Dependent variable:	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)
KR mortality rates using minimum number of deaths in mass graves										
Years of exposure	-0.221***	-0.258***	-0.187***	-0.321***	-0.327***	-0.306***	-0.051**	-0.057*	-0.044	0.040***
	(0.014)	(0.018)	(0.016)	(0.055)	(0.076)	(0.064)	(0.021)	(0.029)	(0.031)	(0.011)
Years of exposure * Min. KR mortality rates	0.004	0.015	0.003	-0.040	-0.092	0.026	0.013	0.019	0.012	-0.008
	(0.042)	(0.064)	(0.036)	(0.107)	(0.137)	(0.130)	(0.043)	(0.054)	(0.059)	(0.034)
Min. KR mortality rates	0.268	-0.014	0.454	0.288	0.333	0.236	-0.082	-0.057	-0.120	-0.114
	(0.415)	(0.448)	(0.420)	(0.766)	(0.828)	(0.845)	(0.304)	(0.335)	(0.345)	(0.233)
R-squared	0.140	0.094	0.074	0.232	0.182	0.139	0.128	0.129	0.134	0.028
KR mortality rates using maximum number of deaths in mass graves										
Years of exposure	-0.221***	-0.257***	-0.187***	-0.318***	-0.321***	-0.308***	-0.052**	-0.057*	-0.046	0.040***
	(0.014)	(0.018)	(0.016)	(0.055)	(0.076)	(0.064)	(0.021)	(0.029)	(0.031)	(0.011)
Years of exposure * Max. KR mortality rates	0.002	0.010	0.004	-0.052	-0.125	0.038	0.017	0.015	0.025	-0.008
	(0.037)	(0.057)	(0.032)	(0.101)	(0.126)	(0.126)	(0.041)	(0.050)	(0.058)	(0.031)
Max. KR mortality rates	0.191	-0.071	0.363	0.349	0.471	0.209	-0.068	-0.033	-0.114	-0.105
	(0.371)	(0.400)	(0.375)	(0.740)	(0.801)	(0.825)	(0.282)	(0.314)	(0.317)	(0.222)
R-squared	0.140	0.094	0.074	0.232	0.183	0.139	0.128	0.129	0.134	0.028
Absolute figure of average deaths in mass graves (divided by 1,000,000)										
Years of exposure	-0.220***	-0.257***	-0.185***	-0.324***	-0.335***	-0.304***	-0.047**	-0.055*	-0.036	0.040***
	(0.014)	(0.018)	(0.016)	(0.052)	(0.074)	(0.060)	(0.021)	(0.028)	(0.031)	(0.011)
Years of exposure * Average KR mortality	-0.027	0.148	-0.059	-0.137	-0.441	0.167	-0.313	0.004	-0.639	-0.133
	(0.327)	(0.548)	(0.200)	(0.520)	(0.692)	(0.725)	(0.290)	(0.330)	(0.406)	(0.292)
Average KR mortality	1.575	-1.669	3.499	-0.254	0.121	-0.198	-1.775	-3.026	-0.499	-1.355
	(2.395)	(2.978)	(2.153)	(4.419)	(5.018)	(4.952)	(1.929)	(2.050)	(2.208)	(2.250)
R-squared	0.140	0.094	0.074	0.232	0.182	0.139	0.129	0.130	0.135	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	5,698	5,252	10,950	5,698	5,252	94,193

Note: Robust standard errors clustered by district are in parentheses. All specifications control for age, age squared, gender (for pooled sample), district fixed effects, and survey-year fixed effects (for CSES). All the regressions include sampling weights. *** significant at 1%, ** significant at 5%, * significant at 10%.

Chapter 3: The Long-term Negative Impact of Conflict on Human Development: The Role of the Sex Ratio

3.1 Introduction

Armed conflicts tend to result in the deaths of large numbers of young males and reduce the sex ratio, the number of men for each woman, which, in turn, affects the marriage outcomes of survivors in post-conflict societies. The negative effects of an imbalanced sex ratio on marital outcomes have been documented in France after World War I (Abramitzky, Delavande, & Vasconcelos, 2011), in Bavaria after World War II (Bethmann & Kvasnicka, 2013, 2014), and in Tajikistan after the Tajik civil war (Shemyakina, 2013). Given the effects of armed conflicts on the sex ratio and marriage outcomes, armed conflict might also have negative intergenerational impacts because of the close relationship between outcomes of parents and children.

This chapter examines the role of the sex ratio in perpetuating the negative impact of civil conflict on human development through intergenerational transmission. To do so, we exploit arguably exogenous geographical variation in the intensity of the Cambodian genocide, which disproportionately killed prime-age males under the Khmer Rouge (KR) regime (1975–1979), as a natural experiment. We show that in districts with high mortality rates during the KR regime, children born to parents who were of prime age for marriage (14–29) during the 1970s and 1980s have lower educational and health outcomes. Our results indicate that adverse educational and health outcomes of these children channel through the imbalanced sex ratio of their parents' generation.

To the best of our knowledge, this is the first study to provide evidence on the intergenerational impact of civil conflict. Previous studies typically have examined the effects of exposure to civil conflict in early life, either *in utero* or infancy, and adolescence on health status (e.g., Bundervoet, Verwimp, & Akresh, 2009; Akresh, Bhalotra, Leone, & Osili, 2012; Akresh, Lucchetti, & Thirumurthy, 2012; Minoiu & Shemyakina, 2014, 2012; Grimard & Laszlo, 2014) and educational attainment (e.g., Akresh & de Walque, 2008; Chamarbagwala & Morán, 2011; Leon, 2012; Shemyakina, 2011). We show that the Cambodian genocide had negative impacts on the health and educational outcomes of children born years after it ended, and we provide evidence that the impacts channel through the imbalanced sex ratio that existed in the parents' generation. Thus, this study bridges the literature on the adverse impacts of armed conflicts and the literature on the direct consequences of gender imbalance on marriage and labor market outcomes (Abramitzky et al., 2011; Angrist, 2002; Brainerd, 2008; Shemyakina, 2013; Bethmann & Kvasnicka, 2013, 2014).

We find that mortality rates under the KR predict the likelihood of lower normal grade progression rates and height-for-age Z-scores among children born to parents who were of prime age for marriage during the 1970s and 1980s. We find that each additional percentage point increase in KR mortality rates reduces the probability of children displaying normal grade progression by 7.5 percentage points and decreases children's height-for-age Z-scores by 1.4–1.5 standard deviations. We also find that in districts with high mortality during the KR regime, the sex ratio is lower in the parents' generation. When the mortality rate under the KR increases by 1 percentage point, the sex ratio in the parents' generation falls by 0.013–0.018 standard deviations. Using KR mortality as an instrumental variable for the sex ratio, the likelihood of children exhibiting normal grade progression decreases by 6.8–7.4 percentage points and height-for-age Z-scores fall by 1.5 standard deviations for every 1 standard deviation decrease in the sex ratio in the parents' generation. Our findings are robust to alternative measures of mortality rates and cohort ranges that define the sex ratio of the parents' generation. Our results are also robust to the possibility of sibling effects when restricting the sample to the oldest children born to parents in the selected birth cohorts.

We examine other channels that could potentially bias the effects of the sex ratio on children's educational and health outcomes. Specifically, we focus on three major channels that are closely linked to children's education and health outcomes: parental educational attainment, income, and health status. First, we find that geographical variations in mortality rates during the KR regime do not influence parents' educational attainment. Second, we find that parents' monthly earnings and household earnings are not associated with mortality rates during the KR regime. Third, we find that a wide range of parents' health measures are not correlated with mortality rates during the KR regime. While it is difficult to conclusively rule out all possible channels, the evidence indicates that mortality rates during the KR regime do not affect these determinants of children's outcomes but do influence the sex ratio of the parents' generation and children's education and health. This evidence suggests that the intergenerational impact of conflict is most likely mediated by changes in the sex ratio.

We also seek to understand whether changes in the sex ratio of the parents' generation capture the impact of genocide on their marital outcomes. We find that a low sex ratio leads to a higher likelihood of women marrying younger husbands and narrower spousal age and education gaps. When the sex ratio decreases by 1 standard deviation, the probability of women marrying a younger husband increases by 4.4 percentage points (19.3% increase relative to the mean). A 1 standard-deviation decrease in the sex ratio reduces the spousal age gap by 0.6 years (23.3% decrease from its mean) and the spousal education gap by 0.3 years (18.4% decrease from the mean). Additionally, the lower sex ratio has no effect on children ever born but does lead to higher mother's age at childbirth and at first marriage. A 1 standard deviation decrease in the sex ratio produces a 0.4–0.5 year increase in mother's age at childbirth when the sample is children 6–17 years old (1.6%–1.7% increase from the mean). Mother's age at first marriage rises by 0.4–0.5 years (1.9%–2.0% increase from the mean) with a 1 standard-deviation decline in the sex ratio.

Finally, we supplement our main results for children's outcomes, by adding parents' age and education, as well as mother's age at marriage and first birth, as additional controls to allow for the direct effects of these variables. The intergenerational effect of the genocide on children's education decreases moderately when including both parents' educational attainment. We show that age of the mother at childbirth is unlikely to explain why height-for-age is lower in districts which had higher mortality rates under the KR. Therefore, we conclude that, although one cannot completely rule out alternative channels through which civil conflict could have adverse intergenerational outcomes, the effect of the genocide on the sex ratio and the ensuing disruption to the marriage market in the parents' generation most likely play crucial roles in perpetuating the intergenerational effects of the genocide.

3.2 Data

The data for the main results in this chapter are drawn from four sources: the 10% micro sample of the 1998 General Population Census of Cambodia (Census 1998), the 2004 Cambodia Socio-Economic Survey (CSES), the Cambodian Genocide Database (CGD), and the 1962 Cambodian Population Census (Census 1962). In robustness checks, we also draw on the Demography and Health Survey 2000 (DHS 2000).

It is worth noting that no population census was undertaken in Cambodia between 1962 and 1998. The Government of the People's Republic of Kampuchea carried out population counts in 1979 and 1980; however, district-level information on the male and female populations is not available. The CSES was conducted in 1993, 1997, 1999, and 2004. The surveys carried out before 2004 cover a narrower range of topics and have smaller sample sizes. CSES 2004 covers the whole country and includes information on a wider range of topics, such as children's health and educational expenditure.

The 10% micro sample of Census 1998 provides information related to marital status, educational attainment, and other socio-demographic characteristics of individuals and households. We use this data to construct the sex ratio for our selected cohorts who were exposed to the KR regime during prime age for marriage. The large sample size allows more precise estimation of the effects of mortality under the KR regime and the imbalanced sex ratio. We limit the Census 1998 sample to individuals born in Cambodia, who make up 99.4% of the total sample, in order to exclude individuals who were not in Cambodia while the KR was in power. The main limitations of Census 1998 include the absence of information on educational expenditure, children's height and weight, health status and earnings profile. Thus, we use CSES 2004 to examine the effects of mortality during the KR regime and the resulting imbalanced sex ratio on children's educational and health outcomes, as well as the influence of parental health and income.

The 1962 census data were drawn from the final report of the General Population Census 1962 "Resultats Finals du Recensement General de la Population 1962", issued by the Ministry of Planning, Cambodia. The 1998 Census data were sourced from the Integrated Public Use Microdata Series, International (IPUMS-I), at the Minnesota Population Center (2014). The CSES data were obtained from the National Institute of Statistics, Ministry of Planning, Cambodia.

3.2.1 Child Sample and Measures of Children's Educational and Health Outcomes

We study children who were born after 1980 and whose parents were of prime age for marriage during the 1970s and 1980s. We use both Census 1998 and CSES 2004 to examine children's educational outcomes and CSES 2004 to examine children's health outcomes.

We use two indicators to evaluate educational outcomes of children aged 6 to 17: 1) whether children exhibit a normal grade progression; and 2) total spending on children's education. We use Census 1998 to code whether a child exhibits normal progression in school. This binary variable equals 1 if a child attends a grade level equal to or higher than the standard grade level for the child's age. We use CSES 2004 to construct total spending on children's education by summing school tuition fees, textbook and school supply costs, transportation and pocket money, as well as other school related expenses (the monetary unit is thousands of Cambodian riels).

We focus on the educational outcomes of children aged 6–17 for two reasons. First, as shown in Figure 3.1, most individuals aged 6–17 in 1998 are children of the cohorts who were of prime marriage age during the KR regime. Second, in the Cambodian education system, children start first grade at 6 years old and should reach twelfth grade and complete high school when they are 17 years old.

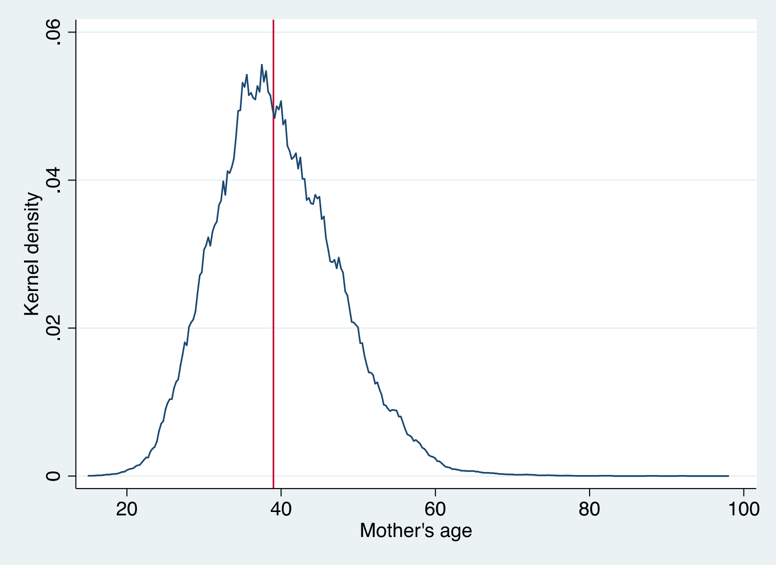


Figure 3.1: Distribution of mothers' ages for children aged 6–17 in 1998 (Census 1998)

For children's health outcomes, we use a sample of children aged 0 to 5 years old in CSES 2004 because it contains information on height and weight only for children younger than 5 years old, while Census 1998 does not contain any of this information. We use height-for-age and weight-for-age Z-scores as measures of children's health outcomes. Anthropometric Z-scores are calculated using the gender-specific 2006 World Health Organization (WHO) child growth standards for children 0 to 5 years old. Height-for-age is a commonly used measure of health for children under age 5 and is widely regarded a good health indicator because it predicts long-term health outcomes. It is also a useful indicator of parent's investment in their children. Duflo (2000) argues that height-for-age depends on accumulated investment in nutrition and healthcare throughout childhood, while genetic factors become more important in adolescence. In contrast, weight-for-age is an indicator of children's current malnutrition and health status. We code children as stunted if their height-for-age Z-scores are lower than −2 (below minus 2 standard deviations from the average of the reference population), and underweight if their weight-for-age Z-scores are below the same threshold.

3.2.2 Sex Ratios of the Parents' Generation

We use data from Census 1998 to construct the district-level sex ratios of individuals who were in the prime age for marriage in the 1970s and 1980s. These women and men were born between 1950 and 1965. The sex ratio for each district is defined as the ratio of men to women born between 1950 and 1965 who lived in that district in 1998.

Restricting our analysis to individuals born between 1950 and 1965 is justified by two reasons. First, the lives of these individuals were the most affected by the KR regime. We use Census 1998 to plot the sex ratio of individuals by birth year (1940–2000 birth cohorts) at the national level. As shown in Figure 3.2, the sex ratio starts to abruptly drop for cohorts born in 1950 onwards and reaches a low point with the 1954–1956 birth cohorts. Although the sex ratio increases slightly for younger cohorts, it remains considerably less than 1. Only for cohorts born after the KR regime ended does the sex ratio come close to the normal level (approximately 1.06). Overall, sex ratios are fairly low for cohorts born between 1950 and 1965. The low sex ratios imply a shortage of men relative to women in local marriage markets. Second, these birth cohorts were of prime marriage age during and after the KR regime. Approximately 98% of all women in CSES 2004 and 97% of women born between 1950 and 1965 were married for the first time between the ages of 15 and 30.

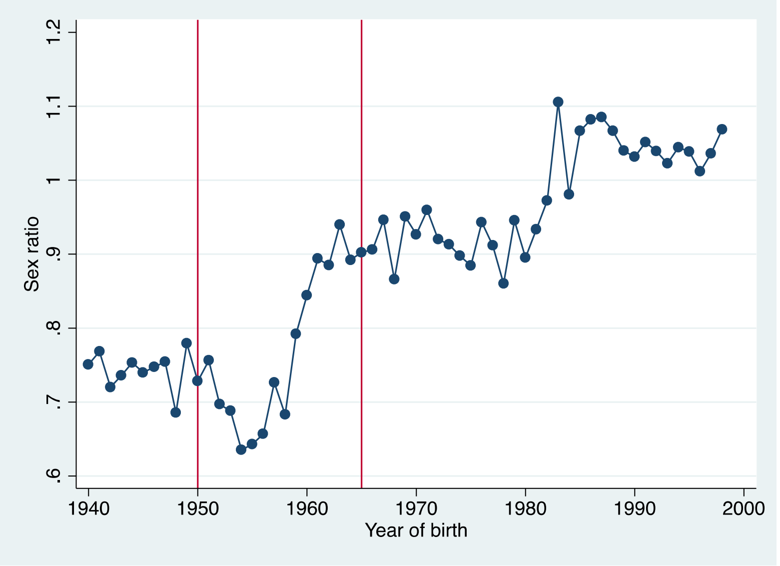


Figure 3.2: Sex ratio by year of birth (Census 1998)

Notes: The first vertical red line indicates the cohort born in 1950 and the second vertical red line indicates the cohort born in 1965.

3.2.3 Khmer Rouge Mortality Rates

We use geographic variation in KR mortality rates across districts to examine the impact of the genocide on children's outcomes and the mediating role played by the conflict-driven gender imbalance. We construct KR mortality rates using information sourced from the CGD and Census 1998. The CGD includes a district identifier of each KR mass gravesite and the estimated number of bodies at each mass grave.[14] Some graves have minimum and maximum estimates of bodies and, in such situations, we use the average of the two estimates to construct district-level KR mortality rates. Specifically, we divide the estimated deaths under the KR 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 Census 1998 data). As we do not have information on the number of individuals who survived the KR regime, but died between

[14] The CGD was initially developed by Yale University and has been updated by the Documentation Center of Cambodia (DC-Cam). We use both information from the original Yale database and data on additional mass gravesites and estimates of the number of deaths from the updated DC-Cam. See http://www.yale.edu/cgp/ and http://www.d.dccam.org/Database/Index1.htm for details on the Cambodian Genocide Program and DC-Cam database.

1980 and 1998 at the district level, the estimated KR mortality rates are noisy. Districts in five provinces (Kaoh Kong, Preah Vihear, Otdar Mean Chey, Krong Kaeb, and Krong Pailin) are excluded from the analysis because no information on the estimated deaths under the KR regime is available in the CGD. In a robustness section, we use alternative measures of KR mortality rates to examine the sensitivity of our results. We find that the main results are robust to alternative ways of constructing mortality rates.

Figure 3.3 plots changes in the sex ratios of the 1950–1965 birth cohorts against KR mortality rates. There is a strong negative relationship between the sex ratio and the KR mortality rate at the district level. The association is statistically different from zero at the 1% level. When the KR mortality rate increases by 1 percentage point, the sex ratio decreases by 0.013–0.018 standard deviations.

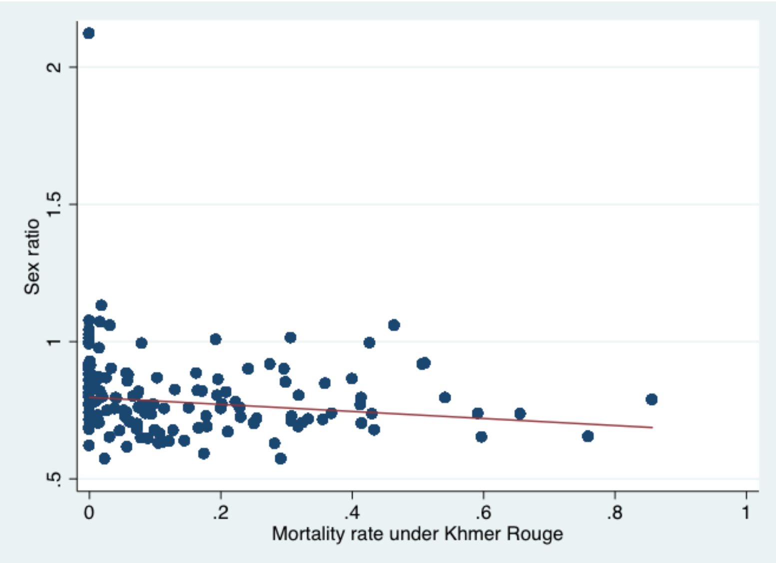


Figure 3.3: Sex ratio of the 1950–1965 birth cohorts against the Khmer Rouge mortality rates (Census 1998 and CGD)

Note: The negative correlation between variation in KR mortality rates and sex ratio is statistically significantly different from zero at the 1% level of significance.

We argue that the geographical variation in KR mortality rates at the district level provides exogenous variation in the sex ratio of individuals born from 1950 to 1965. We assess whether this argument holds by examining the correlation between KR mortality rates and various measures that are potential correlates of outcomes or economic activities: the 1) pre-KR sex ratio; 2) pre-KR population density; 3) geographical distance of a district to an urban center; and 4) geographical distance of a district to a neighboring country. Census 1962 allows us to test whether the variation in KR mortality rates is orthogonal to the pre-KR sex ratio and population density.[15] Table 3.1 shows no statistically significant association between the sex ratios in 1962 and the KR mortality rates at the district level (column 1). The KR mortality rates are also not correlated with various measures of population density in 1962 (columns 2–4). Similarly, district mortality rates under the KR are not correlated with distance to the provincial capital, the nearest Thai border district or the nearest Vietnamese border district. Thus, geographical variation in KR mortality is likely exogenous.

[15] The General Population Census 1962 provides data on commune-level population by gender. First, we match the commune codes in Census 1962 with the district codes in Census 1998. Next, we match the commune-level population with the district-level population based on the district codes in Census 1998. To merge Census 1962 with the CGD, we replaced the sex ratio and population density with the neighboring district's sex ratio and population density for district codes that were not available in Census 1962.

Table 3.1: Exogeneity of mortality rates under the Khmer Rouge regime

	Sex ratio in 1962	Density in 1962	Density— Men in 1962	Density — Women in 1962	Distance to capital district	Distance to nearest Thai border district	Distance to nearest Vietnamese border district
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
KR mortality rates	-0.022	-819.500	-419.917	-399.583	4.191	3.719	7.428
	(0.017)	(659.871)	(334.296)	(325.630)	(9.542)	(32.113)	(48.342)
Constant	1.010***	541.307***	274.367***	266.940***	28.919***	198.946***	139.267***
	(0.005)	(205.022)	(104.262)	(100.772)	(2.223)	(9.127)	(13.214)
R-squared	0.008	0.008	0.008	0.008	0.001	0.0001	0.0001
Observations	145	145	145	145	145	145	145

Note: The number of observations is the number of districts. The values for the dependent variables in columns 1–4 are derived from Census 1962. KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD and the number of individuals born before 1980 based on their district of birth. Robust standard errors are in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

3.2.4 Summary Statistics

Summary statistics for the samples are presented in Table 3.2. The upper panel (A) shows statistics for observations from Census 1998, and the lower panel (B) observations from CSES 2004.

In Panel A of Table 3.2, only 11.3% of children aged 6–17 in the mother sample (the sample of children with a mother born between 1950 and 1965) exhibit normal grade progression, while about 12% in the father sample (the sample of children with a father born between 1950 and 1965) do. For both mother and father samples, about 51% of the children aged 6–17 are boys. The average mother's age at childbirth is about 28 and ranges from 16 to 42 years old. The average mother's age at first marriage is about 22.

In the parents' generation, approximately 79% of women born between 1950 and 1965 are married in 1998, while 96% of men born during the same period are married. The mean years of schooling are only about 2.6 years for women and 4.5 years for men. The mean sex ratios for both the male and female samples are lower than 1, meaning that more women than men are in the 1950–1965 birth cohorts. The standard deviation of the female sex ratio is 0.107, while that of the male sex ratio is 0.122. In the female sample, approximately 23% of women have younger husbands and 18% of men are younger than their wives in the male sample. The mean age gap of spouses is approximately 2.7 years in both the male and female samples. The average spousal education gap in the female sample is about 1.7 years of schooling, compared to 1.5 years in the male sample. On average, about 15% of women in the female sample have less-educated husbands and about 16% of the men in the male sample have less education than their wives. The average number of children ever born and the average number of children surviving in the female sample are 5.4 and 4.7, respectively.

In Panel B of Table 3.2, average expenditure on children's education is slightly higher in the female sample than the male sample. Nearly 46% of children younger than 5 in the mother sample and 43% of children younger than 5 in the father sample are stunted. Approximately 30% of children younger than 5 in the mother sample and 28% of children younger than 5 in the father sample are underweight. The mean height-for-age and weight-for-age Z-scores are -1.05 and -0.80, respectively, in the mother sample. In the father sample, they are -0.85 and -0.53, respectively. The mean mother's age at childbirth and mother's age at first marriage in CSES 2004 is approximately 39 and 21, respectively.

The demographic characteristics of the parents' generation in the CSES 2004 sample are similar to those in the Census 1998 sample, except that all women and men born between 1950 and 1965 were married in the CSES 2004 sample.

Table 3.2: Summary statistics of main variables

	Mother born in 1950–1965 sample					Father born in 1950–1965 sample				
	N	Mean	SD	Min.	Max.	N	Mean	SD	Min.	Max.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Census 1998										
Children aged 6–17										
Age	193288	11.307	3.312	6	17	177877	10.986	3.291	6	17
Years of schooling	193288	2.319	2.542	0	13	177877	2.158	2.457	0	13
Male	193288	0.510	0.500	0	1	177877	0.511	0.500	0	1
Children's normal grade progression likelihood	193288	0.113	0.317	0	1	177877	0.119	0.324	0	1
Mother's age at childbirth*	193288	28.436	4.979	16	42					
Mother's age at first marriage**	192711	22.453	3.960	12	42					
Individuals born 1950–1965										
Sex ratio *(men born 1950–1965 / women born 1950–1965)*	97812	0.777	0.107	0.573	2.122	75683	0.792	0.122	0.573	2.122
KR mortality rates	97812	0.146	0.160	0	0.857	75683	0.142	0.159	0	0.857
Age	97812	39.719	4.542	33	48	75683	39.226	4.548	33	48
Married	97812	0.789	0.408	0	1	75683	0.961	0.194	0	1
Years of schooling	97716	2.595	2.996	0	13	75549	4.457	3.483	0	13
Spouse's age	69185	42.242	7.839	15	91	68462	36.540	5.849	12	66
Younger husband	69185	0.230	0.421	0	1	68462	0.178	0.382	0	1
Spouse's education	69185	4.319	3.401	0	13	68462	2.875	3.060	0	13
Less-educated husband	69185	0.146	0.353	0	1	68462	0.156	0.362	0	1
Spouse age gap *(husband's age to wife's age)*	69185	2.738	5.862	-20	54	68462	2.741	4.402	-20	33
Spouse education gap *(husband's education to. wife's education)*	69185	1.684	3.271	-13	13	68462	1.541	3.205	-13	13
Number of children ever born	69185	5.385	2.547	0	20					
Number of children surviving	68123	4.690	2.017	0	16					
Panel B: CSES 2004										
Children aged 6–17										
Age	7168	12.186	2.958	6	17	7767	11.912	2.984	6	17
Years of schooling	7168	3.705	2.586	0	12	7767	3.557	2.539	0	12
Male	7168	0.524	0.499	0	1	7767	0.516	0.500	0	1
Children's educational expenditure (in thousand Cambodian riels)	7168	90.543	293.424	0	12220	7767	91.039	285.059	0	12220
Children aged under 5										
Age	1267	3.154	1.641	0	5	1717	3.000	1.680	0	5
Male	1267	0.526	0.500	0	1	1717	0.507	0.500	0	1
Height-for-age Z-score	1267	-1.052	3.714	-9.12	19.61	1717	-0.847	3.903	-9.37	19.61
Weight-for-age Z-score	1267	-0.795	2.692	-7.05	16.36	1717	-0.531	2.910	-7.05	16.36
Mother's age at childbirth*	1267	38.860	3.429	33	54					
Mother's age at first marriage	1213	21.271	4.195	15	40					
Individuals born 1950–1965										
Age	5876	45.543	4.591	37	55	4442	45.169	4.598	37	55
Married	5876	0.738	0.440	0	1	4442	0.962	0.191	0	1
Years of schooling	5820	2.837	3.108	0	22	4379	4.854	3.610	0	22
Spouse's age	4159	48.032	7.566	23	88	4186	42.081	6.142	18	70
Younger husband	4159	0.221	0.415	0	1	4186	0.171	0.377	0	1
Spouse's education	4159	4.751	3.536	0	22	4186	3.248	3.185	0	22
Less-educated husband	4159	0.155	0.362	0	1	4186	0.163	0.369	0	1
Spouse age gap	4159	2.830	5.610	-20	38	4186	3.065	4.670	-20	31
Spouse education gap	4159	1.773	3.365	-18	16	4186	1.604	3.263	-11	18

Note: KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD and the number of individuals born before 1980 based on their district of birth. Normal grade progression likelihood takes the value of 1 if a child attends the expected grade level or higher and 0 otherwise. Children's educational expenditure are the sum of school tuition fees, textbook and school supply costs, transportation and pocket money, and other school-related expenses (in thousand Cambodian riels). * Mother's age at childbirth is a proxy variable which is equal to the mother's age minus the child's age because we do not have information for this variable in either Census 1998 or CSES 2004. ** Mother's age at first marriage in Census 1998 is a proxy variable which is equal to the mother's age minus the age of the oldest child in the household minus 1. We assume that the oldest child living in the household is her oldest child because we have no information about children living outside the household. There are some missing values, so some variables used in the analysis do not have the same sample size.

3.3 Effects of KR Mortality on Children's Education and Health

In this section, we examine the intergenerational impact of the genocide under the KR on the outcomes of children born years after the genocide ended. Specifically, we employ the following reduced-form specification to estimate the effects of KR mortality rates on educational and health outcomes of children born after the genocide ended:

$$Chltoutcome_{ij} = \gamma_0 + \gamma_1 DR_j + \gamma_2 \mathbf{C}_{ij} + \varepsilon_{ij} \qquad (3.1)$$

where $Chltoutcome_{ij}$ represents the educational or health outcome of child i in district j. Children's education variables are the likelihood of the children exhibiting normal grade progression and expenditure on children's education. Children's health outcomes are the height-for-age and weight-for-age Z-scores. DR_j denotes the mortality rate during the KR regime in district j. We expect γ_1 to be negative, implying that children's outcomes are negatively affected by the KR mortality rates. $\mathbf{C}_{ij}$ is a vector of the characteristics of the child i (age and gender) living in district j, and ε_{ij} is the error term. We cluster standard errors at the district level and include sampling weights in our estimation.

We report the reduced-form estimates of the effects of civil conflict on children's educational and health outcomes in Table 3.3. Panel A reports the results for the sample of children whose mothers belong to the 1950–1965 cohorts and Panel B presents the results for the sample of children whose fathers belong to the 1950–1965 cohorts.

The results in columns 1 and 2 of Table 3.3 indicate that the likelihood of children experiencing normal grade progression is lower in high mortality districts for both the mother and father samples. The effects in the mother sample are statistically significant at the 10% level. In the mother sample, each additional percentage point in KR mortality rate reduces the likelihood of children experiencing normal grade progression by 7.5 percentage points. However, the results for the father sample are statistically insignificant.

Columns 3 and 4 of Table 3.3 show that expenditure on children's education are negatively associated with variation in KR mortality rates in both the mother and father samples, but the relationship is not statistically significant in either sample. Regarding children's health outcomes, the reduced-form results shown in columns 5 and 6 of Table 3.3 suggest that, in districts with high KR mortality rates, children's height-for-age and weight-for-age Z-scores are smaller in both the mother and father samples. However, the effects are statistically significant only for the height-for-age Z-scores. A 1 percentage point increase in KR mortality rates reduces a child's height-for-age Z-score by 1.5 standard deviations in the mother sample and 1.4 standard deviations in the father sample.

Overall, as anticipated, the genocide in Cambodia under the KR regime negatively affected the educational and health outcomes of children born after 1980, although the effects on educational expenditure on children are not statistically significant.

Table 3.3: Estimates of effects of mortality rates under the Khmer Rouge regime on children's educational and health outcomes

	Census 1998		CSES 2004			
	Children's normal grade progression likelihood		Children's school expenditure		Height-for-age Z-score	Weight-for-age Z-score
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Children of mothers born in 1950–1965 sample						
KR mortality rates	-0.075*	-0.075*	-86.227	-86.652	-1.499**	-0.668
	(0.044)	(0.044)	(58.211)	(57.980)	(0.696)	(0.441)
Age	-0.016***	-0.016***	16.451***	16.470***		
	(0.001)	(0.001)	(2.716)	(2.718)		
Male		0.0005		-5.854		
		(0.002)		(4.982)		
R-squared	0.030	0.030	0.038	0.038	0.005	0.002
Observations	193288	193288	7168	7168	1267	1267
Panel B: Children of fathers born in 1950–1965 sample						
KR mortality rates	-0.073	-0.073	-85.948	-86.265	-1.354*	-0.357
	(0.045)	(0.045)	(66.634)	(66.513)	(0.733)	(0.498)
Age	-0.018***	-0.018***	14.934***	14.953***		
	(0.001)	(0.001)	(2.427)	(2.429)		
Male		-0.0002		-5.840		
		(0.002)		(4.296)		
R-squared	0.033	0.033	0.033	0.033	0.003	0.0004
Observations	177877	177877	7767	7767	1717	1717

Note: KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD and the number of individuals born before 1980 based on their district of birth. All specifications include sampling weights. The samples in columns 1 and 2 include children aged 6–17 in 1998 with a parent born between 1950 and 1965. Normal grade progression likelihood takes the value of 1 if a child attends the expected grade level or higher and 0 otherwise. The samples in columns 3 and 4 are children aged 6–17 in 2004 with a parent born between 1950 and 1965. Children's educational expenditure are the sum of school tuition fees, textbook and school supply costs, transportation and pocket money, and other school-related expenses (in thousand Cambodian riels). The samples in columns 5 and 6 are children younger than 5 in 2004 with a parent born between 1950 and 1965. Robust standard errors clustered by district (145 districts) are in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

3.4 Effects of KR Mortality on the Education, Income and Health of the Parents' Generation

We show in section 3.2.3 that the district-level mortality rates under the KR regime negatively affect the district-level sex ratios of individuals born between 1950 and 1965, whose children are the subjects of the empirical analysis in section 3.3. Therefore, the sex ratio of the parents' generation is likely to be one of the channels through which the genocide has intergenerational impacts on children's outcomes. However, the intergenerational impact of the genocide on children might also channel through its impact on parental education, earnings, and health, as earlier studies have shown that parents' education, earnings, and health affect their ability to invest in children's education and health (e.g., see Black, Devereux, & Salvanes, 2005; Oreopoulos, Page, & Stevens, 2006; Case, Lubotsky, & Paxson, 2002; Thomas, 1994).

Using equation (3.1), we investigate whether the geographical variation in KR mortality rates influence the geographical variation in parental education, income, and health status. We use four sets of observations for our analysis. The first set includes mothers born between 1950 and 1965 who have children aged 0-17. The second set includes the husbands of the mothers in the first set. The third set includes fathers born between 1950 and 1965 who have children aged 0-17. The fourth set includes the wives of the fathers in the third set.

Panel A of Table 3.4 illustrates the effects of the KR mortality rates on parental educational attainment. In all four samples, there is no evidence that geographical variation in KR mortality rates influence parents' completed years of schooling. Therefore, parental education is unlikely to be the channel through which the genocide influences children's educational and health outcomes.

Panel B of Table 3.4 shows the effects of KR mortality rates on parents' monthly earnings and household income. For monthly earnings, we include those who work as employees or are self-employed in CSES 2004. We aggregate the monthly wages and household income for those who reported that they had worked during the past seven days and divide the total household income by the number of adults in the household who reported that they had worked during the past seven days. The estimates in Panel B show no evidence that that mortality under the KR directly affects parents' monthly earnings or household income.

Panel C of Table 3.4 presents the effects of KR mortality on parental health measures. We use a large number of health indicators from CSES 2004, including if the individual had experienced injury or illness during the past 30 days, was disabled, had experienced difficulties with physical movement or had experienced psychological difficulties. In the sample of mothers born between 1950 and 1965 and the sample of fathers married to mothers born between 1950 and 1965, there is no statistically significant relationship between KR mortality rates and any of the health measures examined (columns 1 and 2). In the father sample, the KR mortality rate does not have a statistically significant correlation with the majority of health indicators. The exceptions are difficulty moving (column 3), which is statistically positive at the 10% level, and difficulty speaking (columns 2 and 3), which is statistically negative at the 5% level.[16] The majority of health indicators are not correlated with KR mortality rates, and those coefficients that are significant are small or have the wrong sign; therefore, the evidence that genocide affects children's outcomes through its influence on parental health is weak at best.

[16] In section 3.5, we examine whether the estimated effect of the sex ratio on children's health is sensitive to adding difficulty moving as a control variable.

These findings imply that the estimated effects of the genocide on children's outcomes are unlikely to have come through its effects on parental education, income, and health. The lack of effects of KR mortality on parental education, income, and health and the strong effect of KR mortality on the sex ratio of the parents' generation suggest that the intergenerational impacts of violent conflicts are mediated primarily through the sex ratio.

Table 3.4: Estimates of effects of mortality rates under the Khmer Rouge regime on parents' education, income, and health

	Mother born 1950–1965 with children aged 0–17 (1)	**Her spouse** (2)	**Father born 1950–1965 with children aged 0–17** (3)	**His spouse** (4)
Panel A: Effects of Mortality Rates under the Khmer Rouge Regime on Parents' Education (Census 1998)				
Dependent variable:				
Years of schooling	-0.691	-1.033	-1.121	-0.783
	(0.542)	(0.656)	(0.747)	(0.622)
Observations	66284	66284	65643	65643
Panel B: Effects of Mortality Rates under the Khmer Rouge Regime on Parents' Income (CSES 2004)				
Dependent variable:				
Monthly earnings	-0.263	-0.079	-0.417	-0.353
	(0.825)	(0.324)	(0.390)	(0.680)
Observations	301	961	1102	350
Monthly household income	-0.470	-0.470	-0.373	-0.373
	(0.322)	(0.322)	(0.344)	(0.344)
Observations	3732	3732	3852	3852
Panel C: Effects of Mortality Rates under the Khmer Rouge Regime on Parents' Health (CSES 2004)				
Dependent variable:				
Illness/injury during the past 30 days	0.025	0.077	0.100	0.021
	(0.078)	(0.062)	(0.067)	(0.073)
Disabled	0.058	0.043	0.049	0.028
	(0.054)	(0.045)	(0.046)	(0.045)
Difficulty seeing	0.008	0.010	0.009	0.011
	(0.023)	(0.027)	(0.018)	(0.017)
Difficulty hearing	-0.001	0.009	0.014	-0.0003
	(0.008)	(0.011)	(0.010)	(0.007)
Difficulty speaking	-0.005	-0.007**	-0.007**	-0.004
	(0.005)	(0.003)	(0.003)	(0.004)
Difficulty moving	0.044	0.037	0.047*	0.020
	(0.028)	(0.023)	(0.024)	(0.020)
Difficulty sensing	0.004	0.003	-0.007	0.001
	(0.011)	(0.011)	(0.010)	(0.011)
Psychological or behavioral difficulties	0.007	-0.0001	-0.005	0.003
	(0.013)	(0.007)	(0.008)	(0.009)
Observations	3736	3736	3855	3855

Note: We report the estimated coefficient for KR mortality rates. All specifications include sampling weights. KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD and the number of individuals born before 1980 based on their district of birth. Illness/injury during the past 30 days equals 1 if an individual was sick or injured during the last 30 days and 0 otherwise. Disabled equals 1 if an individual is disabled and 0 otherwise. Difficulty seeing, hearing, speaking, moving, and sensing and psychological or behavioral difficulties are coded in the same way. In the mother sample, when we include either the mother's own age (specification 1) or the father's own age (specification 2), the estimated coefficients for KR mortality rates remain similar. In the father sample, the estimated coefficients for KR mortality rates are similar when we include either the father's own age (specification 3) or the mother's own age (specification 4). Robust standard errors clustered by district are in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

3.5 Effects of the Sex Ratio on Children's Education and Health

We provide evidence that the geographical variation in KR mortality rates is exogenous to the historical sex ratio, historical population density, distance to various centers of economic activities, completed years of schooling, and later-life income and health measures of individuals born between 1950 and 1965. We also show that mortality under the KR regime is a major source of variation in gender imbalances across districts among individuals who were of prime age for marriage in the 1970s and 1980s. Most importantly, KR mortality rates affect the educational and health outcomes of children born years after the KR regime ended. These results imply that mortality under the KR regime during the 1970s influences the outcomes of children in the late 1990s and early 2000s through its impact on gender imbalance in the parents' generation.

In this section, we use variation in KR mortality rates as an instrument for the sex ratio of the parents' generation to examine the effects of the parental sex ratio on children's educational and health outcomes. Our second-stage instrumental variable (IV) specification is as follows:

$$Chltoutcome_{ij} = \delta_0 + \delta_1 \widetilde{Sexratio}_j + \delta_2 \mathbf{C}_{ij} + v_{ij} \qquad (3.2)$$

where $Sexratio_j$ is the sex ratio in district j. $\mathbf{C}_{ij}$ is a vector of the child's characteristics i (age and gender) living in district j, and v_{ij} is the error term. Our focus is on δ_1, and we expect $\delta_1 > 0$. When $\delta_1$is positive, it implies that the higher the sex ratio (more men), the better the outcomes for children. In other words, the more imbalanced the sex ratio is, the worse the outcomes for children. We also cluster standard errors at the district level and include sampling weights.

Panel A of Table 3.5 presents the effects in the mother sample and Panel B displays the effects in the father sample. We also report the first-stage F-statistics.

Columns 1 and 2 of Table 3.5 present the IV results for the effects of the sex ratio on the likelihood of normal grade progression for children aged 6–17 years old. The sex ratio of the parents' generation has a positive effect on the children's educational outcomes. A 1 standard deviation decrease in the sex ratio, which is roughly 0.11 in the mother sample, lowers the likelihood of normal grade progression by 6.8 percentage points (columns 1 and 2 of Panel A). This finding amounts to a 60.4% decrease in the likelihood of normal grade progression from the mean of 11.3%. For the father sample, a 1 standard deviation decline in the sex ratio, which is approximately 0.12, reduces the likelihood of children's normal grade progression by 7.4 percentage points (columns 1 and 2 of Panel B), which represents a 62.3% decrease from the mean of 11.9%.

Parents might favor sons over daughters or vice versa (e.g., Dahl & Moretti, 2008), and if so, it is possible that parents would engage in gender-biased investment in children's health or education. When we control for children's gender in the specifications (column 2), the estimated effects remain unchanged, indicating that gender-biased investment in children's health or education is unlikely to play any role in this setting.

Columns 3 and 4 of Table 3.5 show that the sex ratio of the parents' generation has a positive association with educational expenditure on children in both the mother and father samples, but the association is statistically insignificant. Children's gender also has no effect on parents' spending on children's education.

Columns 5 and 6 of Table 3.5 present the results for the effects of the sex ratio on children's health based on observations in the mother and father samples. The sex ratio is positively associated with height-for-age and weight-for-age Z-scores in both samples, but only the effects of the sex ratio on height-for-age Z-scores are statistically significant (column 5). A 1 standard deviation decrease in the sex ratio decreases children's height-for-age Z-scores by approximately 1.5 standard deviations in both the mother and father samples.

We also test whether the estimated effects of the gender imbalance in the parents' generation on children's outcomes in the father sample are sensitive to controlling for difficulty moving, given that this is the only health measure that is negatively correlated with KR mortality. This health measure is available only in CSES 2004, so we could not include it as a control variable in the estimation of the effects of the sex ratio on the likelihood of children's normal grade progression, which is a variable in Census 1998. We report the results in Table A3.1 in the appendix. When we add the father having difficulty moving as an additional control variable, the estimated effects of the sex ratio on children's educational expenditure (column 1 of Table A3.1), height-for-age Z-scores (column 2) and weight-for-age Z-scores (column 3) are all close to the main results.

Table 3.5: Estimates of effects of the sex ratio on children's educational and health outcomes

	Census 1998		**CSES 2004**			
	Children's normal grade progression likelihood		Children's educational expenditure		Height-for-age Z-score	Weight-for-age Z-score
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Children of mothers born in 1950–1965 sample						
Sex ratio	0.638*	0.638*	865.499	868.581	13.963*	6.227
	(0.370)	(0.370)	(543.333)	(541.715)	(8.321)	(4.544)
Age	-0.016***	-0.016***	16.152***	16.165***		
	(0.001)	(0.001)	(2.371)	(2.370)		
Male		0.001		-4.223		
		(0.002)		(4.587)		
First-stage *F-statistic*	7.337***	7.337***	4.439**	4.452**	5.442**	5.442**
R-squared	0.014	0.014	0.044	0.044	-0.113	-0.035
Observations	193288	193288	7168	7168	1267	1267
Panel B: Children of fathers born in 1950–1965 sample						
Sex ratio	0.608*	0.608*	844.546	847.115	12.653*	3.332
	(0.362)	(0.362)	(591.498)	(590.562)	(7.345)	(4.400)
Age	-0.017***	-0.017***	15.545***	15.562***		
	(0.001)	(0.001)	(2.215)	(2.213)		
Male		-0.001		-4.815		
		(0.002)		(3.992)		
First-stage *F-statistic*	7.536***	7.535***	4.422**	4.427**	5.936**	5.936**
R-squared	0.018	0.018	0.046	0.046	-0.079	-0.005
Observations	177877	177877	7767	7767	1717	1717

Note: KR mortality rates measure the mortality rates under the KR regime based on the estimated deaths by district in the CGD and the number of individuals born before 1980 based on their district of birth. All specifications include sampling weights. The samples in columns 1 and 2 are children aged 6–17 in 1998 with a parent born between 1950 and 1965. Normal grade progression likelihood takes the value of 1 if a child attends the expected grade level or higher and 0 otherwise. The samples in columns 3 and 4 are children aged 6–17 in 2004 with a parent born between 1950 and 1965. Children's educational expenditure are the sum of school tuition fees, textbook and school supply costs, transportation and pocket money, and other school-related expenses (in thousand Cambodian riels). The samples in columns 5 and 6 are children younger than 5 in 2004 with a parent born between 1950 and 1965. Robust standard errors clustered by district (145 districts) are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

3.6 Effects of the Sex Ratio on Marriage and Fertility Outcomes

We establish that the effects of KR mortality on the outcomes of children born years after the genocide are most likely mediated through the genocide's impact on the gender imbalance in the parents' generation. In this section, we examine whether the genocide-driven gender imbalance affects measures of marriage outcomes of individuals born between 1950 and 1965, given that previous studies (e.g., Abramitzky et al., 2011; Angrist, 2002) have found that gender imbalances worsen marriage outcomes for the gender that has a surplus.

Due to the limited information in the available datasets, we focus on measures of marital outcomes that can be constructed based on basic demographic and education characteristics and information about the interrelationships of household members in the datasets. We estimate the effects of the imbalanced sex ratio on marriage outcomes using IV regression. The second-stage IV regression is as follows:

$$Y_{ij} = \beta_0 + \beta_1 \widetilde{Sexratio}_j + \beta_2 \mathbf{Z}_{ij} + \omega_{ij} \qquad (3.3)$$

The outcome variable, Y_{ij}, of individual i in district j includes the likelihood of being married at the time of the survey, the likelihood that a woman marries a younger man (or that a man is a younger husband), the spousal age gap, the likelihood that a woman marries a less-educated man (or that a man is a less-educated husband), the spousal education gap, the number of children ever born, the number of children surviving and the mother's age at childbirth and at first marriage. $\mathbf{Z}_{ij}$ is a vector of the individual's characteristics i (age and education) living in district j, and ω_{ij} is the error term. We also cluster standard errors at the district level and include sampling weights.

Table 3.6 reports the IV estimates for the effects of the sex ratio on the likelihood of getting married and spousal differences in the female sample (Panel A) and the male sample

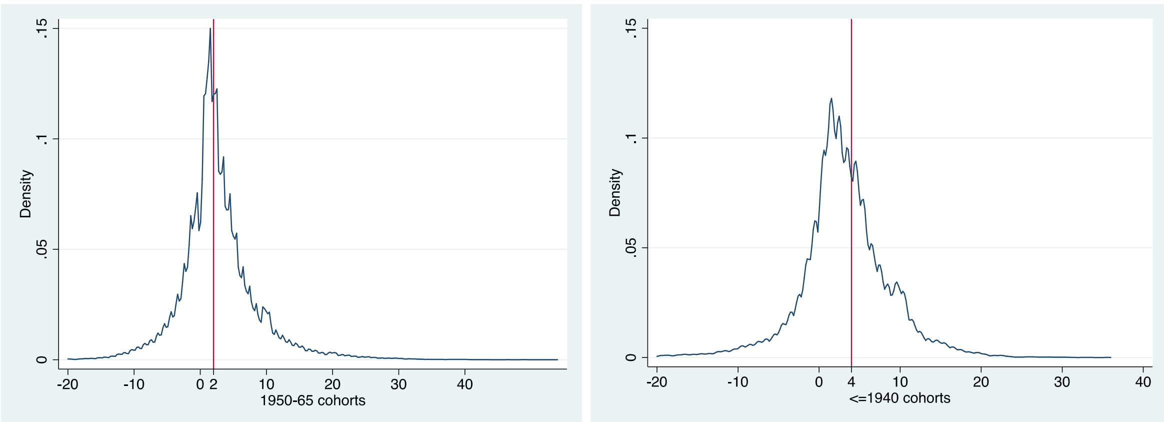
(Panel B). In the female sample, a higher sex ratio increases the likelihood of being married, but the effect is not statistically significant (column 1). Therefore, despite the shortage of men of similar ages, the probability of women being married remains largely unchanged, consistent with the findings of de Walque (2006) and Heuveline and Poch (2007). Our results imply that many of the affected women ended up marrying men outside their age range, perhaps reflecting that their eagerness to get married after the KR regime fell. Indeed, our estimates in Panel A confirm that women are more likely to marry a younger spouse, and the spousal age and education gaps are reduced when there is a decrease in the sex ratio (more women than men). The estimates are mostly statistically significant at the conventional levels, except for the likelihood of marrying a less-educated husband.

A 1 standard deviation decrease in the sex ratio increases the probability of a woman marrying a younger man by 4.4 percentage points (column 2), which represents a 19.3% increase from the mean of 23%. A 1 standard deviation decrease in the sex ratio reduces the spousal age difference by 0.6 years, a 23.3% decrease from the mean (column 3). The median spousal age difference is approximately 4 years for individuals born before 1940, who most likely married before the KR regime, but falls to 2 years for the 1950–1965 birth cohorts (Figure 3.4). This result is consistent with the findings of Abramitzky et al. (2011) from post-World War I France. The lower sex ratio also reduces the spousal education gap (column 5). For every 1 standard deviation decrease in the sex ratio, the spousal education gap falls by 0.3 years, or a 18.4% decrease from the mean. In a traditional society like Cambodia, there is a strong stigma against women who never get married, so parents with unmarried daughters cannot be as demanding of potential sons-in-law (Heuveline & Poch, 2006). The smaller spousal age and educational gaps might indicate poorer marital match quality.

Table 3.6: Estimates of effects of the sex ratio on marriage outcomes

	Census 1998				
	Married (1)	Husband’s age < Wife’s age (2)	Spousal age gap (3)	Husband’s education < Wife’s education (4)	Spousal education gap (5)
Panel A: Mother born in 1950–1965 sample					
Sex ratio	0.138	-0.415**	5.974***	-0.108	2.891**
	(0.165)	(0.176)	(2.076)	(0.125)	(1.415)
Age	-0.007***	-0.008***	0.176***	0.0001	0.034***
	(0.0003)	(0.001)	(0.007)	(0.0003)	(0.004)
First-stage *F-statistic*	7.899***	7.968***	7.968***	7.968***	7.968***
R-squared	0.007	0.004	0.015	-0.001	-0.005
Observations	97812	69185	69185	69185	69185
Panel B: Father born in 1950–1965 sample					
Sex ratio	0.0001	-0.490***	7.392***	-0.172	2.662**
	(0.067)	(0.140)	(1.805)	(0.132)	(1.279)
Age	0.002***	-0.009***	0.143***	-0.002***	0.077***
	(0.0003)	(0.0004)	(0.004)	(0.0003)	(0.003)
First-stage *F-statistic*	8.614***	8.376***	8.376***	8.376***	8.376***
R-squared	0.002	0.007	0.026	-0.002	0.007
Observations	75683	68462	68462	68462	68462

Note: All specifications include sampling weights. In Panel A, the samples are mothers born between 1950 and 1965. In Panel B, the samples are fathers born between 1950 and 1965. Robust standard errors clustered by district (145 districts) are in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.



(A) (B)

(A): The figure indicates husband age – wife age for individuals born between 1950 and 1965. The red line is the median.
(B): The figure indicates husband age – wife age for individuals born before 1940. The red line is the median.

Figure 3.4: Distribution of the husband and wife age gap.

Similar effects from the sex ratio on marriage outcomes also appear in the male sample. Column 1 of Panel B of Table 3.6 indicates that, when the sex ratio decreases, the probability of men being married decreases, but the estimate is not statistically significant. There is a strong negative and statistically significant relationship between the sex ratio and the likelihood of a man being younger than his wife in the male sample (column 2 of Panel B). Similarly, spousal age and education gaps also decrease as the sex ratio falls (columns 3 and 5 of Panel B). In particular, the effect of the sex ratio on the spousal age gap is much larger in the male sample than in the female sample.

Table 3.7: Estimates of effects of the sex ratio on the number of children

	Census 1998					
	Number of children ever born			**Number of children surviving**		
	(1)	(2)	(3)	(4)	(5)	(6)
Sex ratio	-3.248	-2.833	-2.074	-1.918	-1.630	-1.168
	(2.581)	(2.601)	(2.168)	(1.843)	(1.856)	(1.632)
Age		0.177***	0.181***		0.122***	0.125***
		(0.004)	(0.003)		(0.003)	(0.002)
Education			-0.130***			-0.080***
			(0.010)			(0.008)
First-stage *F-statistic*	7.984***	7.968***	7.839***	7.908***	7.893***	7.765***
R-squared	-0.002	0.097	0.123	0.006	0.080	0.093
Observations	69185	69185	69185	68123	68123	68123

Note: All specifications include sampling weights. The samples are mothers born between 1950 and 1965. Robust standard errors clustered by district (145 districts) are in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

Table 3.7 shows that changes in the sex ratio do not have any effect on the fertility of the 1950–1965 birth cohorts. There is no statistically significant relationship between the sex ratio and the number of children ever born or the number of surviving children across different specifications. However, the results in columns 1–3 of Table 3.8 show that there is a significant negative relationship between the sex ratio and the mother’s age at the birth of the children whose outcomes we examine.[17] A 1 standard deviation decrease in the sex ratio increases the age at which the mother gives birth to a child 6–17 years old in Census 1998 by 0.4-0.5 years. This is a 1.6%–1.7% increase from the mean mother's age at childbirth. The older the mother is at childbirth, the higher the risks to the child's physical and mental development.

These results imply that women delay their age of marriage in response to the shortage of men. As shown in columns 4–6 of Table 3.8, there is a significant negative correlation between the sex ratio and the age at first marriage for the mothers of children aged 6-17. We have no information on mother's age at first marriage in Census 1998, but it is available in CSES 2004. Thus, we use a proxy variable for mother's age at first marriage in Census 1998, which is the age of the mother minus the age of the oldest child in the household minus 1. This variable assumes that the oldest child living in the household is the oldest child of the mother as we do not have information on children living outside the household. Every 1 standard deviation decrease in the sex ratio increases the age at first marriage for mothers of children aged 6–17 by 0.4–0.5 years, representing a 1.9%-2.0% increase from the mean. These estimates indicate that women delay marriage as a result of the low sex ratio or the shortage of men resulting from the genocide.

[17] Mother's age at childbirth is a proxy variable which is equal to the mother's age minus the child's age because we do not have information for this variable in both Census 1998 and CSES 2004.

Table 3.8: Estimates of effects of the sex ratio on mother's age at childbirth and at first marriage

	Census 1998: Children aged 6-17						CSES 2004: Children under 5					
	Mother's age at childbirth			Mother's age at first marriage			Mother's age at childbirth			Mother's age at first marriage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sex ratio	-4.155**	-4.446**	-4.301***	-4.075*	-4.407*	-4.170**	9.098	9.547	8.257	2.735	0.133	3.409
	(1.742)	(1.787)	(1.410)	(2.254)	(2.251)	(2.109)	(6.834)	(7.002)	(5.682)	(9.283)	(8.511)	(9.291)
Mother's education		0.058***			0.067***			-0.067*			0.269***	
		(0.009)			(0.009)			(0.040)			(0.058)	
Father's age			0.343***			0.158***			0.262***			-0.118***
			(0.004)			(0.004)			(0.022)			(0.033)
Father's education			-0.016**			-0.004			-0.071*			0.107**
			(0.007)			(0.007)			(0.037)			(0.047)
First-stage *F-statistic*	7.338***	7.247***	7.217***	7.349***	7.258***	7.229***	5.442**	5.350**	5.443**	4.442**	4.328**	4.424**
R-squared	-0.005	-0.004	0.229	-0.011	-0.010	0.067	-0.044	-0.047	0.186	-0.005	0.030	0.027
Observations	193288	193288	193288	192711	192711	192711	1267	1267	1267	1213	1213	1213

Note: All specifications include sampling weights. The samples in columns 1–6 are children aged 6–17 in 1998 with a mother born between 1950 and 1965. The samples in columns 7–12 are children under 5 in 2004 with a mother born between 1950 and 1965. Mother's age at childbirth in both Census 1998 and CSES 2004 is a proxy variable which is equal to the mother's age minus the child's age. Mother's age at first marriage in Census 1998 is a proxy variable which is equal to the mother's age minus the age of the oldest child in the household minus 1. We assume that the oldest child living in the household is her oldest child because we have no information about the children living outside the household. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Our findings so far suggest several possible channels through which the gender imbalance in the parents' generation could lead to negative outcomes for children. The shortage of men in the 1950–1965 birth cohorts led women to marry younger, less educated men and to enter their first marriage at a later age than they would have otherwise, while marriage rates remain unchanged in equilibrium. This means that the combined educational attainment of parents decreased with the lower sex ratio. If the father's educational attainment influences children's outcomes, then the lower combined educational attainment of the parents will also hinder the children's educational attainment. As illustrated in Table 3.9, we compare the main results for the likelihood of children's normal grade progression when including only the child's characteristics (age and gender) with the results when controlling for parents' characteristics (age and education). We re-report the main results in column 1 (Panel A for the mother sample and Panel B for the father sample). The estimated effects of the sex ratio on the likelihood of children's normal grade progression are less in the specification containing both the mother's and father's education (column 3). However, the mother's and father's education do not seem to have any influence on children's height-for-age Z-scores (column 8).

We also show that as the sex ratio falls, completed fertility remains unchanged while age at childbirth increases. This implies that the children studied were born to older mothers whose egg quality at the time of conception might have been poor,[18] affecting the children's education and health. However, when we include mother's age at childbirth as an additional control variable, the effect of the sex ratio on the height-for-age Z-scores is statistically insignificant in the mother sample, but mother's age at childbirth has a significant positive correlation with children's height-for-age Z-scores (column 9). Additionally, the inclusion of this variable even increases the estimated coefficient on the sex ratio in the father sample.

[18] As completed fertility is unchanged, the birth order of the child likely remains unchanged.

The estimated effects are similar when controlling for mother's age both at childbirth and at first marriage (column 10). Thus, mother's age at childbirth is unlikely to explain why height-for-age is lower in districts with high KR mortality.

Another possibility is that the lower sex ratio implies that men might obtain a higher marriage surplus than women as the price of marriage adjusts in men's favor. This advantage could potentially give men greater household bargaining power. As some studies have shown, fathers tend to invest less in children than mothers; therefore, men's stronger bargaining position in the household might lead to poorer outcomes for children (Thomas, 1990, 1994; Duflo, 2000; Rangel, 2006; Qian, 2008). Unfortunately, the Census and CSES data lack suitable measures to capture the relationship between the imbalanced sex ratio and women's bargaining position in the household. However, we explored proxies for married women's bargaining position in the household using variables related to women's empowerment from DHS 2000. Specifically, DHS 2000 asked childbearing-age married women (aged 15–49) whether they or their husbands have the final say on various individual and household decisions, including the money the wife earns, the wife's health care, large household purchases, household purchases for daily needs, visits to family, what food to cook, children's education and medical care for sick children. We find no relationship between KR mortality rates and these proxies of women's relative bargaining position in the household. However, because the sample is fairly small (roughly 4,000 women) and cannot be directly linked to our outcome measures, we cannot completely rule out the possibility that the intergenerational effects of the genocide on children's health outcomes channel through women's bargaining position in the household.

Table 3.9: Estimates of effects of the sex ratio on children's normal grade progression likelihood and height-for-age Z-scores

	Census 1998: Children's normal grade progression likelihood					CSES 2004: Children's height-for-age Z-scores				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Children of mothers born in 1950–1965 sample										
Sex ratio	0.638*	0.522*	0.439*	0.439*	0.436*	13.963*	13.887*	13.935*	11.126	12.316
	(0.370)	(0.281)	(0.246)	(0.246)	(0.245)	(8.321)	(8.375)	(8.358)	(7.513)	(8.655)
Age	-0.016***	-0.017***	-0.018***	-0.018***	-0.018***					
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)					
Male	0.001	0.0003	0.0001	0.0001	-0.0001					
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)					
Mother's age		0.001***	-0.00003				-0.030	-0.020		
		(0.0003)	(0.0004)				(0.037)	(0.041)		
Mother's education		0.024***	0.016***	0.016***	0.016***		0.031	0.025	0.028	0.027
		(0.002)	(0.001)	(0.001)	(0.001)		(0.040)	(0.045)	(0.044)	(0.045)
Father's age			0.0001	0.0001	0.0003			-0.010	-0.101***	-0.111***
			(0.0003)	(0.0003)	(0.0003)			(0.022)	(0.022)	(0.025)
Father's education			0.015***	0.015***	0.015***			0.012	0.036	0.047
			(0.001)	(0.001)	(0.001)			(0.039)	(0.036)	(0.038)
Mother's age at childbirth				-0.00003	-0.001				0.329***	0.382***
				(0.0004)	(0.001)				(0.042)	(0.046)
Mother's age at first marriage					0.001***					-0.028
					(0.0004)					(0.028)
First-stage *F-statistic*	7.337***	7.225***	7.150***	7.150***	7.172***	5.442**	5.294**	5.382**	5.332**	4.313**
R-squared	0.014	0.072	0.097	0.097	0.097	-0.113	-0.111	-0.111	0.003	-0.005
Observations	193288	193288	193288	193288	192711	1267	1267	1267	1267	1213
Panel B: Children of fathers born in 1950–1965 sample										
Sex ratio	0.608*	0.445*	0.415*	0.415*	0.417*	12.653*	12.799*	12.853*	12.918*	14.632*
	(0.362)	(0.261)	(0.234)	(0.234)	(0.236)	(7.345)	(7.376)	(7.467)	(7.366)	(8.202)
Age	-0.017***	-0.018***	-0.018***	-0.018***	-0.019***					
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)					
Male	-0.001	-0.001	-0.001	-0.001	-0.001					
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)					
Mother's age			-0.00001					-0.059**		
			(0.001)					(0.027)		
Mother's education			0.017***	0.017***	0.017***			-0.022	0.012	0.020
			(0.001)	(0.001)	(0.001)			(0.040)	(0.039)	(0.040)
Father's age		0.001**	0.001*	0.001*	0.001**		0.008	0.045	-0.053*	-0.059*
		(0.0003)	(0.001)	(0.001)	(0.001)		(0.025)	(0.033)	(0.032)	(0.035)
Father's education		0.023***	0.016***	0.016***	0.015***		0.042	0.030	0.071**	0.070*
		(0.002)	(0.001)	(0.001)	(0.001)		(0.031)	(0.033)	(0.034)	(0.036)
Mother's age at childbirth				-0.00001	-0.001				0.101***	0.114***
				(0.001)	(0.001)				(0.026)	(0.030)
Mother's age at first marriage					0.002***					-0.042
					(0.001)					(0.027)
First-stage *F-statistic*	7.535***	7.424***	7.363***	7.363***	7.335***	5.936**	6.036**	6.076**	6.064**	5.717**
R-squared	0.018	0.084	0.103	0.103	0.103	-0.079	-0.080	-0.075	-0.067	-0.091
Observations	177877	177877	177877	177876	176409	1717	1717	1717	1717	1676

Note: All specifications include sampling weights. The samples in columns 1-5 are children aged 6–17 in 1998 with a parent born between 1950 and 1965. Normal grade progression likelihood takes the value of 1 if a child attends the expected grade level or higher and 0 otherwise. The samples in columns 6–10 are children under 5 in 2004 with a parent born between 1950 and 1965. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

3.7 Robustness Checks

We check whether the key findings are robust to alternative measures of mortality rates and the age range of cohorts used to construct of the sex ratio for the parents' generation. We also restrict the children's sample to the oldest children born to parents in the 1950–1965 cohorts to examine whether the intergenerational effects of civil conflict on children born years after the genocide channel through a sibling effect.

3.7.1 Alternative Mortality Estimates

In this section, we re-estimate equations (3.1), (3.2), and (3.3) using an alternative measure of KR mortality rates constructed based on the population currently residing in various districts (KR mortality rates–current districts), instead of the population born in various districts. To do so, we divide the estimated number of deaths in a district by the sum of the estimated deaths under the KR and the number of individuals born before 1980, but currently residing in each district (instead of the district in which they were born). The major shortcomings of this alternative measure are that it includes individuals who moved from other districts and is influenced by migration during and after the period when the KR was in power. The results based on this alternative measure of KR mortality rates are reported in Tables 3.10 and 3.11. This alternative measure gives us stronger first-stage F-statistics, while the magnitude, sign, and significance of the estimated effects are comparable to the main results presented in Table 3.3 and Tables 3.5–3.9.

Table 3.10: Robustness: Reduced-form and IV estimates of children's educational and health outcomes using alternative mortality estimates

	Children of mothers born in 1950–1965 sample						**Children of fathers born in 1950–1965 sample**					
	Children's normal grade progression likelihood		Children's educational expenditure		Height-for-age Z-score		Children's normal grade progression likelihood		Children's educational expenditure		Height-for-age Z-score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Reduced-form estimates												
KR mortality rates—current district	-0.105***	-0.071***	-131.853**	-113.631***	-1.498**	-1.478**	-0.105**	-0.068***	-135.516**	-115.387**	-1.561**	-1.523**
	(0.040)	(0.025)	(51.064)	(42.911)	(0.666)	(0.662)	(0.040)	(0.025)	(58.355)	(47.436)	(0.725)	(0.697)
Own age and gender	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	-	-
Parental age and education	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-squared	0.032	0.104	0.042	0.088	0.005	0.007	0.035	0.110	0.038	0.088	0.005	0.018
Observations	193288	193288	7168	7168	1267	1267	177877	177877	7767	7767	1717	1717
IV estimates												
Sex ratio	0.535***	0.364***	747.064***	655.598***	9.234**	9.145**	0.523**	0.346***	728.523**	631.611**	9.082**	8.927**
	(0.207)	(0.132)	(274.903)	(238.921)	(4.499)	(4.501)	(0.205)	(0.127)	(293.879)	(248.913)	(4.039)	(4.013)
Own age and gender	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	-	-
Parental age and education	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
First-stage *F-statistic*	24.142***	23.824***	18.509***	18.007***	14.717***	14.428***	24.495***	24.162***	20.693***	20.975 ***	17.219***	17.543***
R-squared	0.022	0.101	0.055	0.096	-0.046	-0.043	0.025	0.107	0.055	0.099	-0.037	-0.028
Observations	193288	193288	7168	7168	1267	1267	177877	177877	7767	7767	1717	1717

Note: All specifications include sampling weights. KR mortality rates–current district measure the mortality rates under the KR regime based on the estimated deaths in the CGD and the number of individuals born before 1980 but currently residing in the district. The samples in columns 1, 2, 7 and 8 are children aged 6–17 in 1998 with a parent born between 1950 and 1965. Normal grade progression likelihood takes the value of 1 if a child attends the expected grade level or higher and 0 otherwise. The samples in columns 3, 4, 9 and 10 are children aged 6–17 in 2004 with a parent born between 1950 and 1965. Children's educational expenditure are the sum of school tuition fees, textbook and school supply costs, transportation and pocket money, and other school-related expenses (in thousand Cambodian riels). The samples in columns 5, 6, 11 and 12 are children under 5 in 2004 with a parent born between 1950 and 1965. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 3.11: Robustness: Estimates of effects of the sex ratio on marriage and fertility outcomes using alternative mortality estimates

	Census 1998								
	Married	Husband's age < Wife's age	Spousal age gap	Husband's education < Wife's education	Spousal education gap	Number of children ever born	Number of children surviving	Mother's age at childbirth	Mother's age at first marriage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Mother born in 1950–1965 sample									
Sex ratio	0.132	-0.314***	4.597***	-0.058	2.448***	-2.537*	-1.737		
	(0.096)	(0.106)	(1.121)	(0.069)	(0.814)	(1.472)	(1.075)		
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
First-stage *F-statistic*	26.509***	26.568***	26.568***	26.568***	26.568***	26.568***	26.379***		
R-squared	0.007	0.006	0.017	-0.0004	-0.003	0.099	0.080		
Observations	97812	69185	69185	69185	69185	69185	68123		
Panel B: Father born in 1950–1965 sample									
Sex ratio	-0.023	-0.378***	6.452***	-0.095	2.370***				
	(0.039)	(0.076)	(0.955)	(0.070)	(0.761)				
Age	Yes	Yes	Yes	Yes	Yes				
First-stage *F-statistic*	27.090***	28.224***	28.224***	28.224***	28.224***				
R-squared	0.002	0.012	0.029	-0.0000	0.008				
Observations	75683	68462	68462	68462	68462				
Panel C: Children aged 6–17 with a mother born in 1950–1965 sample									
Sex ratio								-3.197***	-2.578**
								(0.901)	(1.254)
Mother's education								Yes	Yes
First-stage *F-statistic*								23.955***	23.917***
R-squared								-0.001	-0.002
Observations								193288	192711

Note: All specifications include sampling weights. KR mortality rates–current district measure the mortality rates under the KR regime based on the estimated deaths in the CGD and the number of individuals born before 1980 but currently residing in the district. In Panel A, the samples are mothers born between 1950 and 1965. In Panel B, the samples are fathers born between 1950 and 1965. In Panel C, the samples are children aged 6–17 in 1998 with a parent born between 1950 and 1965. Mother's age at childbirth in both Census 1998 and CSES 2004 is a proxy variable which is equal to the mother's age minus the child's age. Mother's age at first marriage in Census 1998 is a proxy variable which is equal to the mother's age minus the age of the oldest child in the household minus 1. We assume that the oldest child living in the household is her oldest child because we have no information about children living outside the household. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

3.7.2 Alternative Birth Cohorts Used to Construct the Sex Ratio

We next examine the sensitivity of our main results to the range of birth cohorts used to construct the sex ratio of the parents' generation. We consider four alternative ranges of birth cohorts. The first sample includes the 1954–1965 birth cohorts (excluding the 1950–1953 birth cohorts). The second sample focuses on cohorts born between 1950 and 1960 (eliminating the 1961–1965 birth cohorts). We selected these alternative ranges because Figure 3.2 (as discussed in section 3.2.2) shows that the sex ratio drops sharply, reaching its lowest level with those born in 1954 and then stabilizing with those born in 1961 and later. The third sample includes the 1960–1965 birth cohorts and the fourth sample includes the 1960–1969 birth cohorts. One might argue that older cohorts might have already married before or during the KR regime; therefore, excluding the pre-1960 birth cohorts allows one to test whether the results are confounded by other non-marriage market channels.

The regression results are presented in Tables 3.12 and 3.13. We run separate regressions for each alternative birth cohort. We report both reduced-form and IV results for children's educational and health outcomes. The results have the same signs and statistical significance as the main findings for the effects on the likelihood of children's normal grade progression (columns 1 and 2 of Table 3.12). However, the estimated effects on children's educational expenditure are not significant (columns 3 and 4 of Table 3.12). In addition, the precision of the coefficient for height-for-age Z-scores in the IV estimates decrease, mainly due to the smaller sample size (columns 5 and 6 of Table 3.12). The estimated effects on children's health outcomes become statistically insignificant, but the magnitudes are comparable to the main results, except for the 1950–1960 birth cohorts whose sample size is less than half that used to estimate the main results.

The estimated effects of the sex ratio on marriage outcomes remain statistically significant across the samples, although the magnitudes are slightly smaller (Panel A of

Table 3.13). The estimated effects of the sex ratio on the number of children ever born and the number of children surviving remain statistically insignificant, as in the main results (Panel B of Table 3.13). The statistically significant results for the effects of the sex ratio on mother's age at childbirth for the 1954–1965 and 1950–1960 birth cohorts are marginally lower than the main results (columns 1 and 2 of Panel C of Table 3.13). The coefficients become statistically insignificant for the younger birth cohorts (columns 3 and 4 of Panel C of Table 3.13). In addition, the estimated effects of the sex ratio on mother's age at first marriage are statistically insignificant for all alternative birth cohorts, but the sign remains unchanged compared to the main results.

3.7.3 Sibling Effect

In this section, we investigate whether the intergenerational effects of the civil conflict on children born well after the conflict ended channel through a sibling effect. For instance, the health of older siblings born before or during the conflict could have been affected by direct exposure to conflict. If this is the case, parents might devote greater resources to older siblings, and this intra-household allocation of resources could affect younger siblings. To explore this issue, we restrict our sample to the oldest children born to the 1950–1965 cohorts. We have no information about children not living in the household, so we assume that the oldest child living in the household is the oldest child of the mother or father.

The reduced-form and IV estimates of the likelihood of children's normal grade progression for this restricted sample are reported in Table 3.14.[19] The effects of the KR mortality rates and the sex ratio on the likelihood of children's normal grade progression are larger than when using the total sample. Thus, our main findings are robust to the possibility of a sibling effect.

[19] We do not report the estimates for children's educational expenses and health outcomes because the CSES 2004 samples become too small for any meaningful analysis when we restrict the sample to the oldest child.

Table 3.12: Robustness: Reduced-form and IV estimates of children's educational and health outcomes using different birth cohorts to construct sex ratio of the parents' generation

	Mother Sample						Father Sample					
	Children's normal grade progression likelihood		Children's educational expenditure		Height-for-age Z-score		Children's normal grade progression likelihood		Children's educational expenditure		Height-for-age Z-score	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Born 1954–65												
RF: KR mortality rates	-0.078*	-0.053*	-76.137	-68.921	-1.466**	-1.464**	-0.075*	-0.051*	-76.286	-69.181	-1.257*	-1.304*
	(0.045)	(0.028)	(53.446)	(45.015)	(0.714)	(0.706)	(0.044)	(0.027)	(60.936)	(49.765)	(0.726)	(0.686)
IV: Sex ratio	0.658*	0.457*	894.192	818.937	14.109	14.169	0.629*	0.434*	817.654	742.742	12.537	13.064*
	(0.376)	(0.248)	(617.814)	(555.414)	(8.783)	(8.856)	(0.362)	(0.237)	(608.781)	(528.397)	(7.793)	(7.939)
First-stage *F-statistic*	7.374***	7.221***	3.216*	3.240*	5.052**	4.996**	7.328***	7.159***	3.674*	3.965**	5.141**	5.338**
Observations	158303	158303	6235	6235	1241	1241	140025	140025	6608	6608	1602	1602
Born 1950–1960												
RF: KR mortality rates	-0.076*	-0.050	-99.100	-91.514	-1.285	-1.319	-0.080	-0.052	-105.946	-107.463	-1.022	-0.941
	(0.045)	(0.030)	(74.226)	(62.378)	(1.228)	(1.219)	(0.049)	(0.031)	(84.825)	(69.062)	(0.847)	(0.836)
IV: Sex ratio	0.665*	0.439*	827.491	771.607	7.268	7.433	0.658*	0.439*	1010.771	1003.079	10.266	9.527
	(0.391)	(0.262)	(545.063)	(477.043)	(6.836)	(6.752)	(0.387)	(0.253)	(721.928)	(622.750)	(9.695)	(9.550)
First-stage *F-statistic*	7.103***	6.902***	6.170**	6.272**	10.967***	10.579***	7.508***	7.271***	4.556**	5.266**	4.333**	4.358**
Observations	124274	124274	3985	3985	411	411	116966	116966	4438	4438	693	693
Born 1960–1965												
RF: KR mortality rates	-0.072*	-0.050*	-64.919	-58.173	-1.392*	-1.354*	-0.067*	-0.047*	-60.083	-54.449	-1.636*	-1.689**
	(0.043)	(0.027)	(48.893)	(42.431)	(0.717)	(0.709)	(0.038)	(0.024)	(46.049)	(38.806)	(0.872)	(0.851)
IV: Sex ratio	0.610*	0.427*	790.499	731.830	14.919	14.746	0.556*	0.396*	548.681	498.440	13.924*	14.405*
	(0.359)	(0.242)	(615.550)	(582.469)	(10.147)	(10.214)	(0.325)	(0.223)	(406.481)	(357.996)	(7.311)	(7.502)
First-stage *F-statistic*	7.196***	7.077***	2.959*	2.857*	3.799 *	3.701*	7.606***	7.386***	4.969**	5.287**	6.597**	7.109***
Observations	85336	85336	3755	3755	980	980	76287	76287	4008	4008	1182	1182
Born 1960–1969												
RF: KR mortality rates	-0.076*	-0.056**	-57.727	-46.901	-1.039*	-1.034*	-0.065*	-0.047**	-63.260	-50.763	-1.023	-1.027*
	(0.042)	(0.026)	(49.494)	(41.435)	(0.546)	(0.538)	(0.037)	(0.023)	(40.622)	(33.818)	(0.625)	(0.610)
IV: Sex ratio	0.623*	0.469**	596.662	503.359	9.905	9.838	0.537*	0.391*	595.944	492.080	8.897	9.190
	(0.349)	(0.229)	(479.207)	(436.372)	(6.665)	(6.690)	(0.317)	(0.209)	(365.216)	(323.566)	(6.061)	(6.331)
First-stage *F-statistic*	7.556***	7.492***	4.314**	4.211**	5.715**	5.931**	7.633***	7.463***	4.945**	4.896**	7.121***	7.099***
Observations	120653	120653	6392	6392	1934	1934	102411	102411	6415	6415	2394	2394
Control variables:												
Own age and gender	Yes	Yes	Yes	Yes	-	-	Yes	Yes	Yes	Yes	-	-
Parental age and education	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: All specifications include sampling weights. The samples in columns 1, 2, 7, and 8 are children aged 6–17 in 1998. Normal grade progression likelihood takes the value of 1 if a child attends the expected grade level or higher and 0 otherwise. The samples in columns 3, 4, 9, and 10 are children aged 6–17 in 2004. Children's educational expenditure are the sum of school tuition fees, textbook and school supply costs, transportation and pocket money, and other school-related expenses (in thousand Cambodian riels). The samples in columns 5, 6, 11, and 12 are children under 5 in 2004. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 3.13: Robustness: Estimates of effects of the sex ratio on marriage and fertility outcomes using different birth cohorts to construct sex ratio of the parents' generation

	Census 1998							
	Mother Sample				**Father Sample**			
	Born 1954–1965	**Born 1950–1960**	**Born 1960–1965**	**Born 1960–1969**	**Born 1954–1965**	**Born 1950–1960**	**Born 1960–1965**	**Born 1960–1969**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Effects of the Sex Ratio on the Marriage Outcomes								
Dependent variables:								
Married	0.163	0.129	0.175	0.137	0.005	0.007	0.001	-0.153
	(0.130)	(0.173)	(0.142)	(0.148)	(0.073)	(0.051)	(0.077)	(0.145)
Husband's age < Wife's age	-0.381**	-0.413**	-0.363**	-0.401***	-0.448***	-0.432***	-0.410***	-0.435***
	(0.172)	(0.187)	(0.160)	(0.139)	(0.143)	(0.128)	(0.145)	(0.125)
Spousal age gap	4.641**	6.767**	3.803**	4.884**	6.687***	6.980***	6.292***	6.484***
	(1.860)	(2.769)	(1.911)	(2.020)	(1.741)	(1.894)	(1.686)	(1.516)
Husband's education < Wife's education	-0.052	-0.030	-0.077	-0.225	-0.159	-0.083	-0.203*	-0.217*
	(0.109)	(0.137)	(0.110)	(0.138)	(0.126)	(0.139)	(0.113)	(0.129)
Spousal education gap	2.069	2.736*	1.934	2.807**	2.201*	2.816**	2.083*	2.299*
	(1.258)	(1.405)	(1.379)	(1.351)	(1.261)	(1.340)	(1.107)	(1.321)
Observations	56543	42081	32574	56311	55775	39369	34776	59160
Panel B: Effects of the Sex Ratio on the Number of Children								
Dependent variables:								
Number of children ever born	-2.604	-3.430	-2.301	-2.511				
	(2.567)	(2.945)	(2.347)	(2.062)				
Observations	56543	42081	32574	56311				
Number of children surviving	-1.628	-2.304	-0.919	-1.461				
	(1.828)	(2.142)	(1.640)	(1.391)				
Observations	55650	41529	31976	54842				
Panel C: Effects of the Sex Ratio on Mother's Age at Childbirth and at First Marriage								
Dependent variables:								
Mother's age at childbirth	-2.652*	-3.535**	-1.321	-0.839				
	(1.366)	(1.444)	(0.996)	(1.116)				
Observations	158303	124274	85336	120653				
Mother's age at first marriage	-3.408	-3.565	-2.668	-1.904				
	(2.226)	(2.241)	(2.217)	(1.838)				
Observations	157869	123900	85098	120121				

Note: We report the estimated coefficient for the sex ratio. All specifications include sampling weights. In Panel A and B, regressions are controlled for individual's own age. In Panel C, the samples are children aged 6–17 in 1998 and regressions are controlled for the mother's education. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 3.14: Robustness: Reduced-form and IV estimates of normal grade progression likelihood of the oldest children born after 1980

	Census 1998							
	The oldest children of mothers born in 1950–1965 sample				The oldest children of fathers born in 1950–1965 sample			
	RF	RF	IV	IV	RF	RF	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
KR mortality rates	-0.091**	-0.063**			-0.098**	-0.069**		
	(0.044)	(0.027)			(0.048)	(0.028)		
Sex ratio			0.729**	0.516**			0.763**	0.550**
			(0.371)	(0.242)			(0.379)	(0.238)
Age	-0.014***	-0.015***	-0.013***	-0.014***	-0.017***	-0.016***	-0.015***	-0.015***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.008***	0.008***	0.010***	0.009***	0.003	0.002	0.004	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Mother’s age		0.001		0.001**		0.001*		0.002**
		(0.0004)		(0.001)		(0.0004)		(0.001)
Mother’s education		0.017***		0.017***		0.019***		0.018***
		(0.001)		(0.001)		(0.001)		(0.001)
Father’s age		0.001**		0.0001		0.004***		0.002**
		(0.0002)		(0.0003)		(0.001)		(0.001)
Father’s education		0.016***		0.016***		0.016***		0.016***
		(0.002)		(0.001)		(0.001)		(0.001)
First-stage F stat.			8.090***	7.856***			8.372***	8.134***
R-squared	0.024	0.123	-0.012	0.107	0.037	0.142	-0.001	0.123
Observations	42903	42903	42903	42903	46519	46519	46519	46519

Note: All specifications include sampling weights. The samples are the oldest children aged 6–17 in 1998 with a parent born between 1950 and 1965. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

3.8 Conclusion

This chapter examines the effects of the genocide that occurred in Cambodia during the KR regime on the health and educational outcomes of children born years after the regime fell. We demonstrate that genocide-induced mortality exogenously lowers sex ratios for the 1950–1965 birth cohorts across districts in Cambodia and leads to lower normal grade progression rates and height-for-age Z scores among children born to these cohorts. Since the geographical variation in mortality rates under the KR regime is not correlated with various determinants of children's educational and health outcomes, such as parental educational attainment, earnings, and health, our results suggest that the intergenerational effects of the genocide are primarily mediated through its gender-differentiated mortality effect on the parents' generation.

Although it is difficult to pin down precisely what aspects of the sex ratio channel drive the intergenerational effects, we further explore the role of the disruption to the marriage market during the parents' generation. We show that a lower sex ratio increases the likelihood of marriages of women to younger men, lowers spousal age and education gaps, and increases mother's age at birth (of the children studied) and mother's age at first marriage. The lower sex ratio, though, has no effect on completed fertility. Once we control for the educational attainment of both parents, the intergenerational effects of the genocide lessens, suggesting that the marriage market serves as an important channel. However, the ages at which the mother gives birth and first marries are unlikely to be the channel through which the genocide affects children's outcomes.

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Appendix to Chapter 3

Table A3.1: Estimates of effects of the sex ratio on children's outcomes controlling for fathers having difficulty moving

	CSES 2004: Children of fathers born in 1950–1965 sample		
	Children's educational expenditure	Height-for-age Z-score	Weight-for-age Z-score
	(1)	(2)	(3)
Explanatory variables:			
Sex ratio	831.656	12.129*	3.211
	(592.912)	(7.158)	(4.321)
Father having difficulty moving	-31.045*	-0.817	-0.189
	(17.105)	(0.560)	(0.516)
Age	15.560***		
	(2.216)		
Male	-4.748		
	(3.975)		
First-stage *F-statistic*	4.404**	6.053**	6.053**
R-squared	0.048	-0.071	-0.004
Observations	7767	1717	1717

Note: All specifications include sampling weights. The sample in column 1 is children aged 6–17 in 2004 with a father born between 1950 and 1965. The samples in columns 2 and 3 are children under 5 in 2004 with a father born between 1950 and 1965. Robust standard errors clustered by district are in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Chapter 4: The Long-term Effects of Genocide on Social Preferences and Risk

4.1 Introduction

There is a growing body of literature on the social costs of civil conflicts. Most studies have focused on the impact of conflict exposure on adults shortly after the end of conflicts, and to date, the evidence has been inconclusive. Some studies show that individuals more affected by violent conflicts tend to exhibit pro-social behavior, such as trust, altruism (Voors et al., 2012; Whitt & Wilson, 2007; Gilligan, Pasquale, & Samii, 2014), and egalitarianism (Bauer, Cassar, Chytilová, & Henrich, 2014), and to be more socially and politically engaged (Blattman, 2009; Bellows & Miguel, 2009; Gilligan et al., 2014). Cassar, Grosjean, and Whitt (2013) and Rohner, Thoenig, and Zilibotti (2013), on the other hand, highlight the negative consequences of exposure to conflict on trust, fairness, and willingness to engage in impersonal exchanges. Risk preferences among those affected by war have also been examined (Voors et al., 2012; Callen, Isaqzadeh, Long, & Sprenger, 2014). What is yet to be established is whether the impact of exposure to conflict on these behaviors persists in the long term. Additionally, none of the studies have examined whether exposure to conflict influences anti-social behavior.

Violent and near-death experiences can alter the preferences of violent conflict survivors and affect long-term economic development. For example, the difficulties experienced during war could influence neighbors to band together and create institutions that promote trust and cooperation in order to defend themselves and jointly cope with the negative consequences of war (Gilligan et al., 2014). However, the psychological literature shows that victimization and other traumatic experiences can also have devastating effects on survivors' basic psychological needs for trust, esteem, identity, and positive connections to others (Staub, Pearlman, Gubin, & Hagengimana, 2005) and can induce self-protective behavior in survivors (Weinstein, 1989). Restoring the reduced stock of social capital takes time. Exposure to violent conflicts also might lead to anti-social behaviors (e.g., dishonesty, vindictive or destructive behavior), which can affect a wide range of economic outcomes. When destructive envy is not restrained, individuals who innovate and have an entrepreneurial spirit are stigmatized and sometimes punished by others, which impedes economic development (Grolleau, Mzoughi, & Sutan, 2009). Similarly, dishonesty encourages corruption and financial fraud and discourages economic exchanges. Once a reputation for honesty is lost, the incentive for honest behavior in the future is greatly lessened (Collier et al., 2003). Exposure to war also influences human emotions, which, in turn, affects people's risk evaluations (Lerner & Keltner, 2001; Callen et al., 2014). The traumatic experience of violence can provoke feelings of anger or fear. Feelings of anger are more likely to lead people to make optimistic risk evaluations and be more prone to choosing risky options. In contrast, feelings of fear are more likely to cause people to avoid taking risky options.

In this study, we conduct an artefactual field experiment to examine the effects of direct exposure to genocide during childhood and early adolescence on pro- and anti-social behavior and risk preferences in adulthood. We use exposure to the Cambodian genocide under the Khmer Rouge (KR) regime (1975–1979) to address our research questions. The KR regime was one of the worst in human history, causing the deaths of nearly 2 million people across Cambodia and leaving millions more traumatized. We focus on genocide exposure during childhood and early adolescence because a number of experimental studies show that individuals' social preferences develop over the course of childhood and adolescence (Eckel et al., 2011; Fehr, Glatzle-Rutzler, & Sutter, 2013; Harbaugh, Krause, & Vesterlund, 2002; Sutter & Kocher, 2007; Sutter, Kocher, Glatzle-Ruetzler, & Trautmann, 2013). We conduct trust, altruism, risk, money burning, and self-reporting games among those exposed and not exposed to genocide. The exposed group includes individuals born before the genocide (1960–1974 birth cohorts), while the non-exposed group includes those born during and after the genocide (1975–1982 birth cohorts).[20] We also use different birth cohorts in the exposed and non-exposed groups to examine the sensitivity of the results to alternative ways of defining birth cohorts.

We find strong support for pro-social behavior and risk preferences. We observe significantly less trusting, less altruistic, and more risk-averse preferences among individuals exposed to the genocide. There is, however, little evidence that exposure to genocide during childhood and early adolescence leads to dishonest and vindictive behaviors in adulthood. We corroborate the findings from the experiment using survey data and personality traits questionnaires. The survey results show that being exposed to genocide results in individuals having lower levels of trust in family, neighbors, and friends and lower values for extraversion and agreeableness.

The main findings from the experiment are robust across a variety of sensitivity checks and specifications. In particular, we demonstrate that our results are not affected by the differences in the observed characteristics, such as age, education, and geographic location, of the exposed and non-exposed cohorts. More importantly, our results are robust when using alternative birth cohorts in the exposed and non-exposed groups and when taking into account individuals' personality. We also find that living in the same district or location

[20] Although the non-exposed group includes individuals who were born during the genocide period, the literature on childhood amnesia and autobiographical memory development indicates that children have very little memory of experiences that occurred before the age of 2 and few memories of events that occurred between the age of 2 and 3 years old (Howe, 2013).

since birth has no effect on behavioral differences between the exposed and non-exposed groups. Finally, we show that our experimental outcomes are not driven by variation in the incidences of violence-related experiences or witnessing of violent acts under the KR regime self-reported by the exposed group and individuals born during the KR regime.

This study builds on recent evidence that civil conflicts can have adverse effects on the social behavior of affected individuals and contributes to the literature in a number of ways. First, whereas the existing literature focuses on recent civil conflicts, this study of the Cambodian genocide enables examining the long-term (30 years after the war ended) impact on social behavior and risk. Second, despite diverse evidence on the link between civil conflicts and pro-social behavior and risk preferences, little is known about anti-social preferences which result from direct exposure to civil conflict. Our findings contribute to the literature by showing that exposure to civil conflict during childhood and early adolescence do not lead to dishonesty and vindictive behavior in adulthood. Finally, we examine the relationship between exposure to civil conflict and personality traits. Although the psychological literature shows that childhood traumatic experiences, such as sexual, emotional, and physical abuse, influences individuals' personality (Allen & Lauterbach, 2007; Roys & Timms, 1997), little is known about the effect of exposure to civil conflict on personality traits.

4.2 Study Sample

4.2.1 Selection of Sample

We conducted the experiment in February 2014 in Phnom Penh, Cambodia's capital city, and in six districts in the Kampong Cham province of Cambodia.[21] The districts were

[21] Kampong Cham is one of the five largest provinces in Cambodia by population and is 123 km from Phnom Penh.

selected randomly from a list of districts in the Cambodian Genocide Database (CGD), which includes a district identifier for each KR mass gravesite and the estimated number of bodies in each mass grave.[22] Violence during the KR regime might have caused individuals with stronger pro-social attitudes and risk-seeking preferences to be killed at a higher rate than those who have weaker pro-social attitudes and are more risk averse. To account for this potential sample selection, using the CGD, we randomly select some districts in the Kampong Cham province with high mortality rates and other districts with low mortality rates (as shown in Figure 4.1).

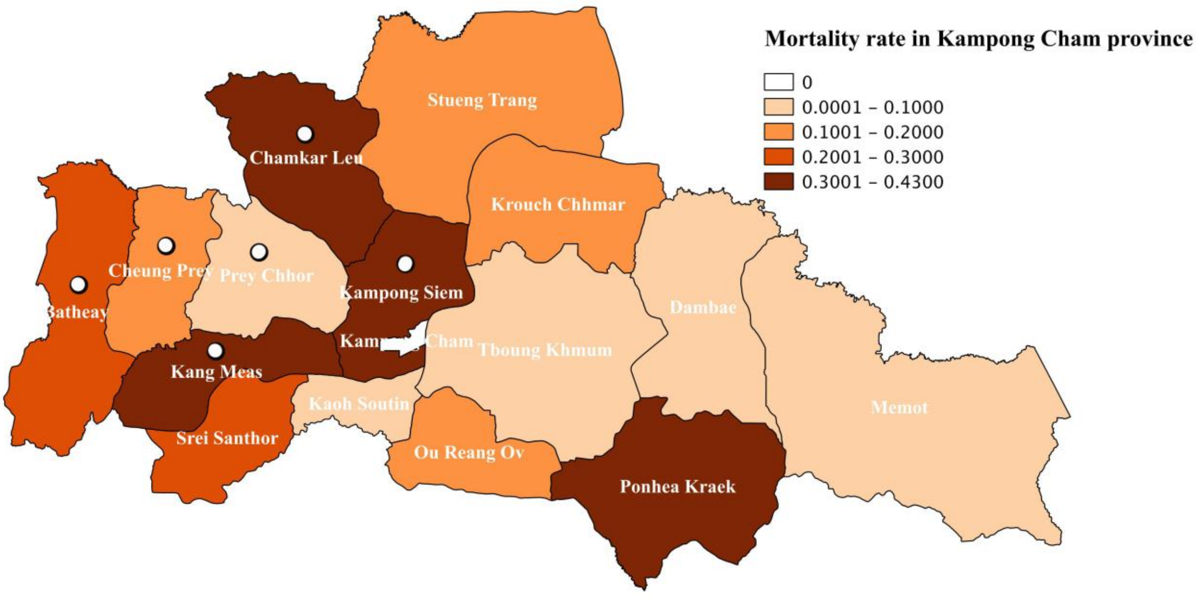

Figure 4.1: Selected districts

Note: White dots denote the selected districts: Kang Meas, Cheung Prey, Kampong Siem, Chamkar Leu, Batheay, and Prey Chhor.

[22] The database was developed by Yale University and has been updated by the Documentation Center of Cambodia (DC-Cam). We use both information from the original Yale database and data on additional mass gravesites and estimates of the number of deaths from 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.

In Phnom Penh, we recruited participants by posting flyers at local coffee shops, on the bulletin boards of local and international organizations, and on social media. We also randomly approached people on the street. In Kampong Cham, local research assistants and school principals assisted in recruitment. In all locations, participants were required to meet the age criterion (born between 1960 and 1982). In addition, we aimed for a gender balance and some diversity in socio-economic backgrounds. During the recruitment process, participants were not made aware that the focus of our study is the effects of genocide.

4.2.2 Exposed vs. non-exposed Group

A total of 492 adults born between 1960 and 1982 participated in the experiment. We divide the sample into two groups: 1) the exposed group (born between 1960 and 1974); and 2) the non-exposed group (born between 1975 and 1982). The 1960–1974 cohorts, who were between 1 and 15 years old when the KR regime came to power in 1975, were heavily exposed to the genocide during their childhood and early adolescence and were most likely to experience and witness violent acts (e.g., killing, torture) or see the results of violent acts (e.g., dead bodies, bombed buildings).

The 1975–1979 cohorts, who were born during the KR regime, are included in the non-exposed group. Although they were born during the KR regime and thus exposed to at most four years of the genocide, they were too young to remember their experience under the KR regime and are less likely to have experience and witnessed violent acts. They are more likely to have learned about the KR regime, their own experiences, and the exposure of their family members through social interactions in the family and community, which could indirectly affect their emotions and behavior. They are also more likely to have experienced nonviolent trauma, such as homelessness and starvation. Hence, we consider individuals born between 1975 and 1979 to have likely developed their social preferences after the KR regime ended. For similar reasons, individuals born after the KR regime (1980–1982) are included in the non-exposed group.

A potential concern that could affect the definition of the exposed and non-exposed groups is related to adult memories of early childhood. The literature on childhood amnesia and autobiographical memory development suggests that the age estimates of earliest childhood memories are generally accurate, with an average age estimate of 3.5 among adults. Howe (2013) finds that people have very little memory of experiences that occurred before the age 2 and few memories of events that occurred between the ages of 2 and 3. The number of retained memories of events increases with individuals' age at the time of the event and mature levels of memories start forming from around age 7 (Howe, 2013). Wang and Peterson (2014), though, argue that people's earliest memories might develop earlier than the first 3 to 4 years of life. For this reason, we recruited about 60% of total subjects in exposed group and 40% of subjects in non-exposed group.[23] In the sensitivity analysis section, we also analyze the sensitivity of the main results using different birth cohorts in the exposed and non-exposed groups.

Table 4.1 reports summary statistics of experiment participants' demographic characteristics and t-tests of means between the exposed and non-exposed groups. A broad balance is achieved across a variety of demographics, including gender, marital status, and ethnicity. The exposed group (n = 296) and the non-exposed group (n = 196) do not differ in background demographics, except for age and education, as shown in column 6. The average ages of the exposed group and the non-exposed group are approximately 47 and 35, respectively (p = 0.000). The average years of schooling of the exposed group and the non-exposed group is 6.1 and 9.2 years, respectively (p = 0.000). It is reasonable that the younger cohorts (non-exposed group) have more education than the older cohorts (exposed

[23] Twenty percent of the non-exposed group born during the KR and other 20% born after the KR.

group). Our sample is nearly perfectly gender balanced. Men account for 47% of the exposed group and 44% of the non-exposed group (p = 0.576). The percentage of married participants is approximately 85% in the exposed and 82% in the non-exposed group (p = 0.436). The majority of participants in the experiment are from the Khmer ethnic group. Only a small number of Cambodian Muslims participated in the experiment.

We also test the representativeness of the sample by comparing the demographic characteristics of the experiment sample to data from the 2011 Cambodia Socio-economic Survey (CSES 2011). Except for the slightly higher education levels in the non-exposed group, our sample is reasonably representative (columns 7 and 8 of Table 4.1). With respect to marital status, there is little difference between our experiment sample and the CSES 2011 data.

Table 4.1: Demographic characteristics of experiment participants

Main Variables	**Experimental sample**						**CSES 2011**	
	All	*SD*	Range	Exposed group	Non-exposed group	T-test of: (*p*-value)	Exposed group	Non-exposed group
	(1)	(2)	(3)	(4)	(5)	(4) – (5)	(7)	(8)
Age	42.31	7.290	32-54	47.30	34.77	0.000	43.40	31.42
Male (=1)	0.46	0.499	0-1	0.47	0.44	0.576	0.46	0.48
Education (years)	7.32	4.460	0-22	6.13	9.15	0.000	6.85	7.49
Married (=1)	0.84	0.369	0-1	0.85	0.82	0.436	0.86	0.78
Khmer (=1)	0.99	0.078	0-1	0.99	1.00	0.158		
Observations	492			296	196		2,699	1,902

Note: The exposed group consists of individuals born between 1960 and 1974 and the non-exposed group of individuals born between 1975 and 1982.

4.3 Experiment and Survey Design

Our research design includes conducting five experimental games, employing attitudinal survey questions, and assessing personality traits.

4.3.1 Experimental Games

To measure pro- and anti-social behaviors and risk preferences, we conducted the trust, dictator, risk, money burning, and self-reporting games. The trust game elicits the degree to which participants can trust one another and the extent of their trustworthiness. The dictator game giving is designed to measure the extent of altruism among participants, which might be an indicator of concern for the well-being of others, instead of self-interest. The dictator game giving or taking likely is an indicator of opportunism and selfishness. The risk game measures risk-taking behavior. The money burning game is designed to understand participants' inclination to destroy others' resources or well-being at a cost to themselves. The self-reporting game can be interpreted as an indicator of dishonesty.

We set different endowment and participation fees for Phnom Penh and the rural areas in Kampong Cham province. In Phnom Penh, the endowment and participation fees are twice those of the rural areas. The average earnings of workers in Phnom Penh are approximately twice those of workers in rural areas, based on CSES 2011. The following sections describe the experimental games with the endowment and participation fees used in the rural areas. One of the first four tasks was randomly chosen for payment purposes, in addition to the earnings in the self-reporting task.

Game 1: Trust

We use the standard trust game protocol to measure trust and trustworthiness. Each participant plays both as player 1 (sender) and player 2 (receiver). The sender receives an endowment of 10,000 riel (AUD 2.8)[24], while the receiver is endowed with 0 riel. In the first stage, all participants are senders and can send any positive amount $x \in \{0, 1{,}000, 2{,}000, \ldots, 9{,}000, 10{,}000\}$ to the anonymous receiver, knowing that the experimenter triples the amount sent and that the receiver receives an amount of $3x$. In the second stage, all participants play as receivers. We aimed to minimize the logistical issues in the field, so the receiver was not informed of the amount sent by the sender. Thus, the receiver decides on an amount $y \in \{3x\}$ to return to the sender for all the corresponding amounts the receiver might receive. The sender is also not informed of the amount sent back by the receiver unless the game is selected for the final payment. If the trust game and the sender's role are selected for the final payment, all participants receive $(10{,}000 - x + y)$. Otherwise, all participants receive $(3x - y)$ if the receiver's role is chosen.

Game 2: Dictator

In the dictator game, there were two stages: 1) giving; and 2) giving or taking. Each participant plays as both player 1 (dictator) and player 2 (recipient). All participants receive an endowment of 10,000 riel. The dictator receives an additional endowment of 10,000 riel, while the recipient does not receive the additional endowment.

1) *In dictator game giving*, the dictator is asked to decide how much of the *additional* endowment to give to the recipient. The dictator can transfer any positive amount $x \in \{0, 1{,}000, 2{,}000, \ldots, 9{,}000, 10{,}000\}$ to the anonymous recipient. The recipient must simply accept it and is only informed of how much the dictator sends if the game is selected for the

[24] The exchange rate was AUD 1 = 3,570Riel (February 23, 2014).

final payment. All participants receive (10,000 + 10,000 – x) if the dictator game, part 1, and the dictator's role are selected for the final payment. However, if the dictator game, part 1, and the recipient's role are chosen, the payoffs for all participants are (10,000 + x).

2) *In dictator game giving or taking*, the dictator can send the *additional* endowment to other players or take other player's initial endowment. This means that the dictator can send a negative or positive amount $x \in \{-1,000, -2,000, \ldots, -10,000, 0, 1,000, 2,000, \ldots, 10,000\}$. As in part 1, the recipient is only told the amount the dictator sends or takes if the game is selected for the final payment. The payoffs of all participants are (10,000 + 10,000 –/+ x) depending on the dictator's decision to take or give if the dictator game, part 2, and the dictator's role are selected and (10,000 –/+ x) if the dictator game, part 2, and the recipient's role are selected.

Game 3: Risk

We use a simple risk game which involves a 50% chance of winning or losing. Each participant receives 10,000 riel and can invest any positive amount $x \in \{0, 1,000, 2,000, \ldots, 9,000, 10,000\}$ in a risky business. The investment yields triple the amount invested with 50% probability and 0 with 50% probability. The outcome is decided by tossing a coin. If the coin shows heads, the investment is successful, and all participants gain (10,000 – x + $3x$). If the coin shows tails, the payoff for all players is (10,000 – x + 0).

Game 4: Money Burning

In the money burning game introduced by Zizzo and Oswald (2001), each player is given an opportunity to pay a fee to reduce the income of the other player. We use a simpler two player version of this game.

All participants receive 20,000 riel (AUD 5.6). Half of them receive an additional amount called a gift. Those who have odd identification (ID) numbers receive a gift of 5,000 riel (AUD 1.4), and those with even ID numbers do not receive any gift. The gift is known to all participants. Participants simultaneously decide how much of the other player's total endowment to eliminate. Participants can also choose not to eliminate any of the other's endowment. Participants have to pay from their own endowment to eliminate the other player's endowment. The fee incurred for eliminating other's endowment is charged at three levels: 5%, 10%, and 20% of the amount a player wants to eliminate of the other player's endowment. We study different costs of eliminating to test whether the cost has any influence on an individual's behavior. In the payment stage, if this game is chosen, the odd-numbered participants choose an even-numbered partner by randomly selecting an even ID number, and vice versa. With this game we aim to understand individual's propensity to destroy others' resources at a cost to themselves in a scenario with unequal endowments.

Game 5: Self-reporting

In the self-reporting game, we aim to measure dishonesty using an individual-level decision-making environment. We design a simple self-reporting task with pictures instead of games with numerical or word tasks to accommodate the low literacy level in Cambodia. The game involves finding the picture of a star from a sheet of 10 tables which each have 9 images (see appendix B). Each participant is given an envelope containing a sheet of 10 tables and is instructed to find the stars within 1 minute.

To ensure that considerable and different opportunities for cheating, not all of the 10 tables have a star. We design 2 different sheets: a sheet with 7 stars in the 10 tables and a sheet with only 4 stars in the 10 tables. These maximum numbers are not known to the participants. The maximum number of 4 or 7 stars for each sheet allows considerable scope for cheating, even for top performers. In rural areas, participant can earn 1,000 riel (AUD 0.28)[25] for each star found. Participants record the total number of stars they find at the end of the sheet, place the sheet back into the envelope, and pay themselves for this task from a small envelope containing ten 1,000 riel notes given to them at the beginning of the task. Participants place any remaining money in the small envelope, seal it, and leave it on their desk for the experimenters to collect. The envelopes are not opened until the experimental session is completed. To reduce scrutiny bias, the experimenters leave the room while participants perform this task.

4.3.2 Experimental Procedure

All the tasks were conducted with paper and pen. Clear instructions with tables and diagrams in Khmer were provided to all participants. Before starting the experiment, participants were randomly assigned to 1 of 3 separate rooms in a local school.[26] On average, there were 24 participants per session. One session was smaller (14 participants) because of the small size of the room available and 3 sessions were larger (30–32 participants). We ran 3 sessions (rooms) simultaneously in Phnom Penh and in each district in Kampong Cham to reduce the spillover effects between sessions.

Participants played with other participants in the same session. They were informed that their partner was another participant in the session and selected their partner during the payment stage by choosing the ID number of another participant in the same session. Participants were also informed that they would be paid for 1 of the first 4 tasks picked at random, plus their earnings in task 5 and participation fees of 20,000 riel (AUD 5.6) in Phnom Penh and 10,000 riel (AUD 2.8) in the rural areas. At the end of the entire

[25] It was 2,000 riel (AUD 0.56) in Phnom Penh.
[26] In Phnom Penh, we conducted the experiment at a research institute: the Cambodia Development Resource Institute.

experiment, the experimenter rolled a dice in front of the participants to determine for which game participants were paid. Participants did not receive any feedback between the tasks or on the tasks not chosen for the final payment.

The games were conducted in two different orders. Game order 1 followed the sequence of trust, dictator, risk, money burning, and self-reporting games. This order was followed in odd-numbered districts (4 of 7 districts). In game order 2, we used the sequence of risk, money burning, trust, dictator, and self-reporting games in even-numbered district (3 districts). We altered the order of the games mainly to test whether participating in the anti-social games first might influence participants' behavior differently.[27] Participants had to make decisions in a booth during each game, except for the self-reporting game. An experimenter was in the booth to assist participants if they could not read or write.

4.3.3 Survey and Personality Traits Questions

After completing all the experimental games, participants were asked to complete a survey questionnaire. The survey covers information about participants' personal characteristics and experiences during the KR period and includes some attitudinal questions and 10 questions related to personality traits. Participants with limited reading and writing ability were interviewed.

The survey asks attitudinal questions related to self-reported trust, risk taking, and dishonest behavior.[28] The trust question focuses on trust in family members, neighbors, and friends, so it is comparable with the experimental setting in which all games were performed

[27] Our results are robust when we control for game order instead of experimental district fixed effects. The self-reporting task was always conducted last as participants paid themselves in this task and the amount they earned in this task could potentially influence their decisions in other tasks if this task were conducted before the others.

[28] We do not use a trust question from the General Social Survey: *"Generally speaking, would you say that most people can be trusted, or that you can't be too careful in dealing with people?*" As mentioned by Knack and Keefer (1997), this question leaves it somewhat ambiguous to what "people" respondents have in mind.

with participants from the same communities. The survey-based question "*trust in family members, neighbors and friends"* is scored from 1 to 5, with higher scores indicating more trust. We also use a survey question designed by Glaeser, Laibson, Scheinkman, and Soutter (2000) to measure self-reported past trusting behavior. We form an index of *past trusting behavior* by summing the scores of 2 survey questions: *"How frequently do participants: 1) lend personal possessions to friend; and 2) lend money to friends?*" The maximum possible value for this index is 10, with higher scores indicating a higher frequency of lending.

The survey also asks participants to score *"How willing are you to take risks regarding your household finances?"* (risk taking regarding household finances), with a maximum value of 5; higher scores indicate more risk taking. Similarly, participants evaluate themselves on the question *"How honest do you consider yourself?"* (being honest). The maximum possible score for this variable is 5, with higher scores indicating that participants are more honest.

We use a short version of the Big Five Inventory-10 developed by Rammstedt and John (2007), which contains 10 questions designed to categorize people in terms of 5 main factors: extraversion, agreeableness, conscientiousness, neuroticism, and openness. Broadly, extraversion reflects sociability, assertiveness, and positive emotionality. Agreeableness reflects altruism and the tendency toward cooperation, maintenance of social harmony, and consideration of the concerns of others. Conscientiousness describes traits related to self-discipline, organization, and self-control. Neuroticism refers to the tendency to experience negative emotion, including anger and emotional ability. Openness reflects imagination, creativity, and intellectual curiosity.

4.4 Estimation

We examine the effects of exposure to genocide on behavioral differences between the exposed and non-exposed individuals. To control for any socio-economic differences between the exposed and non-exposed cohorts, we use a regression framework to account for covariates, such as differences in age, education, and location of residence. We estimate the differences in behavioral outcomes in the experiments, survey outcomes, and Big Five factors of personality traits using equation (4.1):

$$Y_{ij} = \beta_0 + \beta_1 Exposed_i + \beta_2 \boldsymbol{X}_{ij} + \gamma_j + \varepsilon_{ij} \qquad (4.1)$$

where the outcome variable Y_{ij} includes the behavioral outcomes in the experiments, the outcomes in the attitudinal survey, and the Big Five factors for individual *i* in district *j*. The dummy variable $Exposed_i$ takes the value of 1 if the individual i was exposed to genocide and the value of 0 if individual i was not exposed. A set of control variables $\boldsymbol{X}_{ij}$ includes age, gender, and education for individual *i* in district j. γ_j is a set of district fixed effects. ε_{ij} is the error term. We include district fixed effects in the estimation strategy to account for variation between districts because we selected districts with low and high KR mortality rates.[29]

We address a number of potential concerns to show that the main results are not driven by other differences between the exposed and non-exposed cohorts. First, we define $Exposed_i$ based on individuals' birth year, as discussed in section 4.2.2, so age could be a determinant of behavioral differences between the exposed and non-exposed groups. We account for such differences in equation (4.1) by using age as a control. In addition, in the sensitivity analysis section, we examine whether age is a determinant of behavioral

[29] We also estimated specifications that include a set of session fixed effects instead of district fixed effects. The results are similar and available upon request.

outcomes in the experiments within the exposed and non-exposed samples. Second, we examine whether the results are robust to the exclusion of the participants' completed number of years of schooling since education is likely a potential channel for the effects of civil conflict exposure on individual preferences. Third, adult memories of early childhood could also affect the definitions of the exposed and non-exposed groups. To explore the robustness of the main results, we use alternative definitions of cohorts to exclude some birth cohorts that are likely to be sensitive to early childhood memories. Fourth, we control for individuals' characteristics of living in the same district or location since birth to show that the behavioral differences between the exposed and non-exposed groups are not sensitive to displacement during or after the KR regime. Fifth, we add personality traits as additional controls in equation (4.1) because individual personality likely is correlated with social behavior and risk preferences. Finally, we test the effects of variation in experiencing violence or seeing violent acts under the KR regime, for individuals exposed to the genocide and born during the genocide, on the experimental outcomes.

4.5 Results

4.5.1 Summary Statistics

Figures 4.2, 4.3, and 4.4 present the mean differences between the exposed and non-exposed groups in the behavior observed in the experimental games, survey outcomes, and personality traits, respectively. Figure 4.2 shows that the exposed group exhibits lower pro-social preferences, higher levels of risk aversion, and more dishonest preferences than the non-exposed group. In the trust game, individuals exposed to genocide send on average less of their endowment to the other players (27.7% versus 35.8%; p = 0.002) and return less (28.4% versus 34.7%; p = 0.003) than those not exposed. The exposed group also shares less of the endowment in the giving part of the dictator game, compared to the non-exposed group (21.7% versus 28.3%; p = 0.004). In the dictator game giving or taking, each individual has 3 options: (1) take some or all of the other player's endowment; (2) do not give to or take from the other player; and (3) give some or all of one's endowment to the other player. Overall, 36% of the exposed group (105 out of 296) chose option 1, compared to 34% of individuals (67 out of 196) in the non-exposed group. The figures are similar for options 2: 29.7% for the exposed group and 27.6% for the non-exposed group. But, 34.8% of the exposed group chose option 3, compared to 38.3% of the non-exposed group. We define a simple binary variable as equal to 1 if the individual takes some or all of the other player's endowment (option 1); otherwise, it is equal to 0. Figure 4.2 shows that exposed individuals are more likely to take than individuals in the non-exposed group; however, the difference is not statistically significant.

Regarding risk-taking behavior, individuals exposed to genocide invest less than non-exposed individuals (42.9% versus 51.8%; p = 0.001). Figure 4.2 also shows the differences in the means in the anti-social experiments. In the money burning game, we use 3 measures to describe the burning decision: (1) decision to reduce (burn) the other player's money for at least 1 of the 3 prices (costs) of burning (5%, 10%, and 20%); (2) decision to reduce the other player's money for at least 2 prices of burning; and (3) decision to reduce other player's money for all 3 prices of burning. More participants in the exposed group than the non-exposed group choose to eliminate the other player's money for at least 1 price and for at least 2 prices. But the exposed group is less likely to eliminate other player's money at all 3 prices compared to the non-exposed group. However, the differences are not statistically significant for all measures of the burning decision. In the self-reporting game, 35% of the exposed group takes extra money, compared to 24% of the non-exposed group. This difference has strong statistical significance (p = 0.007), suggesting that exposure to genocide can lead to dishonest behavior.

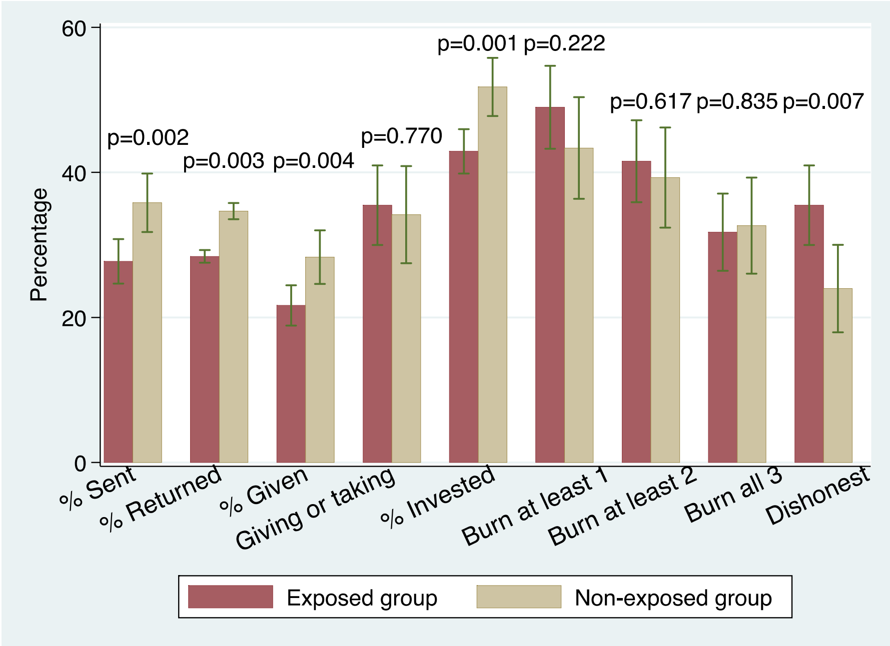

Figure 4.2: Mean differences of experimental outcomes

Note: The exposed group includes individuals born between 1960 and 1974 and the non-exposed group includes individuals born between 1975 and 1982. The giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. The burn at least 1 variable equals 1 if the player decides to reduce (burn) the other player's money for at least 1 of the 3 prices and 0 if player decides not to burn any money. The burn at least 2 variable equals 1 if the player decides to reduce the other player's money for at least 2 prices and 0 if player decides not to burn any money. The burn all 3 variable equals 1 if the player decides to reduce the other player's money for all 3 prices and 0 if player decides not to burn any money. The dishonest variable equals 1 if the player takes extra money to which he or she is not entitled and 0 otherwise.

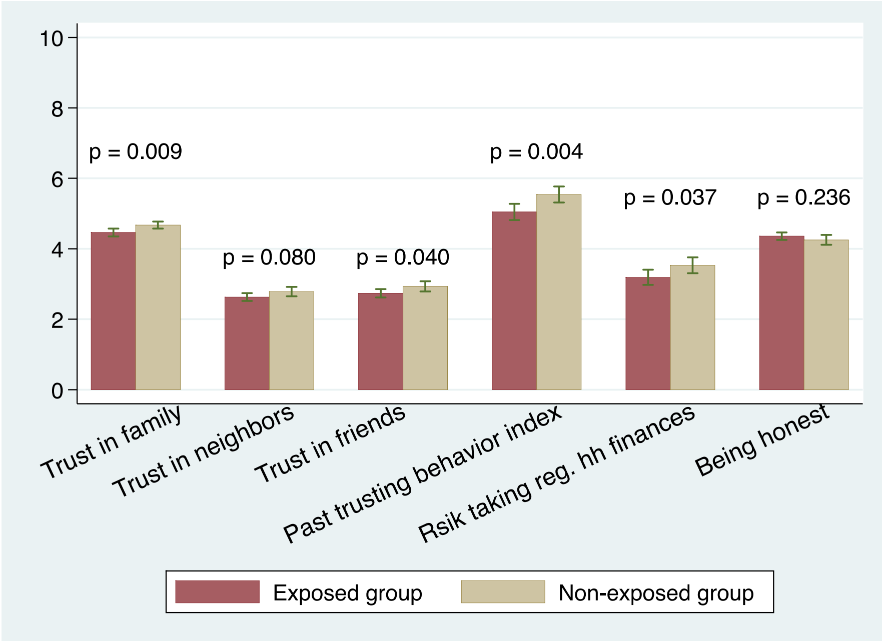

Figure 4.3: Mean differences in the survey outcomes

Note: The exposed group includes individuals born between 1960 and 1974 and the non-exposed group includes individuals born between 1975 and 1982. Trust in family, neighbors, and friends are the self-reported question of "how much trust do you have in your own family members, neighbors, and friends?" (1 = never; 5 = always). The past trusting behavior index is a sum of the values for the question "how often do you lend personal possessions to friends and lend money to friends?" The maximum possible value for this index is 10, and higher values indicate higher frequencies of lending. The risk-taking variable is drawn from the question "how willing are you to take risks regarding your household finances?" (0 = not at all; 5 = completely risk taking). The being honest variable is a self-reported question: "How honest do you consider yourself?" (0 = not at all; 5 = completely honest).

Figure 4.3 presents the mean differences on the attitudinal survey questions. The measures of trust and risk in the attitudinal survey questions provide evidence consistent with the trusting behavior observed in the experiments. Compared to the non-exposed group, the exposed group reports lower values of trust in family members, neighbors, and friends and lower past trusting behavior. These differences are significant (p = 0.009, p = 0.080, p = 0.040, and p = 0.004, respectively). Risk-taking behavior regarding household finances is also lower in the exposed group and is statistically different from zero (p = 0.037).

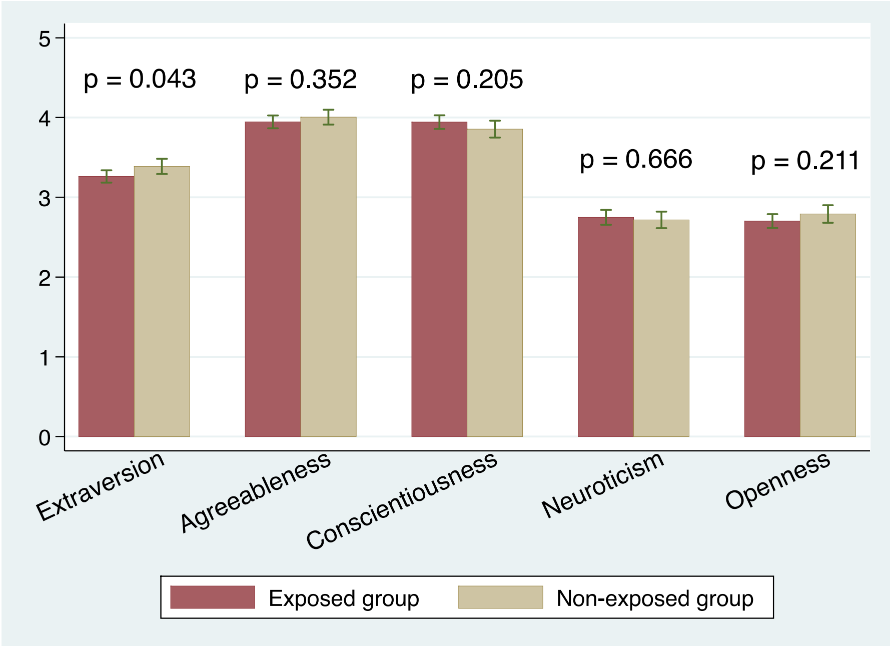

Figure 4.4: Mean differences of personality traits

Note: The exposed group includes individuals born between 1960 and 1974 and the non-exposed group includes individuals born between 1975 and 1982.

Figure 4.4 shows the mean comparison of the five factors of personality in the exposed and non-exposed groups. Those in the exposed group tend to have lower scores for extraversion, agreeableness, and openness than those in the non-exposed group. However, the difference is only statistically significant for extraversion (p = 0.043).

4.5.2 Regression Results

The differences between the exposed and non-exposed samples could be due to differences in individuals' demographic and socio-economic characteristics. To control for any such differences, we use Ordinary Least Squares estimation (OLS) to estimate equation (4.1).

4.5.2.1 Results from the Experiment Games

The regression results, which are controlled for demographic characteristics and district fixed effects, are presented in Table 4.2. The results show that the mean differences in the outcome variables of the exposed and non-exposed cohorts remain after conditioning on controls. The differences are highly statistically significant, except for trustworthiness. Individuals exposed to genocide under the KR regime are less trusting, less altruistic and more risk averse than individuals who did not directly experience the genocide. The results also demonstrate that exposure to genocide increases anti-social preferences, but the point estimate is not statistically significant.

In the case of the trust game, the regression results in Table 4.2 suggest that the exposed group sent 8.6 percentage points less of the endowment (p = 0.036) and returned 3.6 percentage points less than the non-exposed group (columns 1 and 2 of Panel A). However, the difference in trustworthiness is not statistically significant. These results suggest that directly experiencing the KR during early life weakens trust in later life. Individuals exposed to genocide are also less altruistic as measured by the percentage given to the other players, with those exposed giving 8.3 percentage points less of the endowment to the other player (p = 0.034) than non-exposed individuals (column 3 of Panel A). This indicates that individuals who directly experienced genocide are more self-regarding.

Column 4 of Table 4.2 reports the regression results for dictator game giving or taking. We use a binary variable equal to 1 if the individual takes some or all of the other player's endowment; otherwise, it is equal to 0. The results suggest that, when conditioning on controls, individuals exposed are still more likely to choose to take some or all the endowment from other players. However, the coefficient estimates are statistically insignificant. The point estimate suggests that the exposed group is 10.7 percentage points more likely to take the endowment than the non-exposed group. When we run an ordered probit or a multinomial logit/probit regression model in which the dependent variable takes the value of 0 (takes some or all of the other player's endowment), 1 (does not give to or take from the other player), and 2 (gives some or all of the endowment to the other player), we still find that those exposed to genocide are more likely to take other players' endowments. Again, the corresponding coefficient estimate is not estimated with precision.

Next we examine the effect of exposure to genocide on risk preference. The dependent variable is the percentage invested in the risk game. The OLS regression[30] results for risk preference presented in Column 5 of Table 4.2 show that exposure to genocide significantly motivates participants to make more risk-averse investment choices. The exposed group invested, on average, 14.1 percentage points less of the endowment in the risky investment (p = 0.001) than the non-exposed group.

We find a statistically insignificant but positive association between exposure to genocide and anti-social behavior (columns 6 and 7).[31] In column 6 of Table 4.2, we report the results when the dependent variable is whether participants burn other players' money for at least 1 of the 3 prices of burning (5%, 10%, and 20%).[32] Column 7 reports the corresponding results with inclusion of a binary indicator of whether the participant is an

[30] The dependent variable is the percentage invested, so we also use a generalized linear model and find the same results.
[31] We also use a Probit estimation and find similar results.
[32] The magnitude of the estimated coefficients is smaller when using different measures of the burning decision (burn at least two prices of burning and burn all three prices), but the signs of the estimates remain the same and are statistically insignificant.

advantaged player (i.e., received a gift in the money burning game) as an additional control. The results remain unchanged.

The measure of dishonesty in the self-reporting game is a binary indicator of whether participants take more money than that to which they are entitled. We also control for the maximum number of possible correct answers (7 or 4 stars) in the regression. The estimated coefficient of being exposed to genocide is not significant (columns 8 and 9).[33] Column 9 reports that opportunities to cheat are associated with dishonest attitudes. When there are fewer opportunities to cheat, individuals are less likely to be dishonest. The findings from the anti-social games suggest that exposure to genocide under the KR regime can influence vindictive and dishonest behavior, although these effects are not precisely estimated. Individuals' past experience with violence might decrease empathy toward others and increase concern for self-protection as belief in the trustworthiness of others decreases.

We also examine whether males or females exposed to genocide differ. Panel A of Table 4.2 shows that gender plays a role in shaping individual game behavior. Males have more pro-social behavior, are more risk-taking, and engage in less anti-social behavior than females. Compared to females, males send more in both trust and dictator giving (columns 1 and 3), invest more in the risk game (column 5), and are less likely to take or be dishonest (columns 4 and 9). These results are all statistically significant.

However, there is no difference of being exposed to genocide between males and females in our experimental sample. In Panel B of Table 4.2, we report results when we add an interaction term of being male and exposed to genocide in equation (4.1). The interaction term is statistically insignificant across the 5 games. The main results for the coefficient of exposure remain the same after adding this interaction term.

[33] We find the same results when using a Probit estimation.

We also consider if the results in the games are influenced by the other games since participants played a number of experimental games in one session. In particular, one might argue that risk and dishonest attitudes, in addition to the effects of direct exposure to genocide, might influence senders' decisions in the trust game. Similarly, individuals' dishonest behavior in the self-reporting game could also be affected by trust attitudes. In addition, being altruistic might be a factor that influences trustworthiness. To examine these possibilities, we include the percentage invested in the risk game and whether the individual is dishonest as controls in trust game regressions (sender's role). We add the percentage given to the other player in dictator game as an additional control when we run the regression for the trust game (receiver's role). We also control for the percentage sent in the trust game in the self-reporting game regressions. The results in Table 4.3 show that greater risk seeking is associated with sending more in the trust game. The impact of exposure to genocide is marginally smaller and insignificant (p = 0.144) when controlling for the percentage invested in the risk game (column 1). Hence, less risk-seeking behavior is also a factor in lower trust from exposure to genocide. Attitudes toward giving are positively associated with trustworthiness but do not influence the difference in trustworthy behavior between the exposed and non-exposed groups (column 3). Being dishonest is also not associated with sending less (column 2) or vice versa (columns 4 and 5).

Table 4.2: Estimates of effects of exposure to genocide on pro- and anti-social behavior and risk

	Trust		Dictator		Risk	Money Burning		Self-reporting	
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burn (=1)	Burn (=1)	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A									
Exposed vs. non-exposed group	-8.559**	-3.641	-8.260**	0.107	-14.092***	0.046	0.047	0.031	0.036
	(4.059)	(3.662)	(3.882)	(0.084)	(4.274)	(0.083)	(0.083)	(0.077)	(0.075)
Age	0.303	-0.044	0.318	-0.007	0.688**	0.003	0.003	0.002	0.001
	(0.288)	(0.249)	(0.292)	(0.006)	(0.297)	(0.006)	(0.006)	(0.006)	(0.006)
Education (years)	0.543*	0.241	0.398	0.003	1.117***	0.013**	0.013**	-0.016***	-0.016***
	(0.321)	(0.245)	(0.310)	(0.006)	(0.315)	(0.006)	(0.006)	(0.005)	(0.005)
Male (=1)	7.995***	3.038	8.278***	-0.112**	8.264***	-0.017	-0.016	-0.065	-0.069*
	(2.339)	(1.926)	(2.234)	(0.045)	(2.456)	(0.046)	(0.046)	(0.042)	(0.041)
Advantaged player							-0.014		
							(0.044)		
Maximum number of correct answers									-0.115***
									(0.040)
R-squared	0.212	0.110	0.163	-	0.122	0.058	0.058	0.083	0.098
Observations	492	4920	492	492	492	492	492	492	492
Panel B									
Exposed vs. non-exposed group	-8.533*	-5.357	-8.327**	0.085	-14.639***	0.039	0.040	0.036	0.041
	(4.432)	(4.168)	(4.176)	(0.092)	(4.750)	(0.093)	(0.094)	(0.086)	(0.085)
Age	0.303	-0.048	0.318	-0.007	0.687**	0.003	0.003	0.002	0.001
	(0.289)	(0.249)	(0.293)	(0.006)	(0.298)	(0.006)	(0.006)	(0.006)	(0.006)
Education (years)	0.543*	0.225	0.397	0.002	1.112***	0.013**	0.013**	-0.016***	-0.016***
	(0.321)	(0.246)	(0.309)	(0.006)	(0.315)	(0.006)	(0.006)	(0.005)	(0.005)
Male (=1)	8.029**	0.761	8.189**	-0.143**	7.538*	-0.027	-0.026	-0.057	-0.063
	(3.689)	(3.131)	(3.559)	(0.071)	(4.081)	(0.071)	(0.071)	(0.059)	(0.058)
Male x Exposed	-0.057	3.816	0.150	0.052	1.217	0.016	0.016	-0.013	-0.011
	(4.673)	(4.031)	(4.448)	(0.092)	(5.049)	(0.092)	(0.092)	(0.080)	(0.080)
R-squared	0.212	0.111	0.163	-	0.123	0.058	0.058	0.083	0.098
Observations	492	4920	492	492	492	492	492	492	492

Note: The exposed vs. non-exposed group variable equals 1 for individuals born between 1960 and 1974 and 0 for individuals born between 1975 and 1982. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. In column 5, we find the same results when using a generalized linear model. The dependent variable in columns 6 and 7 equals 1 if the player decides to reduce (burn) the other player's money for at least 1 of the 3 prices of burning and 0 if player decides not to burn any amount. The dependent variable in columns 8 and 9 equals 1 if the player takes extra money to which he or she is not entitled and 0 otherwise. The advantaged player equals 1 if the player receives a gift and 0 otherwise. In columns 6–9, the unreported Probit results are similar to those reported above. The maximum number of correct answers equals 1 if the total number of stars in the self-reporting game is 7 and 0 if total number of stars is 4. In Panel B, the regressions in columns 7 and 9 include being the advantaged player and the maximum number of correct answers, respectively. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4.3: Relationship between behavior in the games

	Trust game			Self-reporting game	
Dependent variable:	% Sent	% Sent	% Returned	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)
Panel A					
Exposed vs. non-exposed group	-5.953	-8.527**	-0.915	0.028	0.032
	(4.064)	(4.067)	(3.533)	(0.077)	(0.076)
Risk (% invested)	0.185***				
	(0.048)				
Dishonest (=1)		-1.041			
		(2.603)			
Dictator (% given)			0.330***		
			(0.046)		
Trust (% sent)				-0.0003	-0.0005
				(0.001)	(0.001)
Age	0.176	0.305	-0.149	0.002	0.001
	(0.285)	(0.289)	(0.239)	(0.006)	(0.006)
Education (years)	0.336	0.526	0.109	-0.016***	-0.016***
	(0.326)	(0.325)	(0.223)	(0.005)	(0.005)
Male (=1)	6.467***	7.927***	0.305	-0.062	-0.066
	(2.365)	(2.333)	(1.800)	(0.042)	(0.042)
Maximum number of correct answers					-0.116***
					(0.040)
R-squared	0.242	0.213	0.204	0.083	0.099
Observations	492	492	4920	492	492
Panel B					
Exposed vs. non-exposed group	-5.826	-8.495*	-2.608	0.034	0.037
	(4.411)	(4.440)	(4.026)	(0.086)	(0.085)
Risk (% invested)	0.185***				
	(0.048)				
Dishonest (=1)		-1.041			
		(2.606)			
Dictator (% given)			0.330***		
			(0.046)		
Trust (% sent)				-0.0003	-0.0005
				(0.001)	(0.001)
Age	0.176	0.305	-0.153	0.002	0.001
	(0.286)	(0.289)	(0.238)	(0.006)	(0.006)
Education (years)	0.337	0.526	0.094	-0.016***	-0.016***
	(0.325)	(0.325)	(0.223)	(0.005)	(0.005)
Male (=1)	6.634*	7.969**	-1.941	-0.055	-0.059
	(3.664)	(3.691)	(2.967)	(0.059)	(0.059)
Male * Exposed	-0.282	-0.070	3.767	-0.013	-0.011
	(4.560)	(4.681)	(3.751)	(0.080)	(0.080)
R-squared	0.242	0.213	0.206	0.083	0.099
Observations	492	492	4920	492	492

Note: For the definition of the variables, see Figure 4.2. Column 3 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. In Panel B, the regression in column 5 includes the maximum number of correct answers. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

4.5.2.2 Results from the Survey Questions

We now analyze the survey data and examine the differences in exposed and non-exposed groups. The main purpose of this is to assess whether the behavioral outcomes in the survey support the findings from the experiment.[34] We estimate OLS regressions on behavioral outcomes in the attitudinal survey questions using equation (4.1) with the same set of controls as in Table 4.2. Table 4.4 indicates that individuals exposed to genocide are less trusting (columns 1–3) and have less confidence in others (column 4). In Panel A of Table 4.4, the exposed group shows lower trust in family members (p = 0.085), trust in neighbors (p = 0.080), and self-reported past trusting behavior (p = 0.042) compared to the non-exposed group. However, the estimated coefficient for the effect of exposure to genocide on trust in friends is not statistically significant. Exposure to genocide also results in less risk taking compared to those who were not exposed (column 4), but the coefficient is not statistically significant. Overall, we find that trusting behavior reported in the survey-based measures are greatly consistent with trust in the experiment game, while there is no statistically significant evidence for risk-taking behavior in the survey results.

Panel B of Table 4.4 presents the results with the inclusion of the interaction term of being male and exposed to genocide. The magnitudes and significance levels of the estimated coefficients for the effects of exposure to genocide on trust in family members, neighbors, and friends and past trusting behavior are higher. There is a differential effect between males and females for trust in the survey questions.

[34] We do not aim to test the relationship between the experimental and survey measures of social capital which have been considered in many studies (e.g., Glaeser et al., 2000).

Table 4.4: Estimates of effects of exposure to genocide on social behavior and risk using attitudinal survey questions

Dependent variable:	Trust in family	Trust in neighbors	Trust in friends	Past trusting behavior index	Risk taking regarding household finances	Being honest
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Exposed vs. non-exposed group	-0.242*	-0.342**	-0.204	-0.631**	-0.289	0.142
	(0.141)	(0.162)	(0.169)	(0.309)	(0.291)	(0.158)
Age	0.004	0.022*	0.014	0.039*	0.002	-0.0001
	(0.011)	(0.012)	(0.012)	(0.023)	(0.021)	(0.012)
Education (years)	0.017	0.038***	0.058***	0.111***	0.022	0.014
	(0.010)	(0.012)	(0.012)	(0.023)	(0.022)	(0.012)
Male (=1)	0.056	0.281***	0.175*	0.089	0.211	0.036
	(0.082)	(0.090)	(0.098)	(0.167)	(0.160)	(0.090)
R-squared	0.040	0.083	0.085	0.089	0.046	0.017
Observations	492	492	492	492	492	492
Panel B						
Exposed vs. non-exposed group	-0.448***	-0.501***	-0.432**	-0.696**	-0.293	0.078
	(0.160)	(0.181)	(0.186)	(0.351)	(0.327)	(0.174)
Age	0.004	0.022*	0.013	0.038*	0.002	-0.0003
	(0.011)	(0.012)	(0.012)	(0.023)	(0.021)	(0.012)
Education (years)	0.015	0.036***	0.056***	0.110***	0.022	0.014
	(0.010)	(0.012)	(0.012)	(0.023)	(0.022)	(0.012)
Male (=1)	-0.216*	0.070	-0.127	0.002	0.205	-0.049
	(0.111)	(0.140)	(0.150)	(0.238)	(0.230)	(0.148)
Male x Exposed	0.457***	0.354**	0.506***	0.145	0.009	0.142
	(0.153)	(0.177)	(0.191)	(0.335)	(0.320)	(0.187)
R-squared	0.055	0.091	0.098	0.089	0.046	0.018
Observations	492	492	492	492	492	492

Note: For the definitions of the variables, see Figure 4.3. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

4.5.2.3 Personality Traits

Table 4.5 reports the differences in personality traits between the exposed and non-exposed individuals using equation (4.1). Being exposed to genocide during childhood and early adolescence has a statistically significant association with lower scores for extraversion (p = 0.015) and agreeableness (p = 0.074), indicating less tendency toward sociability and altruism. We also observe lower scores for conscientiousness, neuroticism, and openness in the exposed group compared to the non-exposed group; however, these differences are not significantly different from zero. When adding the interaction term of being male and exposed to genocide to equation (4.1), the effect of exposure to genocide is slightly higher but remains statistically significant at the 5% level for extraversion (column 1 of Panel B). We see no differential effects between males and females for extraversion. However, the precision of the estimates of the coefficient for agreeableness decreases and the effect of exposure to genocide is statistically insignificant (column 2 of Panel B). Male individuals exposed to the genocide have significantly lower scores for agreeableness.

The concern that an individual's personality traits can change throughout the course of life might be raised. There are several opposing theoretical views on personality development. According to McCrae et al. (1999), personality change occurs primarily during young adulthood and plateaus by late middle age. In contrast, various contextual perspectives (e.g., Lewis, 2001; Neyer & Asendorpf, 2001) assert that personality traits are sensitive to environmental influences and, therefore, are likely to change over time and across contexts, especially during development periods characterized by pervasive internal and external change. Our results are consistent with both perspectives. First, the exposed and non-exposed groups in our sample are middle aged. On average, their personality profiles largely have similar levels and are stable. Second, the lower scores on the Big Five personality traits for the exposed group compared to the non-exposed group can be explained by the fewer opportunities to develop and internalize social principles that promote pro-social behavior during exposure to war during childhood and early adolescence.

Further, our findings are consistent with psychological studies on the relationship between childhood trauma experience (e.g., sexual, emotional, and physical abuse) and personality disorder (Allen & Lauterbach, 2007; Roys & Timms, 1997). While there is no evidence for the effect of childhood trauma experience on personality traits in the context of civil conflicts, a few studies have examined the link between personality and coping strategies after exposure to war (e.g. Fiedler et al., 2000).

Table 4.5: Differences in personality traits between the exposed and non-exposed groups

Dependent variable:	Extraversion	Agreeableness	Conscientiousness	Neuroticism	Openness
	(1)	(2)	(3)	(4)	(5)
Panel A					
Exposed vs. non-exposed group	-0.297**	-0.226*	-0.205	-0.082	-0.093
	(0.122)	(0.126)	(0.133)	(0.130)	(0.131)
Age	0.021**	0.012	0.024***	0.002	0.0004
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Education (years)	0.031***	-0.003	-0.003	-0.031***	0.002
	(0.008)	(0.007)	(0.009)	(0.010)	(0.010)
Male (=1)	-0.008	0.005	-0.161**	-0.401***	0.018
	(0.063)	(0.065)	(0.071)	(0.073)	(0.072)
R-squared	0.053	0.021	0.043	0.109	0.031
Observations	492	492	492	492	492
Panel B					
Exposed vs. non-exposed group	-0.326**	-0.111	-0.240	0.060	-0.184
	(0.133)	(0.144)	(0.150)	(0.145)	(0.147)
Age	0.021**	0.012	0.023***	0.002	0.0002
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Education (years)	0.031***	-0.002	-0.003	-0.030***	0.001
	(0.008)	(0.007)	(0.009)	(0.010)	(0.010)
Male (=1)	-0.047	0.158	-0.208*	-0.213**	-0.103
	(0.101)	(0.096)	(0.110)	(0.108)	(0.115)
Male x Exposed	0.065	-0.257**	0.077	-0.314**	0.202
	(0.127)	(0.127)	(0.140)	(0.142)	(0.146)
R-squared	0.054	0.029	0.044	0.118	0.035
Observations	492	492	492	492	492

Note: The exposed vs. non-exposed group variable equals 1 for individuals born between 1960 and 1974 and 0 for individuals born between 1975 and 1982. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

4.6 Sensitivity Analysis

In this section we identify alternative potential channles for our main findings and demonstrate that these do not have explanatory power in our setting. Exposure to genocide hence remains the main driving force guiding our results.

4.6.1 Age Effects

The exposed and non-exposed groups are defined according to birth cohort and thus are highly correlated with age. In this section, we investigate whether age is directly associated with behavioral outcomes in the experiments. We use two different measures of age—age in years and dummies for different age groups—and check the effect of age on the experimental outcomes within the exposed sample. We also run the same regressions for the non-exposed sample. The results presented in Panel A of Table 4.6 indicate that age is not a determinant of behavior within the exposed sample across the five games. Nor is age associated with experimental outcomes within the non-exposed sample (Panel B). Thus, there is no evidence that the main results are driven by age.

Table 4.6: Tests for age effects within the exposed group and the non-exposed group

	Trust		**Dictator**		**Risk**	**Money Burning**	**Self-reporting**
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burn (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Test for Age Effects within the Exposed Group							
Explanatory variable:							
Age	0.213	-0.104	0.300	-0.006	0.526	0.002	0.0002
	(0.334)	(0.276)	(0.331)	(0.007)	(0.328)	(0.007)	(0.007)
Observations	296	2960	296	296	296	296	296
R-squared	0.156	0.089	0.115	-	0.109	0.061	0.067
Explanatory variable:							
Born 1960–1964	3.168	-2.231	2.797	-0.070	5.587	0.076	0.013
	(4.160)	(3.349)	(4.083)	(0.079)	(4.044)	(0.080)	(0.078)
Born 1965–1970	1.597	-3.281	-1.536	-0.056	2.042	0.096	0.093
	(3.753)	(3.229)	(3.541)	(0.073)	(3.802)	(0.075)	(0.072)
R-squared	0.161	0.091	0.114	-	0.107	0.068	0.074
Observations	296	2960	296	296	296	296	296
Panel B: Test for Age Effects within the Non-exposed Group							
Explanatory variable:							
Age	0.246	0.081	0.149	0.005	1.345	-0.002	0.007
	(0.769)	(0.639)	(0.737)	(0.015)	(0.850)	(0.015)	(0.013)
Observations	196	1960	196	196	196	196	196
R-squared	0.262	0.137	0.205	-	0.117	0.064	0.113
Explanatory variable:							
Born 1975–1979	-1.323	-1.005	0.312	-0.038	2.508	0.046	0.043
	(3.636)	(3.079)	(4.396)	(0.071)	(3.787)	(0.075)	(0.062)
R-squared	0.262	0.137	0.205	-	0.106	0.066	0.113
Observations	196	1960	196	196	196	196	196

Note: For the definition of the variables, see Table 4.2. All the regressions include gender, education, and experimental district fixed effects. The base age group in Panel A was born between 1971 and 1974, and the base age group in Panel B was born between 1980 and 1982. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

4.6.2 Exclusion of Education as a Control

We include the participants’ completed number of years of schooling as a control in the main regression estimates. However, education is a potential channel for the effects of civil conflict exposure on individual preferences. Many existing studies looking at civil conflicts in different countries (e.g., Akresh & de Walque, 2008; Chamarbagwala & Morán, 2011; Dabalen & Paul, 2012; Leon, 2012; Shemyakina, 2011) find a negative relationship between exposure to conflict and educational attainment. Thus, we estimate equation (4.1) on the outcomes of interest in the experiment and exclude completed number of years of schooling as an explanatory variable. As reported in Table 4.7, the signs, magnitudes, and significance levels of the coefficients of interest are almost identical to the main results in Table 4.2. Without controlling for education, the results using outcomes in the survey questions and the Big Five factors of personality traits as outcome variables in Tables 4.4 and 4.5, respectively, are also robust, as reported in Tables A4.1 and A4.2, respectively.

4.6.3 Alternative Exposed and Non-exposed Birth Cohorts

As highlighted in section 4.2.2, the definitions of exposed and non-exposed groups are potentially sensitive to adult memories of early childhood. We exclude some birth cohorts whose early childhood memories are likely inconsistent with the definition of exposure based on their birth years. For instance, we drop from the exposed group the 1973 and 1974 cohorts who were approximately 1 to 6 years old during the four years of genocide. From the non-exposed group, we exclude the 1975 and 1976 cohorts who were approximately 0 to 4 years old during the genocide under the KR regime. We re-estimate the main results of Table 4.2 using the same estimation methods and control variables.

As shown in Table 4.8, our main results in Table 4.2 are robust. We find that exposure to genocide reduces trust, altruistic, and risk-seeking behavior across alternative exposed and non-exposed birth cohorts. Also, the effects of exposure to genocide continue to have the expected signs for trustworthiness in the trust game, taking decisions in the dictator game, and dishonesty in the self-reporting game.

4.6.4 Robustness to Living in the Same Locality

We also analyze whether the behavioral differences between the exposed and non-exposed individuals could be attributed to the displacement during or after the genocide. We control for whether individuals have resided in the same district since birth. In the survey data, we ask whether participants in the experiment have always lived in the same district since birth. For individuals born before and during the KR regime, this question indicates that they were not displaced under the KR regime and lived in the same district after the KR regime. Seventy-six percent of the exposed cohorts and 74% of the non-exposed cohorts answered that they have lived in the same district since birth.

Table 4.9 reports the results for the experimental outcomes. The results show that living in the same locality since birth has no significant effect on behavioral differences between the exposed and non-exposed groups. The magnitudes and significance levels of the estimates remain unchanged. Exposed individuals have lower pro-social preferences and risk preferences compared to those who were not exposed. The results from using survey data as outcome variables in Table 4.4 and using the Big Five factors of personality traits as dependent variables in Table 4.5 are also robust, as shown in Tables A4.3 and A4.4, respectively.

Table 4.7: Estimates of effects of exposure to genocide on pro- and anti-social behavior and risk without education as a control

	Trust		Dictator		Risk	Money burning		Self-reporting	
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burn (=1)	Burn (=1)	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A									
Exposed vs. non-exposed group	-8.167**	-3.467	-7.972**	0.109	-13.285***	0.056	0.057	0.019	0.025
	(4.050)	(3.658)	(3.845)	(0.084)	(4.354)	(0.083)	(0.083)	(0.078)	(0.077)
Age	0.153	-0.111	0.208	-0.008	0.378	-0.001	-0.001	0.006	0.006
	(0.273)	(0.241)	(0.267)	(0.006)	(0.287)	(0.006)	(0.006)	(0.005)	(0.005)
Male (=1)	8.766***	3.380*	8.844***	-0.108**	9.851***	0.002	0.003	-0.088**	-0.092**
	(2.292)	(1.917)	(2.168)	(0.044)	(2.476)	(0.045)	(0.045)	(0.041)	(0.041)
Advantaged player							-0.019		
							(0.044)		
Maximum number of correct answers									-0.114***
									(0.040)
R-squared	0.207	0.109	0.159	-	0.100	0.048	0.048	0.066	0.081
Observations	492	4920	492	492	492	492	492	492	492
Panel B									
Exposed vs. non-exposed group	-8.378*	-5.292	-8.214**	0.085	-14.321***	0.043	0.044	0.032	0.036
	(4.427)	(4.172)	(4.166)	(0.092)	(4.864)	(0.094)	(0.094)	(0.087)	(0.086)
Age	0.153	-0.110	0.208	-0.008	0.379	-0.001	-0.001	0.006	0.006
	(0.273)	(0.241)	(0.267)	(0.006)	(0.287)	(0.006)	(0.006)	(0.005)	(0.005)
Male (=1)	8.484**	0.950	8.522**	-0.142**	8.471**	-0.015	-0.015	-0.071	-0.076
	(3.688)	(3.136)	(3.524)	(0.071)	(4.132)	(0.072)	(0.072)	(0.060)	(0.059)
Male x Exposed	0.468	4.034	0.534	0.055	2.292	0.029	0.029	-0.028	-0.026
	(4.698)	(4.021)	(4.458)	(0.091)	(5.121)	(0.093)	(0.093)	(0.081)	(0.080)
R-squared	0.207	0.110	0.160	-	0.101	0.048	0.049	0.066	0.082
Observations	492	4920	492	492	492	492	492	492	492

Note: For the definition of the variables, see Table 4.2. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. In Panel B, the regressions in columns 7 and 9 include being the advantaged player and the maximum number of correct answers, respectively. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4.8: Estimates of effects of exposure to genocide for alternative exposed and non-exposed birth cohorts

	1960–1972 vs. 1975–1982	1960–1972 vs. 1976–1982	1960–1973 vs. 1975–1982	1960–1973 vs. 1976–1982	1960–1974 vs. 1976–1982	1960–1974 vs. 1977–1982
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:						
% Sent	-8.229*	-10.584**	-8.703*	-10.820**	-10.454**	-9.023**
	(4.926)	(5.200)	(4.560)	(4.839)	(4.295)	(4.394)
% Returned	-5.294	-5.945	-4.513	-5.061	-4.090	-3.636
	(4.377)	(4.664)	(3.865)	(4.105)	(3.868)	(4.084)
% Given	-8.497*	-9.607*	-8.010*	-8.948*	-9.163**	-10.421**
	(4.755)	(5.135)	(4.279)	(4.607)	(4.162)	(4.378)
Giving or Taking	0.057	0.107	0.086	0.134	0.152*	0.125
	(0.098)	(0.104)	(0.090)	(0.095)	(0.088)	(0.093)
% Invested	-15.907***	-12.540**	-12.509**	-9.418*	-11.496**	-10.907**
	(5.320)	(5.596)	(4.853)	(5.066)	(4.450)	(4.658)
Burn (=1)	0.039	-0.043	0.061	-0.005	-0.014	-0.005
	(0.099)	(0.108)	(0.092)	(0.099)	(0.090)	(0.095)
Dishonest (=1)	0.075	0.085	0.048	0.053	0.033	0.076
	(0.093)	(0.101)	(0.085)	(0.091)	(0.082)	(0.084)
Observations	456	433	474	451	469	453

Note: For the definition of the variables, see Table 4.2. Table 8 reports the estimated coefficients for effects of exposure to genocide on the experimental outcomes using alternative exposed and non-exposed birth cohorts. The regressions are controlled for age, gender, education, and experimental district fixed effects. Row 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Row 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4.9: Estimates of effects of exposure to genocide on pro- and anti-social behavior and risk with residing in the same district since birth as a control

	Trust		Dictator		Risk	Money Burning		Self-reporting	
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burn (=1)	Burn (=1)	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A									
Exposed vs. non-exposed group	-8.594**	-3.619	-8.210**	0.107	-14.217***	0.045	0.046	0.032	0.038
	(4.078)	(3.665)	(3.879)	(0.084)	(4.252)	(0.083)	(0.084)	(0.077)	(0.076)
Live in the same district since birth	-1.495	0.966	2.090	-0.006	-5.293*	-0.054	-0.054	0.050	0.059
	(3.196)	(2.392)	(2.838)	(0.060)	(3.096)	(0.062)	(0.062)	(0.057)	(0.057)
Age	0.302	-0.043	0.320	-0.007	0.683**	0.003	0.003	0.002	0.001
	(0.289)	(0.249)	(0.292)	(0.006)	(0.297)	(0.006)	(0.006)	(0.006)	(0.006)
Education (years)	0.543*	0.240	0.397	0.003	1.120***	0.014**	0.013**	-0.016***	-0.016***
	(0.322)	(0.245)	(0.310)	(0.006)	(0.315)	(0.006)	(0.006)	(0.005)	(0.005)
Male (=1)	7.929***	3.080	8.370***	-0.112**	8.032***	-0.019	-0.019	-0.063	-0.067
	(2.360)	(1.946)	(2.248)	(0.045)	(2.460)	(0.046)	(0.046)	(0.042)	(0.041)
Advantaged player							-0.013		
							(0.044)		
Maximum number of correct answers									-0.117***
									(0.040)
R-squared	0.213	0.110	0.164	-	0.127	0.060	0.060	0.084	0.100
Observations	492	4920	492	492	492	492	492	492	492
Panel B									
Exposed vs. non-exposed group	-8.623*	-5.307	-8.202*	0.084	-14.960***	0.035	0.036	0.040	0.045
	(4.450)	(4.183)	(4.182)	(0.092)	(4.745)	(0.094)	(0.094)	(0.086)	(0.085)
Live in the same district since birth	-1.497	0.829	2.091	-0.007	-5.353*	-0.055	-0.055	0.051	0.059
	(3.200)	(2.411)	(2.849)	(0.060)	(3.112)	(0.062)	(0.062)	(0.057)	(0.057)
Age	0.302	-0.047	0.320	-0.007	0.682**	0.003	0.003	0.002	0.001
	(0.290)	(0.249)	(0.292)	(0.006)	(0.297)	(0.006)	(0.006)	(0.006)	(0.006)
Education (years)	0.543*	0.225	0.397	0.002	1.113***	0.013**	0.013**	-0.016***	-0.016***
	(0.322)	(0.246)	(0.310)	(0.006)	(0.315)	(0.006)	(0.006)	(0.005)	(0.005)
Male (=1)	7.891**	0.838	8.381**	-0.144**	7.045*	-0.032	-0.031	-0.053	-0.057
	(3.715)	(3.172)	(3.585)	(0.072)	(4.101)	(0.071)	(0.072)	(0.059)	(0.059)
Male x Exposed	0.065	3.749	-0.019	0.053	1.651	0.020	0.021	-0.017	-0.016
	(4.678)	(4.047)	(4.462)	(0.092)	(5.052)	(0.093)	(0.093)	(0.080)	(0.080)
R-squared	0.213	0.111	0.164	-	0.127	0.060	0.060	0.084	0.100
Observations	492	4920	492	492	492	492	492	492	492

Note: For the definitions of the variables, see Table 4.2. The variable of live in the same district since birth equals 1 if an individual has lived in the same district since birth and 0 otherwise. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. In Panel B, the regressions in columns 7 and 9 include being the advantaged player and the maximum number of correct answers, respectively. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

4.6.5 Effects of Personality Traits in Experiment Behavior

Individual personality could be potentially correlated with the social behavior and risk preferences observed in the experiments. To examine whether our results for the effects of genocide exposure on social behaviors and risk preferences are robust, we include the Big Five personality traits as controls in equation (4.1). The results in Table 4.10 show less trusting, less altruistic, and more risk-averse behavior in individuals exposed to genocide (columns 1, 3, and 5). Compared to the main results in Table 4.2, there is little change in the magnitudes of the estimated coefficients and the significance levels, suggesting that exposure to genocide directly affects the individual behavior observed in the experiments.

4.6.6 Variation in Experiences of Violence or Seeing Violent Acts

Additionally, it is likely, from our specifications, that experiencing violence or seeing violent acts under the KR regime might influence the outcomes of the exposed group in the experiment. It is plausible that exposure to genocide has more pronounced effects on outcomes if individuals personally experienced violence or saw violent activities under the KR regime, compared to those who did not experience or see violence. We asked only participants born before and during the KR regime the following questions: "*Did you ever see or experience physical torture during the KR regime?" and "How often did you see or experience physical torture?"* These questions do not apply to individuals born after the KR regime. Thus, we can examine the variation of the impacts of exposure to genocide within: 1) only the exposed group (born before the KR regime); and 2) the exposed group plus individuals born during the KR regime. Table 4.11 shows that experiencing violence or seeing violent acts during the KR regime has no significant effect on the experimental outcomes (columns 1–7) except in the self-reporting game (columns 8 and 9). The results are similar when we use how often (number of cases) an individual saw or experienced physical torture (see Table A4.5).

Table 4.10: Estimates of effects of exposure to genocide on pro- and anti-social behavior and risk with inclusion of the Big Five factors of personality traits as controls

	Trust		Dictator		Risk	Money Burning		Self-reporting	
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burn (=1)	Burn (=1)	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A									
Exposed vs. non-exposed group	-8.104**	-3.270	-7.171*	0.125	-14.178***	0.045	0.047	0.053	0.057
	(4.061)	(3.646)	(3.935)	(0.085)	(4.234)	(0.085)	(0.085)	(0.078)	(0.077)
Extraversion	-1.824	0.997	2.915*	0.018	2.544	-0.022	-0.023	0.025	0.025
	(1.789)	(1.504)	(1.598)	(0.034)	(1.846)	(0.035)	(0.035)	(0.031)	(0.031)
Agreeableness	1.808	1.607	0.563	0.0004	-1.978	0.001	0.002	0.021	0.016
	(1.731)	(1.390)	(1.556)	(0.033)	(1.808)	(0.034)	(0.034)	(0.030)	(0.029)
Conscientiousness	2.659*	-0.555	-0.277	0.047	-1.571	0.013	0.014	0.018	0.020
	(1.608)	(1.272)	(1.442)	(0.031)	(1.647)	(0.032)	(0.032)	(0.028)	(0.028)
Neuroticism	1.365	-1.906	-0.446	0.044	-4.084***	-0.031	-0.032	0.040	0.039
	(1.583)	(1.331)	(1.530)	(0.030)	(1.568)	(0.030)	(0.030)	(0.026)	(0.026)
Openness	-0.733	-0.200	2.024	-0.003	2.790*	0.053*	0.054*	0.037	0.030
	(1.700)	(1.296)	(1.492)	(0.028)	(1.678)	(0.030)	(0.030)	(0.028)	(0.028)
Age	0.256	-0.067	0.256	-0.009	0.700**	0.003	0.003	0.0005	0.00003
	(0.286)	(0.252)	(0.299)	(0.006)	(0.299)	(0.006)	(0.006)	(0.006)	(0.006)
Education (years)	0.657**	0.154	0.290	0.004	0.893***	0.013**	0.013**	-0.016***	-0.016***
	(0.321)	(0.257)	(0.315)	(0.006)	(0.329)	(0.006)	(0.006)	(0.006)	(0.006)
Male (=1)	8.960***	2.188	8.040***	-0.087*	6.354**	-0.028	-0.028	-0.047	-0.051
	(2.403)	(2.000)	(2.438)	(0.047)	(2.507)	(0.047)	(0.048)	(0.042)	(0.042)
Advantaged player							-0.023		
							(0.045)		
Maximum number of correct answers									-0.111***
									(0.040)
R-squared	0.222	0.116	0.172	-	0.146	0.068	0.068	0.092	0.106
Observations	492	4920	492	492	492	492	492	492	492
Panel B									
Exposed vs. non-exposed group	-8.512*	-4.931	-6.979*	0.098	-13.640***	0.046	0.048	0.056	0.059
	(4.471)	(4.094)	(4.160)	(0.094)	(4.683)	(0.095)	(0.095)	(0.087)	(0.086)
Male (=1)	8.423**	0.002	8.292**	-0.124*	7.062*	-0.026	-0.026	-0.043	-0.048
	(3.684)	(3.168)	(3.620)	(0.072)	(4.068)	(0.073)	(0.073)	(0.059)	(0.059)
Male x Exposed	0.921	3.745	-0.432	0.063	-1.213	-0.004	-0.004	-0.006	-0.004
	(4.680)	(4.040)	(4.515)	(0.093)	(5.030)	(0.093)	(0.093)	(0.082)	(0.081)
R-squared	0.222	0.118	0.172	-	0.146	0.068	0.068	0.092	0.106
Observations	492	4920	492	492	492	492	492	492	492

Note: For the definitions of the variables, see Table 4.2. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. In Panel A, all the regressions include experimental district fixed effects. In Panel B, all the regressions include the five factors of personality traits, age, education, and experimental district fixed effects. The regressions in columns 7 and 9 include being the advantaged player and the maximum number of correct answers, respectively. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table 4.11: Tests for effects of variation in experiencing or seeing violence during the Khmer Rouge regime within the exposed group

	Trust		Dictator		Risk	Money Burning		Self-reporting	
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burn (=1)	Burn (=1)	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Within the Exposed Group (Born 1960–1974)									
Experienced/saw violence	-4.412	-3.379	-2.053	-0.098*	4.855	0.034	0.035	0.224***	0.217***
	(3.338)	(2.817)	(2.996)	(0.059)	(3.154)	(0.062)	(0.062)	(0.055)	(0.055)
Age	0.326	0.004	0.348	-0.003	0.477	0.006	0.006	-0.005	-0.005
	(0.367)	(0.292)	(0.330)	(0.007)	(0.340)	(0.007)	(0.007)	(0.006)	(0.006)
Education (year)	0.614	0.492*	0.667	0.010	1.226***	0.010	0.010	-0.022***	-0.022***
	(0.454)	(0.296)	(0.417)	(0.008)	(0.401)	(0.008)	(0.008)	(0.007)	(0.007)
Male (=1)	5.970*	2.989	7.024**	-0.080	8.240***	0.013	0.013	-0.075	-0.076
	(3.259)	(2.695)	(3.119)	(0.059)	(3.117)	(0.062)	(0.062)	(0.057)	(0.057)
Advantaged player							0.015		
							(0.059)		
Maximum number of correct answers									-0.085
									(0.054)
R-squared	0.027	0.016	0.039	-	0.081	0.008	0.009	0.085	0.093
Observations	296	2960	296	296	296	296	296	296	296
Panel B: Within the Exposed Group and Born During the KR regime (Born 1960–1979)									
Experienced/saw violence	-4.416	-3.763	-3.949	-0.029	1.189	0.029	0.029	0.193***	0.190***
	(3.193)	(2.601)	(2.734)	(0.055)	(3.010)	(0.057)	(0.057)	(0.051)	(0.051)
Age	0.117	-0.062	0.107	-0.002	-0.031	0.005	0.005	-0.006	-0.006
	(0.285)	(0.225)	(0.245)	(0.005)	(0.268)	(0.005)	(0.005)	(0.004)	(0.004)
Education (year)	1.067**	0.540**	0.915**	0.004	1.354***	0.010	0.010	-0.023***	-0.023***
	(0.413)	(0.272)	(0.389)	(0.006)	(0.369)	(0.007)	(0.007)	(0.006)	(0.006)
Male (=1)	7.305**	3.346	8.277***	-0.097*	8.752***	0.027	0.027	-0.075	-0.079*
	(2.848)	(2.337)	(2.664)	(0.050)	(2.852)	(0.052)	(0.052)	(0.047)	(0.047)
Advantaged player							-0.006		
							(0.051)		
Maximum number of correct answers									-0.099**
									(0.046)
R-squared	0.055	0.025	0.064	-	0.079	0.009	0.009	0.087	0.098
Observations	390	3900	390	390	390	390	390	390	390

Note: For the definitions of the variables, see Table 4.2. The explanatory variable of experienced/saw violence equals 1 if an individual experienced or saw violence during the KR regime and 0 otherwise. Only individuals born before 1980 were asked to answer this question. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

4.7 Conclusion

The literature on social capital has highlighted the link between social capital and economic outcomes (Guiso, Sapienza, & Zingales, 2004; Knack & Keefer, 1997). A number of experimental studies show that individual social preferences develop over the course of childhood and adolescence. We examine the long-term effects of one of the worst genocide events in human history which has shaped the social behavior of individuals who directly experienced it during their childhood and early adolescence.

We use three different approaches—experiments, survey-based measures, and personality traits questionnaires—to examine the effect of exposure to genocide under the KR regime on pro- and anti-social behavior and risk preferences. We find that individuals exposed to the genocide are less trusting, less altruistic, and more risk averse. Our results are robust to a variety of alternative definitions of cohorts and other sensitivity checks, such as the age of participants, differences in education levels, living in the same locality since birth, and individual personalities. Violence during the KR regime might have caused individuals with stronger pro-social attitudes and higher risk-seeking tendencies to be killed at a higher rate than those with weaker pro-social attitudes and stronger risk-averse preferences. We try to address this issue using two strategies. First, we consider districts with high and low intensity of wars and add district fixed effects in the regression analysis. Second, we find that variation in the personal experience of violence during the KR regime has no effect on the experimental outcomes within the exposed group. Taken together, these findings suggest that direct exposure to genocide during childhood and early adolescence might alter pro- and anti-social preferences and personality traits of individuals. We argue that the KR forced people to adopt its norms and institutions that created feelings of fear and horror which discouraged pro-social behavior.

Amid diverse evidence on the link between civil conflicts and pro-social behavior and risk preferences, our results complement evidence that suggests that direct exposure to civil conflict has negative consequence on pro-social preferences and risk (Cassar et al., 2013; Rohner et al., 2013; Callen et al., 2014). Our research also extends the findings of previous studies in three ways. First, we provide some insight into the effects of direct exposure to civil conflict on anti-social behavior. We find that direct exposure to genocide has some effects on dishonest behavior when we do a simple comparison between the exposed and non-exposed cohorts. However, these results become statistically insignificant when controlling for individuals' characteristics. Therefore, further investigation into this aspect might help generate more conclusive evidence. Second, we demonstrate that the effects found in the experimental data are consistent with the analysis of the survey questions. Finally, our study also shows the long-term effects of civil conflict on social capital. The absence of early problem identification and intervention, the lack of adequate facilities to provide support to survivors, and the low confidence in old forms (or no emergence of new forms) of social, economic, and political vitality in communities in the aftermath of violent events can lead to the long-term persistence of the impacts of exposure to those violent events.

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Appendix to Chapter 4

Appendix A

Table A4.1: Estimates of effects of exposure to genocide on social behavior and risk using attitudinal survey questions without education as a control

Dependent variable:	Trust in family	Trust in neighbors	Trust in friends	Past trusting behavior index	Risk taking regarding household finances	Being honest
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Exposed vs. non-exposed group	-0.230	-0.315*	-0.163	-0.551*	-0.273	0.153
	(0.141)	(0.162)	(0.171)	(0.314)	(0.291)	(0.157)
Age	-0.0002	0.012	-0.002	0.008	-0.004	-0.004
	(0.010)	(0.011)	(0.012)	(0.023)	(0.021)	(0.011)
Male (=1)	0.080	0.335***	0.257***	0.246	0.241	0.056
	(0.081)	(0.088)	(0.098)	(0.170)	(0.160)	(0.089)
R-squared	0.035	0.062	0.043	0.041	0.044	0.014
Observations	492	492	492	492	492	492
Panel B						
Exposed vs. non-exposed group	-0.444***	-0.490***	-0.416**	-0.665*	-0.287	0.082
	(0.160)	(0.181)	(0.188)	(0.358)	(0.327)	(0.174)
Age	-0.0002	0.012	-0.002	0.008	-0.004	-0.004
	(0.010)	(0.011)	(0.012)	(0.023)	(0.021)	(0.011)
Male (=1)	-0.204*	0.101	-0.080	0.095	0.223	-0.038
	(0.110)	(0.142)	(0.152)	(0.241)	(0.230)	(0.148)
Male x Exposed	0.471***	0.388**	0.559***	0.251	0.030	0.156
	(0.155)	(0.178)	(0.195)	(0.339)	(0.318)	(0.186)
R-squared	0.052	0.072	0.060	0.042	0.044	0.015
Observations	492	492	492	492	492	492

Note: For the definitions of the variables, see Table 4.2. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table A4.2: Differences in personality traits between the exposed and non-exposed groups without education as a control

Dependent variable:	Extraversion	Agreeableness	Conscientiousness	Neuroticism	Openness
	(1)	(2)	(3)	(4)	(5)
Panel A					
Exposed vs. non-exposed group	-0.274**	-0.228*	-0.207	-0.104	-0.092
	(0.124)	(0.126)	(0.133)	(0.132)	(0.130)
Age	0.013	0.012	0.024***	0.011	-0.0001
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Male (=1)	0.036	0.001	-0.165**	-0.445***	0.021
	(0.063)	(0.064)	(0.069)	(0.071)	(0.070)
R-squared	0.024	0.021	0.043	0.087	0.031
Observations	492	492	492	492	492
Panel B					
Exposed vs. non-exposed group	-0.317**	-0.111	-0.241	0.051	-0.184
	(0.136)	(0.144)	(0.150)	(0.146)	(0.147)
Age	0.013	0.012	0.024***	0.011	-0.0001
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Male (=1)	-0.021	0.157	-0.210*	-0.239**	-0.102
	(0.101)	(0.096)	(0.109)	(0.107)	(0.115)
Male x Exposed	0.094	-0.259**	0.074	-0.343**	0.203
	(0.128)	(0.126)	(0.141)	(0.144)	(0.146)
R-squared	0.025	0.029	0.043	0.098	0.035
Observations	492	492	492	492	492

Note: The exposed vs. non-exposed group variable equals 1 for individuals born between 1960 and 1974 and 0 for individuals born between 1975 and 1982. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table A4.3: Estimates of effects of exposure to genocide on social behavior and risk using attitudinal survey questions with living in the same district since birth as a control

Dependent variable:	Trust in family	Trust in neighbors	Trust in friends	Past trusting behavior index	Risk taking regarding household finances	Being honest
	(1)		(2)	(3)	(4)	(5)
Panel A						
Exposed vs. non-exposed group	-0.243*	-0.342**	-0.206	-0.626**	-0.284	0.144
	(0.141)	(0.162)	(0.169)	(0.309)	(0.292)	(0.158)
Live in the same district since birth	-0.013	-0.003	-0.089	0.226	0.220	0.092
	(0.121)	(0.115)	(0.116)	(0.213)	(0.229)	(0.127)
Age	0.004	0.022*	0.014	0.039*	0.002	-0.00003
	(0.011)	(0.012)	(0.012)	(0.023)	(0.021)	(0.012)
Education (years)	0.017	0.038***	0.058***	0.110***	0.022	0.014
	(0.010)	(0.012)	(0.012)	(0.023)	(0.022)	(0.012)
Male (=1)	0.056	0.281***	0.171*	0.099	0.220	0.040
	(0.083)	(0.090)	(0.098)	(0.168)	(0.160)	(0.089)
R-squared	0.040	0.083	0.086	0.090	0.048	0.018
Observations	492	492	492	492	492	492
Panel B						
Exposed vs. non-exposed group	-0.450***	-0.502***	-0.438**	-0.683*	-0.280	0.084
	(0.161)	(0.181)	(0.186)	(0.352)	(0.327)	(0.176)
Live in the same district since birth	-0.030	-0.016	-0.108	0.222	0.221	0.087
	(0.120)	(0.116)	(0.118)	(0.216)	(0.229)	(0.129)
Age	0.004	0.022*	0.013	0.039*	0.002	-0.0002
	(0.011)	(0.012)	(0.012)	(0.023)	(0.021)	(0.012)
Education (years)	0.015	0.036***	0.056***	0.110***	0.022	0.014
	(0.010)	(0.012)	(0.012)	(0.023)	(0.022)	(0.012)
Male (1)	-0.219*	0.069	-0.137	0.023	0.226	-0.041
	(0.113)	(0.141)	(0.149)	(0.242)	(0.231)	(0.149)
Male x Exposed	0.459***	0.355**	0.514***	0.127	-0.009	0.135
	(0.154)	(0.178)	(0.192)	(0.338)	(0.320)	(0.189)
R-squared	0.056	0.091	0.100	0.091	0.048	0.019
Observations	492	492	492	492	492	492

Note: For the definitions of the variables, see Table 4.2. The variable of live in the same district since birth equals 1 if an individual has lived in the same district since birth and 0 otherwise. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses.
*** significant at 1%, ** significant at 5%, * significant at 10%.

Table A4.4: Differences in personality traits between the exposed and non-exposed groups with living in the same district since birth as a control

Dependent variable:	Extraversion	Agreeableness	Conscientiousness	Neuroticism	Openness
	(1)	(2)	(3)	(4)	(5)
Panel A					
Exposed vs. non-exposed group	-0.295**	-0.222*	-0.204	-0.080	-0.096
	(0.122)	(0.126)	(0.133)	(0.130)	(0.131)
Live in the same district since birth	0.089	0.164*	0.056	0.074	-0.129
	(0.079)	(0.084)	(0.092)	(0.102)	(0.086)
Age	0.021**	0.012	0.024***	0.002	0.0003
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Education (years)	0.031***	-0.003	-0.003	-0.031***	0.002
	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)
Male (=1)	-0.004	0.012	-0.159**	-0.398***	0.012
	(0.063)	(0.065)	(0.071)	(0.073)	(0.072)
R-squared	0.055	0.028	0.044	0.110	0.035
Observations	492	492	492	492	492
Panel B					
Exposed vs. non-exposed group	-0.321**	-0.100	-0.237	0.065	-0.192
	(0.134)	(0.143)	(0.151)	(0.145)	(0.148)
Live in the same district since birth	0.087	0.173**	0.053	0.086	-0.136
	(0.079)	(0.084)	(0.093)	(0.102)	(0.086)
Age	0.021**	0.012	0.023***	0.002	0.0001
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Education (years)	0.031***	-0.002	-0.003	-0.030***	0.001
	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)
Male (1)	-0.039	0.174*	-0.203*	-0.205*	-0.115
	(0.101)	(0.097)	(0.111)	(0.110)	(0.116)
Male x Exposed	0.057	-0.271**	0.073	-0.321**	0.213
	(0.127)	(0.128)	(0.141)	(0.143)	(0.146)
R-squared	0.056	0.037	0.044	0.120	0.039
Observations	492	492	492	492	492

Note: The exposed vs. non-exposed group variable equals 1 for individuals born between 1960 and 1974 and 0 for individuals born between 1975 and 1982. The variable of live in the same district since birth equals 1 if an individual has lived in the same district since birth and 0 otherwise. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Table A4.5: Tests for effects of variation in experiencing or seeing violence during the KR regime within the exposed group

	Trust		Dictator		Risk	Money Burning		Self-reporting	
Dependent variable:	% Sent	% Returned	% Given	Giving or Taking	% Invested	Burnt (=1)	Burnt (=1)	Dishonest (=1)	Dishonest (=1)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Within the Exposed Group (Born 1960–1974)									
Number of violence experienced/saw	-0.459	-0.460	-0.058	-0.027	0.876	-0.012	-0.012	0.074***	0.072***
	(1.148)	(0.942)	(1.013)	(0.022)	(1.105)	(0.022)	(0.022)	(0.020)	(0.020)
Age	0.249	-0.047	0.300	-0.003	0.534	0.008	0.008	-0.005	-0.005
	(0.364)	(0.284)	(0.331)	(0.007)	(0.340)	(0.007)	(0.007)	(0.006)	(0.006)
Education (year)	0.625	0.501*	0.671	0.011	1.210***	0.010	0.010	-0.023***	-0.023***
	(0.458)	(0.298)	(0.418)	(0.008)	(0.403)	(0.008)	(0.008)	(0.007)	(0.007)
Male (=1)	5.499*	2.668	6.747**	-0.084	8.622***	0.023	0.023	-0.070	-0.072
	(3.265)	(2.679)	(3.065)	(0.059)	(3.099)	(0.061)	(0.061)	(0.057)	(0.057)
Advantaged player							0.012		
							(0.059)		
Maximum number of correct answers									-0.085
									(0.054)
R-squared	0.022	0.012	0.037	-	0.075	0.008	0.009	0.078	0.086
Observations	296	2960	296	296	296	296	296	296	296
Panel B: Within the Exposed Group and Born During the KR regime (Born 1960–1979)									
Number of violence experienced/saw	-0.823	-0.619	-0.598	-0.012	0.028	-0.015	-0.015	0.068***	0.067***
	(1.119)	(0.888)	(0.965)	(0.021)	(1.074)	(0.021)	(0.021)	(0.019)	(0.019)
Age	0.030	-0.144	0.017	-0.002	0.010	0.007	0.007	-0.005	-0.005
	(0.274)	(0.214)	(0.241)	(0.005)	(0.266)	(0.005)	(0.005)	(0.004)	(0.004)
Education (Year)	1.078***	0.549**	0.925**	0.004	1.352***	0.010	0.009	-0.024***	-0.024***
	(0.416)	(0.273)	(0.390)	(0.006)	(0.370)	(0.007)	(0.007)	(0.006)	(0.006)
Male (=1)	7.028**	3.086	7.987***	-0.096*	8.885***	0.035	0.035	-0.072	-0.076
	(2.852)	(2.325)	(2.638)	(0.050)	(2.829)	(0.052)	(0.052)	(0.047)	(0.047)
Advantaged player							-0.007		
							(0.051)		
Maximum number of correct answers									-0.099**
									(0.046)
R-squared	0.051	0.021	0.059	-	0.078	0.010	0.010	0.083	0.094
Observations	390	3900	390	390	390	390	390	390	390

Note: For the definition of the variables, see Table 4.2. The explanatory variable of the number of violence experienced/saw equals 1 if an individual rarely experienced or saw violence during the KR regime (1-2 times), 2 if sometimes experienced or saw violence (3-5 times), 3 if often experienced or saw violence (6-10 times), 4 if very often experienced or saw violence (more than 10 times), and 0 if not experience or saw violence. Only individuals born before 1980 were asked to answer this question. Column 2 reports the coefficients, clustered by individual ID number, for the pooled percentage returned corresponding to each possible amounts that the sender might send. Column 4 reports the marginal effects evaluated at the mean from the Probit estimation when the giving or taking variable equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise. All the regressions include experimental district fixed effects. Robust standard errors are reported in parentheses. *** significant at 1%, ** significant at 5%, * significant at 10%.

Appendix B

Trust Game: Sender's role

DECISION SHEET #1

PARTICIPANT ID: _______________
TASK 1
STAGE 1

Please decide: "how much would you like to allocate to your partner?"
Please select only one option from the following:

☐ 0

☐ 1,000

☐ 2,000

☐ 3,000

☐ 4,000

☐ 5,000

☐ 6,000

☐ 7,000

☐ 8,000

☐ 9,000

☐ 10,000

Trust Game: Recipient's role

DECISION SHEET #2

PARTICIPANT ID: _______________
TASK 1
STAGE 2

The amount in column A is the amount you received from your partner. The amount is already tripled.

Please decide: "how much would you like to return to your partner?" Please write down your decision in each row in column B according to the amount you received from your partner in column A.

A	B
Amount received from your partner (already tripled)	Amount you wish to send back to your partner
3,000 	__________________Riel
6,000 	__________________Riel
9,000 	__________________Riel
12,000 	__________________Riel
15,000 	__________________Riel
18,000 	__________________Riel
21,000 	__________________Riel
24,000 	__________________Riel
27,000 	__________________Riel
30,000 	__________________Riel

Dictator Game: Giving

DECISION SHEET #3

PARTICIPANT ID: _______________
TASK 2
PART 1

Please decide: “how much would you like to allocate to your partner (using additional money 10,000 Riel)?” Please select only one option from the following:

☐ 0

☐ 1,000

☐ 2,000

☐ 3,000

☐ 4,000

☐ 5,000

☐ 6,000

☐ 7,000

☐ 8,000

☐ 9,000

☐ 10,000

Dictator Game: Giving or Taking

DECISION SHEET #4

PARTICIPANT ID: _______________
TASK 2
PART II

Please decide: “how much would you like to transfer to your partner or how much would you like to take from your partner?”
Please select only one option from the following:

☐ - 10,000 minus

☐ - 9,000 minus

☐ - 8,000 minus

☐ - 7,000 minus

☐ - 6,000 minus

☐ - 5,000 minus

☐ - 4,000 minus

☐ - 3,000 minus

☐ - 2,000 minus

☐ - 1,000 minus

☐ 0

☐ 0

☐ 1,000

☐ 2,000

☐ 3,000

☐ 4,000

☐ 5,000

☐ 6,000

☐ 7,000

☐ 8,000

☐ 9,000

☐ 10,000

Risk Game

DECISION SHEET #5

PARTICIPANT ID: _______________
TASK 3

Please decide: "how much would you like to invest in a risky business?" Please select only one option from the following:

☐ 0


☐ 1,000


☐ 2,000


☐ 3,000


☐ 4,000


☐ 5,000


☐ 6,000


☐ 7,000


☐ 8,000


☐ 9,000


☐ 10,000

Money Burning Game

DECISION SHEET #6
(For ODD ID number)

PARTICIPANT ID: _______________
TASK 4

	YOU	**Your Partner**
Initial endowment:	20,000 Riel	20,000 Riel
Gift:	5,000 Riel	0 Riel
Total income:	**25,000 Riel** 	**20,000 Riel** 

Case 1: At 5% price of reducing other person's income	
How much do you want to reduce other person's income? Please select only one option from the following:	if the price for eliminating is:
☐ 0	0
☐ 5,000 	250 
☐ 10,000 	500 
☐ 15,000 	750 
☐ 20,000 	1,000 

Case 2: At 10% price of reducing other person’s income	
How much do you want to reduce other person’s income? Please select only one option from the following:	if the price for eliminating is:
☐ 0	0
☐ 5,000 	500 
☐ 10,000 	1,000 
☐ 15,000 	1,500 
☐ 20,000 	2,000 

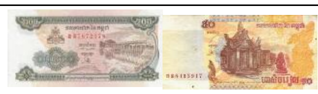
Case 3: At 20% price of reducing other person’s income	
How much do you want to reduce other person’s income? Please select only one option from the following:	if the price for eliminating is:
☐ 0	0
☐ 5,000 	1,000 
☐ 10,000 	2,000 
☐ 15,000 	3,000 
☐ 20,000 	4,000 

Money Burning Game

DECISION SHEET #7
(For EVEN ID number)

PARTICIPANT ID: _______________
TASK 4

	YOU	**Your Partner**
Initial endowment:	20,000 Riel	20,000 Riel
Gift:	0 Riel	5,000 Riel
Total income:	**20,000 Riel** 	**25,000 Riel** 

Case 1: At 5% price of reducing other person's income	
How much do you want to reduce other person's income? Please select only one option from the following:	if the price for eliminating is:
☐ 0	0
☐ 5,000 	250 
☐ 10,000 	500 
☐ 15,000 	750 
☐ 20,000 	1,000 
☐ 25,000 	1,250 

Case 2: At 10% price of reducing other person’s income	
How much do you want to reduce other person’s income? Please select only one option from the following:	if the price for eliminating is:
☐ 0	0
☐ 5,000 	500 
☐ 10,000 	1,000 
☐ 15,000 	1,500 
☐ 20,000 	2,000 
☐ 25,000 	2,500 

Case 3: At 20% price of reducing other person’s income	
How much do you want to reduce other person’s income? Please select only one option from the following:	if the price for eliminating is:
☐ 0	0
☐ 5,000 	1,000 
☐ 10,000 	2,000 
☐ 15,000 	3,000 
☐ 20,000 	4,000 
☐ 25,000 	5,000 

Self-reporting Game

TASK 5

- **Please tick ✓ in the box ☑ if you find a star ★ in the matrix.**
- **Please cross ✕ in the box ☒ if you do not find star ★ in the matrix.**

Example:

☑

✋	✋	①
⓿	★	⑩
○	❁	☺

☒

✋	✋	①
⓿	①	⑩
○	❁	☺

☐

✋	⓿	😐
①	★	①
❁	✋	❁

☐

✋	★	😐
⓿	✋	⓿
😐	☺	✋

☐

✋	⓿	⓿
✋	❁	☺
⑩	★	☺

☐

☺	✋	✋
❁	★	⓿
①	😐	①

☐

⑩	⓿	✋
⑩	⓿	✋
😐	✋	★

☐

❁	✋	❁
☺	✋	★
①	⑩	⓿

☐

⓿	★	😐
⓿	⓿	😐
✋	😐	😐

☐

☺	❁	☺
①	★	❁
⓿	❁	✋

☐

⑩	❁	😐
⓿	❁	①
✋	✋	⓿

☐

★	①	⓿
😐	①	✋
✋	⓿	①

Total number of stars found★: ....

Chapter 5: Conclusion

This chapter summarizes the main findings from each essay, discusses policy implications and suggests some directions for future research.

The first essay shows that exposure to civil conflicts during primary school age reduces the educational attainment of men and women, decreases the earnings of men and increases female completed fertility. The results suggest that educational losses lead to losses in labor productivity for both men and women. While it is difficult to conclusively rule out all other conceivable channels, our results have ruled out several plausible channels through which the conflicts might have affected earnings and fertility later in life. Specifically, we show that variation in years of conflict exposure during primary school age does not systematically explain individuals' health and quality of schooling indicators later in life. We also demonstrate that the estimates are unlikely to be affected by selective survival. Given the findings on gender differences in the effect of educational disruption on loss of earnings, there is a need to target particular groups that might be more adversely affected by conflicts during the post-conflict reconstruction period. The overall findings of the essay also have implications for events other than civil conflicts, such as natural disasters, which can result in similar losses of human capital which have potential long-term effects on earnings and fertility. The first essay could be extended by examining other outcomes. For instance, using the same framework as in the first essay, future research could explore the effect of the disruption of parents' education on offspring's education. This extension will likely be fruitful as previous studies have shown that parents' educational levels have a positive impact on the educational levels of their children (Black et al., 2005; Oreopoulos et al., 2006).

In the second essay, we show that gender-differentiated mortality during the Khmer Rouge (KR) regime has negative intergenerational effects. KR mortality predicts a lower likelihood of normal grade progression and lower height-for-age Z-scores for children born years after the conflict to parents in prime marriage age (14-29) during the conflict. We also provide evidence that parental education, income, and health, which can affect children's educational and health outcomes, are uncorrelated with mortality rates under the KR regime. Finally, we show that a low sex ratio in the parents' generation adversely affects children's outcomes and women's marriage outcomes. The findings suggest that violent conflicts can have negative intergenerational impacts which are transferred through the marriage market channel. One possible policy suggestion stemming from our results is to provide greater public education and health support to children growing up in areas where the sex ratio of the parents' generation is relatively unbalanced.

The third essay is motivated by the empirical observation that the evidence to date on the effects of civil conflicts on pro-social and risk behavior is inconclusive. This essay extends the findings of previous studies by providing insights into the effects of direct exposure to civil conflict on anti-social behavior. We find little evidence that exposure to genocide leads to dishonest and vindictive behavior. Specifically, these results become statistically insignificant when controlling for individual characteristics. Therefore, further investigation is needed to obtain conclusive evidence. Finally, this study also shows the link between the long-term effects of civil conflict and personality traits. Exposure to genocide can make individuals less extraverted and agreeable.

Our findings in the third essay suggest that civil conflicts have long-term negative effects on social capital. It is important to understand the social relations within the affected communities in the aftermath of civil conflicts. Early problem identification and interventions are important to build or restore social capital. If lower trust behavior becomes more commonplace, society can develop a low-level equilibrium of mutual suspicion and widespread opportunism (Collier, 2000; Weitzman & Xu, 1994). This situation increases the costs for all sorts of business transactions and thus generates economic inefficiencies, as discussed in the third essay. As well, many functions upon which other governments could rely simply do not work. For example, the tax collection system, the courts, accountants, and doctors all might have been corrupted by opportunistic behavior (Collier, 1999). Fafchamps (2006) suggests that investing in social capital should be seen as a complement to investing in government capacity. Thus, encouraging community participation can reduce corruption and improve government transparency and accountability.

Given the evidence from the three essays, we conclude that (1) disruption to schooling varies across cohorts but is fairly uniform across Cambodia at any given point in time because it does not vary by geographical variation in mortality during the civil conflicts; (2) the geographical variation in mortality during the KR regime significantly reduced the sex ratio and disrupted the marriage markets of prime-marriage-age individuals during the 1970s and 1980s; (3) civil conflict can affect the labor productivity of the directly affected generation through the channel of education disruption and worsen the educational and health outcomes of future generations through the channel of marriage market disruption; and (4) the direct exposure to civil conflict during childhood and early adolescence had long-lasting impacts on social and risk behavior and can affect the personality of war-exposed individuals.

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