



# The long-term effects of genocide on antisocial preferences

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## ARTICLE INFO

### Article history:

Accepted 5 August 2022

### JEL Codes:

C91

C93

D74

O12

### Keywords:

Civil conflict

Khmer Rouge

Violence exposure

Social preferences

Artefactual field experiment

## ABSTRACT

We conduct an artefactual field experiment to examine the long-term effects of exposure to violence due to the Cambodian genocide (1975–1979), during childhood and adolescence, on individuals' antisocial behaviors. Since antisocial behavior can co-exist with other preferences, we also investigate the effect of this exposure on prosocial and risk-taking behaviors. We find that as district-level mortality rates increase, individuals who directly experienced violence during the genocide period exhibit greater antisocial and risk-taking behaviors decades later. These effects are relatively muted among individuals who did not directly experience genocidal violence. The results imply significant long-term effects on antisocial and risk preferences in association with direct exposure to genocidal violence.

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## 1. Introduction

There is a growing body of literature documenting the social costs of civil conflicts, particularly their impacts on prosocial and risk preferences of adults shortly after the end of the conflict. However, less attention has been paid to the impacts of conflict on antisocial behavior. Antisocial behaviors, such as violating social rules, being deceitful, thieving and being reckless, reflect explicit decisions to harm others, sometimes even when such actions create no obvious private or societal gain. Being antisocial is therefore a distinct step away from being less prosocial as these undeniably harmful decisions can impact the moral fiber of society, leading to inefficiencies in social and economic exchanges.

The main objective of this paper is to investigate the impacts of exposure to violence due to the Cambodian genocide during childhood and early adolescence, on antisocial behaviors in adulthood decades after the genocide. While early childhood conditions have been shown to affect health and economic outcomes later in life (see, for example, Heckman et al., 2012 for a review), whether early

childhood experiences shape (anti)social preferences during adulthood has not been the focus of previous research on conflicts. Because high levels of violence exposure during childhood and adolescence can alter the neuroanatomy of different regions of the brain and disrupt the interconnections between these regions (Dark et al., 2020), and because different regions of the brain are responsible for different kinds of behaviors (Adolphs, 2008), we also assess the effects of this exposure on prosocial and risk-taking behaviors. Given that antisocial and prosocial behaviors have been observed to co-exist (Basurto et al., 2016), it is a priori unclear if violence exposure will have similar effects on antisocial behaviors and prosocial behaviors.

We posit that genocide and civil conflicts can influence the development of a child's social and risk preferences through two important channels. The first is related to their direct experience with violence. As violence escalates, children can become more exposed to frequent violence and observe others being killed in front of them. The direct witnessing of and suffering from violence can traumatize them and shape their social and risk preferences. Studies in neuroscience indicate that high levels of childhood and adolescent maltreatment and direct exposure to violence can decrease amygdala, hippocampal, and prefrontal cortex volumes in the brain as well as disrupt interconnections among the amygdala, hippocampus, and ventromedial, dorsomedial, and

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dorsolateral prefrontal cortex in adulthood (Dark et al., 2020). These regions of the brain are involved in different ways when individuals engage in various kinds of social and risk related behaviors.<sup>1</sup> Consequently, childhood and adolescent violence exposure can influence individuals' antisocial, prosocial, and risk-taking behaviors differently during adulthood through its differential impacts on various regions of the brain and the interconnections among these regions.<sup>2</sup>

The second channel is related to the economic and social disruptions that violence generates at the aggregate level. These disruptions at the aggregate level can lead to economic hardship, family instability, sense of insecurity, threat of loss, and a general sense of resource scarcity. Roux et al. (2015) argue that when resources are scarce, individuals may focus more on advancing their own welfare. Prediger et al. (2014) show that resource scarcity increases antisocial behavior that is consistent with the advancement of one's own welfare. Although selfish behavior may often be aligned with advancing one's own welfare, generous behavior may also be aligned with advancing one's own welfare when it helps to improve one's social standing (Roux et al., 2015). Thus, resource scarcity may also lead to more cooperative and sharing behaviors (Cui et al., 2022). In addition, sympathy may also evolve from a sense of connectedness with others when individuals face common threats. Groups with a shared fate may experience mutual identification, which can become a source of support (Gneezy & Fessler, 2012). In the context of risk, resource scarcity has been linked to both increased risk-taking behavior (Liang et al., 2020) as well as decreased risk-taking behavior (Guiso & Paiella, 2008). Moreover, intergenerational transmission could play a role as parents may indirectly transfer the direct effect of violence they face, to their children (Islam et al., 2017). Consequently, the development of children's social and risk preferences can be indirectly affected by violence even without them witnessing it or experiencing it first-hand, however the directions of the effects on antisocial, prosocial, and risk-taking behaviors are less clear.

Under the Khmer Rouge (KR) regime (April 1975 to January 1979), Cambodia experienced one of the worst genocide events in human history, resulting in the deaths of about 1.7 million people (about 21 % of the country's population) and leaving millions more traumatized. We use the death toll data collected by the Cambodian Genocide Program, which provides an independently verified measure of mortality rates. This measure allows us to quantify the intensity of genocidal violence and socio-economic disruptions that resulted from the violence. Given the scale and extent of the genocide, in addition to the considerable variation

in mortality rates across districts, there is also variation in individuals' personal experience with violence. We use these two types of variations to understand: (1) the effect of genocidal violence through the indirect violence-exposure channel; and (2) the combined effects of the indirect violence-exposure channel and the direct violence-exposure channel associated with different intensity levels of genocidal violence.

We conduct an artefactual field experiment in which participants consist of individuals who were directly exposed to violence and also individuals who did not witness or were indirectly exposed to violence during the genocide period. The directly exposed individuals include people born before or during the KR regime (January 1960 to January 1979 birth cohorts) and those who reported experiencing or witnessing violence during this period. The indirectly exposed group consists of individuals who did not witness or experience violence and includes both individuals who were born before or during the KR regime (January 1960 to January 1979 birth cohorts) and also individuals who were born after (February 1979 to December 1982 birth cohorts).<sup>3</sup> These participants take part in three incentivized games—money burning (capturing financially vindictive behavior), self-reporting (dishonesty)—and a dictator game with an additional taking option (opportunism), which enable us to understand their antisocial behaviors. They also participate in a number of other behavioral games such as dictator and trust games, and a risk task, which allow us to examine their prosocial behaviors and risk attitudes. To identify individuals who were directly exposed to violence, we collect data from participants on their personal experiences relating to violence during the KR period.

Our empirical strategy differentiates between how the behaviors of indirectly exposed individuals and directly exposed individuals vary with district-level mortality rates under the KR regime by exploiting the variation in district-level mortality rates and the variation in individuals' direct exposure to violence during the KR regime. In districts where KR violence was more intense, the disruptions to local economic and social activities were likely to be greater. The disruptions could negatively affect everyone in an area regardless of whether they directly experienced violence or not. District-level KR mortality rates provide a measure of the extent of this socio-economic disruption. In addition, as district-level KR mortality rates increase, the frequency and intensity of individuals being traumatized by witnessing murder, torture and other forms of violence also increase. Individuals who were indirectly exposed to violence during the KR regime experienced the socio-economic disruption but not the direct effect of violence, while individuals who were directly exposed to violence during the KR regime experienced both the disruption and the direct effect. Our empirical strategy enables us to infer the effect of the socio-economic disruption and the combined effects of both socio-economic and direct violence exposure, which helps us quantify the total effect of direct violence exposure.

We find that direct exposure to genocidal violence during childhood and adolescence increases antisocial and risk-taking behaviors in adulthood. In particular, our estimates show that as the mortality rate in a district under the KR regime increases, individuals directly exposed to violence exhibit significantly greater financially vindictive (money burning), opportunistic (dictator taking), and risk-taking behaviors in our experiment. However, their prosocial behaviors in the experiment, especially trust and trustworthiness, do not vary significantly with the mortality rates in the districts in which they resided during the KR regime. These results are related to evidence in the neuroscience literature that demon-

<sup>1</sup> For instance, Hu et al. (2015) find that bilateral striatum and the right lateral prefrontal cortex are involved when individuals help others while bilateral striatum and the left lateral prefrontal cortex are involved when individuals punish others. Functional deficits in left dorsolateral prefrontal cortex of people exhibiting antisocial behaviors have also been documented (Yang & Raine, 2009). On the other hand, Haas et al. (2015) find that the tendency for individuals to trust others is reflected in the structure of dorsomedial prefrontal cortex, whereas Aydogan et al. (2021) find that risky behaviors are associated with smaller grey-matter volumes in amygdala, ventral striatum, hypothalamus and dorsolateral prefrontal cortex.

<sup>2</sup> Different effects of the direct exposure to violence in one's youth have been documented in the literature. Bandura's (1973) social learning theory suggests that, during wartime, children may learn aggressive behaviors by observing directly the violence committed by others, who are often aggressive "role models" perceived as national heroes (Keresteš, 2006). Farver and Frosch (1996) find that children exposed to the Los Angeles riots of 1992 included more aggressive words and thematic content in stories than similar children from other parts of the country who had no direct exposure to the riots. Similarly, McAuley and Troy (1983) report that children affected by riots in urban areas of Northern Ireland exhibited higher rates of antisocial actions. There is also evidence of a lack of a negative effect as shown by Raboteg-Saric et al. (1994), who report that after the war in Croatia, children exhibited no change in instances of aggressive verbal or physical behaviors, instead they report instances of increased sharing, comforting and helping each other.

<sup>3</sup> While these individuals did not witness or experience violence, they were either present during the genocide period or experienced its aftermath, hence we refer to them as indirectly exposed (or not directly exposed).

strates the effect of high levels of violence exposure during childhood and adolescence on different regions of the brains and the interconnections among these regions that are responsible for different social and risk-taking behaviors. It is also consistent with the recent psychology literature that treats prosocial and antisocial behaviors as two distinct behavioral constructs (e.g., Krueger et al., 2001), as opposed to the early literature which considered the two as opposite sides of the same construct (e.g., Wispe, 1972).

We find that the antisocial and prosocial behaviors of individuals indirectly exposed to violence generally do not vary with district-level KR mortality rates. However, indirectly-exposed individuals become more risk taking with greater indirect exposure to violence if they continue to live in the districts in which they lived during the KR regime. Such effect on risk is not observed among those who migrated. The results suggest that migration may attenuate the long-term indirect effect of violence exposure as individuals move away from the place they experience economic hardship and resource scarcity.

Our main findings are robust across different specifications. First, we demonstrate that our results are robust to controlling for a range of predetermined district and individual characteristics. Second, we show that our results are robust to alternative assumptions about the reliability of early childhood memory and alternative definitions of directly exposed and indirectly exposed individuals. Third, we show that our results are robust to potential survivor bias. Fourth, we do not find evidence to support human capital acquisition as a channel for the effects.

This study provides unique insights into the lasting effects of conflict and violence on social behaviors and risk. It builds on recent evidence related to the impacts of civil conflicts on the social and risk-taking behaviors of affected individuals. Some studies show that individuals more affected by violent conflicts exhibit prosocial behaviors. For example, Gilligan et al. (2014), Gneezy and Fessler (2012), Voors et al. (2012), and Whitt and Wilson (2007) document effects on trust and altruism; Bauer et al. (2014) report an increase in egalitarianism; and Bellows and Miguel (2009), Blattman (2009), Bateson (2012), Gilligan et al. (2014), and Bauer et al. (2016) document effects on social, political and civic engagement. Cassar et al. (2013), Rohner et al. (2013) and Nunn and Wantchekon (2011), 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 individuals affected by war or political violence have also been examined.<sup>4</sup> In contrast to this literature, our research emphasis is on understanding the impact of violence and conflict on antisocial behavior.

Existing literature tends to focus on the impact of recent civil conflicts and on the link between civil conflicts and prosocial behaviors and risk preferences. Unlike most of the prior studies in this field, our study focuses on the *long-term effects* of exposure to genocide during childhood and early adolescence on antisocial, prosocial and risk-taking behaviors in adulthood. A number of experimental studies show that individuals' social preferences

develop over the course of childhood and adolescence (Eckel et al., 2011; Fehr et al., 2013; Harbaugh et al., 2002; Sutter & Kocher, 2007; Sutter et al., 2013). Exposure to violent conflicts during this crucial stage, during which significant behavioral development takes place and after which behavior becomes less malleable, can have long-term repercussions.

Social psychologists have documented the impacts of exposure to violence during childhood and adolescence on several social and behavioral outcomes, such as delinquency and aggression (Dubow et al., 2009; Farver & Frosch, 1996; Huesmann, 1988; Loeber, 1990; McAuley & Troy, 1983; Miller et al., 1999; Slone et al., 1999). Some other notable papers in political science and economics that document antisocial behaviors include Blattman and Annan (2010) and Cecchi et al. (2016), which provide survey-based evidence of antagonism among those exposed to severe war violence. Blattman and Annan (2010) find that child soldiers in Uganda display more hostile attitudes. Cecchi et al. (2016) find that among young street football players in Sierra Leone, those that have had more intense exposure to violence were more likely to receive a yellow or red foul card. However, there is little evidence of the impacts of conflict and direct exposure to violence during childhood and early adolescence on financially vindictive, opportunistic and dishonest behaviors decades later. Vindictive behaviors in the form of financial sabotage are costly to the economy (Murphy, 1993). Similarly, opportunism and dishonesty may encourage corruption (Collier et al., 2003) and create fraud and inefficiencies that are harmful to development.

In contrast to these past studies, our approach relies on experimental measures of antisocial behavior obtained from incentivized behavioral games. Experiments have the advantage of allowing the researcher to observe particular, well-defined types of behavior. They are also useful in revealing true preferences of participants as their decisions in these games have real (financial) consequences. Some of the antisocial actions in our experiment include, relevant yet underexplored, situations in which individuals are willing to incur a personal cost to harm others, with no financial benefit to themselves or to society. Finally, as we differentiate between two types of genocide exposure, our estimates inform the long-term effects of being directly and continuously exposed to violence as well as the long-term effects of violence-induced social and economic disruptions.

## 2. Background of the Cambodian genocide

The KR seized power in April 1975 and ruled Cambodia until January 1979. During this period, Cambodia suffered a period of genocide under the KR regime, which was characterized by massive socio-economic disruptions and destruction, violence, and death. The KR attempted to impose an extreme form of Maoist communism, forced all citizens to participate in rural work projects, often without adequate food, to build a new Cambodia based on an agrarian model. To achieve this goal, the KR aimed to destroy traditional Cambodian social norms, cultures, religions, organizations, networks and even family structures (Collier et al., 2003). The regime closed schools, hospitals, and factories, abolished banking, finance and currency, isolated the country from all foreign influences and barred Western medicine (UNESCO, 2011). It banned all religions, and people seen taking part in religious rituals or services were executed. The KR confiscated all private property and relocated people from urban areas to collective farms through agricultural labor.

Before the KR captured Phnom Penh, the population of the city had swelled to several million people between 1970 and 1975, primarily from a flood of refugees from rural provinces to escape armed conflict, aerial bombardment, and forced conscription by the KR (The Ministry of Culture and Fine Arts and The

<sup>4</sup> Recent studies show that adults' risk preferences can be altered by traumatic experiences, which can trigger fear and/or anger; however, evidence showing the direction of this impact is mixed. Callen et al. (2014) find evidence consistent with fearful recollection leading to increased risk aversion in Afghanistan. Kim and Lee (2014) find evidence that exposure to the Korean War between the ages of 4 and 8 permanently increases risk aversion. Lerner and Keltner (2001) also find an association between self-reported fear and less risky decisions. Jakiela and Ozier (2015) show, using evidence from hypothetical lottery choices, that experiencing Kenya's post-election violence sharply increased risk aversion. Sacco et al. (2003) find that after the 9/11 attack, individuals (in a different country) who were not directly exposed to the attacks made less risky decisions. Voors et al. (2012), on the other hand, find that direct exposure to violence during the civil war in Burundi increased risk-seeking behaviors.

Documentation Center of Cambodia, 2014). When the KR gained control of the country in April 1975, they evacuated all cities and residents were forced to evacuate in all directions under the constant surveillance of KR soldiers. KR called these former city dwellers and refugees the “New People” or “April 17 People”, whereas the “Base People” were those who remained in the countryside prior to KR gaining control of the country (The Ministry of Culture and Fine Arts and The Documentation Center of Cambodia, 2014). To maintain control of the population and of production, the KR created security centers and labor camps throughout the country and forced people to live in these camps under the control of the *Angka* (organization) with extremely strict rules and no freedom of movement. Most Cambodians were forced to work long hours each day with insufficient food, no material rewards, limited access to their spouses and children and very little free time (Chandler, 2008). Children faced prolonged parental absence. At the age of eight, most children were sent to live with other children, 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 deep-rooted fear (Collier et al., 2003).

A fundamental characteristic of the KR regime was the constant relocation of people by repeatedly and forcibly moving them from one place to another, often under horrific conditions (The Ministry of Culture and Fine Arts and The Documentation Center of Cambodia, 2014). Because the KR regime aimed to cleanse their perceived enemies, targeting and persecution of specific groups, such as the New People, suspected political opponents or foreign agents, ethnic minorities, individuals from high social and professional classes, and individuals who did not share the KR vision for a new Cambodia, was common during the forced transfers. To avoid being targeted, people hid their identities and tried to be as inconspicuous as possible. According to the Cambodian Genocide Database established by Yale University, approximately 1.7 million Cambodians were killed or died from starvation, exhaustion, or poor health during the KR regime. The intensity of death differed across regions of Cambodia (Islam et al., 2017). Prime-age males were more likely to be forced to do physically draining labor on collective farms and suffered from beatings and physical abuse of supervising KR officers (The Ministry of Culture and Fine Arts and The Documentation Center of Cambodia, 2014). Thus, young adult and adolescent males were the demographic group most likely to die and their likelihood of death increased significantly with age (de Walque, 2006; Neupert & Prum, 2005). Many Cambodians who survived this period either were direct victims of the regime or witnessed violence during the KR's rule. They experienced threats to and the loss of loved ones.

### 3. Research design

#### 3.1. Experimental participants

We conducted an artefactual field experiment in seven locations: Phnom Penh, Cambodia's capital city, and six rural districts in Cambodia's Kampong Cham province.<sup>5</sup> The districts were selected 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.<sup>6</sup> These dis-

tricts were chosen to reflect the representativeness of the CGD in terms of mortality rates under the KR regime.

We recruited participants through the survey contact lists from the local statistical organization. To encourage participation, we also posted flyers at local coffee shops and in the market place, and in addition advertised on social media. We hired local research assistants who received help from school-teachers, principals and village elders to recruit participants. This helped in ensuring the representativeness of the sample, as they were familiar with the age and gender distribution of their communities. In all locations, participants were required to meet the age criterion (born between 1960 and 1982). The age criterion ensures that participants were at most 15 years old at the start of the KR regime. The reason for this age criterion is that the international labor standards on child labor sets the general minimum age for admission to employment or work at 15 years of age, while adolescence falls between the ages of 10 and 19. By using 15 years of age at the beginning of the KR regime as the cutoff, we ensure that all participants were exposed to the KR regime and its aftermath for some time during childhood and adolescence.

During the recruitment process, participants were informed that they would be involved in decision making in different contexts and that they would be paid based on their decisions. We invited eligible individuals to register for the experiment a week before the actual experiment started. In each location, we conducted three experimental sessions, resulting in 21 experimental sessions. 659 individuals showed up on the days of the experiment across the seven locations. In each location, we first verified each individual's age, and randomly selected individuals if more than the required number of participants showed up in a particular location. Individuals who did not participate in this experiment but showed up were given a show up fee.<sup>7</sup> The final sample of participants consist of 492 individuals who participated in the experiment and survey in February 2014.

Overall, the individual participants in the experiment are similar to the rest of the Cambodian population in terms of basic demographic characteristics (Table 1). The mean mortality rates in the districts in which these participants resided during the KR regime are also similar to the 21 % mortality rate at the national level.

#### 3.2. Experimental games and outcome measures

Our outcome variables are drawn from incentivized experimental games. To measure antisocial, prosocial, and risk-taking behaviors, we conduct money burning, self-reporting, dictator, trust, and risk games. These games have been used extensively in the extant literature. We briefly describe the games here. We also detail the design and procedure used in each game in Appendix A and present the instructions seen by the participants in Appendix B.

To measure financially vindictive behaviors, we use a simpler two-player version of the money burning game developed by Zizzo and Oswald (2001). Participants simultaneously decide how much, if any, of the other player's total endowment to eliminate. Participants must pay from their own endowment to eliminate the other player's endowment. The fee incurred for eliminating the other's endowment is charged at three levels: 5, 10 and 20 % of the amount of the other player's endowment a player wants to eliminate. We construct a measure of vindictive behavior, measured by a dummy variable that takes the value of 1 if a person burns other players' money for at least one of the three prices of burning (5, 10 or 20 %) in this money burning game.

<sup>5</sup> Kampong Cham is one of the five largest provinces in Cambodia based on population and is 123 km from Phnom Penh.

<sup>6</sup> 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 death numbers from the DC-Cam updates. For details on the original Yale database and the Cambodian Genocide Program, see <https://www.yale.edu/cgdp/> and <https://www.dccam.org/Database/Index1.htm> for data kept by DC-Cam.

<sup>7</sup> The number of individuals who participated in each session varied slightly across locations, depending on the geographic location of the community and size of each room available to run a session.



**Table 1**  
Demographic characteristics of experiment participants.

Characteristics	Experimental sample			CSES 2011	Mean difference between Experimental Sample and CSES
	All (1)	Std. Dev. (2)	Range (3)	All (4)	p-value of diff. (t-test) (5)
Age	42.31	7.290	32–54	41.88	0.195
Male (=1)	0.46	0.499	0–1	0.47	0.636
Education (years)	7.32	4.459	0–22	7.13	0.358
Married (=1)	0.84	0.369	0–1	0.82	0.297
Khmer (=1)	0.99	0.078	0–1		
District KR mortality rate	0.217	0.153	0–0.542		
Observations	492				

Notes: Age originally reported in CSES 2011 is recoded to reflect the age in February 2014. Column (5) tests the differences of means reported in columns (1) and (4).

Attitudes towards dishonesty are measured using a variant of the self-reporting matrix task that requires participants to pay themselves when there is no probability of being detected for over-payment (Mazar et al., 2008). We design a simple task with pictures instead of numerical or word tasks to accommodate Cambodia's low literacy level. The task involves finding the picture of a star on a sheet with 10 tables, each of which has 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 one minute and pay themselves accordingly. To ensure that there are considerable and different opportunities for cheating, not all of the 10 tables have a star. We design two 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 either 4 or 7 stars per sheet allows considerable scope for cheating, even for top performers. We construct a measure of dishonest behavior, which takes the value of 1 if a participant takes more money than what they are entitled to.

We use two types of dictator games: (1) dictator game with the option to give or take (List, 2007) and (2) dictator game with the option to give only. In each dictator game, each participant plays as both player 1 (dictator) and player 2 (recipient). All participants receive an initial endowment. The dictator receives an additional endowment, while the recipient does not.

The dictator game with the option to give or take (or simply, dictator taking game) provides a measure of opportunism and selfishness. The dictator can send the additional endowment to other players or take other players' initial endowments. This means that the dictator can send either a negative amount or a positive amount. The recipient is only told the amount the dictator sends or takes if the game is selected for final payment. We construct a measure of opportunistic taking, which takes the value of 1 if an individual takes some or all of the other player's endowment in the dictator taking game.

The dictator game with the option to give only (or simply, dictator giving game) provides a measure of altruism among participants, which indicates their concern for the well-being of others (rather than self-interest). The dictator can transfer any positive amount  $x$  of the additional endowment 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 final payment. We construct a measure of altruism based on the percentage of additional endowment given to other players in the dictator giving game.

To measure trust and trustworthiness, we use the trust game (introduced by Berg et al., 1995) protocol in which each participant plays as both player 1 (sender) and player 2 (receiver) and is matched with different participants in each role. In the first stage, all participants are senders and can send any positive amount  $x$  of the total endowment to an anonymous receiver, knowing that the

experimenter triples the amount sent, such that the receiver receives an amount of  $3x$ . In the second stage, all participants play as receivers. To reduce logistical issues, each receiver decides on an amount  $y$  of  $3x$  to return to the sender for all the corresponding amounts the receiver might receive. The sender is only informed of the amount sent back by the receiver if the game is selected for final payment. We construct a measure of trust based on the percentage a sender sends and a measure of trustworthiness based on the percentage a receiver returns.

To elicit risk preferences, we use a risk game with a 50 % chance of winning or losing (Gneezy & Potters, 1997). Each participant receives an endowment and can invest any positive amount in a risky business. The investment yields triple the amount invested with 50 % probability and nothing with 50 % probability. The outcome is decided by tossing a coin. We construct a measure of risk-taking behavior based on the percentage of endowment a person invests in the risk game.

All participants were matched anonymously with another participant for payment purposes. We set different endowments and participation fees for Phnom Penh and for the rural areas in Kampong Cham province. In Phnom Penh, the endowment and participation fees are set to twice those of the rural areas, since the average earnings of workers in Phnom Penh (based on CSES, 2011) are approximately twice those of workers in rural areas.

We construct an index for antisocial behavior using the outcome measures of financially vindictive behavior, dishonesty, and opportunism and an index for prosocial behavior using the outcome measures of altruism, trust, and trustworthiness. The construction of each index follows the approach outlined by Gneezy, Leibbrandt and List (2016).

#### Antisocial index

$$= \left[ \frac{1}{3} \left( \frac{\text{amount took in dictator taking game}}{\text{maximum amount one could take}} \right) + \frac{1}{3} \left( \frac{\text{total amounts burnt of the three prices in money burning game}}{\text{maximum amount one could burn}} \right) + \frac{1}{3} \left( \frac{\text{amount took in self reporting game}}{\text{maximum amount one could take}} \right) \right] \times 100$$

#### Prosocial index

$$= \left[ \frac{1}{3} \left( \frac{\text{amount sent in trust game}}{\text{maximum amount one could send}} \right) + \frac{1}{3} \left( \frac{\text{all possible amount returned in trust game}}{\text{total possible amount one could receive}} \right) + \frac{1}{3} \left( \frac{\text{amount sent in dictator giving game}}{\text{maximum amount one could send}} \right) \right] \times 100$$

Table 2 provides summary statistics of the outcome measures collected from the experimental games.

**Table 2**  
Summary statistics of outcome variables.

Outcome variables	Obs. (1)	Mean (2)	Std. Dev. (3)	Range (4)
Antisocial index	492	13.392	13.996	0–75.556
Prosocial index	492	28.287	20.083	0–100
Risk (% of amount invested)	492	46.443	27.727	0–100
Financially vindictive (burn money or not)	492	0.467	0.499	0–1
Dishonest (take extra money or not)	492	0.309	0.463	0–1
Opportunistic taking (take money or not)	492	0.350	0.477	0–1
Altruism (% of amount given)	492	24.309	25.259	0–100
Trust (% of amount sent)	492	30.955	27.858	0–100
Trustworthiness (% of amount returned)	4920	30.908	24.782	0–100

Note: Antisocial index is composed of the behavior in the money burning, self-reporting, and dictator taking games. Prosocial index is composed of the behavior in trust game (both trust and trustworthiness) and the dictator giving game.

### 3.3. Direct exposure to violence during the KR regime

Participants also complete a post-experiment survey questionnaire. The survey covers information about participants' personal characteristics and experiences during the KR period. Participants with limited reading and writing abilities are interviewed. In the survey, we ask participants born between January 1960 and January 1979 two questions in the survey: "Did you ever see or experience physical torture during the KR regime?" and "Did you ever see someone killed during the KR regime?" If the response to either of these two questions is 'yes', we classify the individual as having been directly exposed to violence during the KR regime. Roughly 55 % of those who were born before or during the KR regime (born between January 1960 and January 1979), or 40 % of the overall sample, responded positively to this question. Note that individuals who were not directly exposed to violence were, by construction, indirectly exposed to violence, since they were either present during or after the genocide period and experienced the social and economic disruptions associated with the violence by the KR regime. These indirectly exposed individuals include those who responded 'no' to the question above (born between January 1960 and January 1979) and those born after the KR regime ended (born between February 1979 and December 1982).

### 3.4. District-level mortality rates during the KR regime

The Cambodian Genocide Database (CGD) provides information which allows us to construct district-level mortality rates during the KR regime.<sup>8</sup> To construct district-level KR mortality rates, we divide the estimated deaths under the KR regime in a given district (based on information from the CGD) by the sum of the estimated deaths under the KR regime and the estimated number of individuals who survived the KR regime in each district. The CGD includes a district identifier of each KR mass gravesite and the estimated number of bodies at each mass grave. Some graves have minimum and maximum estimates of bodies and, in such situations, we use the average of the two to construct estimated deaths. In estimating the number of individuals who survived the KR regime in each district, we utilize the Census 1998 data, instead of the Census 1962 data because the Census 1998 data contain information about each census respon-

dent's district of birth, years of residence in the current district, and previous district of residence.<sup>9</sup>

Fig. 1 illustrates the geographical distribution of KR mortality rates (shaded blue). According to the information available in the CGD, KR mortality rates in the 145 districts fall between 0 and 0.857. For districts in five provinces (Kaoh Kong, Preah Vihear, Otdar Mean Chey, Krong Kaeb and Krong Pailin; shaded white in Fig. 1), no information is available in the CGD.<sup>10</sup>

Since we ask participants in the post-experiment survey whether they have lived in the same district since birth and, if not, in which district they resided during the KR regime, we are able to identify the KR mortality rates in the districts in which they resided during the KR regime and also to link the direct exposure measure with district-level KR mortality rates. The product of direct exposure and district-level KR mortality rates provides a measure of the level of violence intensity that an individual was exposed to during the KR regime.

## 4. Empirical strategy

### 4.1. Regression specification

We are interested in examining how the Cambodian genocide may influence antisocial, prosocial and risk-taking behaviors of individuals in the long term through the indirect violence-exposure channel and the direct violence-exposure channel. We use the following empirical specification:

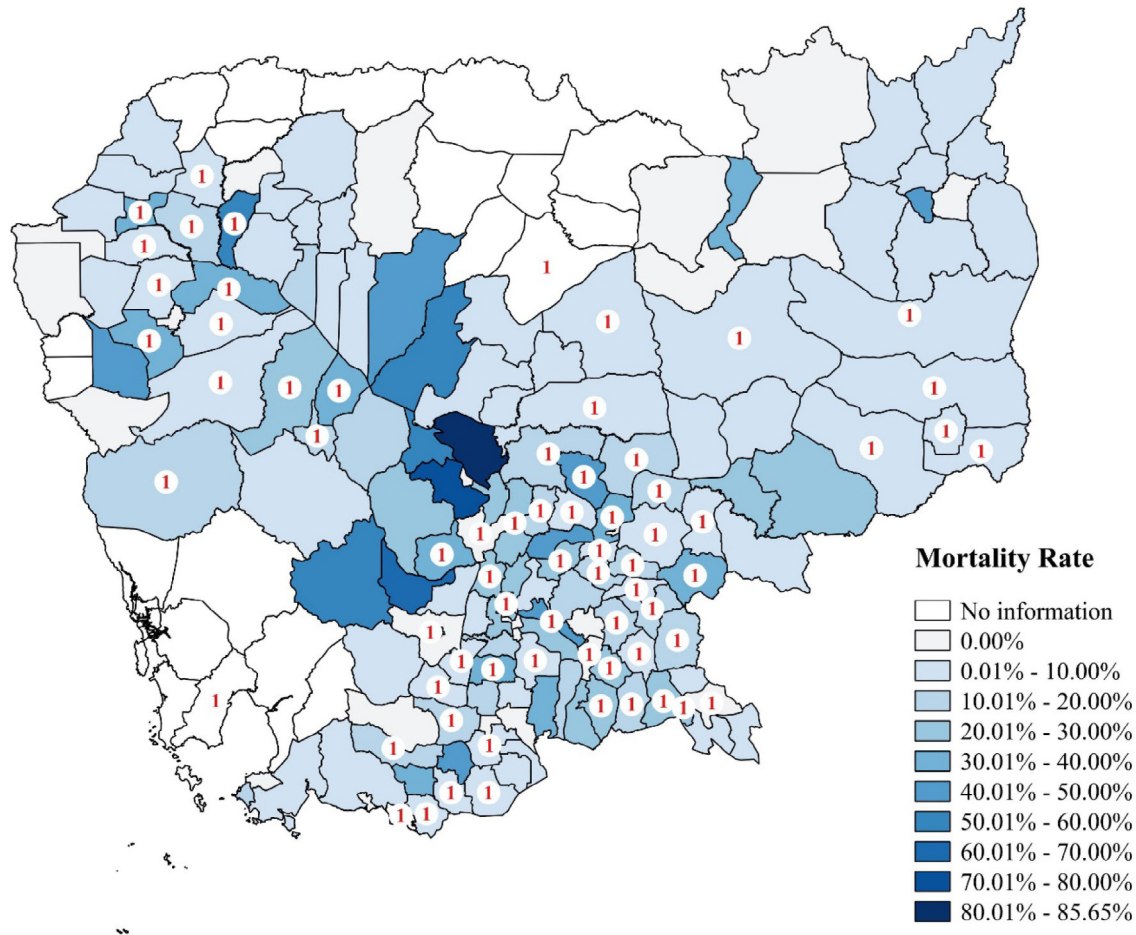
$$y_{ikj} = \beta_0 + \beta_1 KR_k + \beta_2 Exp_{ikj} + \beta_3 Exp_{ikj} \times KR_k + \delta' x_{ikj} + \epsilon_{ikj} \quad (1)$$

$y_{ikj}$  is an antisocial, prosocial or risk-taking outcome measure of individual  $i$  who resided in district  $k$  during the KR regime and participates in the experiment in district  $j$ . The variable  $KR_k$  is the mortality rate in district  $k$  during the KR regime. The dummy variable  $Exp_{ikj}$  takes the value of 1 if individual  $i$  was directly exposed to violence and the value of 0 if individual  $i$  was not directly exposed to violence (i.e., did not experience violence or was indirectly exposed to violence). Eq. (1) also includes a set of controls  $x_{ikj}$  comprising of age, a male dummy, and an indicator of whether individual  $i$  is from the group referred to as the "Base people". Given that the KR regime was significantly more likely to subject prime-age males to physically draining labor on collective farms and labor camps as well as target and persecute the "New people", who were former city dwellers and civil war refugees, it is important that we include age, gender, and "Base people" status as control variables. In addition to these three control variables, we also include an indicator

<sup>8</sup> The CGD was initially developed by Yale University and has been updated by the Documentation Center of Cambodia (DC-Cam). We use data from both sources. For details, see <https://www.yale.edu/cgp/> and <https://www.dccam.org/Database/Index1.htm>.

<sup>9</sup> Since we do not have information on the number of individuals who survived the KR regime but died before Census 1998 at the district level, the estimated KR mortality rates are noisy. Islam et al. (2017) show that alternative ways of constructing mortality rates do not differently affect the estimated impact of KR mortality rates on their outcome variables (education, earnings, fertility and health measures).

<sup>10</sup> Only three participants in our sample were in these provinces during the KR regime. We assume that the intensity of genocide they faced during the KR regime was zero. The results are not sensitive to dropping these participants from the sample or using the KR mortality rates of the districts in which they currently live.



**Fig. 1.** Distribution of KR mortality rates across districts in Cambodia. Note: Blue shaded areas are districts with information of mortality under the Khmer Rouge regime available. White circles with a red marker denote districts in which the participants in our experiment resided during the Cambodian genocide period. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

for whether the experiment was conducted in Phnom Penh, where we paid participants higher endowments and participation fees. In some specifications, we also include controls for certain features of the experiment, such as whether an individual is randomly assigned as the advantaged player in the money burning game or whether an individual is randomly assigned with a greater maximum number of correct answers in the self-reporting game.<sup>11</sup>  $\epsilon_{ikj}$  is the error term. Standard errors are clustered at the level of the districts in which the mortality rates vary (i.e., district  $k$ ).<sup>12</sup>

The coefficient  $\beta_1$  in Eq. (1) indicates how the behavior of a person not directly exposed to violence during the KR regime changes with respect to the mortality rate in the district that the person resided in during the KR regime. This effect allows us to infer the

impact of social and economic disruptions associated with different intensity levels of violence and thus informs us about the effect of the indirect violence-exposure channel of genocide during childhood and adolescence, on an individual's behavior in adulthood. The sum of coefficients  $\beta_1$  and  $\beta_3$  (i.e.,  $\beta_1 + \beta_3$ ) indicates how the behavior of a person directly exposed to violence during the KR regime changes with respect to the mortality rate in the district that the person resided in during the KR regime. Since a person who was directly exposed to violence during the KR regime also experienced social and economic disruptions associated with different intensity levels of violence,  $\beta_1 + \beta_3$  captures the combined effects of the indirect violence-exposure channel and the direct violence-exposure channel associated with different intensity levels of violence. It follows that  $\beta_3$ , the coefficient of the interaction term, captures the effect of direct exposure to different intensity levels of violence. Similar to the intercept term, the coefficient  $\beta_2$  has no specific meaning because it is unlikely for an individual to directly experience or witness violence in a district that had zero KR related deaths. We are most interested in  $\beta_1 + \beta_3$  as it informs the combined indirect violence-exposure and direct violence-exposure effects associated with different intensity levels of violence.

#### 4.2. Potential threats to drawing causal inferences

For us to draw causal inferences from Eq. (1), it is important that the variation in KR mortality rates across districts and the

<sup>11</sup> The advantaged player has a higher endowment and is introduced in the money burning game to induce inequality between the matched participants, which can lead to different motives for antisocial behavior (such as inequality aversion, envy and spite - Zizzo and Oswald, 2001). In the self-reporting game, some participants were assigned sheets where the opportunity to cheat was higher, which enables us to examine how behavior changes when the scope for being dishonest varies. These controls help us confirm that the main results remain robust to the specific design features used in these games in the literature.

<sup>12</sup> We also cluster the standard errors at the experimental session level and conduct two-way clustering of the standard errors at the experimental session level and the level of the districts in which the mortality rates vary, using Cameron, Gelbach and Miller's (2011) multiway clustering approach. The results are similar and reported in Table C1 in Appendix C. In addition, we also use an alternative dummy measure of KR mortality rate where mortality rate that is above the median is coded as one, and zero otherwise. The results are similar and reported in Table C2 in Appendix C.

variation in an individual's direct exposure to violence are both plausibly exogenous. We now discuss some potential concerns and how we seek to address them.

As mentioned in section 2, KR's strategies include the forced evacuation of all cities when it gained control of the country, implementation of forced labor camps, strict control on the movement of people, constant relocation of its population, and targeting and persecuting "New people" and other perceived enemies of the state. As people were repeatedly forced to relocate and work under horrific conditions, mortality rates were high. KR's strategies imply that the geographical distribution of deaths under the KR regime is likely to be uncorrelated with a range of predetermined (pre-treatment) locality characteristics as well as the predetermined characteristics of individuals in these localities. On the other hand, because the KR regime aimed to cleanse their perceived enemies by targeting and persecuting specific groups, such as the "New people", and forced prime-age males to engage in physically draining labor on collective farms, an individual's direct exposure to violence is likely to be correlated with their "Base people" status, age, and gender. Thus, the variation in an individual's direct exposure to violence is more likely to be uncorrelated with predetermined locality characteristics and individual characteristics after controlling for "Base people" status, age, and gender; characteristics that are more observable to the KR regime.

Table 3 demonstrates that the variation in district-level KR mortality rates is uncorrelated with a range of proxies for pre-KR district-level social and economic conditions (panels A and B) and predetermined characteristics of the experimental participants (panel C). Census 1962 and the geographical information system data allow us to construct the following proxies for pre-KR district-level social and economic conditions: the (1) pre-KR sex ratio, (2) pre-KR population density and (3) geographical distance of a district to an urban center.<sup>13</sup> The estimates in panel A are based on the full sample of districts for which the CGD provides mortality figures. The estimates in panel B are based on the sample of districts in which the participants in this study resided during the KR period. Both panels show that the 1962 sex ratios are unrelated with the KR mortality rates at the district level (column 1). The KR mortality rates are also not correlated with various measures of 1962 population density (columns 2 through 4) and distance from the provincial capital (column 5). Similarly, panel C shows that district-level KR mortality rates also are uncorrelated with predetermined characteristics of the experimental participants, including ethnicity (Khmer or not), season of birth (dry season or not), area of birth (urban or not), and school start age.<sup>14</sup>

It is important to note that even though we have only a few pre-KR district-level characteristics in panels A and B, panel D shows that these variables, especially the population density measures, are highly predictive of a range of social, economic, and health outcomes more than three decades after the genocide ended. The estimates in panels A, B and C suggest that the variation in district-level KR mortality rates is plausibly exogenous, and consistent with what KR's strategies could have led to.

Table 4 examines the correlation between the variation in an individual's direct exposure to violence and a range of predetermined district-level characteristics and individual characteristics

of experimental participants. Column 1 reports the unconditional mean difference between individuals directly exposed to violence and individuals not directly exposed to violence, while column 2 reports the conditional mean difference after controlling for age, and gender, and "Base people" status. Column 1 indicates that season of birth and school start age show significant unconditional mean differences between individuals directly exposed to violence and individuals not directly exposed to violence. On the other hand, column 2 indicates that none of the mean differences in predetermined characteristics between the two groups are statistically significant after controlling for age, gender, and "Base people" status. Therefore, Table 4 suggests that the variation in individual's direct exposure to violence is plausibly exogenous conditional on age, gender, and "Base people" status.

As we use Eq. (1) to estimate the effects of genocide on antisocial, prosocial, and risk-taking behavior, we also examine whether the key explanatory variables of interest in Eq. (1) are correlated with predetermined district-level and individual-level characteristics in Table 5. The estimates of interest are  $\beta_1$ ,  $\beta_2$  and  $\beta_1 + \beta_2$ . All of the estimates of interest are not statistically different from zero at conventional levels of significance, with the exception of  $\beta_2$  in column (1). As there are 30 estimates of interest, while only one is statistically significant at the 10 % level, the likelihood of this exception is expected. Table 5 suggests that we can quite confidently use Eq. (1) to draw causal inferences on the effects of genocide.

In our preferred specification, we also include these predetermined district and individual characteristics for several reasons. First, although Table 5 shows that the key estimates of interest are not statistically different from zero, some of the magnitudes seem large. For example, individuals from a wealthier, more urban, and better educated background seem to be exposed to less violence. If these background characteristics are negatively correlated with antisocial behavior, then the positive effect of direct exposure to greater violence on antisocial behavior will be overestimated. Nonetheless, when we include these characteristics as additional control variables, we find that our estimates of interest do not vary substantially. Second, having these additional controls help reduce the standard errors of estimates and improve statistical power.

## 5. Results

We first present the estimates for antisocial index, prosocial index, and risk-taking behavior in Table 6 as an overview of the main results. The sum of coefficients  $\beta_1$  and  $\beta_2$  in column 1 in Table 6 show that as the KR mortality rate in a district increases, antisocial behavior of individuals directly exposed to violence during the genocide period increases significantly. Since the coefficient  $\beta_1$  is not statistically different from zero, while the coefficient  $\beta_2$  is significantly positive, the estimates suggest that the effect of genocide on the antisocial behavior of directly exposed individuals in adulthood is primarily due to their direct exposure to violence during childhood and adolescence. Column 2 shows that the magnitudes of the estimated effects are not particularly sensitive to the inclusion of additional predetermined district and individual characteristics.

Table 6 further shows that as the KR mortality rate in a district increases, prosocial behavior of individuals directly exposed to violence during the genocide period decreases but the decrease is not statistically different from zero (the sum of coefficients  $\beta_1$  and  $\beta_2$  in column 3). Similarly, coefficients  $\beta_1$  and  $\beta_2$  are also not significantly different from zero (column 3). Column 4 shows that the estimated effects are not sensitive to the inclusion of additional predetermined district and individual characteristics. These estimates suggest that the long-term prosocial behaviors of individu-

<sup>13</sup> 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 replace the sex ratios and population densities for district codes not available in Census 1962 with neighboring districts' sex ratios and population densities.

<sup>14</sup> The dry season runs from October until April. As harvesting in agriculture typically occurs during the dry season in Cambodia, it serves as a proxy for socioeconomic background.



**Table 3**

Test of arguably exogenous variation in mortality rates under the Khmer Rouge regime.

District characteristic:	Sex ratio in 1962 (1)	Density in 1962 (2)	Density in 1962: Men (3)	Density in 1962: Women (4)	Distance to capital district (5)
Panel A: CGD sample Test of exogeneity of mortality rates under the Khmer Rouge regime					
KR mortality rates	−0.022 (0.017)	−814.459 (663.015)	−417.384 (335.879)	−397.075 (327.192)	4.054 (9.648)
R-squared	0.008	0.008	0.008	0.007	0.001
Observations	145	145	145	145	145
Mean of dependent variable	1.006	422.292	213.383	208.909	29.92
Panel B: Experiment sample Test of exogeneity of mortality rates under the Khmer Rouge regime					
KR mortality rates	−0.005 (0.022)	−577.343 (1172.329)	−290.417 (592.270)	−286.926 (580.062)	16.750 (16.325)
R-squared	0.001	0.003	0.003	0.003	0.014
Observations	68	68	68	68	68
Mean of dependent variable	0.996	387.435	194.801	192.635	29.929
Panel C: Experiment sample Test of exogeneity of mortality rates under the Khmer Rouge regime					
Individual characteristic:	Khmer (1)	Born in dry season (2)	Born in urban area (3)	School start age (4)	School start age (5)
KR mortality rates	0.018 (0.018)	−0.083 (0.117)	−0.163 (0.134)	0.802 (0.889)	0.667 (0.742)
No education					−8.862*** (0.169)
School start age missing					−8.890*** (0.153)
R-squared	0.001	0.001	0.015	0.002	0.635
Observations	492	492	492	397	492
Mean of dependent variable	0.993	0.392	0.043	8.902	7.183
Panel D: CSES 2004 sample of individuals born before 1950 Correlation of district variables and actual outcomes					
District characteristic:	Mean years of schooling (1)	Mean monthly earnings (2)	Mean monthly household income (3)	Illness/injury during the past 30 days (4)	Disabled (5)
Dependent Variable:					
Sex ratio in 1962	4.469** (1.760)	5.728*** (1.791)	5.834*** (0.794)	−0.314 (0.273)	−0.231 (0.287)
Density in 1962 (in 10,000)	3.521*** (0.245)	1.140*** (0.334)	1.794*** (0.153)	0.105** (0.049)	−0.054* (0.028)
Density in 1962: Men (in 10,000)	6.925*** (0.507)	2.233*** (0.675)	3.527*** (0.315)	0.206** (0.097)	−0.106* (0.056)
Density in 1962: Women (in 10,000)	7.160*** (0.476)	2.327*** (0.662)	3.649*** (0.298)	0.216** (0.098)	−0.110* (0.058)
Distance to capital district (in 10,000)	−288.508*** (42.899)	−113.597** (43.645)	−148.448*** (27.979)	3.122 (6.579)	2.543 (6.315)
Observations	141	125	141	141	141

Notes: The observations in panels A, B and C are measured at the district level and the observations in panel C are measured at the individual level. The values for the dependent variables in columns 1–4 of panels A and B are from Census 1962. In panel B, KR mortality rates are assumed to be zero for the three districts in which KR mortality data are not available (the results are similar if the three districts are excluded). In panel C, the dependent variable in each regression is a characteristic of an individual in the experimental sample. In column 4 of panel C, individuals without school start age information or never attended school are dropped. In column 5 of panel C, individuals without school start age information or never attended school have the school start age value coded as zero and the regression includes an indicator for missing-value observation and an indicator for never attended school. In panel D, each estimate came from a separate regression with one explanatory variable. Panel D shows whether the district variables used for the test of exogeneity are predictive of actual outcomes. Panel A and panel D are from [Islam et al. \(2017\)](#). Robust standard errors reported in parentheses are clustered at the district level in which KR mortality rates vary. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

als directly exposed to violence and individuals not exposed to violence during their childhood and adolescence do not respond to the intensity levels of genocidal violence in their areas.

Finally, [Table 6](#) shows that as the KR mortality rate in a district increases, risk-taking behavior of individuals directly exposed to violence during the genocide period also increases significantly (the sum of coefficients  $\beta_1$  and  $\beta_3$  in column 5). Because the coefficient  $\beta_1$  is not statistically different from zero, the estimate sug-

gests that the indirect exposure channel does not significantly affect risk preferences in the long term. On the other hand, because the coefficient  $\beta_3$  is significantly positive, but  $\beta_1$  is not, the estimates imply that the effect of genocide on the risk preferences of directly exposed individuals in adulthood is primarily due to their direct exposure to violence during childhood and adolescence.

[Fig. 2](#) illustrates the preferred estimates presented in [Table 6](#). The mean antisocial index increases with KR mortality rate for

**Table 4**

Mean differences in predetermined district and individual characteristics between individuals directly exposed to violence and individuals not directly exposed to violence.

	Unconditional mean difference (1)	Conditional mean difference (2)
Panel A: District characteristics		
Sex ratio in 1962	0.001 (0.002)	−0.001 (0.002)
Density in 1962	−130.72 (100.54)	14.860 (23.065)
Density in 1962: Men	−65.967 (50.771)	7.409 (11.651)
Density in 1962: Women	−64.748 (49.771)	7.450 (11.414)
Distance to capital district	1.365 (1.618)	−0.464 (1.280)
KR mortality rates	0.007 (0.018)	0.011 (0.019)
Panel B: Individual characteristics		
Khmer	−0.007 (0.009)	0.018 (0.066)
Born in dry season	−0.092** (0.045)	−0.000 (0.008)
Born in urban area	−0.020 (0.025)	−0.012 (0.022)
School start age	1.505*** (0.219)	0.572 (0.349)

Notes: Mean difference between individuals directly exposed to violence and individuals not directly exposed to violence during the KR period. 196 directly exposed individuals and 296 indirectly exposed individuals. Conditional mean difference is based on a regression specification that includes age, male dummy, and Base people dummy. Individuals without school start age information or never attended school have the school start age value coded as zero and the regression includes an indicator for missing-value observation and an indicator for never attended school. Robust standard errors reported in parentheses are clustered at the district level in which KR mortality rates vary. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table 5**

Tests of conditional exogenous variation in KR mortality rates, direct exposure, and their interaction.

Dependent variable:	District Characteristics					Individual Characteristics				
	Sex ratio in 1962 (1)	Density in 1962 (2)	Density in 1962: Men (3)	Density in 1962: Women (4)	Distance to capital district (5)	Khmer (6)	Born in dry season (7)	Born in urban area (8)	School start age (9)	School start age (10)
KR mortality rate ( $\beta_1$ )	0.002 (0.031)	−471.356 (415.356)	−236.446 (208.761)	−234.910 (206.607)	14.948 (16.543)	0.002 (0.010)	0.147 (0.182)	−0.211 (0.165)	0.850 (0.911)	0.604 (0.721)
Direct exposure ( $\beta_2$ )	0.003 (0.003)	−56.843 (118.418)	−28.415 (59.685)	−28.427 (58.734)	2.419 (3.625)	−0.006 (0.016)	0.127 (0.094)	−0.049 (0.047)	0.827 (0.515)	0.629 (0.404)
KR mortality rate $\times$ Direct exposure ( $\beta_3$ )	−0.019* (0.010)	346.372 (429.780)	173.105 (216.602)	173.266 (213.184)	−13.726 (12.244)	0.024 (0.038)	−0.496 (0.296)	0.178 (0.178)	−0.568 (1.821)	−0.284 (1.490)
Age	0.000 (0.000)	−16.170 (11.990)	−8.155 (6.057)	−8.015 (5.934)	0.168* (0.093)	−0.001 (0.000)	−0.013*** (0.005)	−0.000 (0.002)	0.106*** (0.023)	0.090*** (0.018)
Male	−0.001 (0.002)	86.043 (84.130)	43.594 (42.504)	42.450 (41.627)	−0.011 (0.980)	−0.014 (0.009)	0.100** (0.038)	0.002 (0.023)	0.533** (0.260)	0.424** (0.208)
Base people	0.001 (0.002)	−189.070 (119.580)	−95.215 (60.399)	−93.855 (59.181)	4.884 (3.104)	0.021* (0.012)	−0.136*** (0.040)	−0.100*** (0.033)	0.723*** (0.194)	0.615*** (0.170)
School start age missing									−8.842*** (0.221)	−9.181*** (0.173)
No education										
R-squared	0.011	0.040	0.040	0.040	0.039	0.028	0.059	0.072	0.156	0.682
Observations	492	492	492	492	492	492	492	492	397	492
KR mortality rate + KR mortality rate $\times$ Direct exp. ( $\beta_1 + \beta_3$ )	−0.017 [0.543]	−124.984 [0.338]	−63.341 [0.333]	−61.644 [0.343]	1.222 [0.959]	0.026 [0.454]	−0.349 [0.109]	−0.033 [0.718]	0.282 [0.839]	0.320 [0.784]

Notes: In column 9, individuals without school start age information or never attended school are dropped. In column 10, individuals without school start age information or never attended school have the school start age value coded as zero and the regression includes an indicator for missing-value observation and an indicator for never attended school. Robust standard errors reported in parentheses are clustered at the district level in which KR mortality rates vary. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

those directly exposed, but not for those indirectly exposed. At low levels of KR mortality rate, the mean antisocial index is similar between those directly exposed to violence and those not directly exposed to violence, as the 95 % confidence interval of the mean of the directly exposed group contains the mean of the indirectly

exposed group. However, when KR mortality rate increases above the mean (approximately 0.22), significant differences appear between the directly exposed and indirectly exposed groups as indicated by the confidence intervals. Similar patterns are observed for risk taking. On the other hand, the mean prosocial

**Table 6**  
Effects of exposure to genocide on prosocial and antisocial index and risk.

Dependent variable:	Antisocial Index (1)	Antisocial Index (2)	Prosocial Index (3)	Prosocial Index (4)	Risk % Inv. (5)	Risk % Inv. (6)
KR mortality rate ( $\beta_1$ )	-8.606 (6.649)	-7.850 (5.240)	-4.897 (10.045)	-4.935 (7.551)	11.711 (7.444)	10.676 (7.596)
Direct exposure ( $\beta_2$ )	-2.137 (2.336)	-2.860 (2.483)	-3.002 (3.212)	-3.337 (3.004)	-5.237 (3.464)	-5.464* (3.239)
KR mortality rate $\times$ Direct exposure ( $\beta_3$ )	18.620** (7.035)	20.606*** (6.821)	-2.432 (12.439)	-0.505 (11.059)	27.499*** (9.183)	27.123*** (9.666)
Age	-0.076 (0.082)	-0.071 (0.092)	-0.187 (0.132)	-0.112 (0.151)	-0.401** (0.157)	-0.329* (0.168)
Male	-2.135 (1.967)	-0.899 (1.883)	7.699*** (1.177)	7.033*** (1.232)	10.722*** (2.524)	9.695*** (2.437)
Base people	0.858 (1.779)	1.346 (1.568)	-3.547 (2.206)	-3.265* (1.909)	-9.761*** (2.745)	-9.880*** (2.752)
Phnom Penh	-6.506*** (1.920)	-7.334*** (1.652)	19.472*** (3.634)	19.712*** (3.604)	7.398* (4.156)	6.399 (4.230)
Sex ratio in 1962		16.278 (27.418)		87.854 (93.049)		-70.660 (57.057)
Density in 1962		-0.001*** (0.000)		0.001 (0.001)		0.005*** (0.001)
Distance to capital district		-0.086*** (0.026)		-0.097 (0.094)		0.024 (0.050)
Khmer		8.149 (5.108)		-6.085 (16.571)		-4.275 (13.678)
Born in dry season		1.031 (0.861)		3.150* (1.670)		0.734 (2.673)
Born in urban area		4.291* (2.303)		-9.353* (5.055)		-9.909 (7.555)
School start age		0.024 (0.204)		-0.162 (0.306)		0.137 (0.442)
School start age missing		11.014*** (2.748)		-5.020 (4.037)		-1.631 (5.413)
No education		0.550 (2.238)		-0.302 (4.476)		-0.683 (7.308)
R-squared	0.040	0.115	0.178	0.203	0.095	0.110
Observations	492	492	492	492	492	492
KR mortality rate + KR mortality rate $\times$ Direct exp. ( $\beta_1 + \beta_3$ )	10.015**	12.757***	-7.329	-5.440	39.209***	37.798***
[p-values]	[0.014]	[0.001]	[0.490]	[0.552]	[0.000]	[0.000]

Note: Columns 1 and 2 report the index for antisocial behavior which comprises of the behavior in the money burning, self-reporting, and dictator taking games. Columns 3 and 4 report the index for prosocial behavior, which is composed of the behavior in trust game (both trust and trustworthiness) and the dictator giving game. Columns 5 and 6 report the percentage of endowment invested in the risk game. Robust standard errors reported in parentheses are clustered at the district level in which KR mortality rates vary. \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%.

index of both the directly exposed and the indirectly exposed groups do not vary with KR mortality rate as the 95 % confidence interval of the mean prosocial index of the directly exposed group also contains the mean of the indirectly exposed group at the relevant range of KR mortality rate.

We now investigate which specific behaviors that we measure in the experimental games are driving the main findings for the antisocial and prosocial indices. Table 7 reports the estimates for the six different behaviors based on our preferred specification that controls for additional predetermined district and individual characteristics. It appears that the findings for antisocial index are primarily driven by financially vindictive behavior (i.e., money burning game reported in column 1) and opportunistic taking behavior (i.e., dictator taking game reported in column 3).<sup>15</sup> Interestingly, for dishonest behavior, the significantly negative coefficient  $\beta_1$  suggests that the violence-induced social and economic disruptions that a person experiences during childhood and adolescence significantly reduce their dishonest behavior in the long term. However, the statistically insignificant  $\beta_1 + \beta_3$  suggests that the total effect of social and economic disruptions and direct violence exposure on dishonesty is noisy. On the other hand, for the prosocial behaviors, none of the coefficients of interest are statistically significant (columns 4 to 6). These results are consistent with those for the prosocial index.

Overall, the findings indicate that the more violence an individual directly experiences during childhood and adolescence, the more antisocial and risk taking they become in adulthood. Prosocial behaviors in adulthood, particularly trust and trustworthiness, of those directly exposed to violence do not vary with the intensity of violence exposure. These findings relate to the evidence in the neuroscience literature that indicates high levels of violence exposure during childhood and adolescence can adversely affect various regions of the brain that are responsible for different behaviors. In our sample, it appears that financially vindictive behavior and opportunistic taking behavior in adulthood increase as a result of high levels of direct violence exposure. In contrast, the behavioral effects of the indirect exposure to violence are more muted.

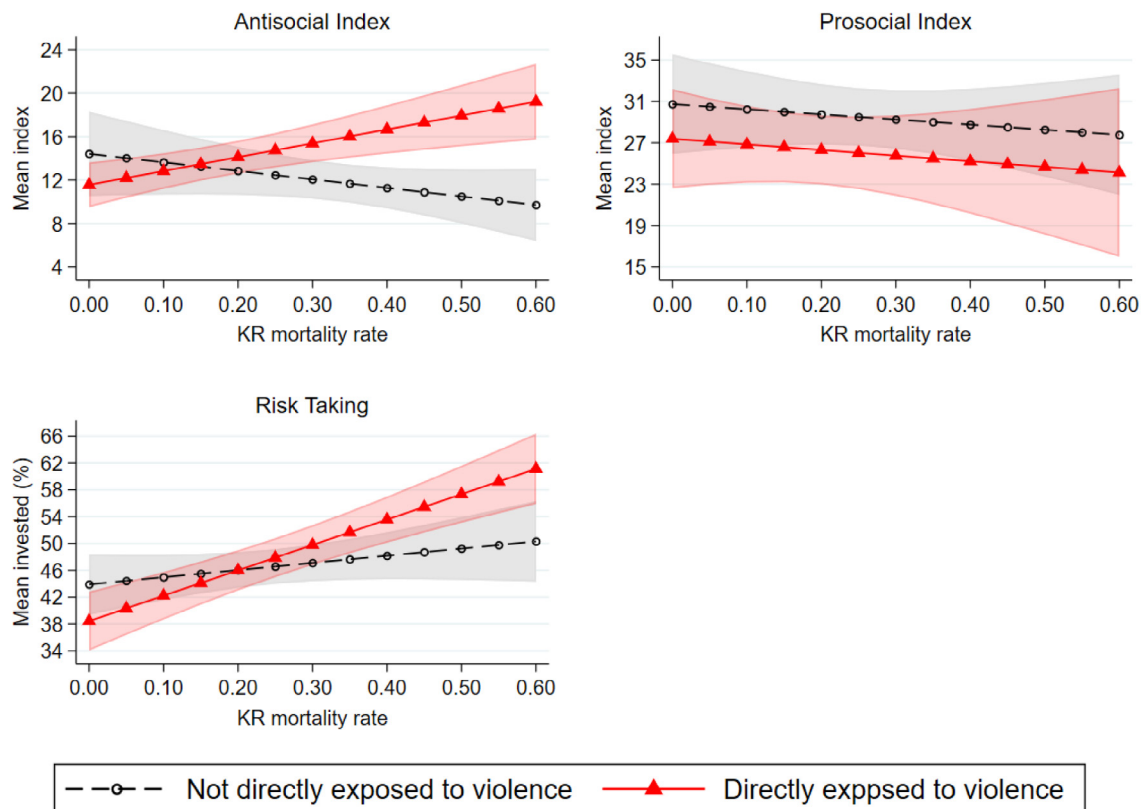
## 6. Sensitivity analysis

In this section, we briefly discuss the robustness of our results to alternative specifications. All relevant tables are reported in Appendix C.

### 6.1. Reliability of direct exposure measure and memories of early childhood events

The literature on childhood amnesia and autobiographical memory development indicates that children can remember very few events before the age of two and only some events that

<sup>15</sup> Results are similar using a probit estimation approach.



**Fig. 2.** Estimated effects of KR mortality rate on behavioral outcomes of individuals directly exposed to violence and individuals not directly exposed to violence. Notes: The means and 95% confidence intervals are based on specifications (2), (4), and (6) in Table 6.

**Table 7**  
Effects of exposure to genocide on antisocial and prosocial outcomes.

Dependent variable:	Antisocial behavior			Prosocial behavior		
	Financially vindictive (1)	Dishonest (2)	Opportunistic taking (3)	Altruism (4)	Trust (5)	Trustworthiness (6)
KR mortality rate ( $\beta_1$ )	0.037 (0.143)	-0.551*** (0.155)	-0.216 (0.199)	-2.910 (7.429)	-7.586 (9.332)	-5.268 (8.978)
Direct exposure ( $\beta_2$ )	-0.069 (0.064)	0.075 (0.068)	-0.134* (0.073)	0.087 (4.434)	-5.594 (4.124)	-3.820 (3.101)
KR mortality rate $\times$ Direct exposure ( $\beta_3$ )	0.599*** (0.167)	0.367 (0.251)	0.637*** (0.208)	-19.901 (15.281)	5.594 (11.819)	10.044 (12.433)
Individual & district controls	Yes	Yes	Yes	Yes	Yes	Yes
Experiment feature control	Yes	Yes	No	No	No	No
R-squared or pseudo R-squared	0.040	0.147	0.070	0.159	0.147	0.090
Observations	492	492	492	492	492	4920
KR mortality rate + KR mortality rate $\times$ Direct exp. ( $\beta_1 + \beta_3$ )	0.636*** [0.000]	-0.185 [0.306]	0.421*** [0.000]	-22.811* [0.085]	-1.992 [0.868]	4.776 [0.567]

Note: The outcome in column 1 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. In column 1, we also control for whether an individual is assigned as the advantaged player in the money burning game, where the advantaged player equals 1 if the player receives a gift and 0 otherwise. The outcome in column 2 equals 1 if the player takes extra money to which they are not entitled and 0 otherwise. In column 2, we also control for the maximum number of correct answers available in the self-reporting game, where the maximum number of correct answers equals 1 if the total number of correct answers in the game is 7 and 0 if total number of correct answers is 4. The outcome in column 3 equals 1 if the player decides to take some or all of the other player's endowment and 0 otherwise in the dictator taking game. The outcome in column 4 measures the percentage of additional endowment given to the other player in the dictator giving game. The outcome in column 5 measures the percentage of endowment sent by the sender in the trust game. The outcome in column 6 measures the percentage returned by the receiver and there are 10 observations per receiver because there are 10 possible values that each sender might send. Robust standard errors reported in parentheses are clustered at the district level in which KR mortality rates vary. \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%.

occurred between the ages of two and three (Howe, 2013). The number of retained memories of events increases with an individual's age at the time of the event, and mature memories begin forming around age seven (Howe, 2013). Bauer et al. (2014) also find that greater exposure to war has no measurable impact on

children below the age of seven, but that the effects are pronounced beginning at around seven years of age.

The potential unreliability of memories about early childhood events imply that: (i) some individuals may falsely recall certain events that never occurred; and (ii) some individuals may not



recall certain events, even when in reality these events had significantly influenced the development of their social and risk preferences. Both types of errors can bias our estimates of the effects of indirect exposure and direct exposure to violence. These two types of errors suggest that we can examine the sensitivity of our results to them by: (i) recoding direct experience of violence as 0 for various birth cohorts whose early childhood memories of exposure to violence are potentially less reliable; and (ii) dropping from the analysis birth cohorts whose early childhood memories are potentially less reliable.

We recode direct experience of violence as 0 for various birth cohorts whose early childhood memories of exposure to violence are less reliable and report the re-estimated results in [Table C3](#). The results suggest that our main conclusion concerning the effect of direct exposure to violence is driven by the sample of children and adolescents who were born before the KR regime began (i.e. born before 1975) and had more mature memories during the KR period.

We drop from our estimating sample the birth cohorts whose early childhood memories of exposure to violence are less reliable (<3.5 years old during the KR period) and report the re-estimated results in [Table C4](#). We find that the magnitude and direction of remain fairly robust. These results also suggest that exposure at an older age when memory is more mature tends to have a stronger effect.

## 6.2. Robustness to inclusion and exclusion of various controls

So far, we have included age, gender and “Base people” status as control variables as these variables help ensure that the variation in direct exposure is uncorrelated with a range of predetermined district and individual characteristics. Even after excluding these three key control variables, while controlling for predetermined district and individual characteristics, the estimates remain robust (see panel A in [Table C5](#)). As the KR targeted prime-age males and “New people”, the robustness of our estimates to the exclusion of these control variables imply that the results are not biased by the unobserved selection of genocide survivors.

Educational attainment can also be a potential channel through which genocide affects social and risk preferences because exposure to genocidal violence may have affected human capital acquisition, which is potentially associated with antisocial behavior.<sup>16</sup> Including educational attainment as an additional control does not materially change the results (see panel B in [Table C5](#)). Thus, there is no suggestive evidence that our estimates of the effects of exposure to genocidal violence on social and risk preferences are mediated by human capital acquisition.

## 6.3. Relocation

We further investigate whether the results are sensitive to migration by splitting the sample according to whether the individuals migrated from the districts they lived during the KR regime or their birth districts (if they were born in the post-KR period).

Panel A of [Table C6](#) reports the estimates for non-migrants and panel B of [Table C6](#) reports the estimates for migrants. For those directly exposed to violence, irrespective of whether they are non-migrants or migrants, antisocial and risk-taking behaviors increase, and prosocial behaviors do not vary with KR mortality rate; though the estimates tend to be noisier given the smaller sample size in each panel. Thus, the findings regarding the effects

of direct exposure to violence reported in [Table 6](#) and [Table 7](#) are robust to migration.

For those who were indirectly exposed to violence, risk taking increases with KR mortality rate if they are non-migrants (panel A), but not if they are migrants (panel B), which is consistent with the interpretation that persistent economic hardship and resource scarcity can increase risk tolerance. Thus, migration weakens the long-term effect of indirect exposure on risk-taking behavior. There is also a tendency for the non-migrants who were indirectly exposed, to exhibit decreased antisocial behaviors with greater indirect exposure to violence, but the estimates are generally noisy. In contrast, migrants who were indirectly exposed to violence are likely to be more financially vindictive if they experienced greater indirect exposure. Thus, it appears that staying on in the community where one experienced economic hardships and social disruptions can potentially lead to decreased antisocial behaviors against community members. These findings are consistent with the literature that argues individuals with shared fate may develop mutual identification and hence are less likely to have animosity against each other.

## 7. Conclusion

Khmer Rouge's violent enforcement of social engineering policies with the goal of transforming Cambodia into a classless agrarian utopia produced one of the worst genocides in human history. We examine the effects of exposure to this genocide during childhood and adolescence on the antisocial, prosocial and risk-taking behaviors of affected individuals in adulthood. To our knowledge this is the first study that explicitly examines the exposure to violent conflict during childhood and adolescence and its effects on antisocial behavior decades later, using more precise measures of antisocial behavior as obtained from incentivized experiments.

We find that as district-level mortality rates increase, individuals who directly experienced violence during the genocide period exhibit greater antisocial and risk-taking behaviors decades later. We do not observe any systematic effects on prosocial behaviors in our sample. The antisocial behaviors that are most adversely affected by direct exposure to violence include financially vindictive behavior and opportunistic taking behavior.

Among individuals who did not directly experience violence during the genocide period, we do not find significant variation in prosocial and risk-taking behaviors but they become less dishonest as district-level mortality rates increase. These effects further depend on individuals' migration status. If they continue to live in the districts where they lived during the KR period, they are more risk taking with greater indirect exposure, indicating that migration may attenuate the impact of economic hardships and resource scarcity on risk. If they relocated from the districts in which they lived during the KR period, they are more financially vindictive with greater indirect exposure. The findings suggest that staying in the community where one experienced economic hardships and social disruptions can potentially lead to decreased antisocial behaviors.

Importantly, our findings suggest greater direct exposure to violence during childhood and adolescence can alter individuals' antisocial preferences and that the effects persist decades after the exposure.

Although our results are robust to a variety of sensitivity analyses, such as reporting errors about direct exposure, potential unreliability of memories, potential survivor bias, and the human capital acquisition channel, there are limitations worth noting. First, our sample includes participants from 68 districts which have an average KR mortality rate that corresponds to the overall KR mortality rate of 21 % in Cambodia. Unfortunately, however, our

<sup>16</sup> Past studies tend to show a negative effect of civil conflicts on educational attainment in different countries (e.g., [Akresh & de Walque, 2008](#); [Chamarbagwala & Morán, 2011](#); [Dabalen & Paul, 2012](#); [Islam et al., 2016](#); [Leon, 2012](#); [Shemyakina, 2011](#)).

sample does not include participants from districts that have mortality rates significantly over 50 %, hence hampering our ability to understand effects of conflicts with extreme violence. In future research, it may be useful to investigate the impact of severe mortality rates on social and risk preferences. Second, it is possible that regions that experienced higher mortality received more aid or government programs in the post-KR period, which could potentially reduce the negative effect of genocide on prosocial behaviors. The presence of this sort of reconciliation effort could potentially explain why we find no effect of violence exposure on prosocial behavior. While we are unable to find information about governmental aid programs in the early post-KR period, Bockers et al. (2011) suggest that various administrations in the post-KR period made no significant attempt at reconciliation until the “Extraordinary Chambers in the Courts of Cambodia” was set up to trial senior leaders of the KR regime more than twenty years after the end of the regime. Similarly, Cambodia's history between 1975 and 1979 was rarely covered in the country's school curriculum and the first textbook about the genocide was only issued in 2009. Third, although we find that sensitivity analysis suggests that the main results are primarily driven by individuals old enough to have mature memories at the time of violence exposure, we do not have data on the timing of each person's exposure to violence to more precisely estimate the differential impacts by age of exposure.

While the KR regime could be dismissed as a uniquely horrific historic event of little general relevance to the world today, the extreme ideology and the general disregard for human life which were the defining characteristics of the Cambodian genocide and which led to human catastrophe can still be seen in several places around the world today.<sup>17</sup> Similar extreme events, for example, have been experienced in China during the Cultural Revolution, in Rwanda, in Yugoslavia and, in recent years, in Syria.

While more research is needed to explore the generalizability of our findings to other contexts, we expect our results to improve the understanding of the long-term effects of violence and how these effects operate through the indirect violence-exposure and the direct violence-exposure channels. The specific motivations of perpetrators may be unique to each conflict; however, repression and murder on a massive scale are all common elements. In sum, the lessons of the Cambodian genocide may apply well beyond the country's borders and could have implications for current events and their effects on long-term behaviors in a post-conflict society.

### CRediT authorship contribution statement

**Lata Gangadharan:** Conceptualization, Methodology, Writing, Asadul Islam: Conceptualization, Methodology, Writing, Supervision, Funding acquisition. **Chandarany Ouch:** Conceptualization, Methodology, Validation, Formal analysis, Investigation, Resources, Data curation, Writing, Visualization, Project administration. **Liang Choon Wang:** Conceptualization, Methodology, Validation, Formal analysis, Data curation, Writing, Visualization.

### Declaration of Competing Interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: There is no conflict of interest. We received funding support from the Australian Research Council, AusAID (The Department of Foreign Affairs and Trade) and Monash University.

<sup>17</sup> Extreme state ideologies have also been found to impact individuals' behaviors in other (non-violent) settings. For example, Cameron et al. (2013) find that individuals who were single children due to the One Child Policy in China exhibit different behavioral characteristics than individuals who had siblings.

### Acknowledgements

We thank the Associate Editor and two anonymous reviewers for detailed comments and suggestions. We also thank Richard Akresh, participants at the Australasian Development Economics Workshop and the ESA World meetings, and seminar audiences at Monash University and Cambodian Development Research Institute for their helpful comments. Ethics (IRB) clearance for the project was obtained from Monash University. We are grateful for generous funding support from the Australian Research Council, AusAID (The Department of Foreign Affairs and Trade) and Monash University. The usual disclaimer applies.

### Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.worlddev.2022.106068>.

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