Civil Conflict and Later Life Crime

Raisa Sara*

January 5, 2020

Abstract

In the 1980s, Peru was marred by a gruesome civil conflict that persisted for over a decade. This paper looks at the impact of exposure to conflict at different stages of childhood on criminal activity later in life. To identify effects, I exploit the temporal and geographic variation in the spread of the war across Peru. Using the birth year and birth location information from the 2016 national penitentiary population census and the 2015-2017 national household survey data, I estimate how exposure to war during different ages affects long-term criminal behavior. I find evidence that exposure to conflict during primary school going ages for men increases their probability of incarceration in adulthood. Unlike other evidences on the long-term impacts of war, exposure during early childhood does not seem to explain criminal behavior in later life in this context.

Keywords: War, Persistence, Crime, Incarceration
JEL Codes: K42, N46

*Department of Economics, Texas A&M University. Email: sara1800@tamu.edu

1 Introduction

Civil wars are sometimes described as "development in reverse". The legacies of conflicts are typically associated with the destruction of factors such as physical infrastructure, human capabilities and social capital which are crucial in the course of growth and development in a society (Collier et al., 2003). The literature on understanding the impact of these conflicts have made great strides in understanding their effects on individual welfare. Several studies found negative and significant impact on human capital accumulation in terms of health and education.[1] Prolonged and intense exposure to conflicts are also found to have behavioral ramifications in terms of trust, identity and risk preferences, which may have important implications for post conflict recovery through overall growth and future conflicts. (Cassar et al., 2011; Voors et al., 2012; Rohner et al., 2013; Jakiela and Ozier, 2019). Yet, a rapidly growing body of literature also documents the emergence of pro-social behavior with greater cooperation, altruism and trust among war exposed individuals (Bauer et al., 2016) and societal reforms in the post conflict era (Cramer, 2006). The competing evidence found in the literature poses a difficult conundrum in assessing the overall cost of these conflicts, especially in the long term. This shifted the focus of the researchers towards understanding the long-term impact of these wars in various contexts.[2] One area that has received relatively less attention in the literature is the impact of such violent conflicts on long term crime. Persistent psychological trauma, economic distress and depletion of social capital are among the many channels that may induce individuals to behave more aggressively. However, since there are mixed evidences on how conflicts affect some of these pathways, it is difficult to predict ex-ante if war exposure is associated with more violent behavior and crime in the future.

Anecdotal evidence suggest that exposure to war makes people more violence prone. The lack of evidences on crime primarily stems from the difficulty in identifying the causal impact of past violence exposure on future violent behavior. In most cases, as individuals exposed to war remain in

[1] In the short term, conflict is associated with reduced years of schooling(Akresh and De Walque, 2008; Leon, 2012) and lower health outcomes measured in terms of height, stunting, birth weights and mortality (Akresh et al., 2011; Arcand and Wouabe, 2009; Sánchez et al., 2010)

[2] In particular, studies found long-term impact on education (e.g., Akresh and de Walque, 2010; Akresh et al., 2017; Leon, 2012; Shemyakina, 2011; Swee et al., 2009), health (e.g., Akresh et al., 2012, 2017; Grimard and Laszlo, 2014), mental health (e.g., Barenbaum et al., 2004; Dyregrov et al., 2000; Derluyn et al., 2004), political participation (e.g., Bellows and Miguel, 2009; Blattman, 2009) and trust and social capital (e.g., Rohner et al., 2013; Besley and Reynal-Querol, 2014; Voors et al., 2012; Cassar et al., 2013; Whitt and Wilson, 2007; Fearon et al., 2009; Gilligan et al., 2011).

the same place, factors of the war affected area, such as weak institutions and ethnic composition, that may have initiated the conflict may also be the factor that increases violent behavior. As such, it is difficult to tease out if the long-term unlawful behaviors of these war exposed individuals are due to the conflict or the underlying characteristics of the environment that was responsible for the breakout of the war.

In this paper I explore empirically if childhood exposure to conflict makes people more violence prone in the long term. Specifically, I analyze how exposure to conflict during different stages of childhood impacts later life incarceration in Peru. In doing so, I attempt to answer two questions- ”Does exposure to conflict induced violent incidents during childhood increases an individual’s propensity to commit crime in the future?" and ”Is exposure to conflict during specific periods in childhood more sensitive than others in explaining unlawful behavior later in life?". I find evidence that individuals who were exposed to conflict during primary school age (6-11 years) are approximately 10% more likely to be incarcerated compared to their counterparts, who are individuals born in the same district but in a different cohort and individuals born during the same year but in a different district.

For this paper, I focus on the civil conflict in Peru. The Shining Path (SP), a Maoist rebel group, launched its internal conflict in Peru in 1980 to use a guerilla warfare to overthrow the government for a full communist revolution. The war lasted for about 20 years and was responsible for approximately 69,000 deaths and disappearances.

The unique structure of the war in Peru helps in the identification of the impact of conflict on future propensity of violence. First, the war was not initiated by the communities that were most affected by it. The pioneer and the main members of the movement were from different ethnic groups than the indigenous population. Second, the war eventually spread across different regions of Peru and was not concentrated in a specific region or towards a specific ethnic or social group. However, it should be acknowledged that the indigenous communities endured the bulk of the atrocities. Given that the majority of the victims were from marginalized communities and the spread of the conflict across the country was arbitrary could pose a threat to the internal validity of the estimates.

To overcome this problem, I exploit the birth district and the age at which individuals were exposed to the war, comparing individuals across the same birth year and, birth district. I assume that at the time of the conflict, the individual resided in their birth district. Using the birth districts to assign exposure to conflict instead of the district of residence insures that the results are driven by the impact of the conflict and not the post conflict characteristic of the district of residence due to selective migration. Furthermore, to insulate the estimated effect from confounders, I restrict the data to individuals born between 1970 and 1993. The literature on conflict has mostly focused on the the impact of conflict on women in the long run. This has been the case since war generally has a larger adverse impact on women than men and, also since data on women are more available than men in developing countries. However, since individuals who are imprisoned are overwhelmingly male[3], I restrict my data to men only.

For my analysis, I use the data on incarceration from the 2016 National Penitentiary Population Census and complement it with the ENAHO National Household surveys from 2015 to 2017 as representative of the non-incarcerated population in Peru. The structure of the combined data follows the design of case-control studies[4], where the incarcerated population are the cases and the non-incarcerated sample are the controls. To identify effects, I exploit the temporal and geographic variation in exposure to conflict and use the birth district and birth year to define exposure to war during different ages. Information on conflict is obtained from the Truth and Reconciliation Commission (CVR) data, which records the date and type of the incident. I only use the data on death and forced disappearances to define violent incidents during the conflict years. The treatment variable is constructed as a dummy variable, which takes a value of one if there was atleast one violent incident in the birth district of the individual during a specified period in childhood and zero otherwise. The periods considered are in utero (-2 to -1 years old), early childhood (0-2 years old), pre-school (3-5 years), primary school age (6-11 years) and secondary school age (12-16 years old).

The data set used in my analysis is a combination of the inmate census data with a sample of the general population data. This however introduces the problem of endogenous sampling since the sampling criteria is correlated with crime and therefore the error term. To correct for this bias, I use sampling weights in my regression analysis.

Furthermore, since the ENAHO data is a stratified random sample of the national population,

[3]94% of the inmates are men in my incarceration data.
[4]Commonly used to study rare events in epidemiology.

my non-incarcerated sample may not capture individuals who were born in every district in Peru. To avoid the problem of overestimating the probability of incarceration for the birth districts not included in the ENAHO national survey data, I only consider the inmate population in my analysis who were born in the districts that are found in the ENAHO data. The data restrictions and adjustments should ensure that exposure to conflict is not correlated with any determinants of future crime.

The results indicate that exposure to conflict during ages 6 to 11 years increases the probability of incarceration in the long term. Specifically, I find that exposure to at least one violent event during ages 6 to 11 years led to a statistically significant 0.0859 percentage point (10%) increase in the probability of being incarcerated for any crime during adulthood. The effect is primarily driven by violent and property crime. I perform a host of tests to show that the estimates are robust to alternative definitions of measuring violence, alternative time trend specification, different choices of birth years to truncate the data, employing a stricter criteria for inclusion of the inmate population in the analysis and the type of inmates.

Although the conflict in Peru lasted for almost two decades, the intensity of human rights violation varied across different years. Particularly, the period between the years 1983-1984 and 1988-1993 witnessed the highest number of atrocities[5]. Since these periods were abrupt and the most brutal during the war, I exploit this to see if the effects are driven by the individuals who were exposed to the war during its most intense periods. I find statistically significant and larger positive effect on incarceration. Indeed, it seems that exposure to conflict during its most violent periods maybe driving the results.

I estimate the impact of exposure to conflict during different ages for women and do not find any statistically significant effect. I also perform a cohort level analysis to estimate the impact of exposure to conflict in the birth district during specific ages for each cohort on the crime rate at the birth district-cohort level and I find a similar trend in my results. However, the magnitude of the effect is smaller than the individual level baseline specification and effects are found for ages 6 to 9 years[6].

[5] a little over 76% of all deaths and disappearances took place during this period according to the CVR conflict data.

[6] I also calculate the effect size in terms of the standard deviation of the dependent variable in the non-war birth districts and I find that the magnitude of the statistically significant effects from both the individual level and the cohort level are equivalent.

I also test for mechanisms that may drive these effects. Previous literature studying the impact of the civil war in Peru found evidence of negative effect on human capital accumulation especially in terms of health and education. In the short term, exposure to war is found to reduce schooling among children of all ages (Leon, 2012), height-for-age in early childhood (Sánchez et al., 2010) and other health outcomes during infancy (Gutierrez, 2017). In the long term, war exposure during early childhood was associated with less years of education (Leon, 2012), reduced earnings and job quality (Galdo, 2013) and reduced height among women (Grimard and Laszlo, 2014)[7]. I find some evidence that exposure to war during early adolescence is associated with lower height during adulthood and reduced earnings and employment. These effects are found using a different dataset and thus I am unable to test directly how it affects the coefficient estimate on crime. I also look at education, the probability of marriage and the probability of having a chronic disease and only find evidence of lower probability of cohabitation or marriage if exposed during later childhood. In my main regression specification, I control for these variables to see how these affect the coefficient estimate for exposure during ages 6 to 11 years on crime. I find that the magnitude of the estimate drops by about 8%. The human capital in terms of education may explain some of the variation in the propensity to commit crime although the magnitude of the indirect affect explained by education is very small.

Another potential mechanism than I cannot directly test for could be the psychological trauma from victimization or witnessing of violence. In comparison to adolescents, young children have a lower cognitive capacity to process and cope with trauma. On the other hand, very young children are believed to be more protected from the scars of trauma as they are unable to fully comprehend the negative consequences. The psychology literature suggests that children aged between 5 and 9 years are at a stage of accumulating the ability to be aware of and understand real events around them, however, lack consolidated identities during these ages which makes them particularly vulnerable to war trauma (Garbarino and Kostelny, 1996; Barenbaum et al., 2004). Studies in behavioral economics also find that that the development of pro-social preferences is particularly active before 12 years of age, specifically between 6 and 12 years (Almås et al., 2010; Bauer et al., 2014, 2018; Fehr et al., 2013). Since I find evidence of a long term impact on crime if exposed to

[7]Most of these studies, except Leon (2012), only focus on the impact of war during in utero or infancy period and ignores later exposure to war. Furthermore, most of these studies use all conflict data instead of just using death and disappearance incidents, which is less likely to suffer from misreporting, to measure exposure to conflict.

war during ages 6 to 11 year, which is also the period when their preferences and attitudes are more susceptible to violent events, psychological trauma could potentially play a role in explaining the long term outcome. Correlations between stressful life events and problems of aggression, social withdrawal and self-reported delinquent behavior are found in school-aged children and youth (Garmezy and Rutter, 1983; Attar et al., 1994; Vaux and Ruggiero, 1983). Specifically, war exposed children are most commonly reported to have elevated levels of post traumatic stress disorder (PTSD), depression, and anxiety disorders (Werner, 2012). Such symptoms of aggression during childhood may either be a precursor to or manifest into co-occurring condition to violence in later life (Loeber and Hay, 1997).

Children were also recruited by the insurgent group to join the militia mostly through coercion and threat of violence. At one point, children under the age of 18 formed about a tenth of all Shining Path militants. Children between the age of 5 and 10 years performed tasks such as delivering messages, spying and cleaning. At age 12, children were taught to use and make various weapons as well as participate in conflicts. Furthermore, these children were completely indoctrinated into the groups ideology as the member of the PCP-SL were their only source of care and education (Landis and Albert, 2012). The industry-specific skills acquired by these children may reduce the cost of participating in a criminal career in the future while the indoctrination of insurgency sentiments may distort their attitude and perception towards violence (Barenbaum et al., 2004).

To the best of my knowledge, this is one of the first papers to study the impact of childhood exposure to war on long term crime in the country of origin. In doing so, it contributes to several bodies of literature.

First, this paper complements the vast literature showing how war affects long term outcomes.[8]. While the detrimental effect of war on human capital accumulation in terms of education and health has been widely established, there remains some ambiguity in the evidences found on the impact of war on risk preferences and pro-social behavior. While some studies find evidences of pro-social behavior and increased risk aversion among the war affected individuals, competing evidences are

[8] In particular, studies found long-term impact on education (e.g., Akresh and de Walque, 2010; Akresh et al., 2017; Leon, 2012; Shemyakina, 2011; Swee et al., 2009), health (e.g., Akresh et al., 2012,?, 2017; Grimard and Laszlo, 2014), mental health (e.g., Barenbaum et al., 2004; Dyregrov et al., 2000; Derluyn et al., 2004), political participation (e.g., Bellows and Miguel, 2009; Blattman, 2009) and trust and social capital (e.g., Rohner et al., 2013; Besley and Reynal-Querol, 2014; Voors et al., 2012; Cassar et al., 2013; Whitt and Wilson, 2007; Fearon et al., 2009; Gilligan et al., 2011).

also found in other war affected context(Voors et al., 2012; Cecchi et al., 2016; Kim and Lee, 2014; Jakiela and Ozier, 2019). On the other hand, only a handful of studies evaluate the influence of conflict on anti-social behavior. Gangadharan et al. (2017) finds war exposed individuals in Cambodia to be less trusting, less altruistic and more risk averse with suggestive evidence that exposure to war during childhood and adolescence are related to long term dishonest and vindictive behavior. They also find that the war affected individuals place lower value on personality traits such as extraversion and agreeableness. Miguel et al. (2011) finds a strong association between the civil conflict and violent behavior by comparing the extent of civil conflict in a soccer player's country of origin and their behavior on field. More recently, Couttenier et al. (2016) finds evidence of higher propensity to commit crime amongst war inflicted asylum seekers in Switzerland and suggests a potential channel for this behavior to be persistence in intra-national grievances. This paper fills the gap in the literature in two important ways. First, I am able to see the long term impact of war on criminal behavior using the incarceration data which is a stronger measure of crime than other self reported measures of violent behavior. Secondly, I am able to see the long term consequence of conflict on how it affects criminal behavior for the people who remain in the country post conflict.

Second, this paper contributes to the growing literature directed toward evaluating later childhood periods as critical periods in development (Akresh et al., 2017; Leon, 2012; Van den Berg et al., 2014; Gangadharan et al., 2017). While fetal and early life is undoubtedly a very critical period that dictates long term outcomes (Currie and Almond, 2011), less attention is given to other childhood periods that may be sensitive to violent events in determining certain long term outcomes. This paper adds to this strand of literature by providing evidence that periods beyond early childhood can also be susceptible to war exposure in determining long term outcome.

Finally, this paper also contributes to the literature on crime. It speaks to the literature on how shocks during childhood affects long term criminal behavior (Sviatschi et al., 2017; Currie and Tekin, 2012; Barr and Smith, 2018). Additionally, this is one of the few papers that looks at the impact on long term criminal behavior in the context of a developing country. It also identifies that exposure to negative shocks during specific periods of childhood can be more influential in determining the propensity to commit crime in adulthood. Studies in criminology mostly focus on the association between exposure to community violence and delinquency in youth and young adults in the US (Eitle and Turner, 2002; Patchin et al., 2006). This paper, unlike the other papers, is able to make a causal link between exposure to violence during childhood on subsequent criminal activity.

The remainder of this paper is organized as follows. In the next section, I present the context of the war in Peru and Section 3 describes the data. Section 4 presents the estimation strategy and the main results, followed by Section 5 examining potential mechanisms. Section 6 shows the robustness for my main specification. Finally, Section 7 presents some discussion and limitations of the paper followed by conclusion in Section 8.

2 Background

2.1 Conflict in Peru

In the 1980s, Peru experienced a decade long intense period of violence with the emergence of an insurgent group Partido Comunista del Peru-Sendero Luminoso (PCP-SL). The group's radical ideology was greatly inspired by the a Maoist revolution in China, and their goal was to convert Peru into a communist society. Sendero Luminoso or Shining Path initiated its actions with the 1980 presidential election by symbolically burning electoral ballots in one of the poorest localities of the country.

In the early 80s, their strategy was to start a "popular war" by gaining the support of the peasant population and creating a vacuum of power in the countryside of the southern Highlands before moving to the more urban areas. Their methods included: selective killing of elected officials and members of the police force; sabotage of elections; bombing of public and private infrastructure and the destruction of electric towers. In 1983, the government sent the National Army to the south of the country to fight these groups which caused a spike in war-related casualties. This forced them to spread to other areas of the country (center Highlands and the Amazonian jungle). The intensity of the conflict greatly reduced in late 1986.

Beginning in August 1987, the atrocities of political violence worsened, when a new terrorist group, the Revolutionary Movement Tupac Amaru (TARM), rebelled against the government. In 1989, there was a second escalation of violence, as SP reorganized and attacked the major cities across the country. Although the conflict did not stop there, PCP-SL virtually lost its power in

1992-93 after its main leaders were captured and the Army intervened in the Highlands and in the Amazonian Jungle.

3 Data

3.1 Conflict Data

Between 2000 and 2002, the Peruvian Truth and Reconciliation Commission or Comision de la Verdad y Reconciliacion (CVR) underwent a massive project to document the human losses and human right violations of the war. They collected around 19,000 testimonies from either victims of the conflict or their relatives from all across Peru. It installed offices in different parts of the country from which testimonies were both received and actively collected by mobile teams assigned to visit all the regions in the country. The information collected was crossed with information collected by other organizations and the State over the 1980-2000 period. Every single instance of civil war violence was coded as an event in a given space and time and placed systematically within a sequence of events. For each recorded act of violence, there is information about the location, time, victim, and perpetrator. Overall, more than 36,000 violent events were documented.

After dropping duplicate cases and those that could not be cross-validated, the sample size drops to 23,149 individual fatalities (only disappeared or dead). Additionally, in a separate data set, the CVR also coded the testimonies as violent acts, which include detention, kidnapping, murder, extra judiciary execution, torture, rape, among others containing about 12,807 observations.

One limitation of the CVR information is that it comes from a non-random sample. The characteristics of the data-generating process make this a self-selected sample, since people voluntarily attended public hearings to tell their stories. This suggests that the data set contains the lower bound of the total incidences of the conflict.

The intensity of violence, as can be measured by the number of violent reports, is more subject to bias if some unobserved characteristics leads to a higher reporting in some areas. For my analysis I only use data on fatalities (only dead or disappeared) to define my exposure to war variables. This is to reduce the probability of incorrect treatment assignment in my estimation.

3.2 Incarceration Data

In order to study the impact of war exposure on crime, I use the data on the census of all individuals in prison in the first quarter of 2016. The National Census of Penitentiary Population (NPPC) was collected by the National Institute of Statistics and Informatics (INEI) from all 66 correctional facilities from all 25 regions in the country on approximately 77,500 inmate population. The data contains extensive information on the characteristics of the respondents as well as information on the type of crime committed, the family, social and health condition of the inmate, health condition, inmate's procedural situation, and condition of the prison.

3.3 Individual Data

To study the impact of war exposure during childhood on crime, I use the Peruvian National Household Survey (ENAHO) from the year 2015-2017 to obtain individual level data of the general population. The study universe includes all private homes and their occupants residing the the rural and urban areas of the country. Members of the armed forces currently living in barracks, camps, ships, etc or people residing in collective dwellings such as hospitals, asylum , prison etc are excluded from the sample population.

The ENAHO survey is designed to be representative at the national and regional (second administrative) level. The sampling frame uses the Population and Housing Census and the updated cartographic material for sample selection. The survey is based on a probabilistic, stratified, multistage sampling method and the sample selections were independent in each region. The primary sampling units are the population centers and the secondary sampling units are the conglomerates that have approximately 120 homes on average. The conglomerates are selected from the primary sampling unit with the probability proportional to their size and with implicit stratification which is based on several socioeconomic variables. The tertiary sampling units are the private homes. The sample selection is proportional to the size in the first and the second stage and a simple random selection in the third stage.

In order to account for the over and under-sampling of certain groups, weights are calculated for each individual using two components: the basic expansion factor and adjustment for non-response. The basic expansion factor is the inverse of the final selection probability which is the product of the selection probability in each stage. The basic expansion factors are adjusted taking into account the population projections by age groups and gender for each month of the survey and the level of the inference.

According to National Institute of Statistics and Informatics (INEI), the total non- response rate is defined as the proportions of occupied home who declined to be interviewed or were not present at the time of the interview. The overall non-response rate was 6.6% with 8% in the urban areas and 2.5% in the rural areas. Also, households in the highest socioeconomic strata had the highest non-response to interview requests of about 18.4%.

The ENAHO collects data primarily on social, demographic and economic information on a nationally representative sample of households and individual household members. It contains data on the district of birth and the date of birth for each member of the household. It also collects detailed information on the characteristics of each member of the household including information on their ethnicity, educational level, employment, marital status and health indicators. For my analysis I restrict the sample to men born between 1970 to 1993.

4 The Impact of Exposure to Conflict on Incarceration in Later Life

The choice of committing a crime is made at the individual level. To understand an individual's propensity to commit crime, ideally the crime data matched to the record of all Peruvian born in the country would be used to perform our analysis. However, due to restricted access to the National Identity document database at the National Registry of Identification and Civil Status and the lack of unique identifiers for the inmates in my data, I am presently unable to perform such a matching exercise.

An alternative could be to append the penitentiary population data with the population census data collected for all individuals living in Peru in the year 2016. This does not require matching the inmates data since the census collects information on all non-institutionalized individuals only. Lochner and Moretti (2004) uses the U.S. Census data to study the impact of education on incarceration which is similar to what is proposed here. The combined dataset should provide a snapshot of my target population[9] in a given period. Unfortunately, this is also not feasible since the census data for 2016 does not exist.

In this paper, I use the national survey of individuals from 2015 to 2017 that is representative of the national population as a proxy data set of the census and supplement that data with the penitentiary census in 2016. I exploit the sample weights of the national survey as expansion factors in my estimation to produce a "pseudo-population" under the assumption that this would be an approximate representation of the national population of the non-incarcerated population in 2016. This approach of combining data is more commonly practiced in epidemiology and political science to perform case control studies of rare events. In this context, "cases" entail the incarcerated population and the "controls" are the non-incarcerated populations. In instances where the total number of controls are several times the number of cases[10], it is argued that a much more efficient way of data collection is to collect data on all cases and select a sample of the controls from the population without losing consistency and much efficiency compared to the full sample of controls (King and Zeng, 2001).[11]

Using the national survey to construct the individual level dataset has some unique advantages in this context. First, since my survey data is collected around the same time as the incarceration data, I observe the individuals of the same birth cohort in the NPPC data and the survey data at similar ages and at the a similar point in time, which excludes any differences that may arise across years or with age. Second, both crime and survey data contains individual level information on education, ethnicity, marital status and health conditions which allows me to explore if these observable factors could explain part of the mechanism. Third, the survey data records the date of birth of the individual compared to the census data that records the age of the respondent at the time of the survey, which is more prone to error and humping.

4.1 Baseline Specification

To circumvent the issues related to selection into war, I exploit the temporal and geographic variation in the occurrences of the conflicts from 1980 to 2000 and the age of the individual at

[9]Target Population=Non-Incarcerated Population + Incarcerated Population

[10]Therefore the name rare events

[11]The epidemiology and political science literature prefers the use of non-linear binary response model such as a binomial logit model in such analysis. Section 7 discusses my preference for using a linear probability model in my main specification instead of a logit model.

which they were exposed to the war.

The main independent variable measures exposure to conflict in the district of birth during different stages of life. The stages of life includes early life as well as school going ages, as defined per the legal standards of the nation for compulsory education at different ages: in utero (-2 to -1 years old), early childhood (0-2 years old), pre-school (3-5 years), primary school age (6-11 years) and secondary school age (12-16 years old). I group the ages into these categories to reduce the impact of measurement error in age and to gain precision.

I estimate the following baseline regression equation to estimate the impact of war exposure on crime,

$$Y_{ijt} = \beta_0 + \beta_k \sum_k WarExposure_{jk} + ethnicity_{ijt} + \alpha_j + \alpha_t + \alpha_j trend_t + \varepsilon_{ijt} \tag{1}$$

where, Y_{ijt} is a binary indicator for individual i from birth cohort t and birth district j incarcerated for some crime. α_t are the birth year fixed effects, α_j are the birth district fixed effects and $\alpha_j trend_t$ are the birth district level linear time trend. By including these fixed effect, I am able to account for any unobserved heterogeneity across districts[12] and cohorts as well as differential trends across birth districts over time during the war. I also control for $ethnicity_{ijt}$ of individual i from birth cohort t and birth district j which takes a value of 1 if the individual has a native ethnic background and 0 otherwise.

$WarExposure_{jk}$ is a dummy variables which is equal to 1 if an individual from birth district j was exposed to war exposure during k stage of their life, where k is in utero (-2 to -1 years old), early childhood (0-2 years old), pre-school (3-5 years), primary school age (6-11 years) and secondary school age (12-16 years old). β_k are the coefficients of interest measuring the crime differential between cohorts exposed to conflict during k stage of their life and cohorts not exposed to conflict during any k stage of life.The identification assumption is that the criminal behavior of an individual exposed to conflict during k stage of their life would have changed similarly to an individual not exposed to conflict during any k stage of their life, absent the conflict.[13]

To minimize the influence of any confounding factor, I restrict the data to individuals born between 1970 and 1993. Also, since the the number of incarcerated population is overwhelmingly

[12] controlling for any invariant difference across the war- affect and non-war affect districts.

[13] Therefore, the identification strategy is similar to a difference-in-difference in birth district and cohort with multiple treatment based on conflict exposure during k stage of their life.

male, I only consider the male population in my main specification. In all my models, I cluster the standard error by district of birth, allowing the errors to be correlated across birth districts over time. To check the validity of my identification assumption, I perform placebo regression analysis with older cohorts and permutation test based on random reassignment of conflict exposure during childhood. These and other checks are discussed in later sections.

For my baseline specification, I use linear probability model with sampling weights. The weights are used to correct for endogenous sampling that arises since the selection criteria of respondents from these two dataset is related to the dependent variable, causing sample selection to be correlated to the error term in our regression. According to Solon et al. (2015), in such cases we should use the sampling weights to obtain correct and consistent estimates of our parameter.

To construct the weight for the combined data, first I use the sample weights from ENAHO. These sampling weights reflects the inverse probability of selection into the sample and the sampling frame is designed to replicate the target population. For my analysis, the sample for the non-incarcerated population is obtained by combining the ENAHO 2015-2017 data. Since the sampling weights are calculated to reflect the total population in each year, it needs to be adjusted such that the pooled ENAHO sample is representative of the non-incarcerated population for a given year[14]. Let the sample weight for individual i in ENAHO for year y be ω_{iy} where $y \in [2015, 2017]$. The composite weight for the pooled ENAHO survey across three years will be $w_i = \alpha_y \omega_{iy}$ where α_y is the proportion of the total observations in year y to the total sample size of the pooled data[15].

The data for the incarcerated population in my analysis includes all observations from the penitentiary census. Since the incarcerated population is represented by the 2016 penitentiary census, the probability that any individual is selected from the inmate population is equal to one. Therefore, for the weights in the combined ENAHO and NPPC data, the non-incarcerated individuals are assigned the composite weight w_i and each incarcerated individual is assigned a weight of one.

[14] I use the ENAHO surveys from 2015-2017 instead of only 2016 to increase the sample size for the non-incarcerated population and improve the precision of my estimates. However, I also perform the regression with only the 2016 ENAHO and the results are similar.

[15] $\alpha_y = \frac{n_y}{\sum_y n}$ where $y \in [2015, 2017]$. Since the design effects of the ENAHO survey data across the three years should be the same, α_y is the optimal value to obtain the composite weights (Chu et al., 1999).

4.2 Baseline Results

Column 1 in Table 2 provides the estimates for the impact of exposure to war during different ages on the probability to be incarcerated for any crime. I find evidence that exposure to war during ages 6 to 11 years or primary school age increases the probability to be incarcerated in the long term by 0.0859 percentage points. This translates into a 9.71% increase in the probability of being incarcerated over the mean and it is statistically significant at the 5% level. I also find some evidence that exposure to conflict during age 12 to 16 years may increase the probability of getting incarcerated by 0.0598 percentage points or about 6.76% over the mean.

To understand how the propensity to commit different types of crime is affected with childhood exposure to conflict, I disaggregate crimes into four broad categories. Violent indicates incarceration due to crimes that caused or may have caused some form of bodily harm which includes sexual crimes, family crimes, aggravated assaults and homicides. Organized indicates crimes that are related to trafficking (drugs, weapons, migrants etc), manufacturing illegal products or counterfeit. Property includes crimes related to theft, usurpation, extortion, cyber-crime etc. Other crimes include state crimes and financial crimes. State crimes are any crime against the state which includes corruption, riots, unrest, acts of terrorism etc. Financial crimes includes tax defraudation and customs evasion. I find evidence that exposure to conflict during primary school and early high school age increases the probability of violent crimes by about 11% and 14% respectively. Exposure during 6 to 11 years are also associated with increased probability of committing property crimes by about 9%. I do not see any significant effect for organized or other crimes. Since I consider different types of crime, I also report the statistical significance of the estimates using the Adjusted False Discovery Rate Q-values proposed by Benjamini and Hochberg (1995) which corrects for multiple comparison. Table 2 reports the FDR q-values which shows that the statistical significance of the estimate remain at the 10% level after adjusting for multiple comparison.

To test if time-series correlation in the exposure of violence are affecting my estimates, I include indicator variables for exposure to violence before birth in my baseline specification. Table A1 shows that the coefficient estimates for the pre-birth exposure to violence years are not statistically significant at the conventional levels. The coefficient estimates for exposure during ages 6 to 11 years remain statistically significant and slightly increases in magnitude compared to the estimates in Table 2.

I also report the results using only the data from ENAHO 2016 and NPPC 2016 in Table A2. I see effects for the same age groups and types of crime as in Table 2, however, the magnitude of the impacts are larger.

4.3 Impact on Crime by the Intensity of Conflict

In this section, I show that the impact of exposure to crime is robust to the definition of my main treatment variable.

4.3.1 Conflict Exposure During High Casualties years

Figure 2(a) shows that the years 1983-1984 and 1988-1993 experienced high number of casualties representing the worst periods during the conflict. To understand how war exposure during the high war intensity years impacts long term crime, I redefine war exposure in equation (1) as exposure to war during different stages of childhood during the high war intensity years (1983-1984 and 1988-1993).

Table 3 shows the result for this specification. The estimates goes up for the exposure to war during ages 6-11 years. For organized crimes the magnitude of the estimate almost doubles. This indicates that the high intensity conflict years may be mostly responsible in driving the results in my baseline specification.

4.3.2 Conflict Intensity as Years of Exposure

An alternative specification of war exposure can be defined in terms of the years of conflict exposure the individual may have experienced during that age as indicated in the independent variable label. Column (3) and (4) in Table 4 indicates the result for the estimation of equation (1) with the treatment specification as defined in this section.

The coefficient estimates indicates how an additional year of conflict exposure during those ages affects future probability of being incarcerated for any crime.

4.3.3 Conflict Intensity as Number of Casualties

Finally, I also consider measuring the intensity of conflict exposure as the total number of war incidences that took place during the ages as specified in the independent variable labels on the table. Column (5) and (6) in Table 4 indicates the result for the estimation of equation (1) with the treatment specification as defined in terms of the total incidences during the specified ages.

The coefficient estimates indicates how an additional war incident during the specified age group affects future probability of being incarcerated for any crime.

5 Potential Channels Explaining the Long Term Impact of Conflict on Crime

Studies on the economics of crime suggests that an individual's decision to commit a crime can be explained by an incentive based model. Becker (1968) argues that an individual chooses to commit an offense if their expected utility from the crime exceeds their expected utility from devoting that time to other resources. Particularly, an individual's participation in illegal activity hinges on the opportunity cost of the illegal activity compared to their return from the legal labor market holding other factors such as taste and preference for crime constant.

Lochner (2004) suggests a human capital framework to explain crime whereby the opportunity cost of crime from forgone work depends on the human capital stock of the individual. Lochner and Moretti (2004) presents empirical evidence to this theory where they find that increase in education is associated with lower probability of incarceration and arrests. Leon (2012) finds that exposure to civil conflict in Peru had long term negative impact on education for individuals exposed before or during pre-school ages. To explore if we see similar evidence in a longer term on the male population in my data, I estimate equation (1) with education as my dependent variable. Column (2) and (3) in Table 5 presents the results for the impact of exposure to conflict during different ages on years of education[16] and probability of completing secondary schooling or high school respectively. Unlike the previous study, I do not find any significant effect on schooling with exposure to conflict

[16]Following Leon (2012), I define years of education by truncating the highest possible year of education to 11 years which is the years needed to complete secondary schooling. This means that individuals who have higher that secondary education are also assigned 11 years of education.

using my preferred specification[17].

It must be noted that the structure of my data is similar to a case-control study with endogenous sampling which may cause problem if we perform auxiliary analysis with the other explanatory variables as our dependent variable. Nagelkerke et al. (1995) discusses how coefficient estimates can be distorted in these cases. If the control group (in my case the ENAHO sample) is representative of the general population, then this bias can be reduced and such auxiliary regression may be valid if using only the control sample. As such the estimates of exposure to conflict on the intermediate variables using my main data[18] may not be correct. Therefore, I also perform the analysis on intermediate variables using the ENAHO 2004-2017 data.

Another potential long term consequence of war can be on health. Grimard and Laszlo (2014) finds that in utero exposure to conflict has long term negative effect on women's height for non-migrants. The main data does not have a direct measure of height or other health measure. However, it contains a self-reported measure stating if the individual suffers from any chronic illness such as diabetes, hypertension, HIV, cholesterol etc. Column (4) in Table 5 estimates equation (1) with an indicator variable which is equal to one if the individual suffers from any chronic disease and zero otherwise. Although we see that for most of the conflict exposure treatments the coefficients are positive, indicating that exposure to war during those ages are associated with higher probability of having a chronic disease in the future, the estimated coefficients are not significant and are very small in terms of their effect sizes. I also do not find any significant effect on chronic disease using the ENAHO 2004-2017 data in Table 6 column (4).

I also use a different data set that contains height and weight measures collected for a sub-sample of the ENAHO survey population between 2007 and 2011 by the National Center for Nutrition and Food (CENAN). Using the measures of weight and height as the dependent variable in equation (1), I estimate the impact of exposure to conflict during different ages. Column (1) and column (2) in Table 9 shows the impact of conflict exposure for men on height and weight respectively. We can see that exposure during adolescent years is associated with a 0.77cm decrease in height as adults

[17] I check the impact of conflict exposure on education using similar specification (province level cubic trends) and conflict data (including all incidents during war- rape, torture, death, disappearance etc) as Leon (2012) using only the ENAHO data from 2004 to 2017 and find results that are similar to his findings. I see that there is long term negative impact on education with exposure to war during early childhood. However, the impact is strong for women and not for men as can be seen in Table A12

[18] NPPC 2016 combined with ENAHO 2015-2016

among men. This result is consistent with Akresh et al. (2017), where they discuss adolescence as an intense period of growth and any nutritional shortages due to war during that period can have long term consequence in health outcomes[19]. Height can affect crime through its association with cognitive skills that can determine labor market outcomes (Vogl, 2014). In Table 8, I find support for this potential channel as exposure to war during the high school years is associated with lower probability for being employed and also lower earnings.

Social stability can also reduce the probability of engaging in criminal activity. Laub and Sampson (1993) discusses how childhood anti-social behavior that attenuates adult social bonds such as job security and marital cohesion can have important effect on an individual's choice of committing crime. Table 5 column (1) shows the impact of exposure to violence on the probability of cohabition or marriage. Exposure to conflict is associated with a decrease in the probability of marriage, albeit statistically insignificant. I also perform the same analysis using the pooled ENAHO data from 2004 to 2017 and find evidence that exposure to conflict during primary school and high school ages are associated with lower likelihood of marriage. Table 6 column (1) shows that exposure to conflict at 6 to 11 years is associated with a 4.2% decrease in the likelihood of marriage and exposure during 12 to 16 years shows a 3% decrease. The coefficient estimates are statistically significant at 5% and 10% level respectively.

To see if some of these variables have an indirect effect of war exposure on crime I add the intermediate variables in my baseline specification to see how my estimates of interest change. We can see in Table 7 that the magnitude of the coefficient estimate for exposure at 6 to 11 years goes down slightly when I control for marriage, education and chronic disease. I also lose some statistical significance from 1% to 5%. Therefore these variables may only explain very small indirect effect in understanding how exposure to conflict affect later life crime.

6 Robustness Checks

In this section, I show that the baseline specification is robust to a battery of tests.

[19] Although there have been several studies that document how height responds to early life nutrition (Deaton, 2007; Bozzoli et al., 2009), in this case we do not see any impact of war exposure during the critical period of early childhood on adult height. This may indicate parental behavior in shifting the limited resource at the time of war towards the younger child

6.1 Regression with Alternative Trends Specification

In my main specification, I control for district specific linear time trend to control for any underlying divergence or convergence in the outcome across birth districts over time. However, these trends may not be linear over time and in this section I test if my baseline coefficient estimates are robust to other forms of time trends across different administrative units.

In Table A3 column (2), I control for district specific quadratic time trends allowing for a more flexible trend over time across districts. In column (3), similar to Leon (2012), I control for a province level cubic time trend instead of a district level linear trend to account for any differential developments across birth provinces over time. Finally, in column (4), I include a birth region by birth year fixed effects, which allow me to control for any time-varying changes across region. Across all specification, the coefficients estimates for exposure to conflict between 6 to 11 years are statistically significant and similar in magnitude.

6.2 Data Selection of Incarceration Census

For my analysis, I rely on the sample weight from the survey data to provide me with an approximate representation of the non-incarcerated population in Peru in 2016. The sample weights in the ENAHO survey are adjusted taking into account population projections by age groups and sex for each month of survey and levels of inference proposed in the sample design.

Since the ENAHO data contains a random sample of individuals representing the population, by design of the sampling procedure we may not have individuals in the data who were born in all districts in Peru. Since I measure exposure to war at the birth district level, for my main analysis, I only consider using data from the penitentiary census of individuals who has the same birth districts that are in the ENAHO data for male born between 1970 and 1993.[20] However, since the sample weights in the survey data does not account for the birth districts of the individuals in the survey, this may impact the magnitude and variance of my estimates.

To see if my estimate is robust to data selection of the incarcerated population, I run the regression with stricter data selection conditions. Table A4 contains the estimates with the alternate sample selection conditions. Column 1 contains the results from the baseline specification with pre-

[20]For our analysis, we restrict our data to male born between 1970 and 1993.

birth exposure indicators in Table A1 column (1). Column 2, only contains the individuals from the the penitentiary data if there is atleast one observation in the ENAHO data, for male born between 1970 and 1993, with the same birth district and the same last district of residence or current district. For Column 3, I only keep observations of inmates who have the same birth district and birth year as in the ENAHO data. In column 4, I keep observations in crime data that matches on birth district, birth year and district of last residence from the ENAHO data. For Column 5, I do not impose any restriction and include all data for male born between 1970 to 1993 from the NPPC.

Overall, I see the same pattern across all specifications assuring the robustness of my estimate to the choice of the incarceration data used for my analysis.

6.3 Alternative Truncation of Birth Years

To reduce the impact of confounding factors, the main analysis restricts the data to individuals born between 1970 and 1993. The choice of these birth years are arbitrary and were chosen to ensure that individuals in the data are exposed to the high intensity conflict years during the key ages. To test if the coefficients estimates are sensitive to the choice of birth year, I run the baseline regression with alternative choice of birth years to be included in the analysis. Table A5, shows the results for the impact of conflict exposure to any crime for different range of birth years as indicates by the column names. We can see that, the coefficient estimates for our main regressor, 6 to 11 years, is similar across different birth cohorts considered in the regression[21].

6.4 Impact of Conflict on Incarceration by Type of Inmates

The penitentiary census comprise of two type of prisoners: pre-trial detainees and sentenced inmates. Pretrial detainees are awaiting trial whereas the sentenced inmates are the convicted criminals who are serving their time. The composition of individuals in these two groups may be different and may indicate different types of crime. Zevallos (2016) discusses the presence of potential bias of judges in the pre-trial stage against lower socioeconomic individuals in determining their placement in pre-trial detention. These bias detentions, however, usually results in the release of the detainees within a few months owing to insufficient grounds for imprisonment. This threatens the validity

[21]Column (4) has the most conservative consideration for the birth cohorts and is the only produces a coefficient estimate that is statistically non- significant and smallest in magnitude.

of my results if exposure to conflict is correlated with lower socioeconomic status and the impact found on crime is an artifact of the judge bias and not the effect of violence. To test the validity of my results, I perform my main regression specification for the two groups separately. Since the bias is observed only in the pre-trial stage, if the estimates are only significant for the detainees, then this may be a cause for concern. However, in Table A6, we see that the impacts are driven by both groups and therefore I can claim that the results are unlikely to be driven solely by selection into the prison of low socioeconomic individuals due to judge bias.

6.5 Cohort level Specification

A common way of defining crime in the literature is the crime rate at the cohort level. Papers looking at the long term effect on crime in the past have used observations at the cohort level (Couttenier et al., 2016; Sviatschi et al., 2017). In this section I estimate the following regression model to see how exposure to conflict during different ages in the birth district affect the birth district and cohort level propensities to commit crime.

$$Y_{jt} = \beta_0 + \beta_k \sum_k WarExposure_{jk} + \alpha_j + \alpha_t + \alpha_j trend_t + \varepsilon_{ijt} \tag{2}$$

The outcome of interest Y_{jt} is crime rate for cohort t born in district j which is defined as follows: $Crime\ Rate_{jt} = \frac{Number\ of\ Inmates_{jy}}{Population_{jy}}$, where $Number\ of\ Inmates_{jy}$ is the number of incarcerated individuals in 2016 born in year y and birth district j and $Population_{jy}$ is the total number of individuals (2007 census) in birth cohort y born in district j.

$WarExposure_{jk}$ is a dummy variables which is equal to 1 if an individual from birth district j was exposed to war exposure during k stage of their life, where k is in utero (-2 to -1 years old), early childhood (0-2 years old), pre-school (3-5 years), primary school age (6-11 years) and secondary school age (12-16 years old). α_t are the birth year fixed effects, α_j are the birth district fixed effects and $\alpha_j trend_t$ are the birth district level linear time trend. Following Couttenier et al. (2016), the cohort level regression is weighted by the size of the cohort as recommended by Angrist and Pischke (2009) for grouped data..

Figure A2 shows the estimates for the different types of crime. Although I see a similar pattern compared to the baseline specification in Table 2, however, I do not find any statistically significant estimates and the magnitude of the estimates are smaller.

Figure A1 has the estimates by grouping the exposure age by 2 years. It also includes dummy variables for exposure years before birth. With further disaggregation of my treatment variables, I find that at the cohort level, exposure to conflict from age 6 to 9 years increases the probability of incarceration in the future. The magnitude of the effect with the cohort level data, however, is lower than at the individual level[22] . I perform the regression analysis using cohort sizes from both 2007 and 2017 census data.

6.6 Drug Industry

Peru is one of the largest coca producing industry in the world. Although, coca leaves have deep spiritual and social value amongst the indigenous population, the remote agricultural valleys producing coca became the centers for drugs trafficking of cocaine. During the 1980s, with the growth of the drugs cartels, Peru was also the target of US anti-drug policies.

In the 1980s, PCP-SL also became established in the drug producing regions. Control of these regions enabled them to make significant profit and the funds received here were vital in their expansion throughout the country.

Furthermore, paper by Sviatschi et al. (2017) finds that ages 11 to 14 years are most sensitive to expansion to the drugs industry as they are used as child labor. She further finds evidence that drug expansion during these years predicts increase in the propensity to commit crime in the future. Although, the birth years used for that paper are not similar the ones used in this paper, there are overlaps. One can argue that the effects I find in this paper are driven by the drug industry and not by exposure to conflict.

To test the robustness of my estimates from the drug industry, I run my baseline specification excluding the drug producing regions. The coca producing districts are identified using the 1994 Agriculture Census. Table A8 shows that my estimates are robust to exclusion of the drug producing regions.

[22] I find that exposure to conflict between 6 to 9 years are associated with about 4% increase in the incarceration rate at the cohort and birth district level

6.7 Falsification Tests

To test my identification assumption, I perform a falsification analysis in this section. I conduct a permutation-based test on the baseline specification by randomizing the exposure to conflict during different ages during childhood. Following Couttenier et al. (2016), I conduct a Monte Carlo simulation by randomly reassigning exposure to conflict during the different age groups according to a binomial distribution based on the observed proportions of exposure in each age group in the data. All characteristics other than the exposure to conflict explanatory variables remains the same as in the data. I estimate the baseline specification with the randomly reassigned treatment repeatedly and estimate the coefficients of the exposure to conflict variable 1000 times. The kernel density plot of the estimated coefficients for each of the exposure to conflict explanatory variables for different ages in Figure A3. As can be seen from plot (3), which shows the distribution of the coefficient estimate for exposure to conflict during 6 to 11 years of age, the probability of spuriously estimating a coefficient larger than my estimated coefficient of 0.00086 from the baseline is close to zero.

I also conduct a placebo test using alternative placebo war dates. I assume the placebo war took place 30 years before the actual war and repeat by baseline specification for an older cohort of males born between 1940 and 1963. Table A7, show the results for the placebo regression analysis. We see that the coefficients are not statistically significant and small in magnitude. It must be noted that this is not a pure placebo group. The individuals in these cohort although were not exposed to war during childhood, were exposed when they were older[23]. Therefore, this exercise can also be seen as a test to confirm our hypothesis that exposure to war is especially detrimental for children.

7 Discussion

Measurement Error in the Treatment Variables

The under-reporting in the violence data that is used to create the exposure to conflict variables may cause an underestimation of my coefficients estimates due to the problem of measurement error. To reduce the probability of measurement error in my regressor, I define my treatment variable as

[23] Between ages 28 and 46

exposure to atleast one violent incident during the age group in the variable. Firstly, more violent incidences such as death and disappearance are less likely to be reported with error compared to acts of sexual violence and tortures. Furthermore, I do not use the total number of incidences as my treatment variable which is more likely to suffer from the problem of underreporting. Finally, the aggregation of age groups to create the explanatory variable measuring exposure to conflict reduces the chances of incorrectly assigning treatment since even with the problem of self-selection into reporting and underreporting, the likelihood of recording atleast one violent incident when there were violent attacks within a range of years increases considerably. However, we should still interpret the coefficient estimates as a lower bound.

Misclassification Error

Another possible bias may arise from the misclassification of my dependent variable. For my analysis, the incarceration data used was collected from the first quarter of 2016. I assume that the control data of the non-incarcerated population can be represented by the ENAHO 2015-2017 data. However, there may be individuals in the ENAHO data who were released after being incarcerated at some point. This would lead us to assign an individual as a non-incarcerated (=0) person, when infact they were incarcerated (=1). Generally classical measurement error in the dependent variable does not lead to bias although it inflates the standard error. However, if the dependent variable is a binary variable, as it is in my case, any misclassification of the variable leads to non-classical measurement error and cause bias (Hausman, 2001). In this paper, the data used has misclassification error only in terms of false negatives, which means that I may incorrectly assign 0 when the true value of the dependent variable should be 1. In this case, irrespective of the nature of the misclassification being conditionally random[24], it will attenuate the coefficient estimates (Meyer and Mittag, 2017). The bias in the linear probability model can be corrected using a closed form solution but it requires information on the conditional probability of assigning false negatives in the data. Unfortunately, I do not have that information. This again indicates that my estimates should be an underestimate of the true effect of exposure to conflict in the population. Meyer and Mittag (2017) discusses that one can still infer the sign of the coefficients from the data with misclassification.

[24]"Conditionally random" means that the misclassification is independent of the covariates (Hausman et al., 1998).

Migration

A potential concern that may cause bias in my coefficient estimates is migration. Anecdotal evidence suggests that individuals who were displaced as a result of war were discriminated in the cities they migrated. In my analysis, I assume that an individual who was born in a specific district lived there during the time of war. The direction of the bias that may result from migration is ambiguous. If the estimates are driven by the individuals who migrated due to war, the effect on long term crime may be a result of discrimination and not exposure to violence. In this case, I would be overstating the effect of conflict on future crime. Since the data does not have information on the migration history of individuals, I cannot test for this bias. The data only contains information on birth district and the last district of residence. Migrants with this data can be defined as individuals whose last district of residence is different from their birth district. However, conducting sub-sample analysis based on this definition does not account for post conflict migration. If individuals exposed to war with lower social capital are more likely to migrate, then we will see larger effects for the migrants. This does not mean exposure to war does not have an effect on long term crime. This only means that individuals who were crime prone as a result of the war are more likely to migrate. Although I cannot directly adjust for migration in my specification, I examine whether the probability of migration[25] is influenced by exposure to war during different ages. In Table A10, I do not find any evidence of differential migration by exposure to war during different ages.

Model Misspecification

In my main specification, I use a linear probability model to estimate the impact of exposure to conflict on crime. Although this model is frequently used in applied work for binary dependent variables, there are arguments against the use of a linear probability model for binary dependent variables especially in the case of rare events such as incarceration (Durlauf et al., 2010; King and Zeng, 2001). The main critique of the linear probability model estimates is that the predicted probabilities are not constrained to the unit interval. Horrace and Oaxaca (2006) formally discusses how the estimates from the linear probability model can be biased and inconsistent. They show that the bias increases as the proportion of the predicted probabilities that fall outside the unit interval

[25] Migration takes a value of one if the last district of residence is different from the birth district and zero otherwise.

increases. If the predicted probabilities lie within the unit interval, the concerns for inconsistency can be reduced. In my case, all predicted probabilities lie with the unit interval.

Furthermore, my identification assumption follows a difference and difference style framework assuming a common trend between the exposed and the non-exposed individuals. Lechner et al. (2011) discusses how using a non-linear model may violate the common trend assumption. This arises from how the fixed effects are treated in the different models. The linear model requires the unobserved differences across group to be constant overtime whereas the non-linear model requires it to be absent. Therefore, using a non-linear model with the difference in difference assumption will lead to an inconsistent estimator. Therefore, following Angrist and Pischke (2009), my preferred specification is a linear probability model.

Although my preferred model is a linear probability model, I also perform a logit analysis and the marginal effects at the mean from the logit model are presented in Table A11. The estimates from the exercise looks similar to my baseline results. With the logit model, I find that exposure to conflict during 6 to 11 years are associated with about a 6% increase in the probability of being incarcerated at the 10% level of significance.

8 Conclusion

This paper studies the impact of exposure to conflict during childhood on long term criminal behavior measured in terms of incarceration. I exploit the temporal and geographic variation in the violent events of the civil conflict in Peru and the birth year of individuals to assign if an individual was exposed to the violent civil conflict during specific ages during childhood. Using the National Penitentiary Population Census from 2016 and the ENAHO National Household Survey from 2015-2017, I estimate the effect of exposure to conflict during different ages on adult criminal behavior.

Results indicate that men who were exposed to the civil conflict between the ages of 6 and 11 year, are more likely to be incarcerated in the long term. Particularly, they are more likely to be incarcerated for violent and property crimes. This is an important finding since it deviates from the commonly held belief that exposure to negative shocks are most critical during in utero and early childhood years. The results are robust to a battery of checks and tests and are more likely to be an underestimate of the true effects.

I also discuss some potential channels that may be driving the results. Unlike Lochner (2004), I do not find education to be the main driver of long term criminal behavior in this context. Other channels could be labor market outcomes and health in the long run. The conflict can also have persistent psychological consequence, which although not directly testable in my context, may play an important role in explaining the pathway to crime.

References

Akresh, R., Bhalotra, S., Leone, M., and Osili, U. O. (2012). War and stature: growing up during the nigerian civil war. *American Economic Review*, 102(3):273–77.

Akresh, R., Bhalotra, S., Leone, M., and Osili, U. O. (2017). First and second generation impacts of the biafran war. Technical report, National Bureau of Economic Research.

Akresh, R. and De Walque, D. (2008). *Armed conflict and schooling: Evidence from the 1994 Rwandan genocide*. The World Bank.

Akresh, R. and de Walque, D. (2010). Armed conflict and schooling: Evidence from the 1994 rwandan genocide.

Akresh, R., Verwimp, P., and Bundervoet, T. (2011). Civil war, crop failure, and child stunting in rwanda. *Economic Development and Cultural Change*, 59(4):777–810.

Almås, I., Cappelen, A. W., Sørensen, E. Ø., and Tungodden, B. (2010). Fairness and the development of inequality acceptance. *Science*, 328(5982):1176–1178.

Angrist, J. D. and Pischke, J.-S. M. H. E. (2009). an empiricistâĂŹs companion.

Arcand, J.-L. and Wouabe, E. D. (2009). Households in a time of war: Instrumental variables evidence for angola. *The Graduate Institute, Geneva Working Paper*.

Attar, B. K., Guerra, N. G., and Tolan, P. H. (1994). Neighborhood disadvantage, stressful life events and adjustments in urban elementary-school children. *Journal of Clinical Child Psychology*, 23(4):391–400.

Barenbaum, J., Ruchkin, V., and Schwab-Stone, M. (2004). The psychosocial aspects of children exposed to war: practice and policy initiatives. *Journal of Child Psychology and Psychiatry*, 45(1):41–62.

Barr, A. and Smith, A. A. (2018). Fighting crime in the cradle: The effects of early childhood access to nutritional assistance. *Work. Pap., Texas A&M Univ./US Milit. Acad., West Point. http://people. tamu. edu/ abarr/AB_AS_Nut_Assistance_Crime_4_3_2018b. pdf Google Scholar Article Location*.

Bauer, M., Blattman, C., Chytilová, J., Henrich, J., Miguel, E., and Mitts, T. (2016). Can war foster cooperation? *Journal of Economic Perspectives*, 30(3):249–74.

Bauer, M., Chytilová, J., and Pertold-Gebicka, B. (2014). Parental background and other-regarding preferences in children. *Experimental Economics*, 17(1):24–46.

Bauer, M., Fiala, N., and Levely, I. (2018). Trusting former rebels: An experimental approach to understanding reintegration after civil war. *The Economic Journal*, 128(613):1786–1819.

Becker, G. S. (1968). Crime and punishment: an economic approach1. *Journal of Political Economy*, 76(2):169–217.

Bellows, J. and Miguel, E. (2009). War and local collective action in sierra leone. *Journal of public Economics*, 93(11-12):1144–1157.

Benjamini, Y. and Hochberg, Y. (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological)*, 57(1):289–300.

Besley, T. and Reynal-Querol, M. (2014). The legacy of historical conflict: Evidence from africa. *American Political Science Review*, 108(2):319–336.

Blattman, C. (2009). From violence to voting: War and political participation in uganda. *American political Science review*, 103(2):231–247.

Bozzoli, C., Deaton, A., and Quintana-Domeque, C. (2009). Adult height and childhood disease. *Demography*, 46(4):647–669.

Cassar, A., Grosjean, P., and Whitt, S. (2013). Legacies of violence: trust and market development. *Journal of Economic Growth*, 18(3):285–318.

Cassar, A., Grosjean, P. A., and Whitt, S. (2011). Civil war, social capital and market development: Experimental and survey evidence on the negative consequences of violence. *Social Capital and Market Development: Experimental and Survey Evidence on the Negative Consequences of Violence (November 10, 2011). UNSW Australian School of Business Research Paper*, (2011ECON14).

Cecchi, F., Leuveld, K., and Voors, M. (2016). Conflict exposure and competitiveness: Experimental evidence from the football field in sierra leone. *Economic Development and Cultural Change*, 64(3):405–435.

Chu, A., Brick, J. M., and Kalton, G. (1999). Weights for combining surveys across time or space. *Bulletin of the International Statistical Institute, Contributed Papers*, 2:103–104.

Collier, P. et al. (2003). *Breaking the conflict trap: Civil war and development policy*. World Bank Publications.

Couttenier, M., Preotu, V., Rohner, D., and Thoenig, M. (2016). The violent legacy of victimization: Post-conflict evidence on asylum seekers, crimes and public policy in switzerland. CEPR Discussion Papers 11079, C.E.P.R. Discussion Papers.

Cramer, C. (2006). *Civil war is not a stupid thing: Accounting for violence in developing countries*. Hurst London.

Currie, J. and Almond, D. (2011). Human capital development before age five. In *Handbook of labor economics*, volume 4, pages 1315–1486. Elsevier.

Currie, J. and Tekin, E. (2012). Understanding the cycle childhood maltreatment and future crime. *Journal of Human Resources*, 47(2):509–549.

Deaton, A. (2007). Height, health, and development. *Proceedings of the National Academy of Sciences*, 104(33):13232–13237.

Derluyn, I., Broekaert, E., Schuyten, G., and De Temmerman, E. (2004). Post-traumatic stress in former ugandan child soldiers. *The Lancet*, 363(9412):861–863.

Durlauf, S. N., Navarro, S., and Rivers, D. A. (2010). Understanding aggregate crime regressions. *Journal of Econometrics*, 158(2):306–317.

Dyregrov, A., Gupta, L., Gjestad, R., and Mukanoheli, E. (2000). Trauma exposure and psychological reactions to genocide among rwandan children. *Journal of Traumatic stress*, 13(1):3–21.

Eitle, D. and Turner, R. J. (2002). Exposure to community violence and young adult crime: The effects of witnessing violence, traumatic victimization, and other stressful life events. *Journal of research in Crime and Delinquency*, 39(2):214–237.

Fearon, J. D., Humphreys, M., and Weinstein, J. M. (2009). Can development aid contribute to social cohesion after civil war? evidence from a field experiment in post-conflict liberia. *American Economic Review*, 99(2):287–91.

Fehr, E., Glätzle-Rützler, D., and Sutter, M. (2013). The development of egalitarianism, altruism, spite and parochialism in childhood and adolescence. *European Economic Review*, 64:369–383.

Galdo, J. (2013). The long-run labor-market consequences of civil war: Evidence from the shining path in peru. *Economic Development and Cultural Change*, 61(4):789–823.

Gangadharan, L., Islam, A., Ouch, C., and Wang, L. C. (2017). The long-term effects of genocide on social preferences and risk. *Unpublished Results*.

Garbarino, J. and Kostelny, K. (1996). The effects of political violence on palestinian children's behavior problems: A risk accumulation model. *Child development*, 67(1):33–45.

Garmezy, N. E. and Rutter, M. E. (1983). Stress, coping, and development in children. In *Seminar on Stress and Coping in Children, 1979, Ctr for Advanced Study in the Behavioral Sciences, Stanford, CA, US*. Johns Hopkins University Press.

Gilligan, M. J., Pasquale, B. J., and Samii, C. (2011). Civil war and social capital: Behavioral-game evidence from nepal. *Available at SSRN 1911969*.

Grimard, F. and Laszlo, S. (2014). Long-term effects of civil conflict on womenâĂŹs health outcomes in peru. *World Development*, 54:139–155.

Gutierrez, F. H. (2017). Infant health during the 1980s peruvian crisis and long-term economic outcomes. *World Development*, 89:71–87.

Hausman, J. (2001). Mismeasured variables in econometric analysis: problems from the right and problems from the left. *Journal of Economic perspectives*, 15(4):57–67.

Hausman, J. A., Abrevaya, J., and Scott-Morton, F. M. (1998). Misclassification of the dependent variable in a discrete-response setting. *Journal of econometrics*, 87(2):239–269.

Horrace, W. C. and Oaxaca, R. L. (2006). Results on the bias and inconsistency of ordinary least squares for the linear probability model. *Economics Letters*, 90(3):321–327.

Jakiela, P. and Ozier, O. (2019). The impact of violence on individual risk preferences: evidence from a natural experiment. *Review of Economics and Statistics*, 101(3):547–559.

Kim, Y.-I. and Lee, J. (2014). The long-run impact of a traumatic experience on risk aversion. *Journal of Economic Behavior & Organization*, 108:174–186.

King, G. and Zeng, L. (2001). Logistic regression in rare events data. *Political analysis*, 9(2):137–163.

Landis, D. and Albert, R. D. (2012). *Handbook of Ethnic conflict*. Springer.

Laub, J. H. and Sampson, R. J. (1993). Turning points in the life course: Why change matters to the study of crime. *Criminology*, 31(3):301–325.

Lechner, M. et al. (2011). The estimation of causal effects by difference-in-difference methods. *Foundations and Trends® in Econometrics*, 4(3):165–224.

Leon, G. (2012). Civil conflict and human capital accumulation the long-term effects of political violence in perú. *Journal of Human Resources*, 47(4):991–1022.

Lochner, L. (2004). Education, work, and crime: A human capital approach. *International Economic Review*, 45(3):811–843.

Lochner, L. and Moretti, E. (2004). The effect of education on crime: Evidence from prison inmates, arrests, and self-reports. *American economic review*, 94(1):155–189.

Loeber, R. and Hay, D. (1997). Key issues in the development of aggression and violence from childhood to early adulthood. *Annual review of psychology*, 48(1):371–410.

Meyer, B. D. and Mittag, N. (2017). Misclassification in binary choice models. *Journal of econometrics*, 200(2):295–311.

Miguel, E., Saiegh, S. M., and Satyanath, S. (2011). Civil war exposure and violence. *Economics & Politics*, 23(1):59–73.

Nagelkerke, N. J., Moses, S., Plummer, F. A., Brunham, R. C., and Fish, D. (1995). Logistic regression in case-control studies: The effect of using independent as dependent variables. *Statistics in medicine*, 14(8):769–775.

Patchin, J. W., Huebner, B. M., McCluskey, J. D., Varano, S. P., and Bynum, T. S. (2006). Exposure to community violence and childhood delinquency. *Crime & Delinquency*, 52(2):307–332.

Rohner, D., Thoenig, M., and Zilibotti, F. (2013). Seeds of distrust: Conflict in uganda. *Journal of Economic Growth*, 18(3):217–252.

Sánchez, A. et al. (2010). Transitory shocks and long-term human capital accumulation: the impact of conflict on physical health in peru. Technical report.

Shemyakina, O. (2011). The effect of armed conflict on accumulation of schooling: Results from tajikistan. *Journal of Development Economics*, 95(2):186–200.

Solon, G., Haider, S. J., and Wooldridge, J. M. (2015). What are we weighting for? *Journal of Human resources*, 50(2):301–316.

Sviatschi, M. M. et al. (2017). Making a narco: Childhood exposure to illegal labor markets and criminal life paths. *Job Market Paper, Columbia University*.

Swee, E. L. et al. (2009). On war and schooling attainment: The case of bosnia and herzegovina. Technical report, Households in Conflict Network.

Van den Berg, G. J., Lundborg, P., Nystedt, P., and Rooth, D.-O. (2014). Critical periods during childhood and adolescence. *Journal of the European Economic Association*, 12(6):1521–1557.

Vaux, A. and Ruggiero, M. (1983). Stressful life change and delinquent behavior. *American Journal of Community Psychology*, 11(2):169–183.

Vogl, T. S. (2014). Height, skills, and labor market outcomes in mexico. *Journal of Development Economics*, 107:84–96.

Voors, M. J., Nillesen, E. E., Verwimp, P., Bulte, E. H., Lensink, R., and Van Soest, D. P. (2012). Violent conflict and behavior: a field experiment in burundi. *American Economic Review*, 102(2):941–64.

Werner, E. E. (2012). Children and war: Risk, resilience, and recovery. *Development and psychopathology*, 24(2):553–558.

Whitt, S. and Wilson, R. K. (2007). The dictator game, fairness and ethnicity in postwar bosnia. *American Journal of Political Science*, 51(3):655–668.

Zevallos, J. C. M. (2016). Overcrowding in the peruvian prison system. *International Review of the Red Cross*, 98(903):851–858.

Figure 1: The Geographic Evolution of the Conflict

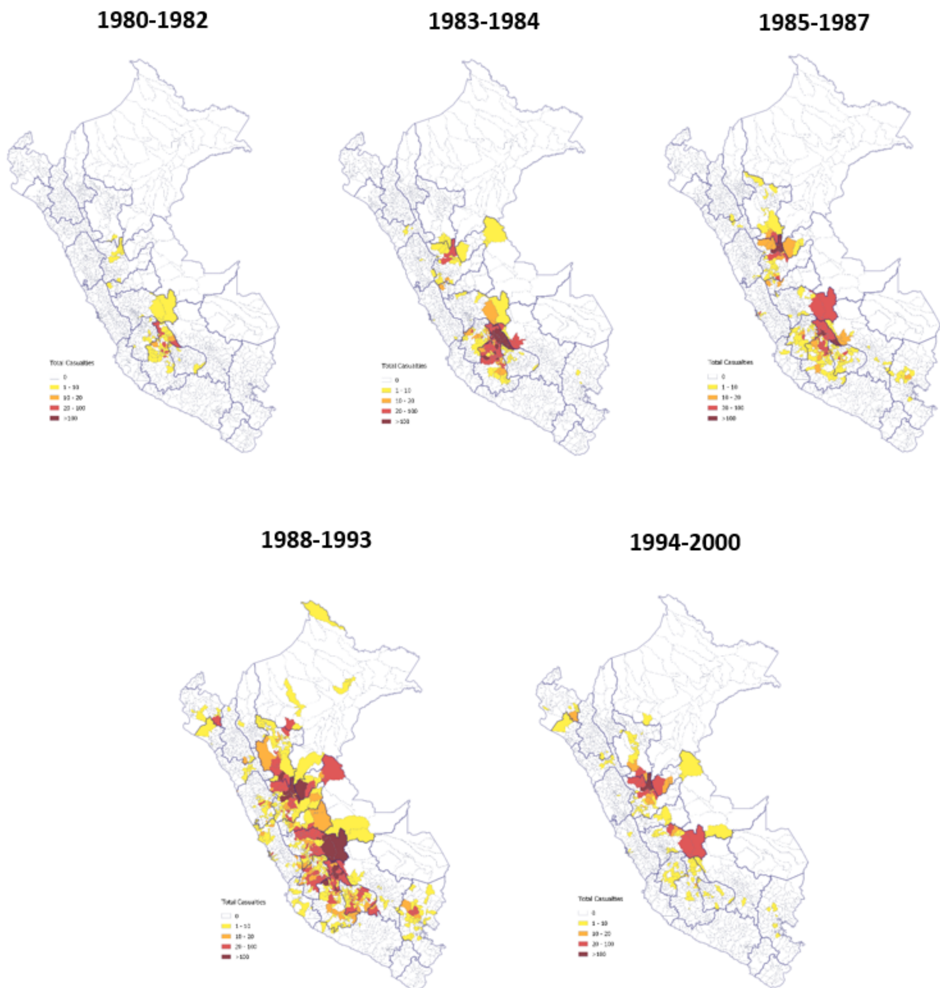
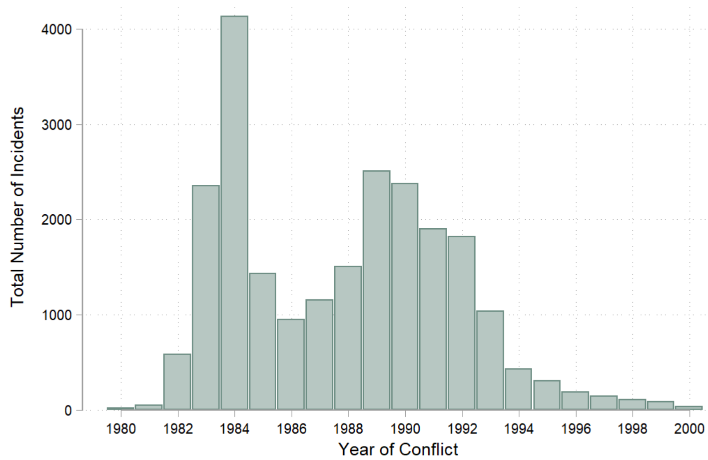
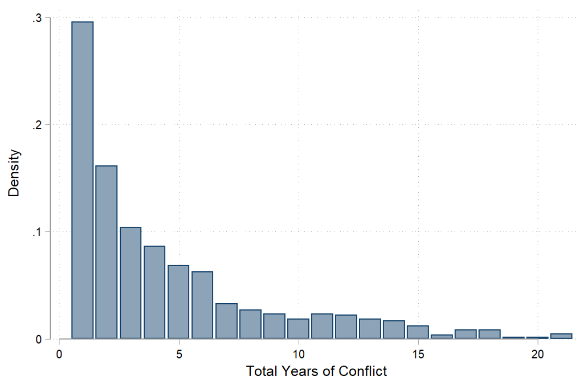


Figure 2: Structure of Conflict Data

(a) Total Conflict Incidents by Year



(b) Distribution of total years of exposure to war



(c) Total Conflict Incidents by Region

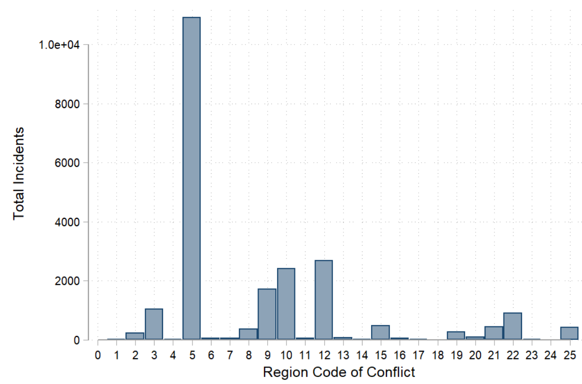


Table 1: Summary Statistics

	ENAHO				NPPC			
	N	Eff. N	Mean	Std.Dev	N	Eff. N	Mean	Std.Dev
Panel A: Characteristics								
Ethnicity (native)	54633	5738064	0.17	0.37	51259	51259	0.10	0.30
Married	54633	5738064	0.60	0.49	51259	51259	0.50	0.50
Years of Education	54626	5736428	9.83	3.18	51163	51163	8.55	3.05
Completed Primary School	54626	5736428	0.92	0.27	51163	51163	0.84	0.36
Completed High School	54626	5736428	0.71	0.45	51163	51163	0.41	0.49
Migration	54633	5738064	0.56	0.50	50719	50719	0.53	0.50
Chronic Disease	54633	5738064	0.29	0.45	51259	51259	0.32	0.47
Panel B: Conflict Exposure (%)								
In Utero	54633	5738064	0.14	0.35	51259	51259	0.19	0.39
0 to 2	54633	5738064	0.22	0.41	51259	51259	0.29	0.45
3 to 5	54633	5738064	0.24	0.43	51259	51259	0.33	0.47
6 to 11	54633	5738064	0.35	0.48	51259	51259	0.44	0.50
12 to 16	54633	5738064	0.28	0.45	51259	51259	0.31	0.46
Conflict Birth District	54633	5738064	0.65	0.48	51259	51259	0.74	0.44

Table 2: Impact of Conflict Exposure on Long Term Crime

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
In Utero	-0.000310	0.0000150	0.0000233	-0.000312	-0.0000352
	(0.000563)	(0.000150)	(0.000180)	(0.000346)	(0.0000293)
0 to 2	-0.000169	-0.0000176	-0.000107	-0.0000215	-0.0000222
	(0.000409)	(0.000130)	(0.000140)	(0.000244)	(0.0000307)
3 to 5	0.000509	0.000112	0.0000561	0.000365	-0.0000239
	(0.000486)	(0.000138)	(0.000134)	(0.000311)	(0.0000270)
6 to 11	0.000859**	0.000288**	0.000177	0.000393**	0.00000137
	(0.000339)	(0.000121)	(0.000111)	(0.000199)	(0.0000250)
12 to 16	0.000598*	0.000361***	0.00000564	0.000198	0.0000337
	(0.000353)	(0.000115)	(0.000125)	(0.000222)	(0.0000297)
Observations	105892	105892	105892	105892	105892
R^2	0.012	0.008	0.008	0.007	0.003
Mean	0.00885	0.00255	0.00195	0.00417	0.000177
StdDev	0.0937	0.0504	0.0442	0.0645	0.0133
Districts	1678	1678	1678	1678	1678
Effect Size for 6 to 11 years	9.71%	11.29%	9.08%	9.43%	0.77%
Effect Size for 12 to 16 years	6.76%	14.16%	0.29%	4.75%	19.03%
FDR q-value for 6 to 11		0.069	0.15	0.098	0.96
FDR q-value for 12 to 16		0.007	0.964	0.499	0.499
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict. Effect sizes are the percentage change in the estimated coefficients for that age group over the mean times 100%. The FDR q-values are the adjusted False Discovery Rate to correct for multiple comparison for the estimates of the exposure at the respective age groups and are interpreted similar to p-values.

Source: Combined data NPPC and ENAHO 2015-2017.

Table 3: Impact of Conflict Exposure During High Intensity War Years on Long Term Crime

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
In Utero	0.000659	0.000144	0.000269	0.000205	0.0000382
	(0.000598)	(0.000154)	(0.000220)	(0.000384)	(0.0000333)
0 to 2	-0.000221	-0.0000190	-0.00000217	-0.000226	0.0000247
	(0.000496)	(0.000159)	(0.000188)	(0.000276)	(0.0000277)
3 to 5	0.000773*	0.000193	0.000203	0.000359	0.0000184
	(0.000468)	(0.000142)	(0.000163)	(0.000277)	(0.0000323)
6 to 11	0.00106***	0.000232*	0.000352**	0.000430**	0.0000470*
	(0.000370)	(0.000132)	(0.000137)	(0.000219)	(0.0000276)
12 to 16	0.0000651	0.000251*	-0.000201	-0.0000160	0.0000315
	(0.000367)	(0.000138)	(0.000123)	(0.000228)	(0.0000346)
Observations	105892	105892	105892	105892	105892
R^2	0.012	0.008	0.008	0.007	0.003
Mean	0.00885	0.00255	0.00195	0.00417	0.000177
Std. Dev.	0.0937	0.0504	0.0442	0.0645	0.0133
Districts	1678	1678	1678	1678	1678
Effect Size for 6 to 11 years	11.98%	9.10%	18.05%	10.31%	26.55%
FDR q-value for 6 to 11 years		0.0885	0.0404	0.0885	0.0885
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future during the high war intensity years 1983-1984 and 1988-1993. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.

The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict. Effect size for 6 to 11 years are the percentage change in the estimated coefficients for that age group over the mean times 100%. The FDR q-values are the adjusted False Discovery Rate to correct for multiple comparison for the estimates of the exposure at the respective age groups and are interpreted similar to p-values.

Source: Combined data NPPC and ENAHO 2015-2017.

Table 4: Impact of Conflict Exposure/ Intensity on Long Term Crime

Independent Variable:	Exposure Dummy		Years of Exposure		Total Casualties	
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Crime	Any Crime	Any Crime	Any Crime	Any Crime	Any Crime
-6		0.000156		0.00103		0.0000919
		(0.000868)		(0.00114)		(0.0000988)
-5		0.00102		0.00181*		0.0000220
		(0.000826)		(0.00107)		(0.0000694)
-4		-0.000113		0.000565		0.0000287
		(0.000655)		(0.000705)		(0.0000700)
-3		0.000242		0.000904		0.000116*
		(0.000615)		(0.000700)		(0.0000687)
In Utero	-0.000310	-0.000198	-0.000245	0.000238	-0.0000175	-0.0000105
	(0.000563)	(0.000606)	(0.000399)	(0.000480)	(0.0000223)	(0.0000285)
0 to 2	-0.000169	0.0000297	0.0000274	0.000474	0.00000493	0.0000252
	(0.000409)	(0.000466)	(0.000266)	(0.000428)	(0.0000177)	(0.0000198)
3 to 5	0.000509	0.000656	0.000277	0.000675*	-0.00000994	0.00000905
	(0.000486)	(0.000498)	(0.000275)	(0.000366)	(0.0000112)	(0.0000149)
6 to 11	0.000859**	0.00103***	0.000183*	0.000535**	0.000000702	0.0000181*
	(0.000339)	(0.000392)	(0.000110)	(0.000222)	(0.00000870)	(0.00000999)
12 to 16	0.000598*	0.000708*	0.0000827	0.000292	0.000000453	0.0000138
	(0.000353)	(0.000362)	(0.000147)	(0.000204)	(0.0000121)	(0.0000144)
Observations	105892	105892	105892	105892	105892	105892
R^2	0.012	0.012	0.012	0.012	0.012	0.012
Mean	0.00885	0.00885	0.00885	0.00885	0.00885	0.00885
StdDev	0.0937	0.0937	0.0937	0.0937	0.0937	0.0937
Districts	1678	1678	1678	1678	1678	1678
Effect Size for 6 to 11 years	9.7%	11.6%	2.1%	6.1%	0.007%	0.21%
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes	Yes
The F-test of joint significance for the coefficients before year -2:						
F-stat		0.511		1.081		0.921
Prob > F		0.727		0.364		0.451

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The treatment variables are defined differently to measure conflict exposure as labelled on the top o the column. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise in column 1 and 2. In column 3 and 4, the independent variable is equal to the total number of years exposed to war during those ages in childhood. In column 5 and 6, the independent variable takes the value of the total number of conflict casualties during those ages in childhood. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict. The F-test statistics of joint significance for co-efficients before year -2 years in Column 2, 4 and 6 is presented in F-stat and Prob > F. In all three cases, I fail to reject the null that they are jointly equal to zero.

Source: Combined data NPPC and ENAHO 2015-2017.

Table 5: Impact of Conflict Exposure on Long Term Intermediate Variables (Main Data)

	(1) Married	(2) Education	(3) Secondary	(4) Chronic
-6	-0.0166	-0.0396	-0.00768	-0.00864
	(0.0231)	(0.0849)	(0.0149)	(0.0206)
-5	0.0130	-0.0253	0.0138	0.0189
	(0.0235)	(0.0724)	(0.0144)	(0.0170)
-4	0.00313	-0.0165	-0.00547	-0.0255*
	(0.0180)	(0.0782)	(0.0134)	(0.0133)
-3	0.0130	-0.0757	-0.0136	0.00151
	(0.0171)	(0.0650)	(0.0132)	(0.0140)
In Utero	-0.00707	-0.00226	-0.00287	0.00355
	(0.0122)	(0.0616)	(0.0118)	(0.0144)
0 to 2	-0.00741	0.0959*	0.00149	0.00718
	(0.0121)	(0.0568)	(0.0105)	(0.0123)
3 to 5	-0.000383	0.0289	-0.000261	0.00185
	(0.0147)	(0.0554)	(0.0108)	(0.0110)
6 to 11	-0.00821	-0.0372	-0.00277	0.00612
	(0.0122)	(0.0639)	(0.0111)	(0.0121)
12 to 16	-0.0147	-0.0449	-0.00804	-0.00247
	(0.0137)	(0.0716)	(0.0129)	(0.0131)
Observations	105892	105788	105788	105892
R^2	0.298	0.306	0.277	0.114
Mean	0.596	9.456	0.706	0.291
StdDev	0.491	2.957	0.455	0.454
Districts	1678	1677	1677	1678
Birth Year FE	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.
The dependent variables are Married which is equal to 1 if the individual is married or cohabiting, Education is the years of education, Secondary is equal to 1 if the individual completed more that secondary education, Chronic is equal to 1 if the person has any chronic disease.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.
Source: Combined data NPPC and ENAHO 2015-2017.

Table 6: Impact of Conflict Exposure on Long Term Intermediate Variables (ENAHO 2004-2017)

	(1) Married	(2) Education	(3) Secondary	(4) Chronic
-6	-0.00382	-0.00346	-0.000280	0.00907
	(0.00706)	(0.0510)	(0.00776)	(0.00865)
-5	0.000529	-0.00483	-0.00391	0.00956
	(0.00766)	(0.0351)	(0.00611)	(0.00723)
-4	0.00288	0.0126	-0.00424	0.00283
	(0.00687)	(0.0398)	(0.00643)	(0.00693)
-3	0.00792	-0.00247	0.00750	-0.00248
	(0.00671)	(0.0378)	(0.00581)	(0.00515)
In Utero	-0.00659	0.0283	0.00567	0.00322
	(0.00762)	(0.0403)	(0.00638)	(0.00493)
0 to 2	-0.0000649	-0.0371	0.000794	0.00193
	(0.00996)	(0.0391)	(0.00773)	(0.00448)
3 to 5	-0.00546	-0.0351	0.00123	0.00353
	(0.00804)	(0.0380)	(0.00657)	(0.00435)
6 to 11	-0.0183**	0.0388	0.0129*	-0.00174
	(0.00872)	(0.0361)	(0.00658)	(0.00428)
12 to 16	-0.0133*	0.00490	-0.000790	-0.00402
	(0.00799)	(0.0362)	(0.00652)	(0.00504)
Observations	233676	233665	233665	233676
R^2	0.369	0.249	0.246	0.063
Mean	0.431	9.016	0.596	0.204
StdDev	0.495	3.141	0.491	0.403
Districts	1792	1792	1792	1792
Birth Year FE	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993. The controls include ethnicity (native=1), age, survey years, birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The dependent variables are Married which is equal to 1 if the individual is married or cohabiting, Education is the years of education, Secondary is equal to 1 if the individual completed more that secondary education, Chronic is equal to 1 if the person has any chronic disease.

The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.

Source: ENAHO 2004-2017.

Table 7: Impact of Conflict on Long Term Crime- Potential Mechanism

	(1)	(2)	(3)	(4)	(5)	(6)
	Any Crime	Any Crime	Any Crime	Any Crime	Any Crime	Any Crime
In Utero	-0.000198	-0.000218	-0.000168	-0.000219	-0.000204	-0.000192
	(0.000606)	(0.000607)	(0.000610)	(0.000665)	(0.000614)	(0.000616)
0 to 2	0.0000297	0.00000835	0.000232	0.0000865	0.0000161	0.000199
	(0.000466)	(0.000458)	(0.000475)	(0.000517)	(0.000469)	(0.000470)
3 to 5	0.000656	0.000655	0.000727	0.000669	0.000653	0.000723
	(0.000498)	(0.000500)	(0.000503)	(0.000538)	(0.000496)	(0.000504)
6 to 11	0.00103^{***}	0.00101^{***}	0.000974^{**}	0.000989^{**}	0.00102^{***}	0.000945^{**}
	(0.000392)	(0.000390)	(0.000400)	(0.000432)	(0.000389)	(0.000396)
12 to 16	0.000708^{*}	0.000667^{*}	0.000619^{*}	0.000546	0.000713^{**}	0.000584
	(0.000362)	(0.000366)	(0.000360)	(0.000373)	(0.000358)	(0.000361)
Native	-0.00388^{***}	-0.00353^{***}	-0.00618^{***}	-0.00739^{***}	-0.00386^{***}	-0.00582^{***}
	(0.000483)	(0.000491)	(0.000623)	(0.000679)	(0.000481)	(0.000646)
Married		-0.00280^{***}				-0.00264^{***}
		(0.000658)				(0.000708)
Education			-0.00183^{***}			-0.00182^{***}
			(0.000216)			(0.000213)
Secondary				-0.0194^{***}		
				(0.00186)		
Chronic					0.00182^{***}	0.00138^{***}
					(0.000314)	(0.000278)
Observations	105892	105892	105788	105788	105892	105788
R^2	0.012	0.012	0.014	0.018	0.012	0.014
Mean	0.00885	0.00885	0.00884	0.00884	0.00885	0.00884
StdDev	0.0937	0.0937	0.0936	0.0936	0.0937	0.0936
Districts	1678	1678	1677	1677	1678	1677
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The regression specification includes the pre-birth conflict exposure indicators for exposure upto 6 years before birth. The coefficient are not presented in the table but are all statistically not significant. The independent variables that are cumulatively added are Married which is equal to 1 if the individual is married or cohabiting, Education is years of education, Secondary is equal to 1 if the individual completed more that secondary education, Chronic Illness is equal to 1 if the person has any chronic disease.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.

Source: Combined data NPPC and ENAHO 2015-2017.

Table 8: Impact of Conflict Exposure on Long Term Labor Market Outcomes

	Ages: 18 to 30 years			Ages: >30 years		
	(1)	(2)	(3)	(4)	(5)	(6)
	Employed	Log Earnings	Large Firm	Employed	Log Earnings	Large Firm
In Utero	0.0047	0.0669*	0.01	0.000511	0.111	-0.0135
	(0.00527)	(0.035)	(0.00612)	(0.0112)	(0.187)	(0.0257)
0 to 2	-0.000278	0.0671	0.00856	-0.00221	-0.0584	-0.0134
	(0.00586)	(0.0465)	(0.00569)	(0.00704)	(0.111)	(0.0158)
3 to 5	0.00701	0.0503	0.0000284	0.004	-0.039	-0.00115
	(0.00491)	(0.042)	(0.00539)	(0.00565)	(0.0802)	(0.0107)
6 to 11	-0.00328	-0.0598	-0.00849	0.00187	-0.0606	0.00541
	(0.00523)	(0.0488)	(0.0067)	(0.00486)	(0.0708)	(0.00934)
12 to 16	-0.0173***	-0.140***	-0.00457	0.000528	-0.0286	0.00538
	(0.00637)	(0.0502)	(0.00725)	(0.00445)	(0.0596)	(0.00842)
Observations	115242	115244	94608	88593	88595	84730
R^2	0.128	0.107	0.133	0.051	0.149	0.138
Mean	0.765	2.935	0.24	0.942	3.13	0.275
StdDev	0.424	3.231	0.427	0.233	3.489	0.446
Districts	1701	1701	1680	1719	1719	1713
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on long term labor market outcomes The independent variable employed takes a value of 1 if the individual has a permanent job at the time of survey and zero otherwise, log earning is the log of (1+earnings) where earning is equal to 0 for unemployed individuals, large firms takes a value of one if the individual works in a firm with more than 2000 workers and 0 otherwise. The sample only contains male who were born between 1970 and 1993. The controls include ethnicity (native=1), age, survey year fixed effects, birth year fixed effects, birth districts fixed effects and birth district level linear time trends. Sampling weights are used to obtain population estimates. The standard errors are clustered at the birth district level. The regression are separately for individuals whose age during the survey was between 18 and 30 years and those who were older than 30.

The Mean and Std. Dev represents the mean and standard deviation of the dependent variable.

Source: ENAHO 2004-2017.

Table 9: Impact of Conflict Exposure on Long Term Health

	(1) Height	(2) Weight
In Utero	-0.0220	0.226
	(0.427)	(0.535)
0 to 2	-0.324	-1.050*
	(0.360)	(0.555)
3 to 5	-0.623*	-0.591
	(0.355)	(0.522)
6 to 11	-0.190	-0.393
	(0.379)	(0.480)
12 to 16	-0.773**	-0.345
	(0.363)	(0.517)
Observations	17270	17270
Adjusted R-squared	0.112	0.290

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

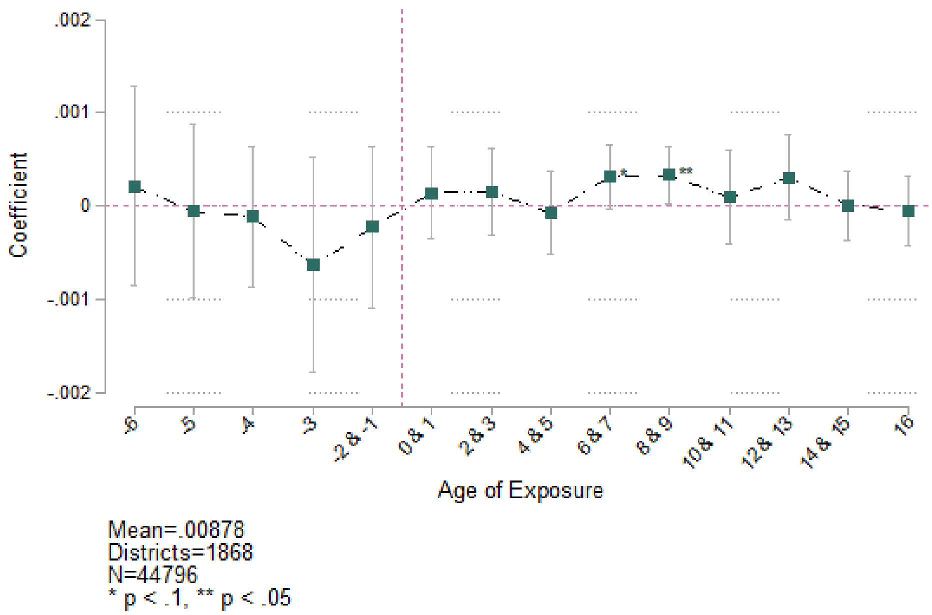
Note: The table shows the impact of exposure to conflict during different ages on long term health outcomes. The dependent variable height is measured in centimeters and weight is measured in kilograms . The sample only contains male who were born between 1970 and 1993. The controls include ethnicity (native=1), age, married, urban, survey year fixed effects, birth year fixed effects, birth districts fixed effects and birth district level linear time trends. Sampling weights are used to obtain population estimates. The standard errors are clustered at the birth district level. The regression specification includes the pre-birth conflict exposure indicators for exposure upto 6 years before birth. The coefficient are not presented in the table but are all statistically not significant.

Source: ENAHO 2007-2011.

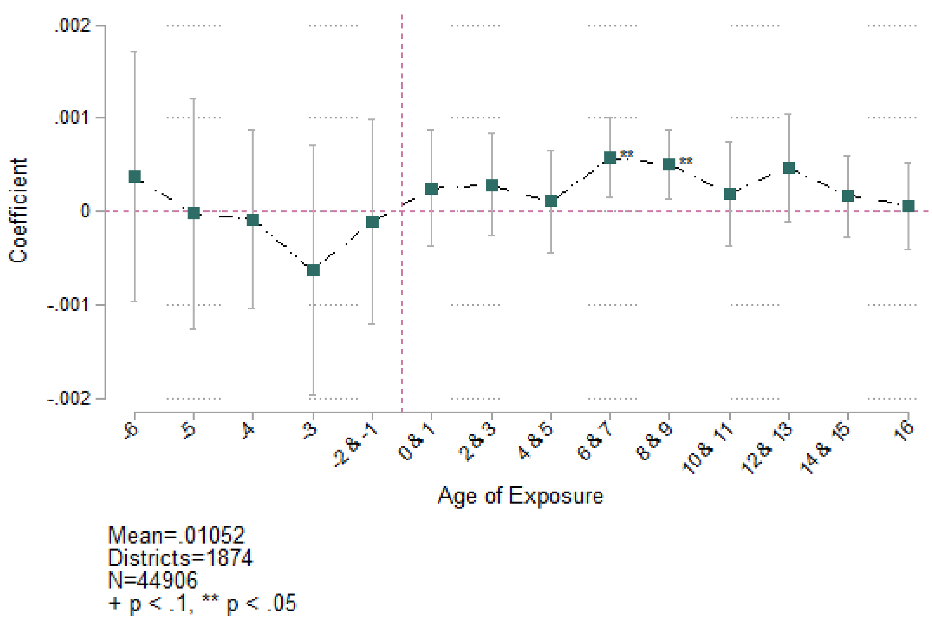
Appendix

Figure A1: Impact of Conflict Exposure on Long Term Crime using Cohort Level Data

(a) NPPC 2016 with Census 2007 to create birth cohort level data.



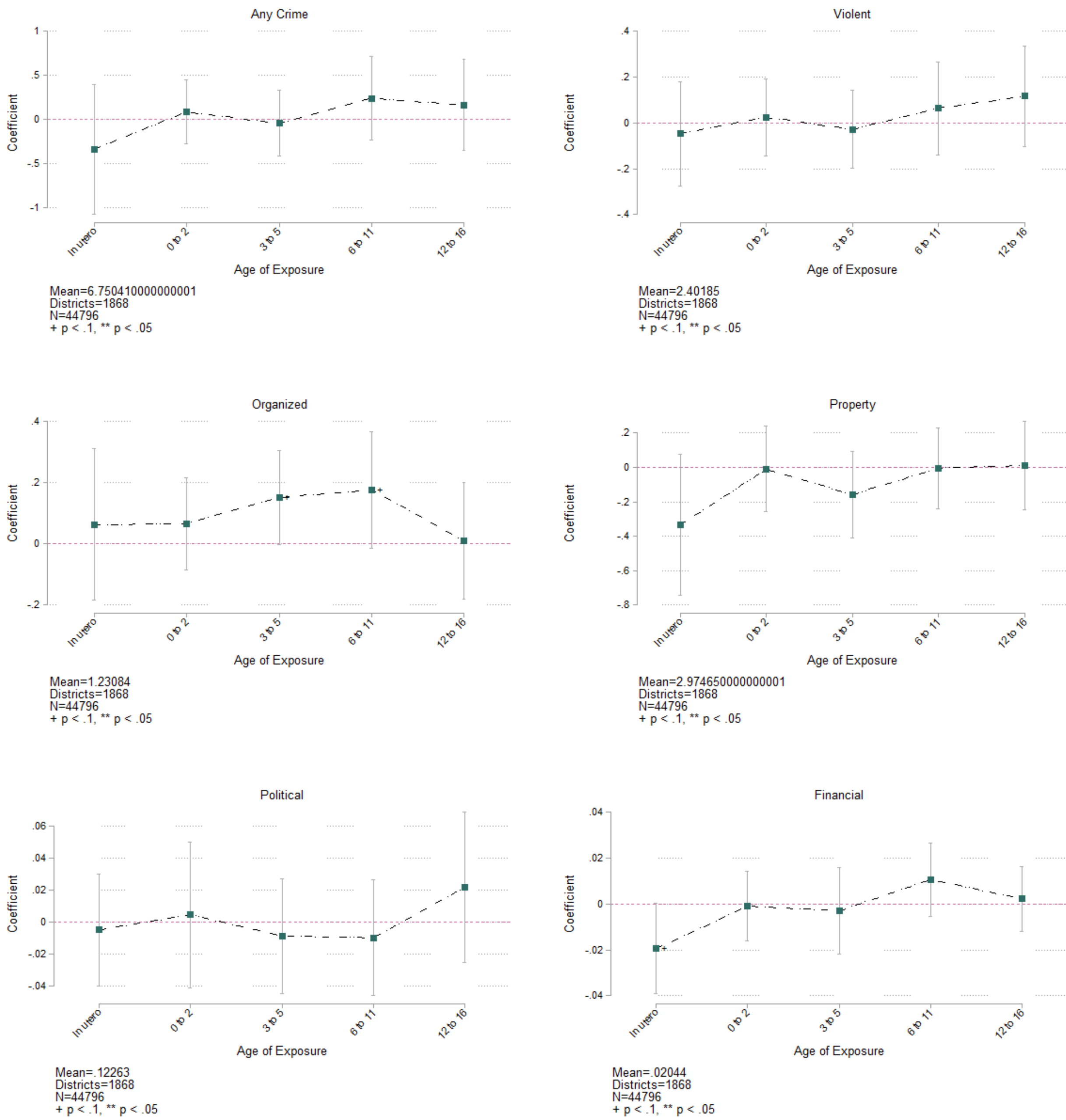
(b) NPPC 2016 with Census 2017 to create birth cohort level data.



Note: The graphs shows the estimates of the impact of exposure to war during different ages. It shows the baseline specification in equation (1) with different age grouping using the cohort level measure which is the crime rate per male in that birth district and cohort. The x-axis indicates an age group and it takes a value of one if the individual/ cohort is exposed to war during any age in that age group and 0 otherwise. The analysis uses the cohort sizes as weights.

Source: NPPC 2016 with Census 2007 or 2017 to create birth cohort level data.

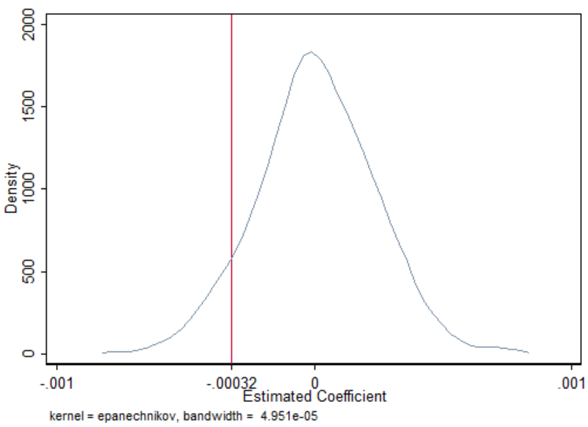
Figure A2: Impact of Conflict Exposure on Long Term Crime using Cohort Level Data



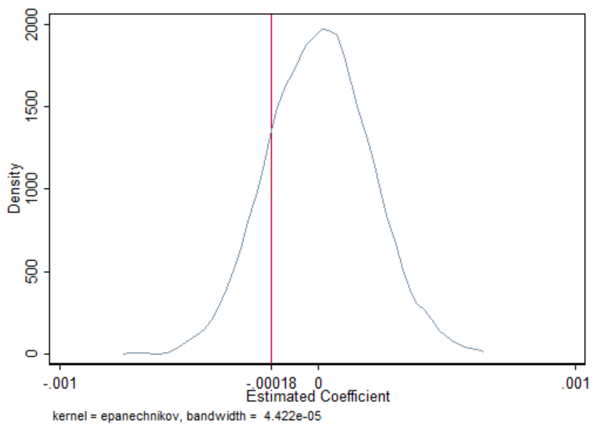
Note: The graphs shows the estimates of the impact of exposure to war during different ages. The observation is at the birth district and cohort level and the outcome variable is the crime rate per 1000 male in that birth district and cohort. The x-axis indicates an age group and it takes a value of one if the cohort is exposed to war during any age in that age group and 0 otherwise.
Source: NPPC 2016 with Census 2007 to create birth cohort level data.

Figure A3: Impact of Conflict Exposure on Long Term Crime: Placebo Estimates on Any Crime

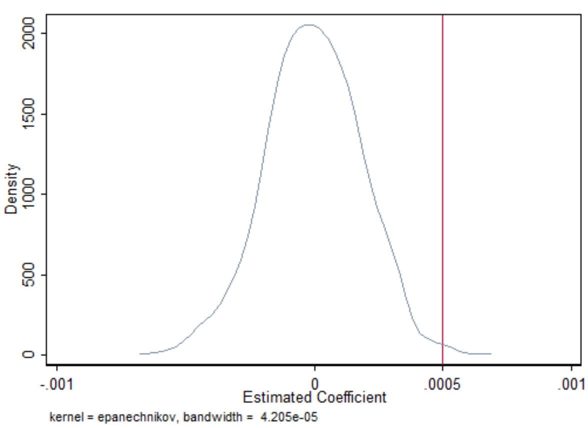
(a) In Utero



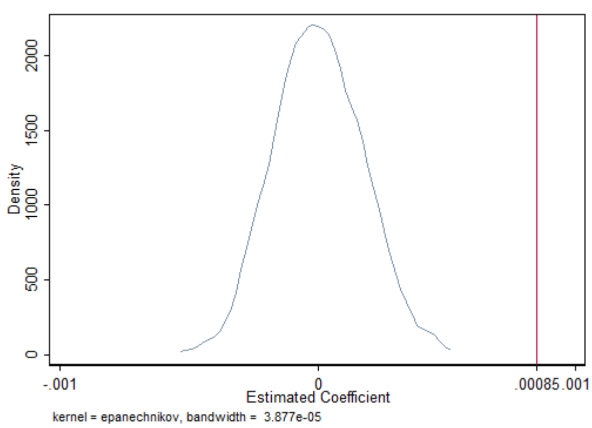
(b) 0 to 2 years



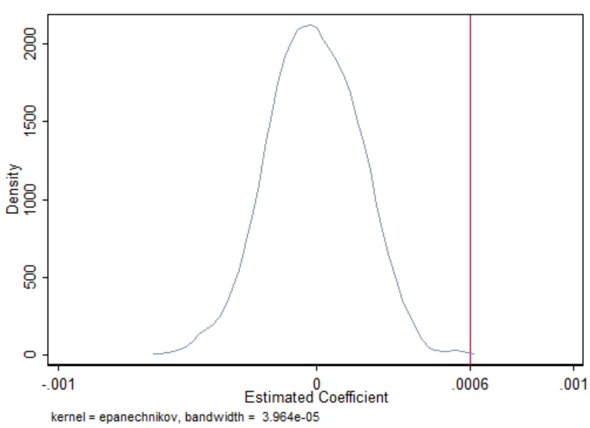
(c) 3 to 5 years



(d) 6 to 11 years



(e) 12 to 16 years



Note: The Figures plot the density of 1000 estimates from equation 1 with exposure to conflict treatment randomly assigned for each of the five age group. each graph specifies the density of estimates for each of the war exposure treatment age group. The red line indicates the actual coefficient estimates from Table 2 column 1.
Source: NPPC 2016 and ENAHO 2015-2017

Table A1: Impact of Conflict Exposure on Long Term Crime with Pre-Birth Conflict Exposure Indicators

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
-6	0.000156	0.0000618	-0.000274	0.000313	0.0000542
	(0.000868)	(0.000207)	(0.000257)	(0.000595)	(0.0000522)
-5	0.00102	-0.0000588	0.000536**	0.000467	0.0000713*
	(0.000826)	(0.000170)	(0.000229)	(0.000572)	(0.0000395)
-4	-0.000113	0.000147	-0.000143	-0.0000426	-0.0000743**
	(0.000655)	(0.000156)	(0.000222)	(0.000421)	(0.0000372)
-3	0.000242	-0.00000239	0.000192	0.0000299	0.0000233
	(0.000615)	(0.000166)	(0.000210)	(0.000393)	(0.0000329)
In Utero	-0.000198	0.0000312	0.0000329	-0.000233	-0.0000275
	(0.000606)	(0.000155)	(0.000198)	(0.000372)	(0.0000333)
0 to 2	0.0000297	0.00000569	-0.0000510	0.0000894	-0.0000135
	(0.000466)	(0.000140)	(0.000156)	(0.000282)	(0.0000341)
3 to 5	0.000656	0.000135	0.0000917	0.000448	-0.0000185
	(0.000498)	(0.000144)	(0.000138)	(0.000314)	(0.0000291)
6 to 11	0.00103***	0.000312**	0.000214	0.000497**	0.0000101
	(0.000392)	(0.000133)	(0.000151)	(0.000226)	(0.0000277)
12 to 16	0.000708*	0.000378***	0.0000341	0.000259	0.0000378
	(0.000362)	(0.000120)	(0.000135)	(0.000218)	(0.0000312)
Observations	105892	105892	105892	105892	105892
R^2	0.012	0.008	0.008	0.007	0.003
Mean	0.00885	0.00255	0.00195	0.00417	0.000177
StdDev	0.0937	0.0504	0.0442	0.0645	0.0133
Districts	1678	1678	1678	1678	1678
Effect Size for 6 to 11 years	11.6%	12.2%	10.9%	11.9%	5.7%
Effect Size for 12 to 16 years	8%	14.8%	1.8%	6.2%	21.4%
FDR q-value for 6 to 11		0.056	0.206	0.056	0.715
FDR q-value for 12 to 16		0.007	0.801	0.312	0.312
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable. Effect sizes are the percentage change in the estimated coefficients for that age group over the mean times 100%. The FDR q-values are the adjusted False Discovery Rate to correct for multiple comparison for the estimates of the exposure at the respective age groups and are interpreted similar to p-values.

Source: Combined data NPPC and ENAHO 2015-2017.

Table A2: Impact of Conflict Exposure on Long Term Crime- Using Only ENAHO 2016 and NPPC 2016

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
-6	-0.000242	-0.000108	-0.000815**	0.000627	0.0000546
	(0.00123)	(0.000319)	(0.000395)	(0.000734)	(0.0000547)
-5	0.000454	-0.000242	0.000564	0.0000497	0.0000834*
	(0.00116)	(0.000236)	(0.000343)	(0.000764)	(0.0000447)
-4	-0.000234	0.000110	0.0000457	-0.000327	-0.0000627
	(0.000825)	(0.000220)	(0.000291)	(0.000495)	(0.0000410)
-3	0.000960	0.000183	0.000393	0.000374	0.00000850
	(0.000914)	(0.000241)	(0.000307)	(0.000559)	(0.0000384)
In Utero	0.0000261	0.0000538	0.000244	-0.000237	-0.0000340
	(0.000964)	(0.000231)	(0.000331)	(0.000544)	(0.0000371)
0 to 2	0.000155	0.0000292	-0.0000664	0.000205	-0.0000127
	(0.000714)	(0.000207)	(0.000232)	(0.000398)	(0.0000409)
3 to 5	0.000989	0.000203	0.0000846	0.000719	-0.0000172
	(0.000854)	(0.000218)	(0.000209)	(0.000529)	(0.0000369)
6 to 11	0.00194**	0.000583***	0.000334	0.00100**	0.0000211
	(0.000781)	(0.000219)	(0.000243)	(0.000426)	(0.0000332)
12 to 16	0.00107*	0.000632***	0.000150	0.000235	0.0000523
	(0.000627)	(0.000191)	(0.000206)	(0.000341)	(0.0000399)
Observations	69837	69837	69837	69837	69837
R^2	0.019	0.013	0.012	0.011	0.003
Mean	0.00877	0.00251	0.00194	0.00415	0.000177
StdDev	0.0933	0.0500	0.0440	0.0643	0.0133
Districts	1477	1477	1477	1477	1477
Effect Size for 6 to 11 years	22.1%	23.2%	17.2%	24.1%	11.9%
Effect Size for 12 to 16 years	12.2%	25.2%	7.7%	5.7%	29.6%
FDR q-value for 6 to 11		0.032	0.23	0.037	0.525
FDR q-value for 12 to 16		0.0037	0.49	0.49	0.38
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2016 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict. The FDR q-values are the adjusted False Discovery Rate to correct for multiple comparison for the estimates of the exposure at the respective age groups and are interpreted similar to p-values.

Source: Combined data NPPC and ENAHO 2016.

Table A3: Robustness Check: Impact of Conflict Exposure on Long Term Crime-With Different Trend Specifications

	(1) Any Crime	(2) Any Crime	(3) Any Crime	(4) Any Crime
-6	0.000156	0.000158	0.000311	0.000445
	(0.000868)	(0.000869)	(0.000687)	(0.000791)
-5	0.00102	0.00102	0.000707	0.000715
	(0.000826)	(0.000827)	(0.000675)	(0.000847)
-4	-0.000113	-0.000112	0.0000260	-0.000127
	(0.000655)	(0.000655)	(0.000602)	(0.000700)
-3	0.000242	0.000240	0.000362	-0.0000553
	(0.000615)	(0.000615)	(0.000639)	(0.000696)
In Utero	-0.000198	-0.000198	0.0000582	0.0000806
	(0.000606)	(0.000606)	(0.000472)	(0.000439)
0 to 2	0.0000297	0.0000289	-0.0000555	-0.000106
	(0.000466)	(0.000466)	(0.000398)	(0.000401)
3 to 5	0.000656	0.000657	0.000679^{*}	0.000487
	(0.000498)	(0.000498)	(0.000400)	(0.000442)
6 to 11	0.00103^{***}	0.00103^{***}	0.000708^{**}	0.000630^{*}
	(0.000392)	(0.000392)	(0.000349)	(0.000333)
12 to 16	0.000708^{*}	0.000708^{*}	0.000390	0.000553^{*}
	(0.000362)	(0.000362)	(0.000318)	(0.000333)
Observations	105892	105892	105892	105892
R^2	0.012	0.012	0.006	0.006
Mean	0.00885	0.00885	0.00885	0.00885
StdDev	0.0937	0.0937	0.0937	0.0937
Districts	1678	1678	1678	1678
Birth Year FE	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes
Birth District Linear Trend	Yes	No	No	No
Birth District Quadratic Trend	No	Yes	No	No
Birth Province Cubic Trend	No	No	Yes	No
Birth Region*Birth Year FE	No	No	No	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for any crime.. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects. For each column, a different time trend specification is used as indicated at the end of the table. The standard errors are clustered at the birth district level.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable.
Source: Combined data NPPC and ENAHO 2015-2017.

Table A4: Robustness Check: Impact of Conflict Exposure on Long Term Crime- Incarceration Data Selection

	(1) Any Crime	(2) Any Crime	(3) Any Crime	(4) Any Crime	(5) Any Crime
-6	0.000157	0.000281	0.000124	-0.000140	0.000157
	(0.000868)	(0.000760)	(0.000771)	(0.000442)	(0.000868)
-5	0.00101	0.000920	0.000664	-0.000112	0.00101
	(0.000826)	(0.000704)	(0.000759)	(0.000344)	(0.000826)
-4	-0.000113	-0.000203	0.000127	-0.000194	-0.000113
	(0.000655)	(0.000552)	(0.000572)	(0.000374)	(0.000655)
-3	0.000243	0.000259	0.000277	0.000184	0.000243
	(0.000615)	(0.000525)	(0.000539)	(0.000298)	(0.000615)
In Utero	-0.000197	-0.0000689	0.0000795	-0.000172	-0.000197
	(0.000606)	(0.000508)	(0.000547)	(0.000393)	(0.000606)
0 to 2	0.0000306	0.0000178	0.000362	0.000156	0.0000306
	(0.000466)	(0.000392)	(0.000391)	(0.000253)	(0.000466)
3 to 5	0.000656	0.000673^{*}	0.00105^{**}	0.000585^{**}	0.000656
	(0.000498)	(0.000407)	(0.000468)	(0.000253)	(0.000498)
6 to 11	0.00103^{***}	0.000859^{***}	0.000901^{***}	0.000565^{**}	0.00103^{***}
	(0.000392)	(0.000319)	(0.000326)	(0.000226)	(0.000392)
12 to 16	0.000709^{*}	0.000589^{*}	0.000534^{*}	0.000588^{**}	0.000709^{*}
	(0.000362)	(0.000305)	(0.000310)	(0.000233)	(0.000362)
Observations	105892	93518	98011	79129	105892
R^2	0.012	0.006	0.005	0.005	0.012
Mean	0.00885	0.00676	0.00753	0.00427	0.00885
StdDev	0.0937	0.0819	0.0864	0.0652	0.0937
Effect Size for 6 to 11 years	11.64%	12.71%	11.97%	13.23%	11.64%
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes
Incarceration Data Selection Criteria:					
Birth District	Yes	Yes	Yes	Yes	No
Birth Year	No	No	Yes	Yes	No
Current District	No	Yes	No	Yes	No

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The dependent variable takes a value of 1 if the individual is incarcerated for any crime and 0 otherwise. The sample only contains male who were born between 1970 and 1993. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.Effect size for 6 to 11 years are the percentage change in the estimated coefficients for that age group over the mean times 100%. Incarceration Data Selection Criteria indicates based on which criteria I select observations from the incarceration data in each column. E.g, in column (4), I only keep the an observation from the incarceration data, if I have atleast one individual in the ENAHO 2015-2017 data who was born in the same district, in the same year and has the have last district of residence of current residence. The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.

Source: Combined data NPPC and ENAHO 2015-2017.

Table A5: Robustness Check: Impact of Conflict Exposure on Long Term Crime- Birth Cohort Selection

	(1) 1970-1993	(2) 1975-1993	(3) 1970-1998	(4) 1975-1989	(5) 1965-1993	(6) 1926-1998
-6	0.000154	0.000426	0.0000417	0.000194	0.000190	-0.000408
	(0.000869)	(0.000967)	(0.000377)	(0.00145)	(0.000828)	(0.000344)
-5	0.00102	0.00124	0.000128	0.00231	0.000979	-0.000230
	(0.000826)	(0.000914)	(0.000370)	(0.00151)	(0.000786)	(0.000329)
-4	-0.000112	-0.000283	-0.000244	-0.0000968	-0.0000223	-0.000437
	(0.000655)	(0.000739)	(0.000377)	(0.00130)	(0.000638)	(0.000356)
-3	0.000241	-0.0000150	0.000293	0.000267	0.000330	0.0000619
	(0.000615)	(0.000623)	(0.000387)	(0.00121)	(0.000594)	(0.000356)
In Utero	-0.000198	-0.000460	0.000253	-0.000917	-0.0000504	0.0000398
	(0.000606)	(0.000669)	(0.000404)	(0.00108)	(0.000581)	(0.000380)
0 to 2	0.0000289	-0.000210	0.000433	-0.000636	0.0000139	0.000115
	(0.000466)	(0.000539)	(0.000378)	(0.000839)	(0.000426)	(0.000325)
3 to 5	0.000655	0.000540	0.000967**	-0.000520	0.000664	0.000766**
	(0.000497)	(0.000580)	(0.000404)	(0.000680)	(0.000458)	(0.000341)
6 to 11	0.00103***	0.000987*	0.00113***	0.000102	0.00102***	0.000995***
	(0.000391)	(0.000566)	(0.000364)	(0.000669)	(0.000325)	(0.000261)
12 to 16	0.000709*	0.000771	0.000544	0.000537	0.000517*	0.000682***
	(0.000362)	(0.000535)	(0.000337)	(0.000615)	(0.000299)	(0.000244)
Observations	105892	87532	126546	67631	121829	190800
R^2	0.012	0.013	0.010	0.017	0.010	0.008
Mean	0.00885	0.00934	0.00771	0.00928	0.00862	0.00648
StdDev	0.0937	0.0962	0.0875	0.0959	0.0924	0.0803
Districts	1678	1623	1696	1593	1717	1801
Effect Size for 6 to 11 years	11.64%	10.57%	14.66%	1.1%	11.83%	15.36%
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of commit-ting crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The dependent variable takes a value of 1 if the individual is incarcerated for any crime and 0 otherwise. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.Effect size for 6 to 11 years are the percentage change in the estimated coefficients for that age group over the mean times 100%. The baseline specification on column (1) uses sample only containing male who were born between 1970 and 1993. the other columns indicates alternative birth year range that I use to estimate the regression for males only. The last column is equivalent to considering all birth cohorts since the incarceration data only has inmates born between 1926 and 1998. The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict. Source: Combined data NPPC and ENAHO 2015-2017.

Table A6: Impact of Conflict Exposure on Long Term Crime

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
Panel A: Sentenced to Prison					
In Utero	-0.000131	0.0000885	0.0000356	-0.000223	-0.0000315*
	(0.000298)	(0.000104)	(0.000115)	(0.000172)	(0.0000182)
0 to 2	-0.00000493	0.0000628	-0.0000317	-0.0000199	-0.0000157
	(0.000234)	(0.0000939)	(0.0000865)	(0.000140)	(0.0000151)
3 to 5	0.000203	0.000117	-0.0000155	0.000133	-0.0000317**
	(0.000247)	(0.0000927)	(0.0000776)	(0.000155)	(0.0000150)
6 to 11	0.000387**	0.000163*	-0.0000109	0.000231**	0.00000347
	(0.000197)	(0.0000866)	(0.0000713)	(0.000115)	(0.0000115)
12 to 16	0.000268	0.000199**	-0.0000136	0.0000598	0.0000230
	(0.000195)	(0.0000863)	(0.0000804)	(0.000114)	(0.0000159)
Effect Size for 6 to 11 years	8.98%	11.73%	-1.23%	11.67%	6.19%
Observations	79463	79463	79463	79463	79463
R^2	0.009	0.008	0.006	0.006	0.006
Mean	0.00431	0.00139	0.000886	0.00198	0.0000561
StdDev	0.0655	0.0373	0.0297	0.0444	0.00749
Districts	1660	1660	1660	1660	1660
Panel B: Detained to Prison					
In Utero	-0.000196	-0.0000758	-0.0000168	-0.000100	-0.00000441
	(0.000324)	(0.0000916)	(0.000102)	(0.000217)	(0.0000216)
0 to 2	-0.000166	-0.0000739	-0.0000897	0.00000444	-0.00000795
	(0.000242)	(0.0000726)	(0.0000914)	(0.000150)	(0.0000278)
3 to 5	0.000313	-0.00000365	0.0000695	0.000240	0.00000636
	(0.000290)	(0.0000787)	(0.0000896)	(0.000191)	(0.0000230)
6 to 11	0.000473**	0.000123*	0.000188***	0.000164	-0.00000222
	(0.000204)	(0.0000737)	(0.0000728)	(0.000124)	(0.0000229)
12 to 16	0.000336	0.000177**	0.0000185	0.000130	0.0000111
	(0.000213)	(0.0000725)	(0.0000817)	(0.000140)	(0.0000253)
Effect Size for 6 to 11 years	10.33%	10.51%	17.41%	7.39%	-1.82%
Observations	81019	81019	81019	81019	81019
R^2	0.010	0.008	0.007	0.006	0.002
Mean	0.00458	0.00117	0.00108	0.00222	0.000122
StdDev	0.0675	0.0341	0.0328	0.0470	0.0110
Districts	1653	1653	1653	1653	1653
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The regression are run based on inmates type (if they are sentenced or if they are detained and not sentenced yet). The Mean and Std. Dev represents the mean and standard deviation of the dependent variable. Effect sizes are the percentage change in the estimated coefficients for that age group over the mean times 100%. **Source:** Combined data NPPC and ENAHO 2015-2017.

Table A7: Robustness Check: Impact of Conflict Exposure on Long Term Crime- Placebo Test using Birth Cohort 1940 to 1963

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
Placebo -6	-0.000155	-0.000350	0.000140	0.000113	-0.0000567
	(0.000526)	(0.000255)	(0.000296)	(0.000154)	(0.0000754)
Placebo -5	-0.000492	0.0000392	-0.000250	-0.000269*	-0.0000127
	(0.000410)	(0.000226)	(0.000204)	(0.000163)	(0.0000630)
Placebo -4	-0.000684*	-0.000374*	-0.000382*	-0.0000240	0.0000956
	(0.000410)	(0.000224)	(0.000205)	(0.000113)	(0.0000656)
Placebo -3	-0.000312	-0.000178	-0.0000405	-0.000158	0.0000649
	(0.000368)	(0.000219)	(0.000173)	(0.000102)	(0.0000607)
Placebo In Utero	-0.000223	-0.000106	-0.000183	0.0000456	0.0000194
	(0.000426)	(0.000252)	(0.000174)	(0.000110)	(0.0000606)
Placebo 0 to 2	-0.000247	-0.0000992	-0.000210	0.000105	-0.0000434
	(0.000308)	(0.000176)	(0.000166)	(0.0000915)	(0.0000483)
Placebo 3 to 5	-0.000162	0.0000265	-0.000241	0.0000402	0.0000125
	(0.000286)	(0.000163)	(0.000157)	(0.0000816)	(0.0000417)
Placebo 6 to 11	-0.0000424	-0.0000355	-0.000106	0.0000879	0.0000116
	(0.000304)	(0.000196)	(0.000137)	(0.0000729)	(0.0000485)
Placebo 12 to 16	-0.0000734	0.0000215	-0.0000951	0.0000109	-0.0000104
	(0.000329)	(0.000186)	(0.000156)	(0.0000855)	(0.0000582)
Observations	39317	39317	39317	39317	39317
R^2	0.022	0.025	0.013	0.010	0.014
Mean	0.00318	0.00170	0.000859	0.000469	0.000151
StdDev	0.0563	0.0413	0.0293	0.0217	0.0123
Districts	1634	1634	1634	1634	1634
Effect Size for 6 to 11 years	-1.3%	-2.1%	-12.3%	18.8%	7.7%
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The regression produces placebo estimates for individuals who were exposed during older ages.The sample only contains male who were born between 1940 and 1963 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The conflict years are assumed to take place 30 years before the actual conflict years. The independent variables takes a value of one if the individual were exposed to placebo conflict during any of the respective ages of that variable and 0 otherwise. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable. Effect sizes are the percentage change in the estimated coefficients for that age group over the mean times 100%.
Source: Combined data NPPC and ENAHO 2015-2017.

Table A8: Robustness Check: Impact of Conflict Exposure on Long Term Crime- Excluding Drug Producing Areas

	(1) Baseline	(2) Excluding Coca Districts	(3) Excluding Coca Provinces	(4) Excluding Huanuco & San Martin
-6	0.000154	0.000133	0.000232	0.000488
	(0.000869)	(0.000897)	(0.00101)	(0.000951)
-5	0.00102	0.000814	0.00135	0.000992
	(0.000826)	(0.000871)	(0.00102)	(0.000879)
-4	-0.000112	0.000243	0.0000809	-0.0000272
	(0.000655)	(0.000691)	(0.000743)	(0.000699)
-3	0.000241	0.000416	0.000180	0.000335
	(0.000615)	(0.000630)	(0.000753)	(0.000637)
In Utero	-0.000198	-0.000371	-0.000504	0.0000546
	(0.000606)	(0.000606)	(0.000534)	(0.000620)
0 to 2	0.0000289	0.0000505	-0.000111	0.000113
	(0.000466)	(0.000476)	(0.000532)	(0.000482)
3 to 5	0.000655	0.000789	0.000813	0.000839
	(0.000497)	(0.000511)	(0.000564)	(0.000516)
6 to 11	0.00103***	0.00117***	0.00122***	0.00118***
	(0.000391)	(0.000402)	(0.000454)	(0.000407)
12 to 16	0.000709*	0.000685*	0.000727*	0.000718*
	(0.000362)	(0.000367)	(0.000415)	(0.000372)
Observations	105892	92851	74590	97389
R^2	0.012	0.011	0.011	0.011
Mean	0.00885	0.00854	0.00849	0.00863
StdDev	0.0937	0.0920	0.0917	0.0925
Districts	1678	1493	1181	1525
Effect Size for 6 to 11 years	11.6%	13.7%	14.4%	13.6%
Birth Year FE	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for any crime. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The coca districts are defined using data from the 1994 Agriculture Census.

The Mean and Std. Dev represents the mean and standard deviation of the dependent variable. Effect sizes are the percentage change in the estimated coefficients for that age group over the mean times 100%.

Source: Combined data NPPC and ENAHO 2015-2017.

Table A9: Impact of Conflict Exposure on Long Term Crime for Women

	(1) Any Crime	(2) Violent	(3) Organized	(4) Property	(5) Other Crime
-6	0.0000278	0.00000294	0.0000533	-0.0000157	-0.0000128
	(0.000101)	(0.0000263)	(0.0000728)	(0.0000595)	(0.000012)
-5	-0.000129	0.000011	-0.0000816	-0.0000665	0.00000852
	(0.000118)	(0.0000314)	(0.0000643)	(0.0000654)	(0.0000159)
-4	-0.0000693	-0.00000119	-0.0000397	-0.0000276	-0.000000731
	(0.0000818)	(0.000027)	(0.0000602)	(0.0000592)	(0.0000114)
-3	0.0000119	-0.0000417	0.00000848	0.0000494	-0.00000424
	(0.0000803)	(0.0000263)	(0.0000463)	(0.0000384)	(0.0000113)
In Utero	-0.0000809	-0.0000167	-0.0000663	-0.00000356	0.00000568
	(0.0000603)	(0.0000223)	(0.0000485)	(0.000024	(0.00000999)
0 to 2	0.0000275	0.0000355	0.00000571	-0.0000051	-0.00000857
	(0.0000605)	(0.0000236)	(0.0000506)	(0.000025)	(0.0000104)
3 to 5	-0.0000689	0.00000456	-0.0000434	-0.0000264	-0.00000372
	(0.0000571)	(0.0000178)	(0.000043)	(0.0000303)	(0.00001)
6 to 11	0.0000391	0.0000254	-0.0000149	0.0000431*	-0.0000145
	(0.000061)	(0.0000183)	(0.0000482)	(0.0000247)	(0.00000994)
12 to 16	0.00000631	0.0000117	-0.0000075	0.0000102	-0.00000808
	(0.0000507)	(0.0000186)	(0.0000387)	(0.0000244)	(0.0000126)
Observations	62591	62591	62591	62591	62591
R^2	0.005	0.007	0.004	0.004	0.001
Mean	0.000545	0.000078	0.000295	0.000147	0.0000253
StdDev	0.0233	0.00883	0.0172	0.0121	0.00503
Districts	1581	1581	1581	1581	1581
Birth Year FE	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains **women** who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level.
The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.

Source: Combined data NPPC and ENAHO 2015-2017.

Table A10: Impact of Conflict Exposure on Long Term Migration

	(1) Migration
-6	-0.0115
	(0.0209)
-5	-0.0247
	(0.0165)
-4	0.00442
	(0.0153)
-3	-0.0259
	(0.0161)
In Utero	0.00447
	(0.0127)
0 to 2	-0.00886
	(0.0145)
3 to 5	-0.00819
	(0.0136)
6 to 11	-0.0139
	(0.0126)
12 to 16	-0.0176
	(0.0125)
Observations	105352
R^2	0.298
Mean	0.55
StdDev	0.5
Districts	1678
Birth Year FE	Yes
Birth District FE	Yes
Birth District Trend	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of current migration. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The dependent variable takes a value of 1 if the last district of residence for the individual is different from their birth district and 0 otherwise. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.
Source: Combined data NPPC and ENAHO 2015-2017.

Table A11: Impact of Conflict Exposure on Long Term Crime: Marginal Effect of the Logit Model

	(1)
	Any Crime
-6	0.000451
	(0.0005923)
-5	0.0009664*
	(0.000573)
-4	0.0001117
	(0.0004695)
-3	0.0003158
	(0.0004757)
In Utero	-0.0001143
	(0.0004217)
0 to 2	-0.0000571
	(0.0003583)
3 to 5	0.0003866
	(0.0003702)
6 to 11	0.0005582*
	(0.0003353)
12 to 16	0.0003179
	(0.000329)
Observations	105119
Mean	0.0089
StdDev	0.09424
Districts	1585
Birth Year FE	Y
Birth District FE	Y
Birth District Trends	Y

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on the probably of committing crime in the future using a logit model with iteratively re-weighted least squares (IRLS) technique. The independent variables takes a value of one if the individual were exposed to conflict during any of the respective ages of that variable and 0 otherwise. The sample only contains male who were born between 1970 and 1993 and in the birth districts contained in the ENAHO 2015-2017 data for male born during those years. The dependent variable takes a value of 1 if the individual is incarcerated for the crime as listed in the columns. The controls include ethnicity (native=1), birth year fixed effects, birth districts fixed effects and birth district level linear time trends. The standard errors are clustered at the birth district level. The reported coefficients are the marginal effect estimated at the mean values from the logit model with delta method standard errors. The Mean and Std. Dev represents the mean and standard deviation of the dependent variable for the individuals who were born in districts that were never exposed to the conflict.
Source: Combined data NPPC and ENAHO 2015-2017.

Table A12: Impact of Conflict Exposure on Long Term Education- Using ENAHO 2004 to 2017

	(1) Male	(2) Female
-6	-0.0137	-0.0753
	(0.0367)	(0.0510)
-5	-0.0430	-0.00737
	(0.0334)	(0.0443)
-4	0.0409	-0.0919**
	(0.0336)	(0.0423)
-3	-0.0521*	-0.0501
	(0.0284)	(0.0409)
In Utero	-0.0323	-0.0788**
	(0.0256)	(0.0385)
0 to 2	-0.0512*	-0.125***
	(0.0304)	(0.0402)
3 to 5	-0.0487*	-0.0751**
	(0.0272)	(0.0362)
6 to 11	-0.00535	-0.0369
	(0.0354)	(0.0361)
12 to 16	-0.0186	0.0271
	(0.0292)	(0.0340)
Observations	207858	214482
R^2	0.237	0.361
Mean	9.262	8.461
StdDev	3.060	3.756
Districts	1789	1804
Birth Year FE	Yes	Yes
Birth District FE	Yes	Yes
Birth Province Cubic Trend	Yes	Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The table shows the impact of exposure to conflict during different ages on long term education. The sample only contains individuals who were born between 1970 and 1993. The controls include ethnicity (native=1), age, survey year fixed effects, birth year fixed effects, birth districts fixed effects and birth district level linear time trends. Sampling weights are used to obtain population estimates. The standard errors are clustered at the birth district level. In this case, following Leon (2012) I use all conflict data, include torture and rape, to define exposure and use province level cubic trends instead of birth district level linear trends.
Source: ENAHO 2004-2017.