ESSAYS IN APPLIED MICROECONOMICS

A Dissertation

by

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ABSTRACT

This dissertation introduces three essays using quasi-experimental methods to examine the effects of the long term impact of civil conflict on crime and the impact of labor market conditions and policy interventions on household well-being. The first essay examines the effect of exposure to conflict-induced violent incidents during childhood on the decision to engage in criminal activity in the long term. The second essay evaluates the impact of a government policy targeted towards the victims of domestic violence in Peru on the health and cognitive development of children under the age of five. The last essay looks at how closing the earnings gap between women and men in the local labor market affects gender-based violence using a shift-share instrument approach. All three essays use data from Peru.

First, in the 1980s, Peru was marred by a gruesome civil conflict that persisted for over a decade. In the first essay, "Civil conflict and later life crime" I look 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 ages for men increases their probability of incarceration in adulthood. Unlike other evidences on the long-term impacts of war, in utero exposure does not seem to explain criminal behavior in later life in this context.

Second, maternal contribution in the nurture and growth of their children is indispensable. However, they may be faced with unfavorable situations that can adversely affect the children. One such common and pervasive problem is domestic violence. Peru, as a country with one of the highest rates of domestic violence in the world, introduced Women's Emergency Centers (WEC) that were found to be effective in reducing domestic violence towards women. In the second essay, "Maternal condition and child well-being", we examine whether a program targeted towards the

victims of domestic violence can impact the cognitive and health outcome of children. We find evidence that exposure to the WEC within 2 km of the household improves the health outcomes of children under the age of five in terms of their weight-for-age z-scores, wasting and underweight. We also find some evidence that access to these centers improve the cognitive development of children in terms of their symbolic function. The improvement in the health outcomes is posited to be driven by reduction in the probability of experiencing domestic violence of the mothers with closer proximity to the Women's Emergency Centers. We also find evidence that maternal-child attachment improves with exposure to these centers. This is consistent with maternal involvement being impaired by domestic violence.

Finally, despite the substantial implications of increased female labor market opportunities for women, relatively less is known about the impact of improved outside option for women on domestic violence, especially in the context of developing countries. Economic theory on household bargaining model predicts that better outside options for women should reduce the level of domestic abuse through greater bargaining power. In the third essay, "The Many faces of abuse: labor market opportunities and domestic violence on women", we exploit the exogenous variation in labor demand induced by differing gender composition across industries to show the impact of changes in the relative labor market condition for women on various forms of abuse. Using nine waves of Demographic and Health Survey (DHS) from Peru, we find evidence of lower psychological and emotional abuse with improvement in the labor market opportunities for women. However, we do not see any effect on physical or sexual violence.

DEDICATION

I dedicate this dissertation to Allah (SWT), my parents, my husband and my son.

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1. INTRODUCTION

This dissertation revolves around understanding the long term impact of shocks during childhood and the influence of social policies on the welfare of women and children. Using quasi-experimental methods, I causally infer the determinants of social inequality and the effectiveness of policies in mitigating these gaps.

In the early 80's, Peru witnessed the beginning of more than a decade long intense period of violence with the emergence of an insurgent group Partido Comunista del Perú-Sendero Luminoso (PCP-SL), a Maoist rebel group that was a self-proclaimed agent of a world history destined to conclude in a communist revolution. The war prompted gross human rights violations with the use of excessive force and killing of innocent civilians by both the rebel group and the government military personnel responding to these rebels. In 2003, the Truth and Reconciliation Commission estimated approximately 70,000 died or disappeared as a result of the internal conflict in Peru between 1980 and 2000.

Recent studies found that people who were exposed to the war in Peru at a young age were negatively affected in their long-term health, education and labor market outcomes. Literature in psychology indicates that exposure to community violence such as in the case of an on-going civil conflict, can lead to an increased risk for various types of externalizing behavior. The persistent exposure to the civil conflict in Peru may induce children to display anti-social behavior, which combined with the long-term economic disadvantage as a result of the war may lead these individuals to a criminal life path in the long run. Section 2 extends the existing literature on the long term effects of conflict by looking at how it affects later life criminal behavior. I find that exposure to conflict during childhood is associated with an increased probability of incarceration during adulthood.

Most developing countries around the world suffer from a deep-rooted culture of patriarchy, granting limited autonomy to women as well as tolerating the use of domestic violence towards women and children. This frequently results in the inadequate exposure of women and children to health care and education which eventually leads to the intergenerational transmission of poverty. Peru has one of the highest rates of domestic violence in the world. The government of Peru introduced reformations in their legislation to ease access to justice for victims of abuse. This led to the subsequent establishment of one-stop specialized centers for victims of domestic violence across the country. A recent study found that introduction of these services led to a decrease in gender-based abuse which suggests a relative improvement in the overall household environment. Section 3 studies if there were any spillover effects from these centers on the children within the household. Particularly, it investigates how health and cognitive outcomes for children less than five years of age are affected. We find that children living within 2 km of a center have greater weight-for-age and lower likelihood of wasting and underweight. They are also more likely to have better cognitive development measured in terms of their symbolic function.

Relatedly, Section 4 looks at how reducing the earnings gap between women and men in the local labor market affects gender-based violence using a shift-share instrument approach. Following the non-cooperative bargaining model, reducing the female to male earnings gap should improve the outside option for women even if they are unemployed. This results in raising the relative bargaining power of women and the paper finds that it leads to a decrease in intimate partner violence in Peru. This indicates that the local labor markets could also play a role in improving the condition of women in a country with a high level of domestic violence.

2. CIVIL CONFLICT AND LATER LIFE CRIME

2.1 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

[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, 2008, 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).

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 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

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

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, 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

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

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.

To evaluate if exposure to war increases the probability of incarceration for women, I estimate the impact of exposure to conflict during different ages for women and do not find any statistically significant effect for any ages of exposure. Following the crime literature, I also perform a cohort level analysis where my outcome variable is measured as the incarceration rate for each cohort born in a specific birth district and divide it by the size of the cohort in that birth district. I estimate the impact of exposure to conflict in the birth district during specific ages for each cohort on the incarceration rate at the birth district-cohort level and 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].

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]. In the short-run, I find evidence that exposure to conflict during primary school age is associated with lower likelihood to attending school and higher probability of being involved in child labor. 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

[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.

[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.

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 from 0.00103 to 0.000945. 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. The data used in this paper only contains years of education as the measure of human capital. However, conflict exposure can affect other measures of human capital such as quality of education, cognitive and non-cognitive skill which may not be reflected through our measure of years of education. Due to data limitations, I cannot test for these channels. These measures can affect an individual's propensity to commit crime and therefore we cannot claim that human capital cannot be a channel.

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 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 also found in other war affected context(Voors et al., 2012, Cecchi et al., 2016, Kim and Lee,

[8]In particular, studies found long-term impact on education (e.g., Akresh and De Walque, 2008, 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).

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.

2.2 Background

2.2.1 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.

2.3 Data

2.3.1 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. Figure 2.1 depicts the temporal and geographic evolution of the war.

2.3.2 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.

2.3.3 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, multi-stage 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.

Table 2.1 contains the summary statistics of the data used in my main analysis.

2.4 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.

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[9], 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).[10]

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.

[9]Therefore the name rare events

[10]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.

2.4.1 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 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,

$$Y_{ijt} = \beta_0 + \beta_k \sum_k WarExposure_{jk} + ethnicity_{ijt} + \alpha_j + \alpha_t + \alpha_j trend_t + \varepsilon_{ijt} \tag{2.1}$$

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[11] 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 variable 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

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

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.[12]

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 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[13]. 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

[12]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.

[13]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.

is the proportion of the total observations in year y to the total sample size of the pooled data[14].

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.

2.4.2 Measurement of Treatment

As mentioned earlier, one of the main drawbacks of the conflict data is that it comprises of a non-random sample of the total violent incidents that took place during the war. Although the commission estimates the total fatalities during the period to be about 70,000, the total recorded fatalities in the data is about a quarter of that.

The data collected by the Truth and Reconciliation Commission is stored in two ways.

- *"Cases" of deaths and disappearances:* In this database, each record represents a dead or missing person as a result of the internal armed conflict between 1980 and 2000. This information comes from the first stage of the analysis that consisted in the registration of all persons reported as dead or missing in the testimonies collected by the CVR.
- *Violent "acts":* In this data set each record represents an act of violence perpetrated against one and only one person, at a specific date and place, by one or more responsible groups. This information comes from the second stage of analysis: the reconstruction of the event and the classification of acts, and represents 70% of the testimonies received. The same person may suffer several acts at different times, each of these constitutes a different record in this table. A violent act is defined as killing, forced disappearance, sexual violence and, torture.

For my main analysis, I use the first data set which defines a violent incident as a death or disappearance of a person. I use this over the second data set for two reasons. First, the data set

[14] $\alpha_y = \frac{n_y}{\sum_y n}$ 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).

includes information on all reported person, whereas, the second data set represents 70% of the total testimonies received. Furthermore, more violent acts such as death and disappearance of an individual are less likely to suffer from measurement error in recording the time and place of the incident compared to specific acts such as torture and rape.

Using my preferred data, I define treatment as a dummy variable which takes a value of 1 if an individual is exposed to atleast one death or disappearance during the specified age group and 0 otherwise. Each coefficient indicates the impact of the average person experiencing violence at the specified age group. The definition of treatment purposefully use broad periods in life (i) to capture the critical stages during childhood, (ii) to reduce the possibility of error in treatment assignment due to the missing incidents in the data and to account for any misreported date of birth.

Alternatively, I can also define treatment as the number of years exposed or total number of violent events per 1000 people within the specified age group. I prefer my original treatment definition over using the number of incidents per 1000 people for a few reasons. As mentioned before, the conflict data set suffer from under-reporting. The sources for these under-reporting is likely to be correlated with the characteristics of the districts or the victims. The camps to collect the war testimonies were set up in the relatively big towns which could exclude some of the more vulnerable population from reporting. Under-reporting could also stem from the less affected and less proactive population or conversely by victims most affected by the violence such that reporting itself could be traumatic for them. This suggest that the measure of war intensity is more likely to have a non-classical measurement error, which could bias the estimates in either direction. This problem is most pronounced when measuring treatment in term of the number of total fatalities. Furthermore, to define the number of incidents per 1000 people, I divided the total number of incidents by the population size in each district in 1981. The district level population size data is obtained from the 1981 census. However, the 1981 census has data from only 22 out of the 25 regions in Peru [15]. Finally, when we define violence in terms of intensity, we assume that the impact of an additional incident or an additional year of exposure should affect our outcome

[15] The missing regions are Apurimac, Loreto and San Martin

variable linearly. This may not be necessarily true.

Therefore defining treatment as exposure instead of intensity in terms of the number of years or the total number of incidents helps us reduce some of the these issues and allows us to observe how the average treated person should be affected by the violence. While to get an accurate measure for the number of incidents, you need everyone to report to the commission, for an reasonable measure to capture exposure during a specified period, we only need one incident to be reported during that period of time. This does not get rid of the measurement error problem completely, but the bias using the total incident measure is likely to be much larger. Nonetheless, I also present the results using the second data set as well as the alternative definitions of treatment in the later sections.

2.4.3 Baseline Results

Column 1 in Table 2.2 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 2.2 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 2.12 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 2.2.

I also report the results using only the data from ENAHO 2016 and NPPC 2016 in Table 2.13. I see effects for the same age groups and types of crime as in Table 2.2, however, the magnitude of the impacts are larger.

Table 2.20 reports the estimated impact of exposure to conflict during childhood on long term incarceration for women and we do not see any statistically significant impact for them.

2.4.4 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.

2.4.4.1 Conflict Exposure During High Casualties Years

Figure 2.2(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 2.3 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.

2.4.4.2 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 2.4 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.

2.4.4.3 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 2.4 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.

2.4.5 Cohort-level Analysis

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.

$$Y_{jt} = \beta_0 + \beta_k \sum_k WarExposure_{jk} + \alpha_j + \alpha_t + \alpha_j trend_t + \varepsilon_{ijt} \tag{2.2}$$

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/ 2017 census) in birth cohort y born in district j.

$WarExposure_{jk}$ is a dummy variable which is equal to 1 if the cohort from birth district j was exposed to war exposure during k ages of their life, where $k \in [-6, 16]$. 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 (2008) for grouped data..

Figure 2.3 has the estimates by grouping the war exposure age by 2 years. It also includes dummy variables for exposure years before birth. I perform the regression analysis using cohort sizes from both 2007 and 2017 census data. 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[16]. I test whether the sum of all the coefficients for exposure during ages after birth is different from zero. The null hypothesis that the sum of the coefficients is zero is rejected at the 5% significant level. This indicates that the estimated effects may not just be driven by the statistically significant coefficients for ages 6 to 9 years, other ages at exposure may also be important.

Figure 2.4 Panel (a) and (b) represents the cohort-level estimation with alternative definitions of treatment where instead of defining the treatment as a dummy variable for each age group, I define $WarExposure_{jk}$ as the total years of violence or total violent incidents per 1000 people that the cohort from birth district j experienced during k stage of their life. The estimated effects looks similar to the main specification when I define treatment intensity as the total years exposed in Figure 2.4 Panel (a). However, when I measure treatment as the total number of violent incidents per 1000 people in a specific group, the estimates looks slightly erratic. Although I find a positive

[16] 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

and significant impact for exposure to one more incident per 1000 people when the child was 8 to 9 years old, I also find positive and significant impact for exposure during younger and older ages in Figure 2.4 Panel (b). If I assume the estimate to be correct, this could indicates that an additional violent incident even during very early childhood is associated with an increase in the probability of crime in the future and my preferred treatment measure which is a dummy variable is unable to capture this effect. Alternatively, this could be a result of the non-classical measurement error discussed in Section 4.2.

I also estimate the cohort-level regression equation (2) using the second CVR data to define the main independent variable. As mentioned before, this data set record a violent incident as an act of violence such as murder, kidnapping, torture or rape perpetrated against one and only one person. Figure 2.4 Panel (c) presents the estimated effect of exposure to violence, as defined in the second data, during specific ages during childhood. The results indicate that exposure to violence during the early childhood period is associated to increased probability of incarceration in the long term.

Therefore, there is significant effect across many ages depending on the definition of the treatment variable, but only those affected around primary school years is consistently impacted across all the specification.

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

2.5.1 Short-term Impact of Conflict

In this section, I explore the short-term impact of the civil conflict on the educational and labor market outcomes for children aged 6 to 17 in the year 1993. Using a 5% random sample of the 1993 population census, I estimate the impact of exposure to conflict on short-term schooling and labor market behavior of school aged children using equation (1). Particularly, I look at the years of education, the probability of attending school in 1993, the probability of never attending school by 1993 and the probability of working for income or non-income generating activities.

The results are presented in Table 2.5. Being exposed to violence during the primary school going age is associated with lower probability of attending school and higher probability of working as presented in Column (2) and (4) respectively. In terms of schooling, I find that exposure during the pre-school age is associated with lower years of education as can be seen in Column (1). Although we see a negative impact on schooling with exposure to conflict during primary school age, the coefficient estimate is statistically non-significant.

The results suggest that a potential mechanism driving the main results is that conflict exposure makes children less likely to attend school and more likely to be involved in child labor. The size of the impact on child labor is also non-trivial, about 18% over the mean. In the context of Peru, child labor may impact long term criminal behavior in a few ways.

First, working during childhood could be associated with reduction in schooling. Children who work may also simultaneously choose to attend school, however, the hours and the strenuousness of the tasks involved may cause increased absenteeism and greater likelihood of dropout. This could result in fewer labor market opportunities and lower wages in the future, increasing the future probability of committing crime. However, I do not find any statistically significant evidence of a decrease in schooling if exposed to war during primary school age. However, its could affect other measures of human capital such as cognitive and non-cognitive skills development that I are unable to measure with this data.

Second, increase in child labor may also be a response to socioeconomic depletion or loss of income earning adults in the household due to the conflict. The poor socioeconomic condition may cause persistence in poverty which may also increase involvement in crime in the future.

To explore the type of work the children are involved in, I estimate the impact of exposure to war on working in different sectors. Table 2.6 shows that exposure to war during 6 to 11 years of age is associated with a greater likelihood of working in agriculture. I do not see any impact of war exposure during earlier ages on working or the impact of war exposure on working for an industry (e.g mining or construction) or in service. After 1983, the Shining Path became established in the coca-growing region including the Huallaga Valley and the Apurimac and Ene River Valleys. Since we see an increased probability of working in agriculture, it could indicate an increased involvement in coca farming. Children as young as 6 years old start working in the coca farm and are well-suited for tasks such as picking leaves. Children are involved in different production processes including transporting and processing coca into cocaine. The areas controlled by the Shining Path were involved with the illegal market of cocaine. Therefore, one could also imagine that as children work in the illegal coca industry, they gain industry-specific criminal capital which increase the benefit of future involvement in the illegal industry (Sviatschi et al., 2017). This, combined with exposure to violence could explain the future propensity to commit crime.

To provide further evidence to my claim, I estimate the impact of exposure to crime on short-term outcomes by the child's migration status. A child is a non-migrant if he remains in his district of birth and is a migrant if he lives in a different district[17]. Table 2.7 shows that the short-run impact of exposure to war seems to be driven by the non- migrant children. This suggests that the negative impact of exposure to war is more likely to be driven by the exposure to violence rather than displacement.

2.5.2 Long-term Impact of Conflict

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

[17]Table 2.21 shows the probability of migration by exposure to war during different ages. We do not see an impact of migration by the age at exposure.

the male population in my data, I estimate equation (1) with education as my dependent variable. Column (2) and (3) in Table 2.8 presents the results for the impact of exposure to conflict during different ages on years of education[18] 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 using my preferred specification[19]. (Leon, 2012) finds that the impact of conflict on education in the long term is smaller than in the short term. Since I used the data from an even longer period, we may not see an effect if the negative impact of war on education subsides with time.

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 2.8 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 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 2.11 shows the impact of conflict exposure for men on height and weight re-

[18]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.

[19]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 2.24

spectively. We can see that exposure during adolescent years is associated with a 0.77cm decrease in height as adults 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[20][21]. Height can affect crime through its association with cognitive skills that can determine labor market outcomes (Vogl, 2014). In Table 2.10, 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 2.8 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.

For the analysis of the intermediate variables, it must be noted that the data used is representative of the non-institutionalized population. As such, if we assume that exposure to conflict increases the probability of incarceration during later childhood, the probability of observing the "treated" individuals in our data set may go down with time. This may lead us to find an impact of exposure to violence on intermediate variables that are smaller in magnitude than the true effect and are statistically non-significant. When I estimate the impact of exposure to conflict during primary school age on the health and labor market outcomes, the coefficient estimates has the expected negative sign albeit smaller effect size and is statistically not significant. This could be as a result of the conflict not having an effect on these outcomes or be a result of the underestimation of the impacts due to the fact the individuals who are most affected by war exposure are more likely

[20] 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 or could be a consequence of child labor.

[21] It must noted that, the CENAN data set is a self-selected sub-sample of the ENAHO population and may not be representative of the population.

to “migrate out” of my sample of analysis. Therefore, I cannot conclusively claim that these are not potential channels explaining my main findings.

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 2.9 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.

2.6 Robustness Checks

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

2.6.1 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 2.14 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.

2.6.2 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.[22] 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 2.15 contains the estimates with the alternate sample selection conditions. Column 1 contains the results from the baseline specification with pre-birth exposure indicators in Table 2.12 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.

2.6.3 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

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

regression with alternative choice of birth years to be included in the analysis. Table 2.16, 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[23].

2.6.4 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 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 2.17, 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.

2.6.5 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

[23]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.

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 2.19 shows that my estimates are robust to exclusion of the drug producing regions.

2.6.6 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 2.5. 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 2.18, 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[24]. Therefore, this exercise can also be seen as a test to confirm our hypothesis that exposure to war is especially detrimental for children.

2.7 Potential Biases and Concerns

2.7.1 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 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.

2.7.2 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-

[24]Between ages 28 and 46

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[25], 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.

2.7.3 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-

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

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[26] is influenced by exposure to war during different ages. In Table 2.21 and 2.22, I do not find any evidence of differential migration by exposure to war during different ages in the short and long term respectively.

2.7.4 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 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 (2008),

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

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 2.23. 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.

2.8 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. The results are robust to a battery of checks and tests and are more likely to be an underestimate of the true effects.

This is an important finding since it deviates from the commonly held belief that exposure to negative shocks are most critical during in utero period. While I do not find a significant impact with early childhood exposure for my preferred specification, I cannot reject the possibility that exposure during ages other than the primary school age may also have an impact on long term incarceration.

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. However, I do find that in the short-term, exposure to conflict during primary school age is associated with lower probability of attending school and a higher probability of working as a child. 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.

Figure 2.1: The Geographic Evolution of the Conflict

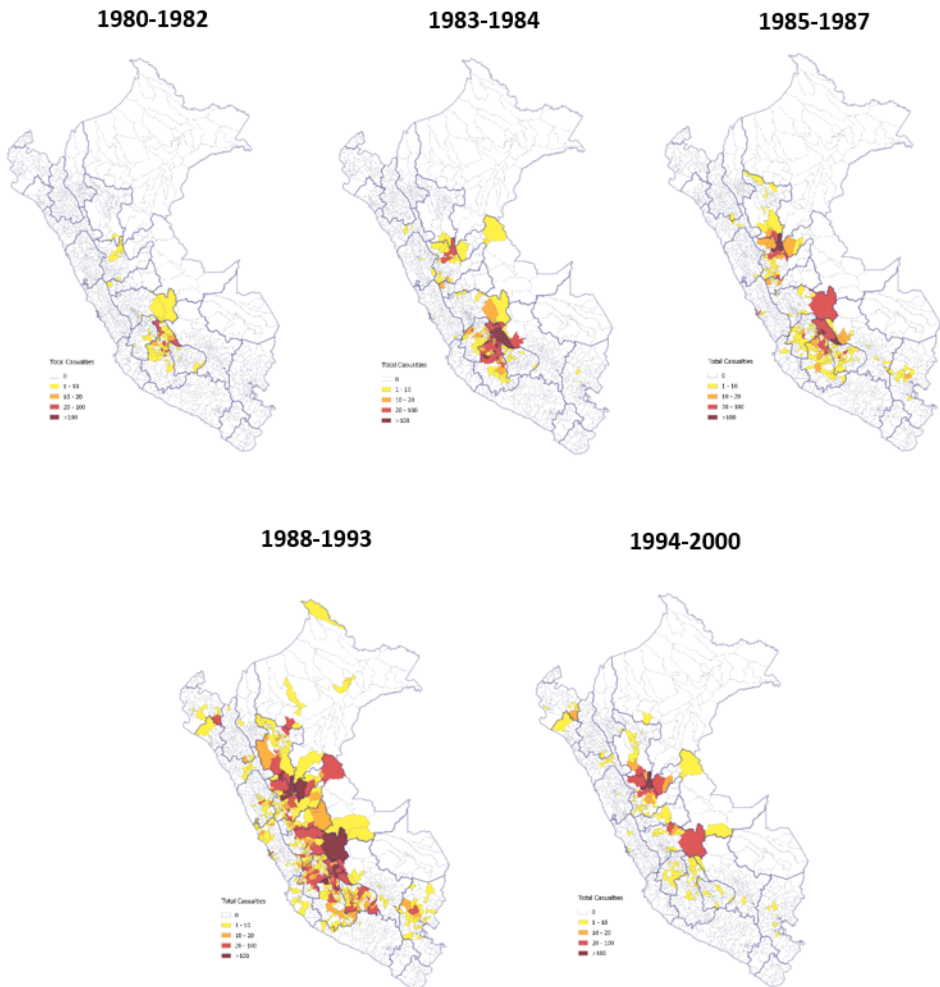
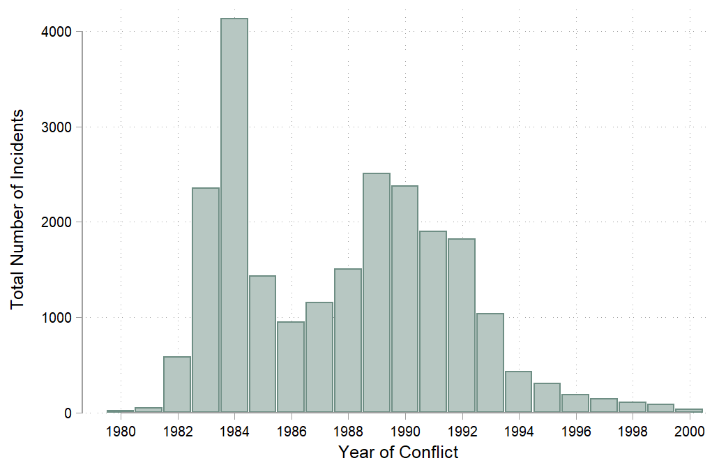
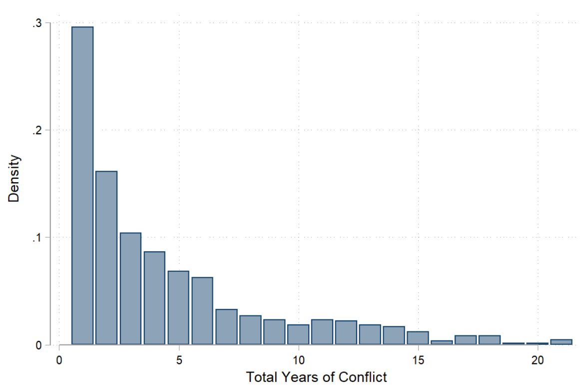


Figure 2.2: Structure of Conflict Data

(a) Total Conflict Incidents by Year



(b) Distribution of total years of exposure to war



(c) Total Conflict Incidents by Region

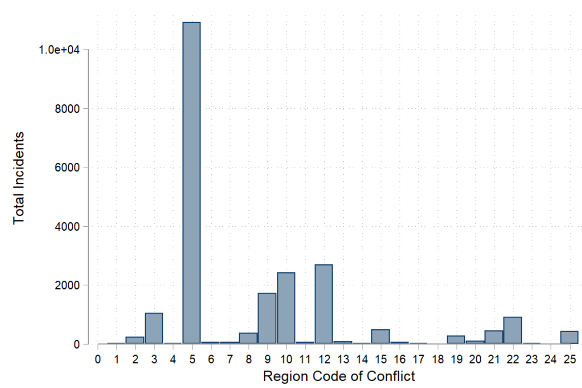


Table 2.1: Summary Statistics

	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 2.2: 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 2.3: 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 2.4: 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 coefficients 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 2.5: Impact of Conflict Exposure on Short Term Education and Labor Market Outcomes

	(1) Education	(2) Attend School	(3) Never School	(4) Work
In Utero	0.0195	0.0129	-0.0171	-0.000818
	(0.0214)	(0.0138)	(0.0117)	(0.00670)
0 to 2	-0.0130	-0.00373	-0.00602	0.00367
	(0.0225)	(0.00992)	(0.00791)	(0.00800)
3 to 5	-0.0457**	-0.0109*	-0.00168	0.00691
	(0.0215)	(0.00563)	(0.00365)	(0.00456)
6 to 11	-0.0353	-0.0198***	0.00862	0.0145**
	(0.0219)	(0.00592)	(0.00546)	(0.00583)
Observations	149894	154606	154606	154606
Adjusted R^2	0.728	0.132	0.102	0.190
Mean	4.579	0.824	0.0372	0.0787
StdDev	3.122	0.381	0.189	0.269
Districts	1760	1762	1762	1762
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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Impact of Conflict Exposure on Short Term Work by Sector

	(1) Agriculture	(2) Industry	(3) Service
In Utero	-0.00898	0.00115	0.00245
	(0.00829)	(0.000891)	(0.00206)
0 to 2	-0.00160	0.000722	0.00249
	(0.0102)	(0.00115)	(0.00273)
3 to 5	0.00406	0.000307	0.00212
	(0.00506)	(0.00106)	(0.00232)
6 to 11	0.0134**	0.00207	-0.00146
	(0.00582)	(0.00129)	(0.00227)
Observations	154606	154606	154606
Adjusted R^2	0.210	0.032	0.040
Mean	0.0407	0.00730	0.0152
StdDev	0.197	0.0851	0.122
Districts	1762	1762	1762
Birth Year FE	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes

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

Table 2.7: Impact of Conflict Exposure on Short Term Outcome by Migration Status

	Education		Attend School		Never School		Work	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-Migrant	Migrant	Non-Migrant	Migrant	Non-Migrant	Migrant	Non-Migrant	Migrant
In Utero	0.0153	0.0107	0.00911	0.00320	-0.0126	-0.00616	0.00568	-0.00103
	(0.0282)	(0.0303)	(0.0115)	(0.0106)	(0.00813)	(0.00683)	(0.00567)	(0.00592)
0 to 2	-0.0169	-0.0159	-0.00768	-0.00763	-0.00434	-0.000185	0.0125**	0.000414
	(0.0283)	(0.0300)	(0.0105)	(0.00942)	(0.00745)	(0.00577)	(0.00634)	(0.00762)
3 to 5	-0.0520*	-0.0368	-0.0111	-0.00980	-0.00327	0.00221	0.0127**	0.00346
	(0.0276)	(0.0332)	(0.00768)	(0.00621)	(0.00522)	(0.00321)	(0.00530)	(0.00577)
6 to 11	-0.0408	-0.0339	-0.0213***	-0.0116*	0.00534	0.00764	0.0197***	0.00972
	(0.0300)	(0.0311)	(0.00813)	(0.00665)	(0.00550)	(0.00475)	(0.00603)	(0.00977)
Observations	92952	56781	96239	58204	96239	58204	96239	58204
Adjusted R^2	0.703	0.760	0.139	0.150	0.113	0.093	0.204	0.219
Mean	4.185	5.223	0.814	0.840	0.0491	0.0176	0.0809	0.0750
StdDev	3.006	3.200	0.389	0.367	0.216	0.131	0.273	0.263
Districts	1717	1538	1723	1541	1723	1541	1723	1541
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth District Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The treatment variable is a dummy variable that takes a value of 1 if the child was exposed to atleast on death or disappearance during the specified age and zero otherwise.

Table 2.8: 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 potential intermediate variables. 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 2.9: Impact of Conflict on Long Term Crime- Potential Mechanism

	(1) Any Crime	(2) Any Crime	(3) Any Crime	(4) Any Crime	(5) Any Crime	(6) 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 2.10: Impact of Conflict Exposure on Long Term Labor Market Outcomes

	Ages: 18 to 30 years			Ages: >30 years		
	(1) Employed	(2) Log Earnings	(3) Large Firm	(4) Employed	(5) Log Earnings	(6) 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 2.11: 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

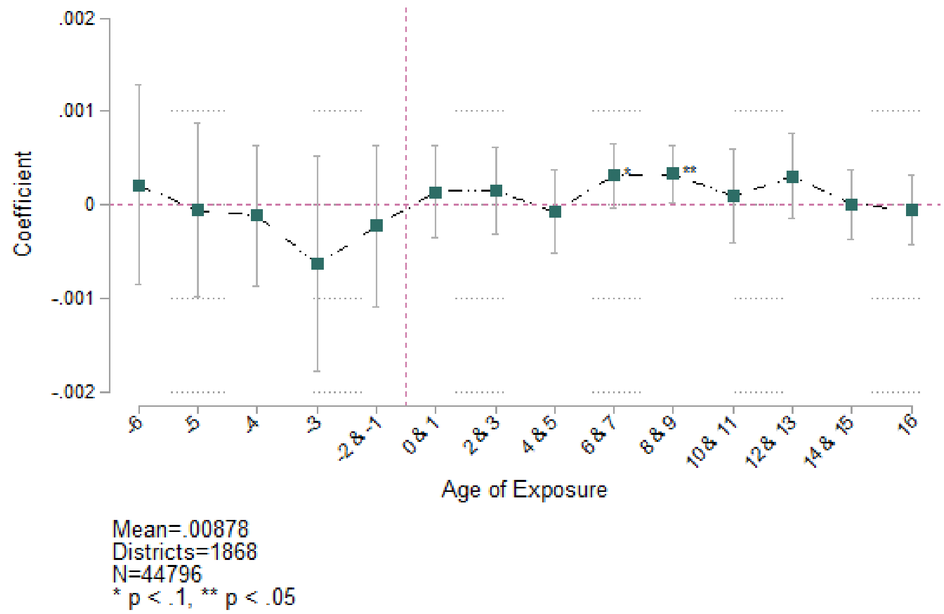
Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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.

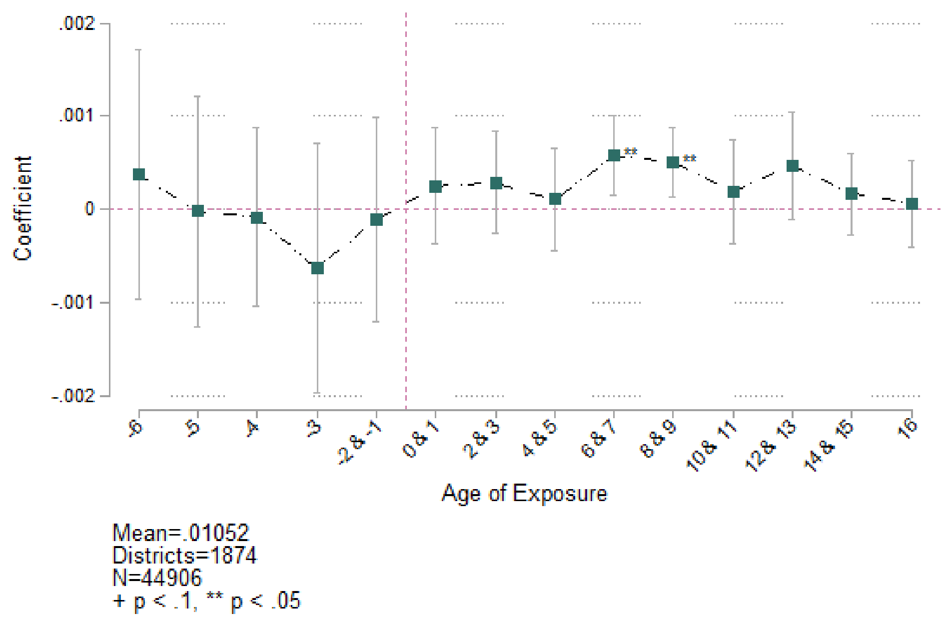
Source: ENAHO 2007-2011.

Figure 2.3: Impact of Conflict Exposure on Long Term Crime Using Cohort Level Data

(a) NPPC 2016 with Census 2007 to Create Birth Cohort Level Data.



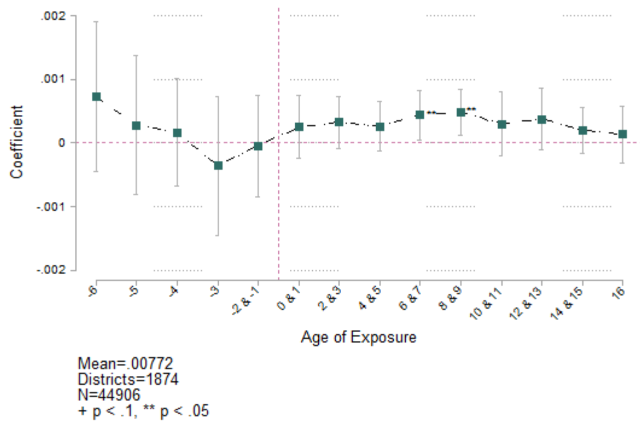
(b) NPPC 2016 with Census 2017 to Create Birth Cohort Level Data.



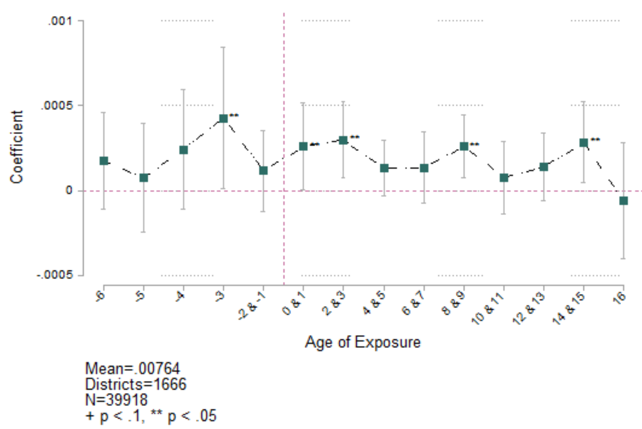
Note: The graphs shows the estimates of the impact of exposure to war during different ages. It shows the baseline specification in equation (1) with different age grouping using the cohort level measure which is the crime rate per male in that birth district and cohort. The x-axis indicates an age group 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 2.4: Impact of Conflict Exposure on Long Term Crime Using Cohort Level Data: Alternative Treatment Definitions

(a) Intensity: Total Years Exposed



(b) Intensity: Total Incidents Per 1000 People



(c) Exposure to Conflict: Using the Violent Acts Data

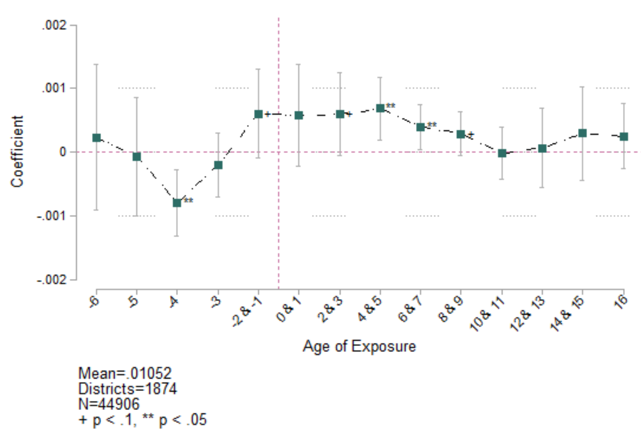
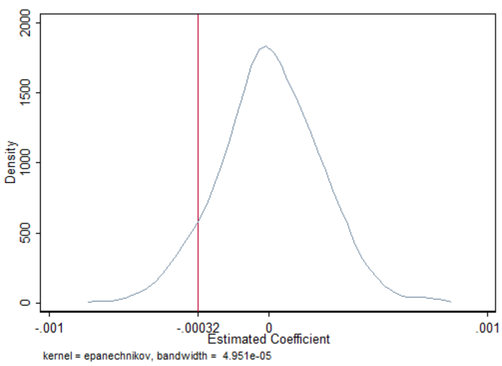
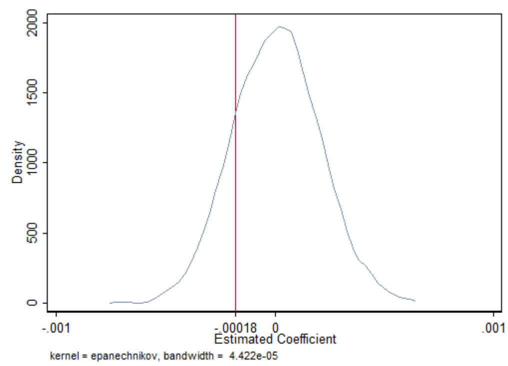


Figure 2.5: Impact of Conflict Exposure on Long Term Crime: Placebo Estimates on Any Crime

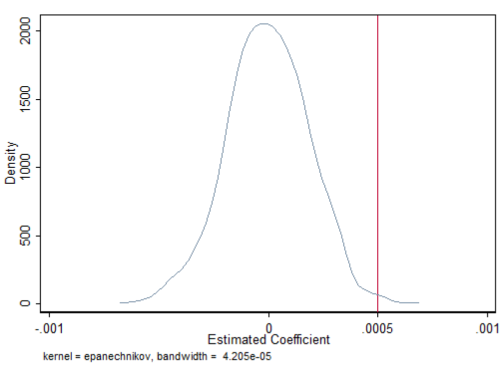
(a) In Utero



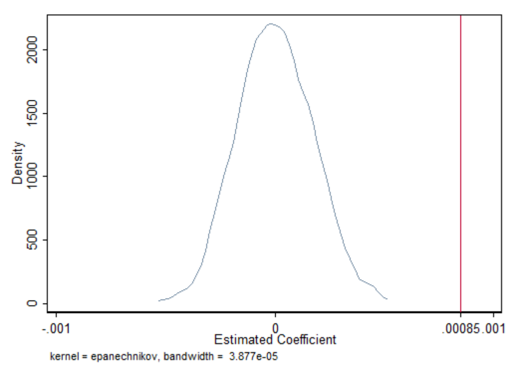
(b) 0 to 2 years



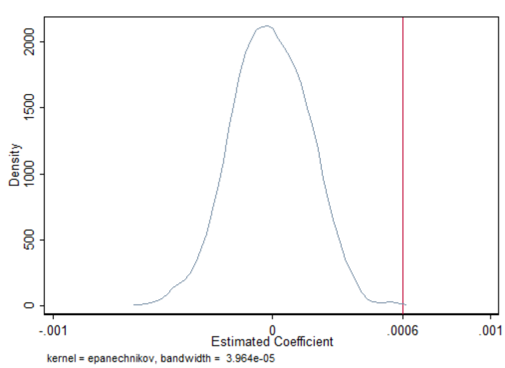
(c) 3 to 5 years



(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 2.12: 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 2.13: 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 2.14: 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 2.15: 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 2.16: 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 2.17: Robustness Check: Impact of Conflict Exposure on Long Term Crime- Type of Inmates

	(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 2.18: 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 2.19: 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 2.20: 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 2.21: Impact of Conflict Exposure on Short Term Migration

	(1)
	Migration
In Utero	-0.00240
	(0.00733)
0 to 2	0.00175
	(0.00733)
3 to 5	-0.00497
	(0.00536)
6 to 11	-0.00545
	(0.00563)
Observations	161351
Adjusted R^2	0.352
Mean	0.373
StdDev	0.484
Districts	1767
Birth Year FE	Yes
Birth District FE	Yes
Birth District Trend	Yes

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

Table 2.22: 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 2.23: 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 2.24: 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.

3. MATERNAL CONDITION AND CHILD WELL-BEING

3.1 Introduction

Violence against women has been recognized by the UN as a human rights violation with important implications for public health (Bott et al., 2012). Growing evidence across the globe documents the pervasiveness of this issue. About 35% of women worldwide reports of ever experiencing some form of physical or sexual violence, of whom less than 40% seek help of any sort in most countries (García-Moreno et al., 2013, UN, 2015b).

Within the Latin American countries, Peru has one of the highest rates of domestic violence against women. The persistence of this problem over the years in Peru have been attributed to the widespread culture of patriarchy and the ineffective implementation of law against violence (UN, 2015a, US, 2017). As a response to tackling the problem of violence, the government in Peru responded with the Women's Emergency Centers (WEC) which are one-stop multi-disciplinary centers to assist victims of domestic violence.

Exposure to physical, emotional or sexual abuse during pregnancy and/or early childhood have also been linked to disrupt brain development and result in adverse consequences in their future life. Whilst an association has been established between domestic violence and cognitive inadequacy of children directly or indirectly exposed to such negative experiences, there is a dearth in studies exploring their causal relationship (Gustafsson et al., 2015). Interventions similar to the WECs are gaining popularity in the developing countries and the focus of such programs are usually improving the condition of women especially the ones suffering from violence. Although there has been some work evaluating the effectiveness of these programs to improve the deterrence and reporting of domestic violence, little attention is directed towards understanding how these interventions may affect children.

To mitigate the gap in this literature, this paper examines whether access to Women's Emergency Centers (WEC) impacts the health and cognitive development of the children under the age of five. Our main aim in this paper is to improve our understanding of how environmental factors that are driven by policy or family dynamics affect child development. Human capital accumulation in the early years of life has been gaining considerable attention in recent times. Children at the age of less than three years are found to be at a critical stage of forming their cognitive ability and social skills (Richter, 2004). Events during early childhood have long-term consequences for the child since they are particularly malleable during these years, or conversely extremely vulnerable to negative environmental factors and shocks. Heckman and his colleagues have been strong advocates of the importance of early childhood and policy makers are increasingly inclined towards policies during the early years. Recently, developing countries are also recognizing the importance of these early years. Policy interventions such as early childhood stimulation programs and conditional cash transfer programs are designed particularly recognizing the large gap in human capital between the rich and the poor in the developing world. Although such programs have shown promise, most of these programs are focused on a specific sub-sample of the population and are generally expensive to implement. While several papers have shown the importance of early childhood intervention, less is known about the impact of other policies on children that are designed to improve maternal conditions especially in terms of their cognitive development. This void in literature stems from the lack of data on cognitive development measures that are nationally representative and is found outside of randomized controlled trials.

The Women Emergency Centers in Peru provides a unique opportunity of testing how an intervention intended to improve the condition of victims of domestic violence, primarily women, impacts children still at the critical age of their brain development without any additional program for health or cognitive stimulation. To estimate the impact of improving access to Women Emergency Centers, we employ a difference-in-differences strategy and exploit the temporal and geographic variation in the rollout of the program. We define treated or exposed group as the children who reside within 2 km of a WEC in their province. In essence, we compare the children living closer to a WEC to the children living farther away from a WEC before and after the WEC opened. To achieve a suitable comparison group, we only include children who are living within

10 km of ever having a WEC. Furthermore, since WEC mostly operated in urban areas, we restrict our main analysis to children living in urban areas. We also only include children who have been living in their current place of residence before the opening of the WEC to take into account any compositional changes due to selective migration. Furthermore, for the main analysis we restrict our sample to 2015-2017 since we focus on understanding the impact of the WEC on cognitive development.

We find evidence that exposure to these center reduces the probability of experiencing domestic violence suggesting that the centers were effective in their mission. With strong evidence of our first stage, we next explore how these center may affect child outcomes. We find that exposure to the WEC within 2 km of the household improves the weight-for-age measures of children by 0.065SD[1]. Malnutrition indicators such as wasting and underweight reduces by about 0.0795 SD and 0.09 SD respectively. We also find that cognitive development measured in terms of their symbolic function improves by about 0.146 SD with exposure to these centers within 2 km. With our main specification we do not find an impact on other measures of cognitive development or stunting.

To support the validity and robustness of our identification assumption, we perform a host of checks. First, we perform an event study analysis for the domestic violence and health outcomes to see if the pre-WEC period estimates support our common trends assumption. We find that the coefficient estimates for the years prior to the opening of the WEC are not statistically significant. We are unable to perform a similar test for the cognitive development outcomes since we only have data for these measures from 2015-2017. We also test if our estimates using the data from 2015-2017 are robust by estimating the impact on the health and domestic violence outcomes using all available data from previous years for these measures. We find that both analysis yield similar results. Furthermore, since we restrict our sample analysis to only non-migrant household, we are able to mitigate any confounding impact of compositional changes in our estimates. We obtain similar estimates when we include the households that moved in after the opening of the WEC. The

[1]The estimates are presented in the standard deviation units of the outcome variable. SD indicates standard deviation.

estimates are also robust to our definition of the comparison group. The coefficient estimates are similar when we consider all urban households in our sample instead of 10 km maximum distance from the WEC. For our estimates on the health outcomes, we find positive and significant effect when we use an alternative identification strategy to define treatment as exposure in the district of residence. Our estimates are also robust to the inclusion of control variables. We also perform a balancing test to see how exposure to the WEC affects household characteristics and mother's pre-determined characteristics respectively. The estimated coefficients are not statistically significant suggesting that treatment induced compositional changes are unlikely to be driving our results.

The presence of the Women's Emergency Centers may affect children well-being in two ways. First, it can directly reduce the mother's probability of enduring intimate partner violence with the ease of access to the the justice system. This should improve the child's cognitive development through reduced exposure to toxic environment in the household and the increased maternal ability to form stronger parent-child relationship and a stimulating environment. Second, the presence of the WECs improves the outside option for the women which increase their threat point under a non-cooperative bargaining model (Tauchen et al., 1991, Lundberg and Pollak, 1993, Farmer and Tiefenthaler, 1997). The improvement in the outside option can be a result of the improved woman's access to seek help or from the increase in social awareness that is directly spread by these centers or through the increase in the number of women using the services of these centers. This improves the intra-household bargaining power of the women, even if they are not the direct beneficiaries of the centers, which should shift the household resources towards the children and improve their cognitive and health outcomes.

To test our hypothesis, we examine the potential mechanisms that could be driving our results. We posit that the main variable driving our results is the reduction in domestic violence. We also explore if these centers are associated with improvements in parental investments. We do not find any impact of access on postnatal investment measured in terms of household environment, health visits and vaccination uptake. However, we do find that access to WEC within 2 km improves the maternal-child attachment significantly. Domestic violence and maternal attachment could potentially explain the impact we find on child well-being. Due to data limitations, we cannot explore other mechanisms such as parental investment in terms of resource allocation within the household. Therefore we are unable to distinguish amongst the different channels responsible for our main results.

This paper contributes broadly to two strands of literature. First, it complements the literature on policies or laws that are intended to improve the well-being of women and their impact on children (Borga, 2018, Menon et al., 2014, Allendorf, 2007). While most of these policies such as divorce laws or improvement in the rights of the women in terms of ownership of wealth, there is almost no study that looks at the impact of policies designed specifically for victims of domestic violence on child outcomes. Sviatschi et al. (2018) is the only paper that looks at the impact of the WEC on women and children's educational outcomes. This paper adds to this small but growing literature by exploring the role of these centers on cognitive development and health outcomes. Second, it provides new evidence on the impact of gender-based violence on the physical health and cognitive development of children under the age of five.

The paper is organized as follows. Section 2 discusses the background of the Women's Emergency Centers (WEC) and Section 3 describes the data, sample selection and identification strategy. Section 4 presents the main results. Section 5 discuss the validity and robustness of our estimates and Section 6 talks about the potential mechanisms. Finally, the paper concludes in Section 7.

3.2 Background

Domestic violence is a serious and long-standing issue in Peru. The Law for Protection from Family Violence was adopted in 1993 to ensure effective and swift protection and access to justice to the victims of domestic violence. It was subsequently strengthened in 1997 reforming and redefining the roles and responsibilities of the justice system involved with the cases. This initiated the creation of several specialized services that were instituted in response to the needs of the victims including a system of one-stop centers for victims of domestic violence in 1999 . These centers are called the Women's emergency Centers (WEC) or "Centros de Emergencia para Mujeres" and were created by the Peruvian Ministry for Women and Vulnerable Population (MIMP) as a part of the National Program against Sexual and Family Violence.

The WECs provides free and multidisciplinary care under one roof including legal, social, psychological and medical services. It was created to help the law enforcement agencies to detect and assist victims to use the different services that are available to them. This includes everything from seeking health care to legal advice and filing a complaint. Accordingly, these centers are frequently located close to police stations, prosecutors office and health centers. Besides the services provided by these centers, volunteer women executes local campaigns and programs to raise awareness about the issues of domestic violence in that region.

The first center opened in 1999, gradually expanding throughout the years. By 2016, 245 centers operated in all over Peru and between 2017 and 2018 about 100 centers were set up inside police stations (comisaria). Majority of the centers are concentrated in the urban areas. According to Sviatschi et al (2018), locations were initially prioritized based on population density and level of infrastructure. After, 2006 the responsibility of the centers were decentralized to the local government and additional criteria such as proximity to the police and health centers were considered in choosing the location. Although priority were supposed to be given to poorer districts and districts with high rates of violence, in most cases the order of opening of the centers involved political influence.

From 2017, the centers were set up inside police stations. The functions of the centers remained the same as the centers located outside the police stations. The geographic distribution of the centers are displayed in Figure 3.1. Figure 3.2 shows the number of centers opened in each year during the period 1999-2018 and Figure 3.3 shows the number of new districts where a WEC opened for the first time by year.

3.3 Empirical Approach

3.3.1 Data

The main data set used in this paper comes from the Peruvian Demographic and Health Survey (DHS), which was collected over a period of 2000-2017[2]. These surveys are repeated cross-

[2]For this paper, we use the DHS data from 2005 to 2017.

sections representative at a national and regional[3] level.

- *Women data:* The DHS collects rich data on women aged 15-49 years and children under the age of five. For the women, this includes information on their demographic characteristics, fertility, health, educational attainment and labor market participation, partner's characteristics and socioeconomic status among others. The data also collects information on self reported domestic violence towards women from their partner. It asks whether they were exposed to some form of controlling behavior, emotional or, physical violence ever in their life by their partner and if they experienced any of these aforementioned forms of violence in the last 12 months. In this paper, we use the measure that reports domestic violence experienced in the last 12 months by mothers with alteast one child under the age of five. The questions used to construct the different types of domestic violence are presented in Table 3.21.
- *Child health measures:* For children under the age of five, the DHS contains information on their health and cognitive development. Measures related to the health outcomes include the measurement of their height, weight and as well as their birth weight. Furthermore, it also collects extensive information on their prenatal investment in terms of their antenatal care as well information on their health visits and vaccination intake. Using the anthropometric measures, we calculate the weight-for-age and height-for-age z-scores following the WHO child growth standards. We also calculate indicators of malnutrition including stunting, wasting and underweight which are the marker of poor health.[4]. The DHS data does not have the child's health measure for the year of 2006.
- *Cognitive development measures:* The DHS data started collecting data on cognitive development for children younger than 5 years of age from the year 2015. To measure cognitive growth, DHS collects data on four developmental markers. These are verbal communication, motor development, symbolic function and regulation of emotion and behavior. The type of

[3]Region or Departamentos are the second geographic administrative unit in Peru.

[4]Also following WHO;
Stunting: 1 if height for age z-score is less than -2 std dev. from the median; 0 otherwise
Wasting:1 if the Weight for Height z-score is less than -2 of a std. dev. from the median; 0 otherwise,
Underweight: 1 if Weight for age z-score is less than -2 std. dev from the median; 0 otherwise

questions related to each category and age group are listed in Table 3.18.

The response to these questions are reported by the mothers and are mostly recorded as a "Yes" or a "No". Therefore each response is recorded as a 1 if "Yes" and 0 if "No". In some cases, the response to the question may include that the child does not do this activity. For example, for one of the question on symbolic function, the mother is asked "When the child shows a scribble or drawing he has made, does he tell you what he drew?". The response may include "Yes", "No" or "Do not doodle or draw". In such case, we record the response as 1 if "Yes", 0.5 if "No" and 0 if "Do not doodle or draw".

The different categories of the cognitive development measures are age dependent. The questions measuring each category are designed for children from specific age group and different cognitive development measures are relevant for children from different age groups. The communication measures are collected for children aged 9-12 months, 15-18 months, 30-36 months and 53-59 months. It intends to measure the communication and language development of the children. Symbolic Function and behavior regulation data is collected for children aged 30-36 months and 53-59 months. Symbolic function measures the child's ability to mentally represent objects and emotion and behavior regulation measures the child's ability to identify emotions as well as measure their level of patience and temperament. Motor development measures if the child can sit up or walk and is collected for children aged 9-12 months and 15-18 months.

We take the average of the responses to the questions in each category to calculate a mean score of cognitive development of the child in that category. To construct our cognitive development outcome measure, we calculate an internally age-adjusted z-score using category and survey year specific mean and standard deviation[5]. We also construct an overall average score of cognitive development by taking the mean of the average scores of the four categories[6]. For the outcome measure of the overall cognitive development, we adopt the same

[5]This is following the construction of the z-score in Fernald et al. (2012)

[6]Notice in Table 3.18 that the categories are age dependent. Therefore an average score is a more appropriate measure than the total score.

strategy as before and calculate an age-adjusted z-score for each child across the different survey years.

These measures are reported by the mothers of the children aged under five years. For certain age groups, the survey also collects data on the maternal attachment and household environment for the child. Maternal attachment measures the relationship between the mother and the child using information on the child's responsiveness to the mother and the mother's contingent responsiveness to the child. This data is collected for mother's with children aged 9-12 months and 15-18 months. The data set also contains information on the environment around the child. It is measured using how many hours the mothers spends away from the child in a day, the total number of people the child interacts with, safe space to play and has books and toys to play with. The question asked in this category is age dependent and is collected for all age-groups.

The unique feature of this data is that it contains cognitive development measures that are nationally representative and collected annually. Usually collection of such data are limited towards a small group of individuals and are designed to test outcomes of randomized control trials. Furthermore, these RCTs are usually targeted in rural areas. Cognitive development measures in developing countries for the urban areas are rare.

- *Children location:* The DHS data also contains information on the district of current residence as well as the GPS coordinates of the cluster of residence[7]. The GPS data is only available for the years 2005-2009 and 2014-2017. The GPS locations of the DHS clusters of households are displaced before public release to preserve the confidentiality of the respondents. The GPS displacement is randomly carried out so that: urban clusters are uniformly displaced up to 2 kilometers and rural clusters are displaced up to 5 kilometers, with 1% of the rural clusters displaced up to 10 kilometers. In addition, the displacement is restricted so that the points stay within the second administrative level, which is the province. However this means the the GPS coordinates may be positioned outside the district after displacement.

[7]Districts are the smallest administrative unit of Peru. Cluster can be thought as a smaller unit within the district.

- *WEC data:* Information on the locations and founding dates of the WEC centers are provided by the Ministry of Women and Vulnerable Population (MIMP). We have the data on all the WEC that were opened from 1999 to 2018. We convert the addresses to GPS coordinates which allows us to know the exact location of the centers. Figure 3.2 contains the distribution of the number of WECs that were rolled out over the years.

3.3.2 Sample Selection

To identify the impact of the WEC on children, I use the GPS location information from the DHS to exploit the distance of the child's cluster of residence to the WEC. However, since the GPS locations in the DHS data are randomly displaced, using the continuous measure of distance introduces random measurement error in our treatment variable that can substantially affect the results (Burgert et al., 2013). (Sviatschi et al., 2018) discusses why this may not be a major problem in this context. Perez-Heydrich et al. (2016) suggest that the amount of measurement error depends on the spatial density of the resource facilities. As the density of the resource facilities decreases, the probability that a DHS cluster is linked to the correct closest WEC center increases for all types of locations (urban and rural). A total of 345 centers opened all over Peru causing the spacial density of the centers to be low[8]. This should largely reduce the issue of measurement error. However, it is still suggested that treatment should be defined within a buffer distance instead of using the actual distance to the facility. As we will discuss in the next section, we define our treated group as the children living in clusters that are within 2 km of a WEC in their province. Therefore we define our buffer distance as 2 km.

To further circumvent the potential bias in our estimates we employ certain sample restrictions. The WECs are mostly located in densely populated areas near police stations and hospital which means that they are mostly located in urban areas. Also, in the DHS data, the magnitude of the displacement of the urban areas are much less than the rural areas. Therefore, restricting the sample to urban areas should also reduce the measurement error. For our analysis in this paper, we restrict our sample to only urban areas.

[8]Peru had about 1874 districts by 2017.

For my preferred regression analysis, we employ two other restrictions in my sample to strengthen our the support for our identification assumption. To create a valid comparison group for the treatment group, we restrict our analysis to children living in clusters that are almost 10 km of having a WEC. The introduction of these centers may also induce selective migration. To avoid capturing the impact of compositional changes in our estimated effects, we only consider the non-migrant children in our analysis. Non-migrants are children who have been living in their cluster of residence since before the introduction of the WEC within 10 km[9].

Finally, since the main focus of this paper is to evaluate the impact of the WEC on the cognitive development outcomes, for my main analysis I only use data from 2015-2017. We will perform validation exercises using data available from all years (2005, 2007-2009 and 2014-2017) to show the robustness of our estimates and provide evidence for parallel trends. It must be noted that the health measures are collected for a larger sample of children that the cognitive measures. This is due to the fact that the cognitive development measures are designed for children within specific age groups. This suggests not all children under five qualify for the cognitive development module.

3.3.3 Identification Strategy

To identify the effects of the WEC on child health and cognitive development, we employ a difference-in-difference design exploiting the variation created by the differential timing in the opening of the WECs as well as the spatial variation in the exposure to the centers within a province. Our primary approach uses the individuals in household clusters that are farther from the WEC as a comparison group for the individuals residing in clusters that are closer to the centers. In our main specification, we define our treated group as individuals living in clusters that are within 2 km of the WEC. The identification assumption is that changes in children health and cognitive development in our treated group would have evolved similarly to the comparison group in the absence of the WEC.

[9] About 75% of the selected sample were non-migrants.

Formally, we estimate the following regression equation,

$$Y_{idt} = \beta_0 + \beta_1 WEC_{pt} + \alpha_{dc} + \alpha_{pt} + X_{idt} + \varepsilon_{idt} \tag{3.1}$$

where, Y_{idt} is the outcome of interest for child i, in district d, at time t. The outcomes variables are cognitive development and health measures of children aged 0-5 years. WEC_{pt} is the treatment variable which takes a value of one if the child is residing in a cluster that is located within 2 km of the WEC in the province p at time t and 0 otherwise. α_{pt} is the province by year fixed effect that should account for any differential trends in the outcome associated with the WEC placement. This should take care of any concerns that our results are driven by changes that differs by province and year. α_{dc} is a district by cluster fixed effect that controls for any time-invariant unobserved characteristics at the district-cluster level. We define cluster c as a dummy variable that takes a value of 1 if the household is located within 2 km of ever getting a WEC within the province of residence and 0 otherwise[10]. We control for the unobserved time-invariant characteristics of the district by the proximity to the household cluster to the WEC. Since we define our treatment area are household clusters located within 2 km of the WEC, we use the 2 km proximity to define our cluster. X_{idt} consists of child, mother and household specific time-varying controls. These controls include child's age fixed effect, gender of child, first born child dummy, mother's age, married dummy, years of education, age at first birth, female headed household dummy, household size, number of living children, number of children under five, wealth index and if the mother is ethnically indigenous. Standards errors are clustered at the district level.

As discussed in the previous section, we restrict our sample to the urban and non-migrant households that are located within **10 kms** of the WEC. This is done to ensure that we have a more comparable control group to our treated households[11].

The coefficient of interest β_1 measures the average change in the outcome for children living

[10]Note that "ever getting a WEC" suggests that a WEC exists at the time of the survey within 2 km of the DHS cluster or will open within 2 km by 2018.

[11]Since the type of household that are located closer to the WECs may be different from the ones that a located really far away, we only include household that will at some point get a WEC that is located within 10 kms of the WEC.

within 2 km of the WEC compared to the outcome for children living further away from the current or future WEC. We assume that in the absence of these centers, the outcome measures for the children living within 2km and living within 2-10 km would evolve similarly.

3.4 Main Results

3.4.1 Estimated Effects on Domestic Violence

The Women's Emergency Centers are tasked with aiding victims of domestic violence. The intervention does not involve any nutritional or cognitive initiative. Therefore, the WEC is unlikely to directly affect the health and cognitive development of the children under the age of five. We assume that the WECs can impact children in two way. First, it can directly reduce domestic violence in the household which may improve the cognitive capacity of the children. Second, the presence of the WEC can be associated with improvement in the bargaining power of women which can also explain the increase in the health and cognitive outcomes.

We begin by estimating the impact of the intervention on intimate partner violence toward women. Figure 3.4 represents the estimated impact on three different types of self-reported domestic violence in the last 12 months: controlling behavior, emotional violence and physical violence. We only analyze the sample of mother's with atleast one child under the age of five and estimate equation (1) without the child controls for our analysis. We find that exposure to WEC within 2 km has a negative and statistically significant effects on all measures of domestic violence. This provides strong evidence for our first stage and suggests that the centers have been effective in fulfilling its purpose of deterrence to gender based violence.

3.4.2 Estimated Effects on Cognitive Development

To estimate the impact of WEC on cognitive development, we use four categories of measures which includes communication, motor skills, symbolic function and regulation of emotion and behavior. We also have an overall cognitive development score that takes into account all fours categories.

Figure 3.5 provides estimates of the impact of the WEC on the cognitive health measures for the children. We see that the symbolic function of children improves by about 0.146 SD[12] with access to WEC within 2 km and its is statistically significant at the 5% level. However, we do not see any effect of exposure to the WEC centers on the overall or the other three measures of cognitive development. The estimates effects are small in magnitude and non statistically significant.

3.4.3 Estimated Effects on Child Health

We define child health using anthropometric measures and indicators of malnutrition. The anthropometric measures include height-for-age and weight-for-age z-scores[13] which are used to assess the general growth status of children. The indicators for malnutrition include stunting, wasting and underweight. These measures, especially stunting and wasting, indicates sub-optimal health and nutritional condition where stunting has been associated with lower cognition and educational performance and wasting have been associated with increased risk of mortality.

Figure 3.6 represents the impact of exposure to WEC within 2km on the health outcomes of children under the age of five as specified in equation (1). The coefficients are presented in terms of the standard deviations of the outcome variable. We find statistically significant and positive impact of exposure to WEC on the weight-for-age z-score by 0.065 SD. We also find the exposure to WEC reduces the probability of wasting and underweight by about 0.0795 SD and 0.091 SD respectively and are statistically significant at the 5% level. We do not find any impact on height-for-age or stunting.

The results indicate that the improvement in the malnutrition measures is coming from the improvement in the weight-for-age of the children.

3.4.4 Heterogeneous Effects

In this section we explore if proximity to the WEC affect the children health and cognitive development differently by different subgroups of the population.

First, we explore how the WEC affects the health and cognitive development by the gender of the child. In Figure 3.10 Panel (a) we find that although the impact on symbolic function is positive

[12]SD indicates that the effect size is present in terms of the standard deviations of the outcome variables
[13]These z-scores as calculated using the WHO reference population.

for both male and female, it is not statistically significant. We do not find any effect of WEC on the other measures of cognitive development by the gender of the child as well. Figure 3.10 Panel (b) indicates some variation in the impact of different health measure by the gender of the child. We find some evidence that the impact on the height and weight for age z-scores are positive and significant for male children whereas we find suggestive evidence that the malnutrition indicators of wasting and underweight goes down for female children.

We also explore heterogeneity by the birth order of the child. The availability of nutrient to children are largely determined by the intrahousehold allocation of resources. These resource allocations may be affected by the birth order of the children as the literature suggests that there may be an association between parental preference and birth order (Behrman, 1988, Ejrnæs and Pörtner, 2004, Jayachandran and Pande, 2017). We examine if the effect of WEC on the children outcomes vary by the birth order of the child. In Figure 3.10 Panel (c) we see that the impact on symbolic function is larger and statistically significant for first born children. Again, similar to gender, we do not see any differential impact on the other measures of cognitive development by the birth order. For the health outcomes in Panel (d), we find mixed results. Wasting is more likely to go down for the first born children whereas probability of underweight goes down by the children born after the first child. However, for the other health measures, we do not find any statistically significant effect.

We also analyze the impact of exposure to WEC by the age group of the mothers. Figure 3.10 Panel (e) shows that the cognitive development in terms of symbolic function improves for the children with mothers ages less than 30 years. In terms of the health outcomes, Panel (f) shows that the weight increases for children with mother's less than 30 years old, however, the wasting and underweight measures improves for children with older mothers.

We also explore the heterogeneity in maternal-child attachment in Figure 3.11. We find that attachment z-score improves for girls and children with higher birth order. It is also driven by children with mother's under the age of 30 years.

3.5 Validity and Robustness

In this section, we present a set of sensitivity tests to provide additional support for our identification assumption. Our main approach which is a difference-in-differences design that relies on the assumption that the changes in the outcome variable in the treatment and control areas should not be systematically different in the absence of the WEC. Specifically, this would suggests that the introduction of the centers is the only factor driving our estimated effect on the child health outcomes.

One of the main limitations of the cognitive development data is that it is available only for the years 2015 to 2017. Therefore, we are unable to exploit the variations in the rollout of the WEC in the prior years and are also unable to test for parallel trends. To test for the presence of the parallel trend assumption we exploit the fact that data on weight, height and domestic violence is available for more additional years. Data on child health and domestic violence were collected from 2005, 2007-2009, 2014-2017[14] with the GPS data. Figure 3.13 compares the estimated effect on the health outcomes and the domestic violence outcomes. We see similar effects for both cases, however, the estimated effect using the data from the later years are slightly larger in magnitude. We should keep this in mind when we interpret the result for the cognitive development measures.

The main threat to our empirical strategy is the possibility that differential trends in population movements or changes in the demographics within the treatment and the control clusters may confound our estimates. We present several pieces of evidence from our analysis to suggests this is unlikely to be the case. First, in Figure 3.9 we present the event-study graphs showing the difference-in-differences estimates of the effects on different measures of child health overtime, with the periods of time prior to the opening of the WEC within 2 km[15]. In each panel, we present the estimated effects from the event study analysis in Section 3.8.

Overall, the results indicate that the health measures for children in the treated and control

[14]Although the DHS data is collected annually every year since 2005, the GPS data is only available for the years mentions.

[15]We only perform the event study analysis for the child health outcomes since the cognitive development data is only available for the years 2015 to 2017.

areas followed a similar trajectory before the WEC starts to operate within a 2 km distance. The magnitude of the estimates are small and statistically non-significant, especially for our height-for-age and weight-for-age z-scores. We also perform an event study analysis for our domestic violence measures in Figure 3.8. Similar to the health outcomes, we find that the pre-period estimates for the domestic violence measures are not statistically significant and small in magnitude. This provides support for our common trends assumption.

Our estimates so far are based on a single measure of treatment which provides us with a weighted average of all possible two group two period treatment effect (Goodman-Bacon, 2018). To see a more disaggregated effect of the WEC over time, we explore if there is heterogeneity in the magnitude of the impact by the length of exposure to the center. The event-study graphs also allow us to explore the dynamic impact of the program on health outcomes after the introduction of the WEC over the years. In Figure 3.9, we see that both height-for-age and weight-for-age z-score exhibit an increasing pattern in their estimated impact over time. We find that the health outcomes in terms of height and weight improves one year after the opening of the center. Wasting and underweight seems to go down in the short-run after the center opens as well. However, for wasting we find a negative impact in the longer period as well. We also explore the dynamic impact on the domestic violence in Figure 3.8. We find that controlling behavior and emotional violence goes down gradually over the years and a negative and statistically significant impact is observed if a center has been open for longer than 4 years. For physical violence, we find that it goes down one year after the center opens and also when the center has been open for more than 4 years.

Additionally, our main analysis also accounts for selective migration as we only consider the non-migrant population which are the individuals who were living in their current place of residence before the beginning of the program. Ideally, we would also like to test if pre-program changes in demographics or the outcomes variable are associated with the timing of the future introduction of the program. Since we only have data with the GPS location of the household clusters from 2005, we are unable to perform such a test for our treatment and control clusters. For the same reason, we are unable to perform a test with placebo treatment using the data before the year 2005.

We also perform a balancing test to explore if the introduction of these centers affect our covariates which includes household and the mother's characteristics. Figure 3.12 shows the impact of the centers within 2 km on the probability that the mother is from an indigenous background, the total number of household members, the age of the household head, the probability of living in a female headed household and the wealth index. It also show the estimated impact on mother's fertility, educational and labor market outcomes and partner's characteristics. As it can be seen in the Figure, all the estimated effects are statistically non-significant, providing additional evidence that treatment induced compositional changes in the households is unlikely to be responsible for our treatment effect. As we do not see an impact on the total number of children born to a mother, it excludes the concern about selective fertility.

For our main analysis we define treatment as the opening of a center within 2 km of the cluster of residence. We assume that the treated group are individuals who have access to a center that is within a distance of around 2 km. To show how the treatment effect varies with change in the maximum distance to define our treatment areas, we perform the main regression analysis from equation (1), however, we change the definition of our main independent variable WEC_{pt} to a dummy variable that takes a value of 1 if there is a WEC center within x km within the province p at time t where x can take any integer value between 1 and 9. The definitions for all other variables and the sample selection remains the same as in the main specification except for the definition of c which now indicates a cluster that takes a value of 1 if they will ever be exposed to a WEC within x km of the cluster and 0 otherwise. Figure 3.14 and 3.15 presents the results for this analysis for the cognitive development child health outcomes. The x-axis in the figure represents the maximum distance x as explained previously. Different color within a graph shows the coefficient estimates for different regression analysis based on their definition of the treated areas. For the cognitive development analysis, we find a positive and significant effect on symbolic function for people living closer to the center. We also see a positive impact on communication score when the treated area is defined within 3 km. For weight-for-age, we see a positive effect of exposure to WEC within a larger treatment area around 6km. For wasting and underweight, we find effects for children living closer to the center.

We also analyze if an alternative specification, that exploits the district level variation in the rollout of the program, affects the estimated impact on the health and cognitive development outcomes. We define treatment as exposure to the WEC within the district in the urban clusters and we only consider the non-migrant population in our analysis[16]. Table 3.4 shows a positive and statistically significant effect of a WEC in the district on child's weight-for-age measures. We do not find any statistically significant impact on the height-for-age z-score but we find a reduction in the probability of wasting and underweight with exposure to a center in the district. We do not find any statistically significant impact on the cognitive development outcomes with exposure to a WEC within the district. Furthermore, exposure at the district level decreases the probability of controlling behavior. Although we see a negative effect on physical violence, the effect is not statistically significant. As we can see, the estimated effects are smaller when the exposure to WEC is measured at the district level which indicates a larger catchment area. This indicates that the effect are stronger for individuals living closer the the WEC.

In our main specification, we restrict our sample to only non-migrant individuals in the urban clusters that are within 10 km of the WEC within the province. Here, we explore if our estimates are robust to the sample selection for our analysis. In Table 3.5, 3.6 and 3.7, we see that the estimates are robust if we select all urban areas instead of urban areas within 10 and both urban and rural areas. We also show that our estimate are robust to an even tighter comparison group where we only include individuals living within 5 km instead of 10 km, thus making the comparison group more identical to the treatment group.

We also explore how the control variables affect our coefficient estimates. In Tables 3.8, 3.9 and 3.10, we estimate the regression equation (1) with no control variables, with limited control variables[17] and with all the control variables as in our main analysis. The estimates are robust to

[16]Since we are not using the GPS data, we use all the data from 2005 to 2017 that contains the health measures for the child. We do not use data from 2006, since anthropometric measures for 2006 were not collected

[17]Limited controls include mother's age, ethnicity,gender of the child and the age of child

the inclusion of the control variables.

Finally, I estimate if exposure to the WEC within 2km when the child was in utero affects outcomes that are associated with the birth of the child. This estimation strategy allows us to exploit the birth year of the child as well as the rollout of the center providing us with additional variation to identify effect. In Table 3.1 we find that exposure to these centers while in utero increases the birth weight and decreases the probability of low birth weight for the children, however, the effects are not statistically significant. We find evidence that the probability of very low birth weight goes down and is statistically significant at the 5% level. We also see whether exposure in utero affect the prenatal or postnatal investment in children measured by the prenatal and postnatal index measures in the Table. Prenatal index is calculated using the number of prenatal visits, wantedness of the pregnancy measured with a dummy variable and iron intake during pregnancy. Using factor analysis, I create the index. For postnatal index, I use postnatal health visits, total vaccines, if the mother breastfed the child upto the age of 2 years and questions that measures the household environment in terms of safe play area, books and toys in the household and interaction with multiple people. Although the estimated effect on both the prenatal and postnatal index is positive, they are not statistically significant.

3.6 Mechanisms

3.6.1 Impact on Intermediate Channel

We found evidence that exposure to the WEC was associated with reduction in domestic violence. Although domestic violence can directly impact child outcomes, it may also affect them indirectly.

We explore how exposure to these centers are associated with investments towards the children under the age of five in our sample and behavior toward children in the household. To assess investment towards children under five, we have four measures- maternal attachment, household environment, number of growth check up visits in the health center and the total number of vaccines. Each measure is constructed as an internally age-adjusted z-score. Figure 3.7 Panel (a) represents the estimated impacts of exposure to the WEC centers on the investment outcomes using equation (1). We find a positive and statistically significant impact on maternal attachment. Maternal attachment measures the mother-child bonding by measuring the child's responsiveness to the mother and the mother's responsiveness to the child. This is consistent with the theory that reduction in domestic violence can improve the parent-child relationship. We do not find any evidence of a change in the other investment measures towards children under five as a result of the WECs. We also estimate how exposure to these centers affect mother's and other household members behavior towards children in the household in general. We use three measures for this analysis- mother's use of physical punishments to discipline their children, other members of the household using physical punishment to discipline children and if the children's feeding practice meets the Minimum Dietary Diversity[18]. Each measure is a binary variable which takes a value of one if the mother use physical disciplining, others use physical disciplining and if the Minimum Dietary Diversity is met respectively and 0 otherwise[19]. Figure 3.7 Panel (b) displays the estimated impact of WEC on the behavior towards children. We do not find any statistically significant effect on these measures.

We do not have detailed information on intrahousehold resource allocation to assess parental investment in more depth. While the variables we explore may capture some of those investment behavior, there may be other channels that we cannot observe due to data limitations. However, they are likely to be a consequence of the reduction in domestic violence.

3.7 Discussion and Conclusion

In this paper, we analyze the impact of introduction to WEC center within 2 km of the household cluster on the health outcomes of children under the age of five. We exploit the temporal and geographic variation in the opening of the WECs and use a difference-in difference approach to estimate the impact. To ensure that our identification strategy hold, we define our control group as

[18]Minimum Dietary Diversity is met if child receives atleast 4 out of 7 food groups. In the DHS, this question is referred to the last living child.

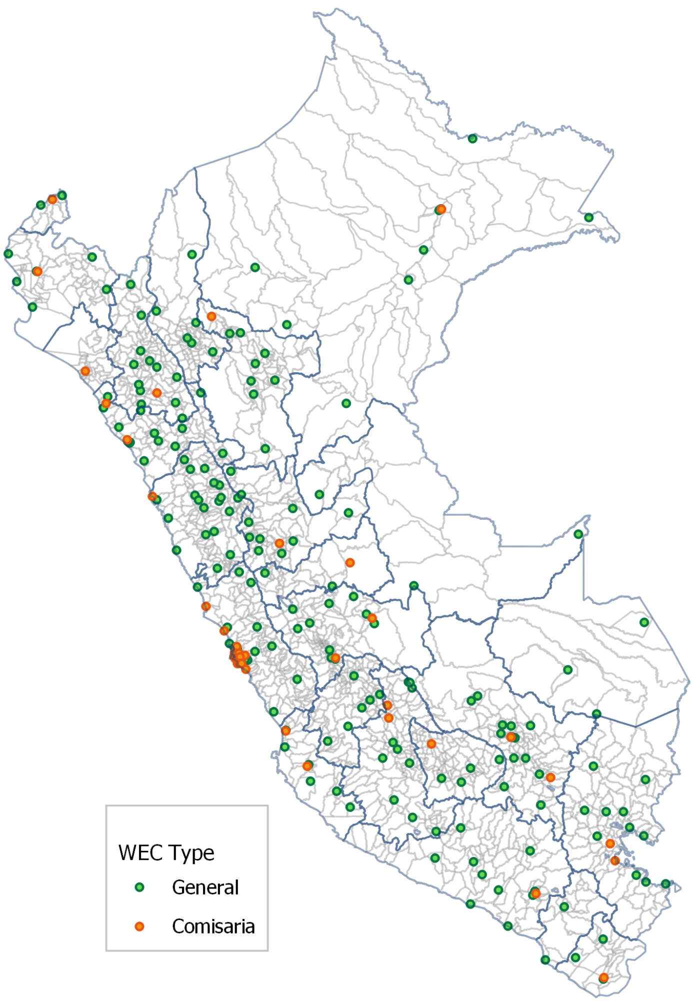
[19]The effects on Figure 3.7 Panel (b) is obtained by estimating equation (1) using the data on mother's with atleast one child under the age of five whose health measures were recorded in the DHS and without any time-varying controls.

the individuals living in clusters that are within 10 km of an already existing or future WEC.

We find evidence that living within 2 km of a WEC improves the health outcomes of children under the age of five primarily in terms of their general health measured with weight-for-age z-scores by 0.065 SD and the decrease in the probability of suffering from undernutrition as measure by wasting (0.079 SD) and underweight (0.09 SD). Furthermore, we find that access to these centers increases the cognitive development of the children in terms of their symbolic function by about 0.14 SD.

The Women Emergency Center (WEC) were initiated as a part of the law for the victims of domestic violence. This paper shows that these centers were indeed successful in reducing the incidents of domestic violence for mothers with very young children. We hypothesize that the centers role in reducing domestic violence is the main driver in improving the well-being of the children living close the centers. We also find that access to these centers improves maternal-child attachment. As extensive research in developmental psychology and neurobiology show, maternal-child attachment in the first years of life is an important determinant of child's development (Aizer and Cunha, 2012).

Figure 3.1: Geographic Distribution of WEC



Note: The map displays the distribution of the WECs across Peru from the year 1999 to mid- 2019. The green dots indicate the (General) WECs that were built between 1999-2016. The orange dots indicates the (Comisaria) WECs that were build inside police stations from 2017 onwards. There are sometimes overlaps between a general WEC and a WEC in Comisaria in the same district. The blue solid lines indicate the region borders and the gray lines represents the district borders.

Figure 3.2: Number of WEC Rollout by Year

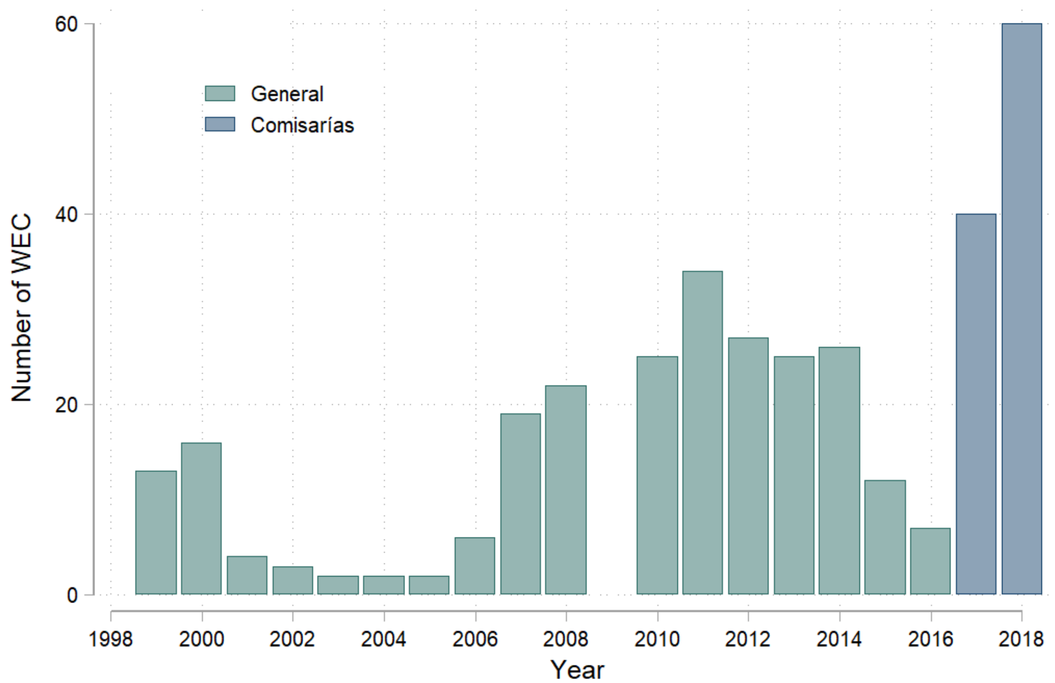


Figure 3.3: Number of New Districts with First WEC by Year

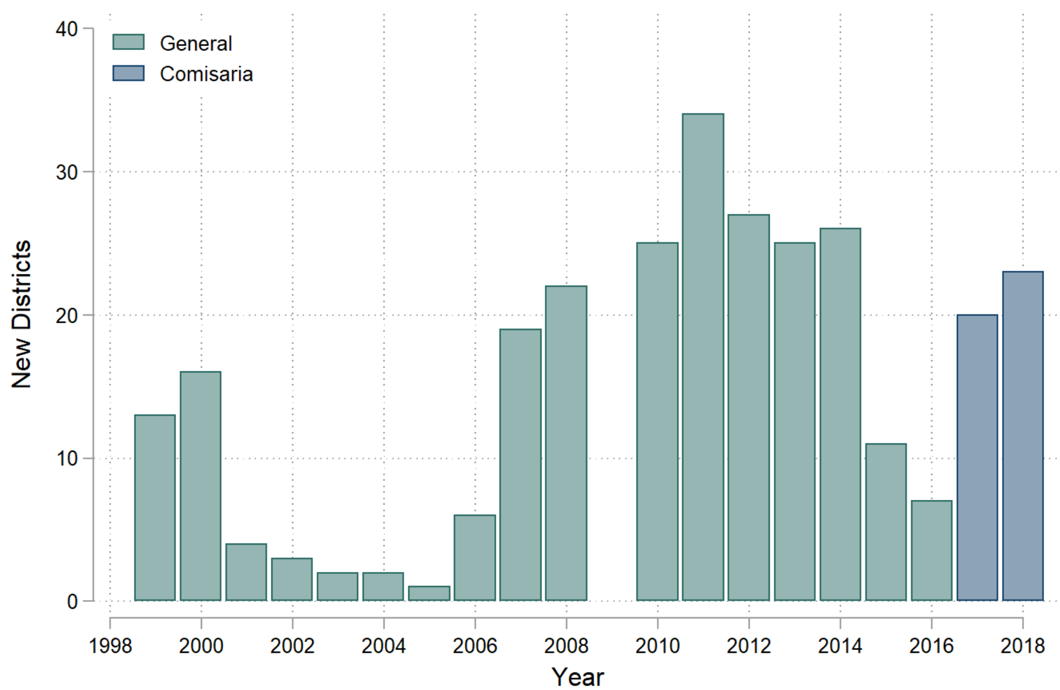
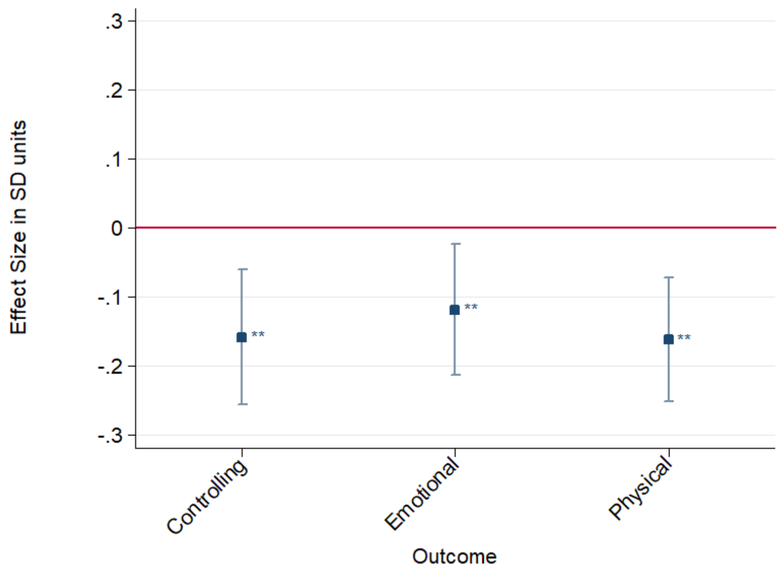


Figure 3.4: Estimated Effects on Intermediate Outcomes (Domestic Violence) Within 2 km Distance of the WEC



Note: The graphs shows the impact of exposure to WEC within 5km on domestic violence and the behavior of mother's with atleast one child under the age of five. The independent variable takes a value of one if there was a WEC within 2km of the cluster of the household at the time of the survey. Definitions of the domestic violence measures can be found in Figure 3.21. The controls include household size, mother's age at 1st birth, mother's years of education, total living children, total children under 5, household head female dummy, married dummy, indigenous dummy and wealth index. The standard errors are clustered at the district level. The coefficients are presented in terms of the standard deviations of the outcome variable. Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.5: Estimated Effects on Cognitive Development Within 2 km Distance of the WEC

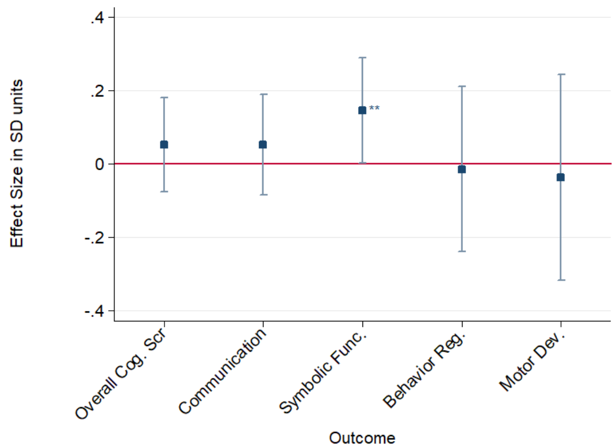
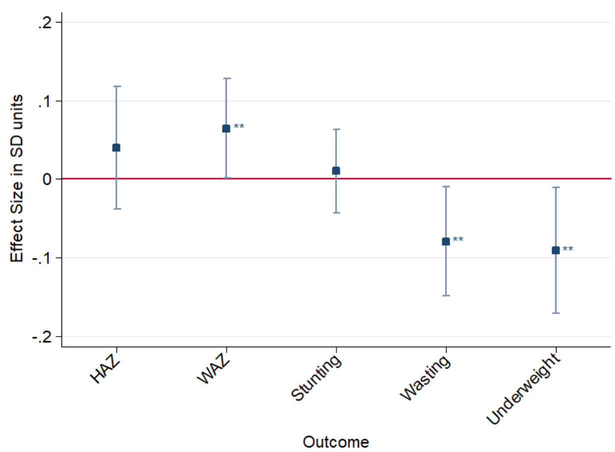


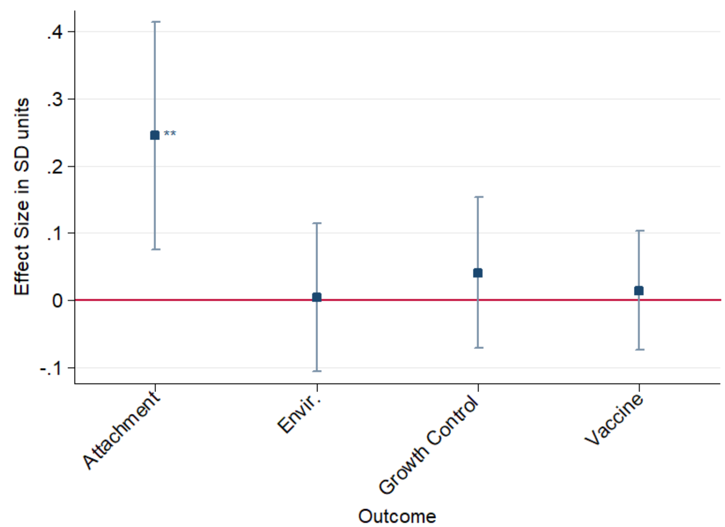
Figure 3.6: Estimated Effects on Child Health Within 2 km Distance of the WEC



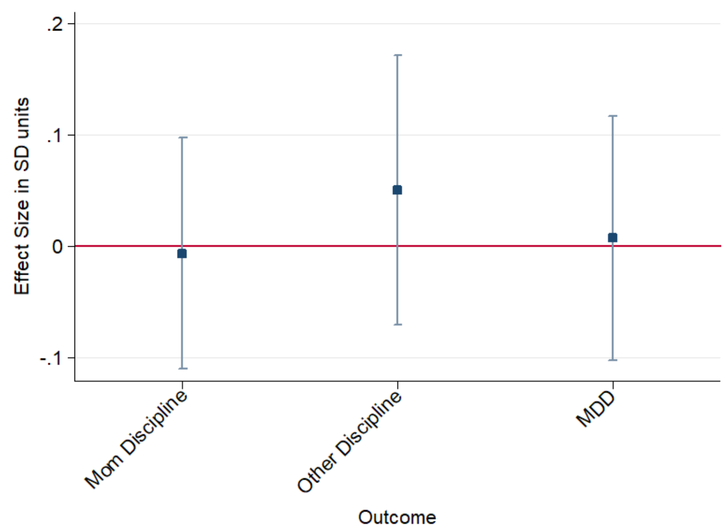
Note: The graphs shows the impact of exposure to WEC within 2km on cognitive development and health measures. The independent variable takes a value of one if there was a WEC within 2km of the cluster of the household at the time of the survey. Overall Cog. Scr is the age-adjusted z-score of the average cognitive score across the four categories in the graph. Based on the age of the child, they overall average score could use either two or three of the categories to calculate the score. Communication indicates age-adjusted z-score that measures the child's ability to comprehend and verbalize. Symbolic Func. indicates the age-adjusted z-score for symbolic function to measure symbolic role play and drawing ability. Behavior Regulation indicates the age-adjusted z-score of behavior and emotional regulation which measures their patience and emotions.Motor Dev. indicates the age-adjusted z-score to measure fine motor skills such as sitting and walking. HAZ measures the height for age z-score, WAZ is the weight for age z-score, Stunting is 1 if height for age z-score is less than -2 std dev. from the median 0 otherwise, Wasting is 1 if the weight for height z-score is less than -2 of a std. dev. from the median 0 otherwise, Underweight is 1 if weight for age z-score is less than -2 std. dev from the median 0 otherwise. The controls include gender of child, firth born dummy, child's age in years, household size, mother's age at 1st birth, mother's years of education, total living children, total children under 5, household head female dummy, married dummy, indigenous dummy and wealth index. The standard errors are clustered at the district level. The coefficients are presented in terms of the standard deviations of the outcome variable. Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.7: Estimated Effects on Intermediate Outcomes with Exposure to WEC Within 2 km

(a) Investment Towards the Child



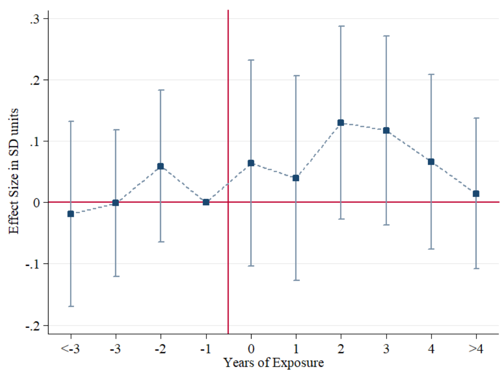
(b) Behavior Towards all Children



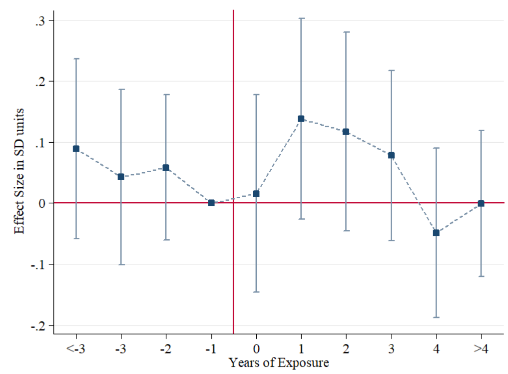
Note: The graphs shows the impact of exposure to WEC within 2km on investment and behavior measures toward children. The independent variable takes a value of one if there was a WEC within 2km of the cluster of the household at the time of the survey.
Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.8: Event Study: Estimated Effects on Domestic Violence for Years of Exposure to the WEC Within 2 km

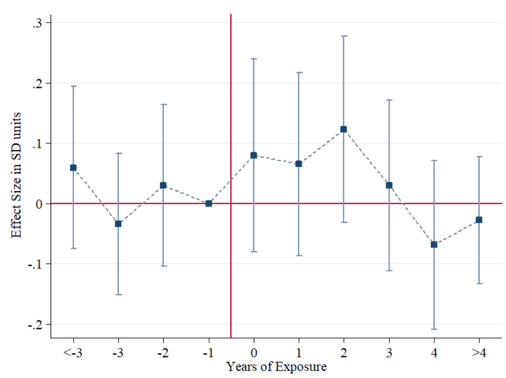
(a) Controlling Behavior



(b) Emotional Violence



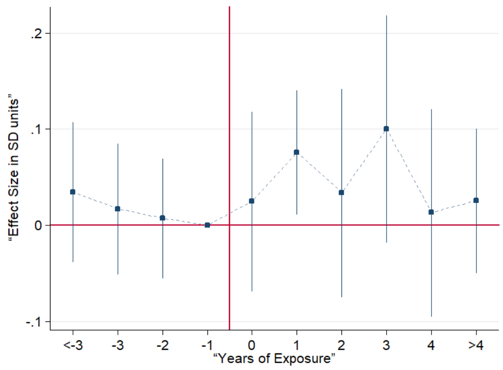
(c) Physical Violence



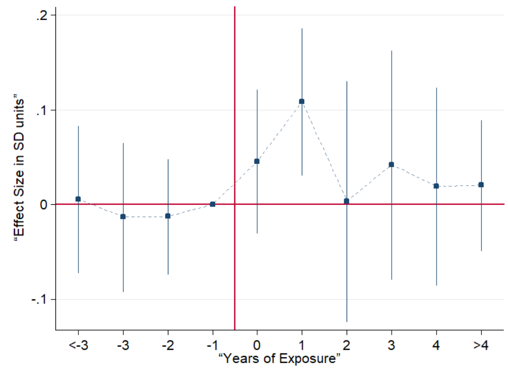
Note: The graphs estimates the length (years) of WEC exposure within 2km from equation (2) where the x-axis value of "i" represents the number of years for which the respondent was exposed to the WEC. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: DHS 2005,2007-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.9: Event Study: Estimated Effects on Anthropometric Measure for Years of Exposure to the WEC Within 2 km

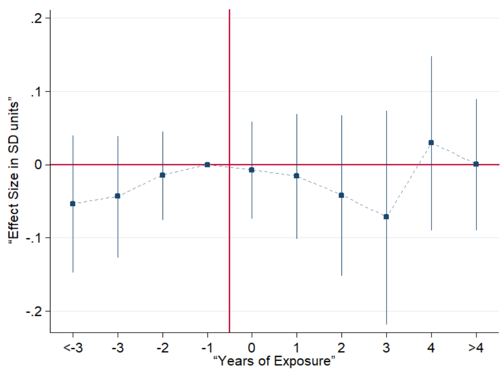
(a) Height-for-age Z-score



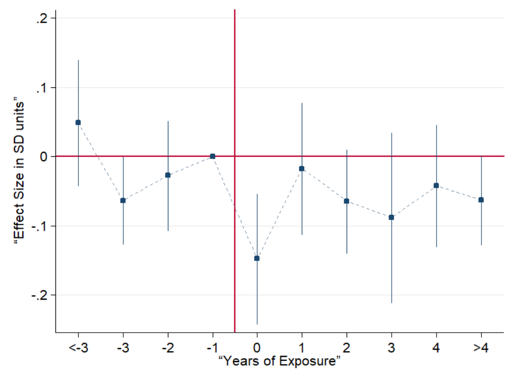
(b) Weight-for-age Z-score



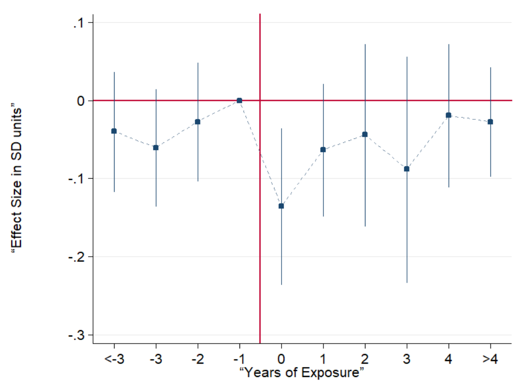
(c) Stunting



(d) Wasting



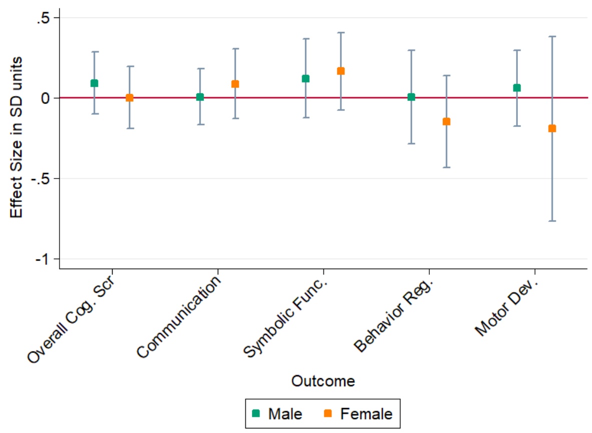
(e) Underweight



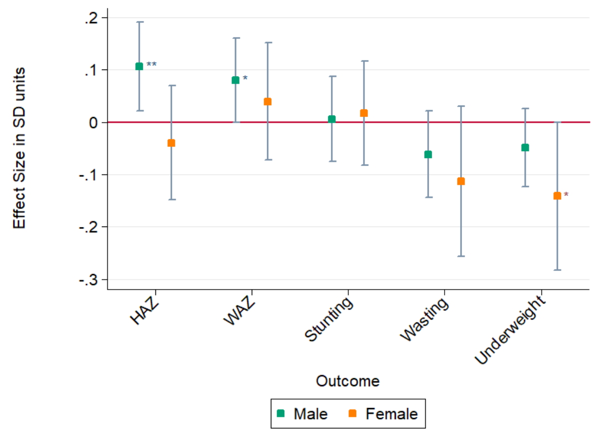
Note: The graphs estimates the length (years) of WEC exposure within 2km from equation (2) where the x-axis value of "i" represents the number of years for which the respondent was exposed to the WEC. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: DHS 2005,2007-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.10: Heterogeneity: Estimated Effects on Cognitive and Anthropometric Measure of Exposure to the WEC Within 2 km by Child's Gender, Birth Order and Mother's Age

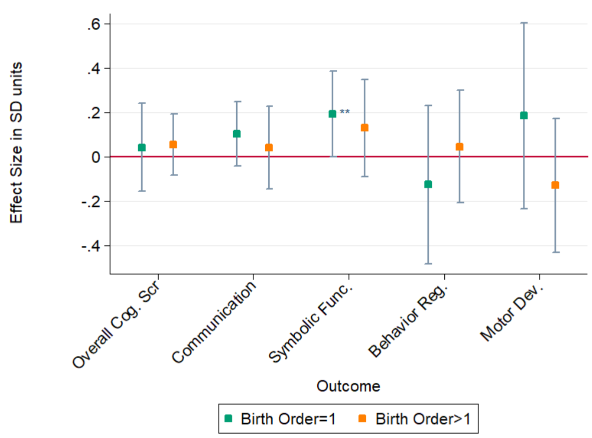
(a) Cognitive Development (By Gender)



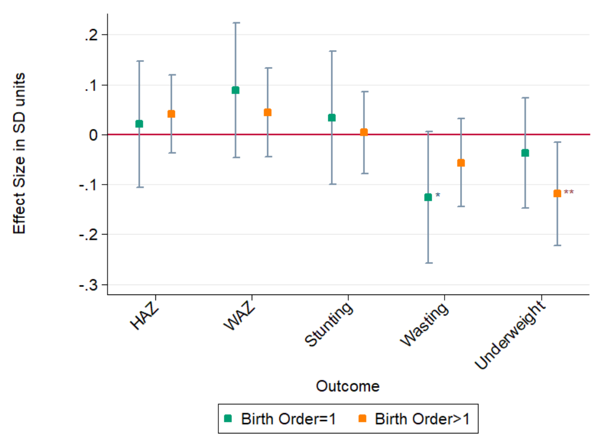
(b) Child Health (By Gender)



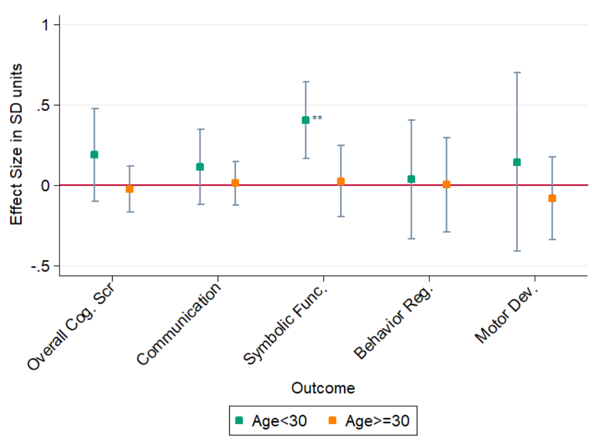
(c) Cognitive Development (By Birth Order)



(d) Child Health (By Birth Order)



(e) Cognitive Development (By Mother's Age)



(f) Child Health (By Mother's Age)

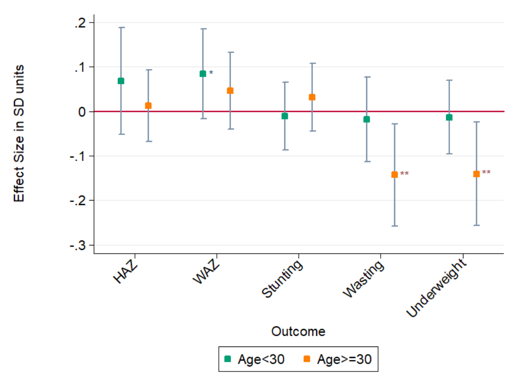
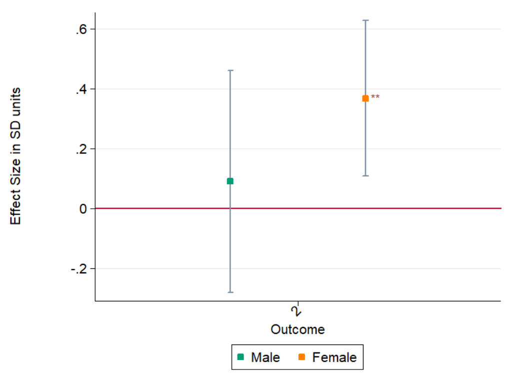


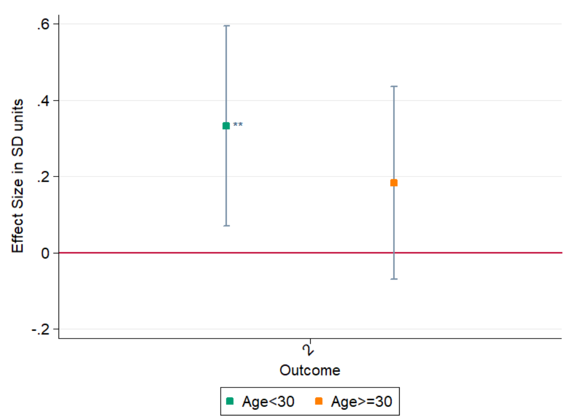
Figure 3.11: Heterogeneity: Estimated Effects on Maternal Attachment with Exposure to the WEC Within 2 km by Child's Gender, Birth Order and Mother's Age

(a) By Gender



(b) By Birth Order

(c) By Mother's Age



Note: The coefficients are presented in terms of the standard deviations of the outcome variable. The regression was estimated using equation (1) but with no control variables. Source: DHS 2015-2017. $*p < 0.10$, $** \ p < 0.05$

Table 3.1: Impact of In-Utero Exposure to WEC Within 2km on Child Birth Related Outcomes

	(1) Birth Weight	(2) LBW	(3) VLBW	(4) Prenatal Index	(5) Postnatal Index
In Utero Exp within 2km	0.001000	-0.0113	-0.0475**	0.0433	0.0141
	(0.0280)	(0.0259)	(0.0226)	(0.0320)	(0.0640)
Observations	37320	37320	37320	33328	10255
Mean	3.301	0.0632	0.00506	0.0966	-0.0796
StdDev	0.549	0.243	0.0710	0.507	0.652
Districts	349	349	349	349	303
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes

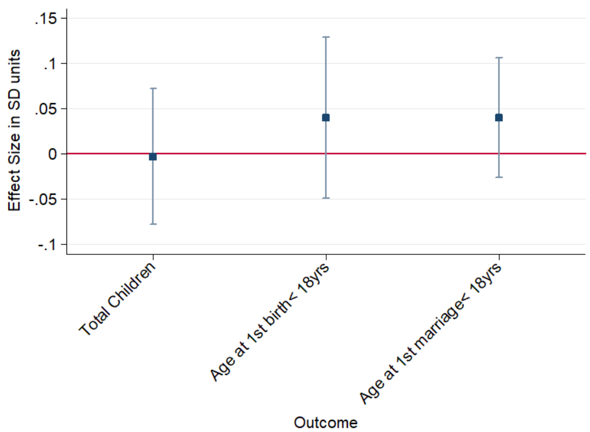
Standard errors in parentheses

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

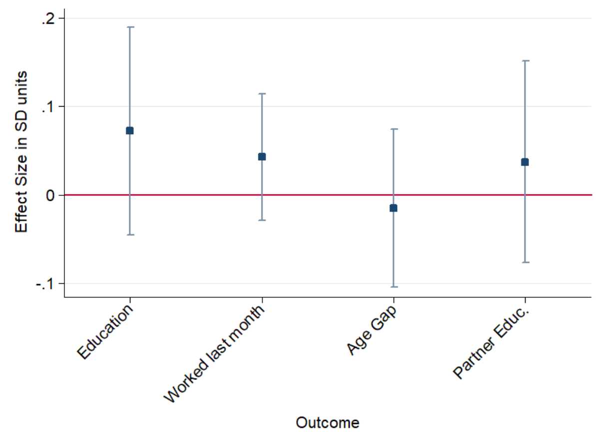
Note: The tables shows the impact of exposure to WEC within 2km in utero on birth measures. The controls include gender of child, firth born dummy, child's age in years. The sample includes all urban areas within 10 km and non-migrant population. The standard errors are clustered at the district level. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: Demographic and Health Survey 2005, 2007-2009, 2014-2017.

Figure 3.12: Balancedness of Covariates: Estimated Effects on Intermediate Outcomes (Household and Mother Characteristics) Within 2 km Distance of the WEC

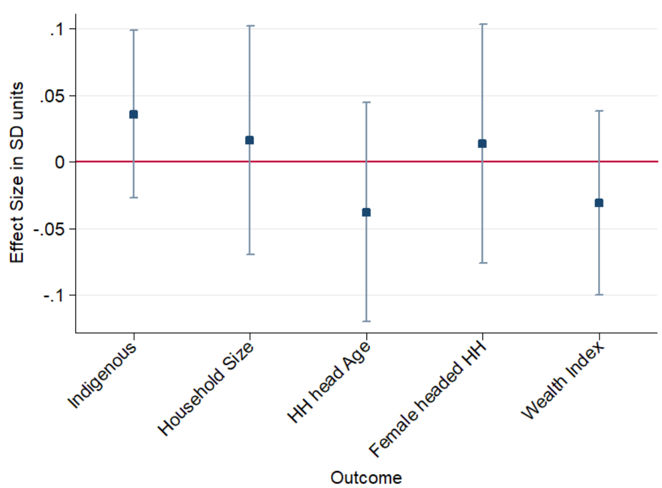
(a) Fertility



(b) Mothers Characteristics

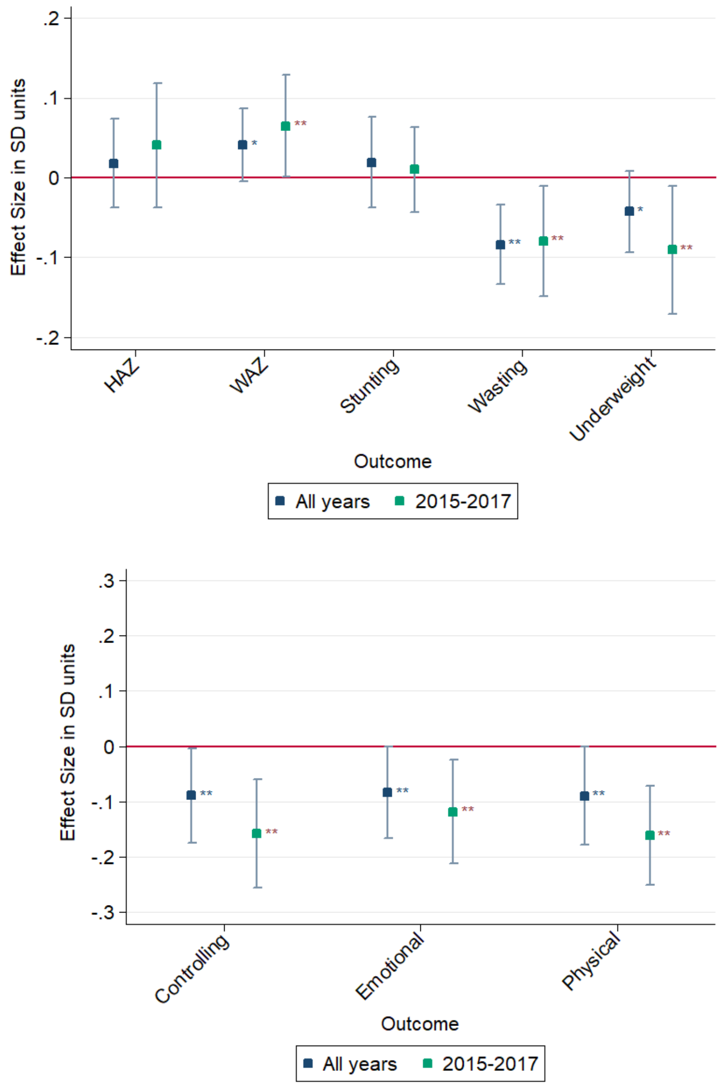


(c) Household Characteristics



Note: The coefficients are presented in terms of the standard deviations of the outcome variable. The regression was estimated using equation (1) but with no control variables. Source: DHS 2015-2017. *$p < 0.10$, ** $p < 0.05$

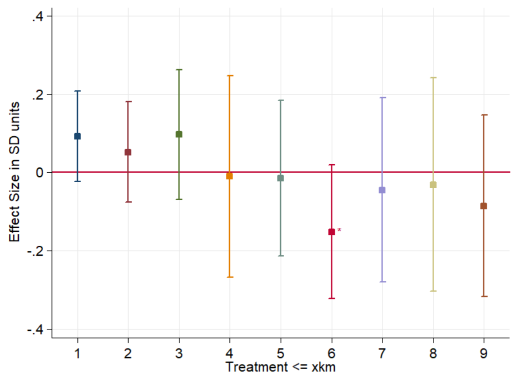
Figure 3.13: Robustness: Estimated Effect on Child Health Within 2 km Distance of the WEC Restricted to Years 2015-2017



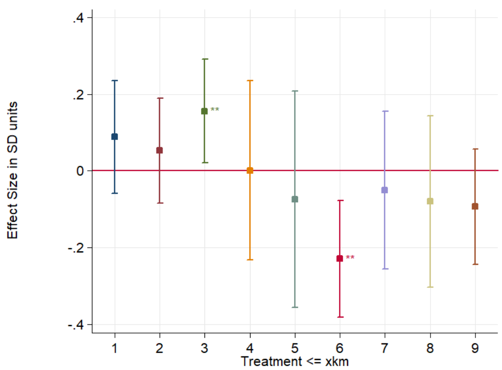
Note: The coefficients are presented in terms of the standard deviations of the outcome variable. Source: Demographic and Health Survey. $*p < 0.10$, $**\ p < 0.05$

Figure 3.14: Alternative Treatment Definition: Estimated Effects on Cognitive Development with Exposure to WEC Within "x" km

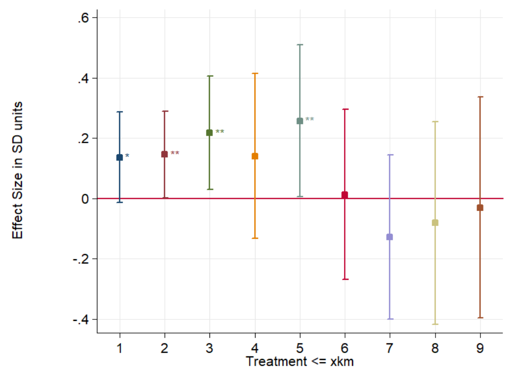
(a) Overall Cognitive Score



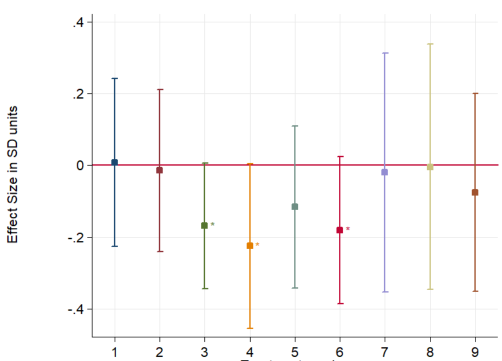
(b) Communication



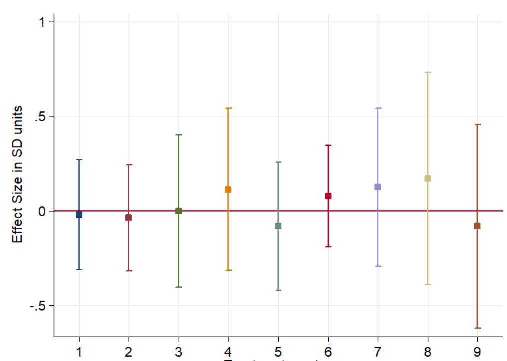
(c) Symbolic Function



(d) Behavior Regulation



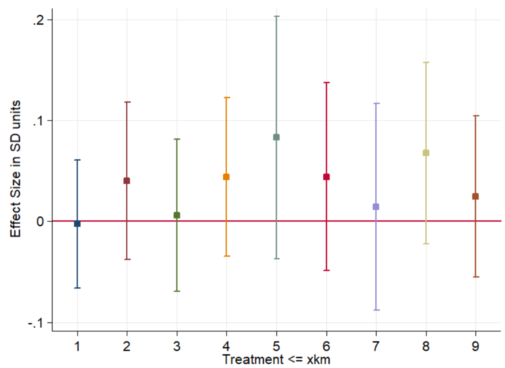
(e) Motor Development



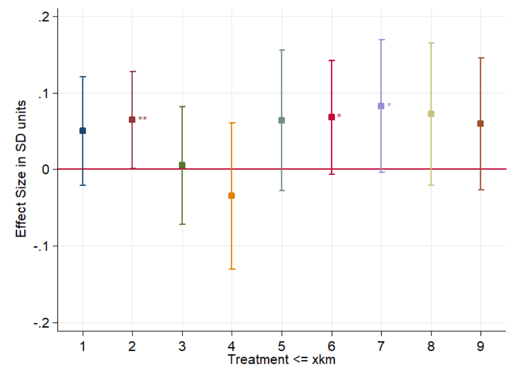
Note:The coefficients are presented in terms of the standard deviations of the outcome variable. Each dot represent a separate regression were we estimate equation(1) by changing the treatment area to "x" as indicated by the x-axis. Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.15: Alternative Treatment Definition: Estimated Effects on Anthropometric Measure with Exposure to WEC Within "x" km

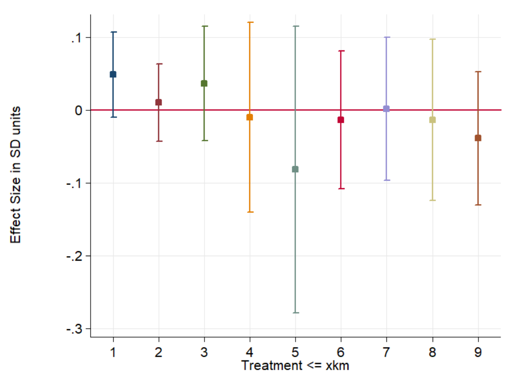
(a) Height-for-age Z-score



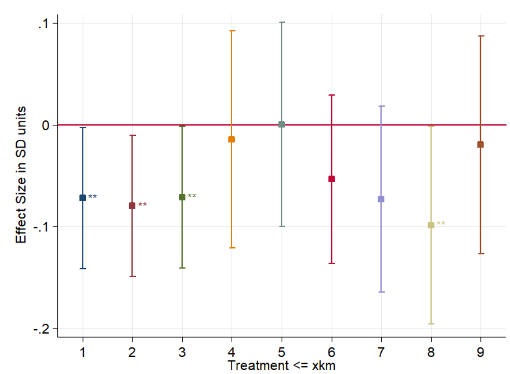
(b) Weight-for-age Z-score



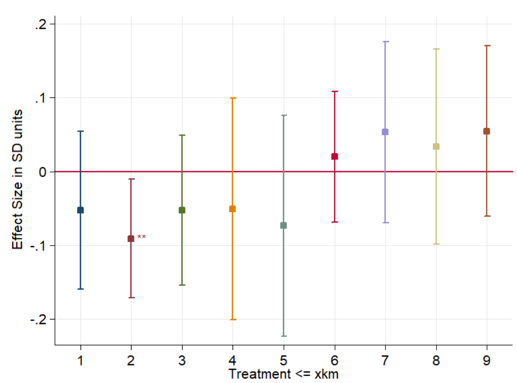
(c) Stunting



(d) Wasting



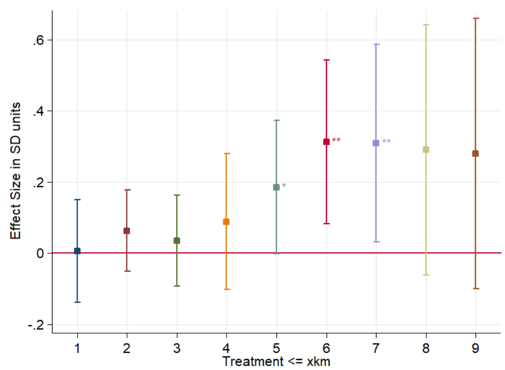
(e) Underweight



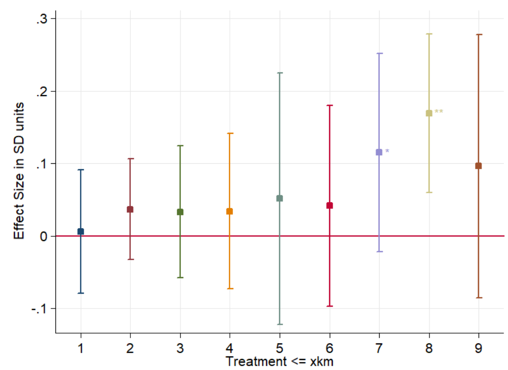
Note:The coefficients are presented in terms of the standard deviations of the outcome variable. Each dot represent a separate regression were we estimate equation(1) by changing the treatment area to "x" as indicated by the x-axis. Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.16: Alternative Treatment Definition: Estimated Effects on Intermediate Measure with Exposure to WEC Within "x" km

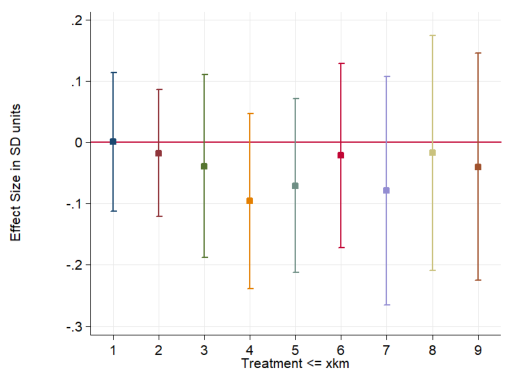
(a) Education



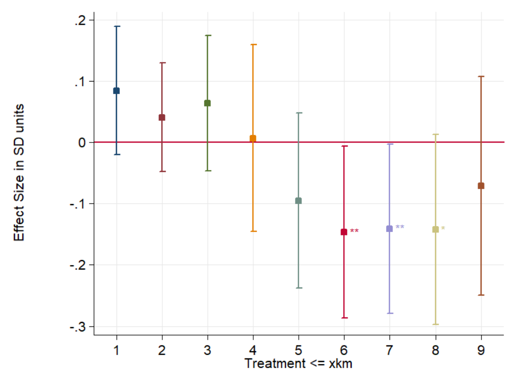
(b) Prob(Worked in the Last 12 Months)



(c) Prob(Physical Discipline (Mom))



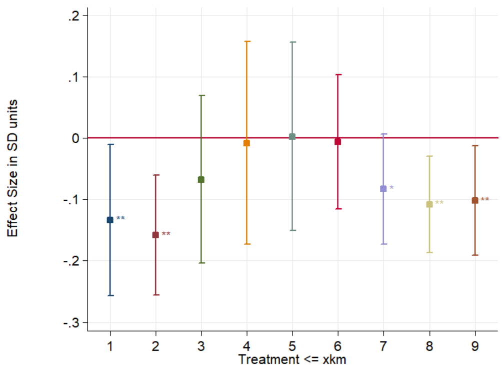
(d) Prob(Age at First Birth < 18 Years)



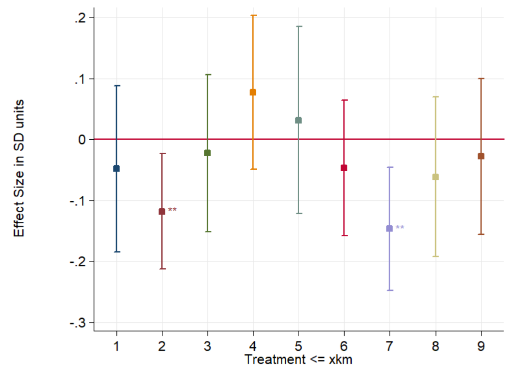
Note:The coefficients are presented in terms of the standard deviations of the outcome variable. Each dot represent a separate regression were we estimate equation(1) by changing the treatment area to "x" as indicated by the x-axis. Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Figure 3.17: Alternative Treatment Definition: Estimated Effects on Domestic Violence with Exposure to WEC Within "x" km

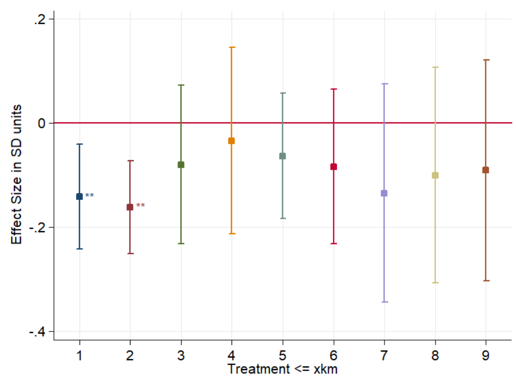
(a) Controlling Behavior



(b) Emotional Violence



(c) Physical Violence



Note:The coefficients are presented in terms of the standard deviations of the outcome variable. Each dot represent a separate regression were we estimate equation(1) by changing the treatment area to "x" as indicated by the x-axis.
Source: DHS 2015-2017.*$p < 0.10$, ** $p < 0.05$

Table 3.2: Impact of WEC in District on Domestic Violence

	(1) Control	(2) Emotional	(3) Physical
WEC in district	-0.0498*	0.0289	-0.0127
	(0.0295)	(0.0384)	(0.0313)
Observations	46272	46271	46273
Mean	0.623	0.172	0.149
StdDev	0.485	0.378	0.356
Districts	600	600	600
Province by Year FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC in the district on domestic violence measures. The controls include household size, mother's age at 1st birth, mother's years of education, total living children, total children under 5, household head female dummy, married dummy, indigenous dummy and wealth index.We also use province by year and district level fixed effect. The sample includes all urban areas and non-migrant population. The standard errors are clustered at the district level. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: Demographic and Health Survey 2005,2007-2017.

Table 3.3: Impact of WEC in District on Child Cognitive Development

	(1) Overall Cog. Scr	(2) Communi-cation	(3) Symbolic Func.	(4) Behavior Regulation	(5) Motor Dev.
WEC in district	-0.109	-0.00245	-0.143	-0.179	0.0138
	(0.0883)	(0.0851)	(0.114)	(0.124)	(0.128)
Observations	12449	12449	7747	7747	4529
Mean	0.0284	0.0352	-0.0144	-0.0154	0.0717
StdDev	0.952	0.957	0.985	0.991	0.897
Districts	469	469	443	443	387
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC in the district on cognitive measures. The controls include gender of child, firth born dummy, child's age in years, household size, mother's age at 1st birth, mother's years of education, total living children, total children under 5, household head female dummy, married dummy, indigenous dummy and wealth index. We also use province by year and district level fixed effect. The sample includes all urban areas and non-migrant population. The standard errors are clustered at the district level. The coefficients are presented in terms of the standard deviations of the outcome variable.

Source: Demographic and Health Survey 2015-2017.

Table 3.4: Impact of WEC in the District on Child Health

	(1) HAZ	(2) WAZ	(3) Stunting	(4) Wasting	(5) Underweight
WEC in district	0.0320	0.0599**	-0.000827	-0.0534**	-0.0585***
	(0.0245)	(0.0277)	(0.0249)	(0.0270)	(0.0216)
Observations	67422	67422	67422	67422	67422
Mean	-0.763	0.00371	0.116	0.00639	0.0260
StdDev	1.078	1.104	0.320	0.0797	0.159
Districts	601	601	601	601	601
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC in the district on health measures. The controls include gender of child, firth born dummy, child's age in years, household size, mother's age at 1st birth, mother's years of education, total living children, total children under 5, household head female dummy, married dummy, indigenous dummy and wealth index. We also use province by year and district level fixed effect. The sample includes all urban areas and non-migrant population. The standard errors are clustered at the district level. The coefficients are presented in terms of the standard deviations of the outcome variable.

Source: Demographic and Health Survey 2005, 2007-2017.

Table 3.5: Impact of WEC Within 2 km on Mother's Domestic Violence by Different Sample Selection

	(1) Control	(2) Emotional	(3) Physical
Panel A: Restricted to Urban Areas that are within 5 km			
WEC within 2 km	-0.155***	-0.136***	-0.164***
	(0.0543)	(0.0506)	(0.0469)
	19691	19689	19691
Panel B: Restricted to Urban Areas that are within 10 km			
WEC within 2 km	-0.158***	-0.118**	-0.162***
	(0.0497)	(0.0480)	(0.0454)
	19885	19883	19885
Panel C: Restricted to All Urban Areas			
WEC within 2 km	-0.164***	-0.123***	-0.169***
	(0.0488)	(0.0469)	(0.0445)
	24091	24089	24091
Panel D: All Urban and Rural Areas			
WEC within 2 km	-0.162***	-0.118**	-0.165***
	(0.0484)	(0.0483)	(0.0445)
	37378	37377	37379
Province by Year FE	Yes	Yes	Yes
District×Clus FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC within 2km on domestic violence measures. Each panel represents the maximum distance used to define the control group. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: Demographic and Health Survey 2015-2017.

Table 3.6: Impact of WEC Within 2 km on Child Cognitive Development by Different Sample Selection

	(1) Overall Cog. Scr	(2) Communi-cation	(3) Symbolic Func.	(4) Behavior Regulation	(5) Motor Dev.
Panel A: Restricted to Urban Areas that are within 5 km					
WEC within 2 km	0.0461	0.0541	0.142*	-0.0373	-0.0389
	(0.0650)	(0.0678)	(0.0805)	(0.124)	(0.145)
	8882	8882	5530	5530	3218
Panel B: Restricted to Urban Areas that are within 10 km					
WEC within 2 km	0.0521	0.0526	0.146**	-0.0144	-0.0369
	(0.0651)	(0.0698)	(0.0728)	(0.115)	(0.143)
	10343	10343	6450	6450	3749
Panel C: Restricted to All Urban Areas					
WEC within 2 km	0.0677	0.0672	0.147**	0.00884	-0.0498
	(0.0640)	(0.0694)	(0.0727)	(0.111)	(0.133)
	12448	12448	7744	7744	4515
Panel D: All Urban and Rural Areas					
WEC within 2 km	0.0657	0.0843	0.0822	0.0182	-0.0406
	(0.0609)	(0.0649)	(0.0791)	(0.105)	(0.117)
	19323	19322	12047	12048	7061
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District×Clus FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC within 2km on cognitive measures. Each panel represents the maximum distance used to define the control group. The coefficients are presented in terms of the standard deviations of the outcome variable.

Source: Demographic and Health Survey 2015-2017.

Table 3.7: Impact of WEC Within 2 km on Child Health by Different Sample Selectionn

	(1) HAZ	(2) WAZ	(3) Stunting	(4) Wasting	(5) Underweight
Panel A: Restricted to Urban Areas that are within 5km					
WEC within 2 km	0.0470	0.0652*	-0.00176	-0.0886**	-0.0936**
	(0.0396)	(0.0332)	(0.0290)	(0.0389)	(0.0424)
	24843	24843	24843	24843	24843
Panel B: Restricted to Urban Areas that are within 10 km					
WEC within 2 km	0.0404	0.0646**	0.0102	-0.0795**	-0.0908**
	(0.0397)	(0.0323)	(0.0271)	(0.0353)	(0.0409)
	28679	28679	28679	28679	28679
Panel C: Restricted to All Urban Areas					
WEC within 2 km	0.0431	0.0592*	0.00672	-0.0754**	-0.0815**
	(0.0388)	(0.0320)	(0.0253)	(0.0339)	(0.0375)
	34464	34464	34464	34464	34464
Panel D: All Urban and Rural Areas					
WEC within 2 km	0.0386	0.0513*	0.00472	-0.0952**	-0.0681**
	(0.0367)	(0.0304)	(0.0200)	(0.0405)	(0.0302)
	53221	53221	53221	53221	53221
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District×Clus FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC within 2km on health measures. Each panel represents the maximum distance used to define the control group. The coefficients are presented in terms of the standard deviations of the outcome variable.

Source: Demographic and Health Survey 2015-2017.

Table 3.8: Impact of WEC Within 2 km on Domestic Violence With and Without Controls

	(1) Control	(2) Emotional	(3) Physical
Panel A: With no controls			
WEC within 2km	-0.160***	-0.118**	-0.161***
	(0.0508)	(0.0527)	(0.0457)
Panel B: With limited controls			
WEC within 2km	-0.167***	-0.122**	-0.166***
	(0.0511)	(0.0518)	(0.0463)
Panel C: With all controls			
WEC within 2km	-0.158***	-0.118**	-0.162***
	(0.0497)	(0.0480)	(0.0454)
Province by Year FE	Yes	Yes	Yes
District×Clus FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Standard errors in parentheses

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

Note: The tables shows the impact of exposure to WEC within 2km on domestic violence measures with different control variables. "No control" suggest we do not include any control variable, "limited control" include mother's age and ethnicity and "all controls" includes all the controls used in our main analysis. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: Demographic and Health Survey 2015-2017.

Table 3.9: Impact of WEC Within 2 km on Child Cognitive Development With and Without Controls

	(1) Overall Cog. Scr	(2) Communi-cation	(3) Symbolic Func.	(4) Behavior Regulation	(5) Motor Dev.
Panel A: With no controls					
WEC within 2km	0.0585	0.0585	0.149*	-0.0173	-0.0392
	(0.0662)	(0.0698)	(0.0795)	(0.115)	(0.142)
Panel B: With limited controls					
WEC within 2km	0.0601	0.0588	0.159**	-0.00955	-0.0343
	(0.0650)	(0.0682)	(0.0790)	(0.117)	(0.143)
Panel C: With all controls					
WEC within 2km	0.0521	0.0526	0.146**	-0.0144	-0.0369
	(0.0651)	(0.0698)	(0.0728)	(0.115)	(0.143)
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District×Clus FE	Yes	Yes	Yes	Yes	Yes

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

Note: The tables shows the impact of exposure to WEC within 2km on cognitive development measures with different control variables. "No control" suggest we do not include any control variable, "limited control" include mother's age, ethnicity, child's age, gender and birth order and "all controls" includes all the controls used in our main analysis. The coefficients are presented in terms of the standard deviations of the outcome variable.

Source: Demographic and Health Survey 2015-2017.

Table 3.10: Impact of WEC Within 2 km on Child Health With and Without Controls

	Anthropometric		Malnutrition Indicators		
	(1)	(2)	(3)	(4)	(5)
	HAZ	WAZ	Stunting	Wasting	Underweight
Panel A: With no controls					
WEC within 2km	0.0350	0.0616*	0.0140	-0.0793**	-0.0889**
	(0.0453)	(0.0346)	(0.0280)	(0.0350)	(0.0381)
Panel B: With limited controls					
WEC within 2km	0.0412	0.0651**	0.00971	-0.0799**	-0.0910**
	(0.0401)	(0.0325)	(0.0282)	(0.0351)	(0.0388)
Panel C: With all controls					
WEC within 2km	0.0404	0.0646**	0.0102	-0.0795**	-0.0908**
	(0.0397)	(0.0323)	(0.0271)	(0.0353)	(0.0409)
Province by Year FE	Yes	Yes	Yes	Yes	Yes
District×Clus FE	Yes	Yes	Yes	Yes	Yes

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

Note: The tables shows the impact of exposure to WEC within 2km on child health measures with different control variables. "No control" suggest we do not include any control variable, "limited control" include mother's age, ethnicity, child's age, gender and birth order and "all controls" includes all the controls used in our main analysis. The coefficients are presented in terms of the standard deviations of the outcome variable.
Source: Demographic and Health Survey 2015-2017.

3.8 Event Study

To measure the dynamic impact of the WECs, we perform an event study analysis to estimates the effect of the WEC overtime.We explore the leads and lags in the introduction of the WECs using the following regression equation,

$$Y_{idt} = \beta_0 + \beta_k \sum_{k=-4}^{5} WEC_p I(\tau_t = k) + \alpha_{pt} + \alpha_d c + X_{idt} + \varepsilon_{idt} \tag{3.2}$$

where, τ_t is the event year[20] such that $\tau_t = 0$ indicates the year on which the WEC was introduced. β_k capture the impact of k years of exposure to WEC within 2km of the residence cluster where $k \in [-4, 5]$[21]. All coefficients are measured relative to $\tau = -1$ which is the year prior to the opening of the WEC. For each outcome, we expect that coefficients on dummies for years -4, -3 and -2 (the years prior to the WECs opening) to be close to zero and not statistically significant to satisfy the parallel trends assumption of our identification strategy.

α_{pt} is the province by year fixed effect that should account for any differential trends in the outcome associated with the WEC placement. α_{dc} is a district by cluster fixed effect that controls for any time-invariant unobserved characteristics at the district-cluster level. We define cluster c as a dummy variable that takes a value of 1 if the household is located within 2 km of ever getting a WEC within the province of residence and 0 otherwise[22]. X_{idt} consists of child, mother and household specific time-varying controls. These controls include child's age fixed effect, gender of child, first born child dummy, mother's age, married dummy, years of education, age at first birth, female headed household dummy, household size, number of living children, number of children under five, wealth index and if the mother is ethnically indigenous. Standards errors are clustered at the district level.

[20] $\tau_t = Survey\, year - WEC\, year$

[21] $\tau = -4$ indicates for all years equal to or greater than -4 years and $\tau = 5$ indicates all years of exposure greater than or equal to 5 years.

[22] Note that "ever getting a WEC" suggests that a WEC exists at the time of the survey within 2 km of the DHS cluster or will open within 5 km by 2018.

Figure 3.18: Cognitive Development Category Description

Category	9-12 months	15-18 months	30-36 months	53-59 months
Effective Verbal Communication	1. Semantic level: Understand your name 2. Phonological level: Verbalize syllables	1. Semantic level: Includes simple indications 2. Phonological level: Verbalize name of objects	1. Semantic level: Includes "large" and "small" 2. Phonological level: Verbalize sentences with subject and action	1. Semantic level: Understand the use of "because" 2. Phonological level: Use the word "mine" in phrases
Symbolic Function			1. Game: Use of the object for another purpose 2. Game: Play sequence of actions of daily life 3. Drawing: Chance Realism	1. Game: Role representation 2. Game: Symbolic use of the body 3. Drawing: Representation of the human figure
Regulation of Emotions and Behavior			1. Identification of sadness (or joy) in others 2. Waiting capacity 3. Ability to wait without attacking or attacking	1. Identification of annoyance (or fear) in others 2. Waiting capacity 3. Ability to wait without attacking or attacking
Motor Development	1. Can sit by themselves	1. Can walk by themselves		

3.9 Validating the Cognitive Development Measures

In this section we explore the correlation of our cognitive development measures with measures of child and maternal health and characteristics. In order to gain more confidence in our measure of cognitive development, we examine if the other measures that we know to be correlated with cognitive development move in our expected direction.

The child's health is measure using his birth weight, if the child is anemic, height for age z-score and weight for age z-score. Mother's health is measure using her BMI. Child's characteristics include their gender, birth order and the total living children of the mother which is labelled as total sibling in the table. Mother characteristics include mother's age at the birth of the child, years of education, marital status, ethnicity, and if the mother currently smokes. Household characteristics include rurality and wealth level (if the household in the the lowest two quintile). We also include two other measures: Postnatal index and Prenatal index which is a proxy for parental investment and prenatal investment respectively.

To create our postnatal index we use the following variables:

- Age-adjusted total vaccines z-score: We calculate the total number of all vaccines that were asked about and calculate the mean and standard deviation by the survey year and age in months to get the internally age-adjusted total vaccines. We do this because at a given age, only certain number of vaccines can be taken.

- Age-adjusted total growth check-up: We calculate the total number of health visit of the child for growth check-up and calculate the mean and standard deviation by the survey year and age in months to get the internally age-adjusted total growth visits. We do this as you get older, the ideal number of visits you should go for increases.

- Breastfeeding: We assign 1 if the child was breastfed upto the age 2 years old (which is recommended by WHO along with other supplemental food) and 0 otherwise. For kids older than 2 years, the duration of breastfeeding need to have been atleast 24 months. For kids younger that 2, the mother should be still breastfeeding or the duration should be equal to their age in months.

- Household environment z-score: This includes questions about the environment of the household based on age of the child. It contains questions such as, how many hours did the mother stay away from the child on average in the last two weeks, how many people the child interact with, if the child's play area is safe and clean, if their are books at home for the child to read, materials at home that they can use to play, and does the child play with other children of their age. We take the mean of the responses to these questions and calculate the internally survey year and age in months adjusted z-scores.

Some other variables that were related to postnatal care but were included is if the child meets the minimum dietary diversity score and the attachment[23] score of the mother. We did not include this since these questions are only available for the children less than 2 years old which severely reduces our sample size and provides noisy estimates. We perform factor analysis on these four variables and create an index based on the scoring coefficients. The factor loadings are shown in Figure 3.19.

[23]It includes questions such as if the child is responsive to the mother, if the mother is responsive to the child when they are under distress and if the mother understands why the child is under distress.

Figure 3.19: Factor Analysis Output for the Postnatal Index

```
Factor analysis/correlation                        Number of obs    =     23435
    Method: principal factors                      Retained factors =         2
    Rotation: (unrotated)                          Number of params =         6
```

Factor	Eigenvalue	Difference	Proportion	Cumulative
Factor1	0.71844	0.71433	1.5354	1.5354
Factor2	0.00411	0.00705	0.0088	1.5442
Factor3	-0.00295	0.24874	-0.0063	1.5379
Factor4	-0.25168	.	-0.5379	1.0000

```
LR test: independent vs. saturated:  chi2(6)  = 6236.34 Prob>chi2 = 0.0000
```

Factor loadings (pattern matrix) and unique variances

Variable	Factor1	Factor2	Uniqueness
zvac_number	0.5948	-0.0083	0.6462
zgrowth_co~r	0.5953	0.0011	0.6456
breastfed2	0.0510	0.0624	0.9935
zdhs_envir	0.0875	0.0122	0.9922

Scoring coefficients (method = regression)

Variable	Factor1	Factor2
zvac_number	0.40019	-0.01085
zgrowth_co~r	0.40086	0.00260
breastfed2	0.02655	0.06223
zdhs_envir	0.04550	0.01234

To create the prenatal index, I used the following variables,

- Number of prenatal visits.: Total number of prenatal visits during pregnancy.
- Wantedness of pregnancy: It is equal to one if the mother wanted to get pregnant when she was pregnant with the child. It is equal to zero if the mother wanted a child later or no more child.
- Iron intake: It is equal to one if the mother got or took iron pill or syrup during pregnancy.

We should mention that there are missing observations for prenatal visits and iron intake that drop our our observations by about 3000 children with cognitive development responses. These missing values seems to be equally missing across survey years and gender.

We perform factor analysis like before to create the prenatal index. The factor analysis output in in Figure 3.20.

Figure 3.20: Factor Analysis Output for the Prenatal Index

```
Factor analysis/correlation                        Number of obs    =    108299
    Method: principal factors                      Retained factors =         1
    Rotation: (unrotated)                          Number of params =         3
```

Factor	Eigenvalue	Difference	Proportion	Cumulative
Factor1	0.38580	0.38919	2.2224	2.2224
Factor2	-0.00339	0.20542	-0.0195	2.2029
Factor3	-0.20881	.	-1.2029	1.0000

LR test: independent vs. saturated: chi2(3) = 9841.82 Prob>chi2 = 0.0000

Factor loadings (pattern matrix) and unique variances

Variable	Factor1	Uniqueness
antenatal_~s	0.4368	0.8092
wanted	0.2291	0.9475
preg_iron	0.3776	0.8574

Scoring coefficients (method = regression)

Variable	Factor1
antenatal_~s	0.33909
wanted	0.16501
preg_iron	0.28409

We use the variable above to run a regression with our different measures of cognitive development as our dependent variable. Other than the variables mentioned above, we include age in months fixed effects, year and district fixed effect. We cluster the error at the district level.

Table 3.11: Determinants of Cognitive Development (2015-2017)-Urban Only

	(1) Overall Cog. Scr	(2) Communi- cation	(3) Symboli Func.	(4) Behavior Regulation	(5) Motor Dev.
Postnatal Index	0.0532**	0.0359*	0.100***	0.0208	0.00590
	(0.0165)	(0.0156)	(0.0208)	(0.0220)	(0.0217)
Prenatal Index	0.0704***	0.0584**	0.0400	0.0686**	0.0200
	(0.0188)	(0.0201)	(0.0260)	(0.0227)	(0.0277)
Birth weight	0.0318*	0.0241	0.0320	-0.0294	0.0608*
	(0.0161)	(0.0159)	(0.0209)	(0.0231)	(0.0276)
Height-for-age z-score	-0.00570	-0.0107	0.0186	0.0113	-0.0175
	(0.0121)	(0.0143)	(0.0199)	(0.0185)	(0.0187)
Weight-for-age z-score	0.00817	0.0156	-0.0259	-0.0117	0.0520**
	(0.0120)	(0.0125)	(0.0178)	(0.0177)	(0.0182)
Anemic (Child)	0.00147	-0.0204	0.00210	0.0562*	-0.0180
	(0.0183)	(0.0205)	(0.0291)	(0.0264)	(0.0230)
Total Siblings	0.0117	0.0106	0.00476	0.0156	0.00635
	(0.0117)	(0.0124)	(0.0157)	(0.0144)	(0.0178)
Gender (Female)	0.161***	0.121***	0.245***	0.121***	-0.0271
	(0.0163)	(0.0153)	(0.0230)	(0.0235)	(0.0247)
Birth Order (First)	0.0376	0.0387	0.00535	0.0347	-0.0131
	(0.0251)	(0.0265)	(0.0327)	(0.0324)	(0.0351)
Mother Smoke	-0.0893	-0.145+	-0.101	-0.0242	0.158+
	(0.0627)	(0.0857)	(0.106)	(0.0661)	(0.0893)
Mother age at birth	0.00918	0.0116	0.00899	0.00759	-0.00646
	(0.0105)	(0.00988)	(0.0178)	(0.0143)	(0.0172)
Mother age at birth sq	-0.000209	-0.000242	-0.000152	-0.000155	0.0000407
	(0.000176)	(0.000166)	(0.000301)	(0.000239)	(0.000295)
Years of Education	0.0170***	0.0135***	0.0220***	0.00668+	0.0118*
	(0.00319)	(0.00341)	(0.00426)	(0.00395)	(0.00485)
Mother BMI	0.000304	-0.000822	0.00193	0.00335	-0.00141
	(0.00207)	(0.00213)	(0.00259)	(0.00251)	(0.00290)
Married	0.0285	0.0211	0.00975	0.0373	0.0105
	(0.0183)	(0.0180)	(0.0248)	(0.0258)	(0.0281)
Ethnicity (Indigenous)	0.0728	-0.0207	0.265**	-0.0959	0.0528
	(0.0653)	(0.0631)	(0.0901)	(0.0996)	(0.123)
Wealth Level (Poor)	-0.00566	-0.00195	0.00697	0.000194	-0.0171
	(0.0204)	(0.0215)	(0.0266)	(0.0356)	(0.0326)
Constant	-0.486**	-0.404*	-0.659*	-0.284	-0.0872
	(0.162)	(0.156)	(0.286)	(0.225)	(0.283)
Observations	13589	13589	7735	7735	5776
Adjusted R^2	0.027	0.018	0.042	0.017	0.037
Mean	0.0273	0.0266	-0.00757	-0.0116	0.0596
StdDev	0.953	0.964	0.977	0.991	0.909
Districts	474	474	446	446	415
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.10 Domestic Violence

To measure the impact of WEC on domestic violence, we use the data from DHS. To construct our outcomes variables the questions on domestic violence are aggregated into three categories: Control, Emotional and Physical. The questions are presented int Table 3.21.

The questions in the control category include responses that record any controlling behavior of the partner resulting in psychological maltreatment. The response to the questions in this category applies to the relationship with their (last) husband or partner which we assumes pertains to the current state of their relationship in regards to psychological abuse. The response to each question is equal to one if the respondent suffers from the abuse indicated in the question and zero otherwise.

The questions in the emotional category records emotional violence and the questions in physical violence contains questions regarding physical and sexual violence. The responses in these questions are a dummy variable that takes a value of one if the respondent indicates that they suffered from the domestic violence in the question in the last 12 months and zero otherwise.

The outcome variable for each category of domestic violence takes a value of one if the response to atleast one question in the category is recorded as one and zero otherwise.

Figure 3.21: Domestic Violence Category Description

Category	Questions
Control	Does your husband get jealous or upset if you talk to another man?
	Does he frequently accuse you of being unfaithful?
	Does he prevent you from visiting your friends?
	Does he try to limit your visits or contacts to your family?
	Does he always insist on knowing all the places she goes to?
	Does he distrust you with money?
Emotional	Has he told you or done things to humiliate you in front of others?
	Has he threatened to harm you or someone close to you?
	Has he threatened to leave the house, take away children or stop the financial aid?
Physical	Did he push, shove or throw anything at you?
	Did he slap you?
	Did he hit you with his fist or something that might have hurt you?
	Did he kicked or dragged you?
	Did he try to strangle or burn you?
	Did he threaten you with a knife, pistol or other weapon?
	Did he attack or assault you with a knife, pistol or other weapon?
	Did he use force to have sex even if you did not want to?
	Did he forced you to perform sexual act that you do not approve?

4. THE MANY FACES OF ABUSE: LABOR MARKET OPPORTUNITIES AND DOMESTIC VIOLENCE ON WOMEN

4.1 Introduction

In recent work, it has been established that the economy plays a role in dictating domestic violence towards women. There are different nuances and forms of abuse that a woman may face and the literature thus far has only been able to establish a causal link between the relative labor market condition and only the most severe form of abuse (Aizer, 2010). However, it does not speak to the less severe but more common forms of abuses that women are more likely to suffer from. Emotional and psychological abuses have been found to be more strongly associated with low self-esteem and a diminished sense of worth and independence (Packota, 2000) whereas physical abuse is more likely to cause marital breakdown (Mullen et al., 1996). The conspicuous and lethal nature of physical abuse and the struggle of academics and professionals to develop a measurable and accurate definition of emotional and psychological abuse has resulted in the small amount of work that focuses exclusively on emotional violence especially in economics. The lack of focus for these women, however, may be contributing to the trivialization of this phenomenon in our society (Barling et al., 1987) especially in under developed societies. Despite that, there is no denying of the immense costs associated with the more prevalent psychological abuses that may restrict women from entering the labor market, affect their overall productivity at work and their ability to care for their children.

In this paper will attempt at examining an additional dimension of emotional and psychological abuse by looking at the role of the relative economic condition for women. Specifically, we analyze how the female to male earnings ratio, which is a measure of the female labor market condition relative to the men, affects different forms of intimate partner violence. To obtain an exogenous measure of earnings, we use a Bartik- like approach which exploits the local industry labor shares and the regional change in earning by industry over time by gender. We find that an increase in the female to male earnings ratios decreases the likelihood of controlling and abusive behavior from the husband or partner by about 5% and 8% respectively. The estimate is robust to alternative definitions of domestic violence.

Improvement in the relative labor market condition and non-labor income of women have shown to improve women's status in the household, especially in the form of reduced severe physical domestic violence on women (Aizer, 2010, Bobonis et al., 2013). Recent theoretical models on household decision-making has seen a shift from the early unitary model to a collective model that considers the dynamics of decision making within households. Cooperative bargaining models, which can be labelled as a subset of the collective model, uses a game theoretic model of households in which bargaining power is a function of the outside options of the two individuals bargaining (Doss, 2013). The increase in the relative income of women translates into improved outside option for the women. Based on the model, this should increase the bargaining power of women within the household, whether or not they work in the paid labor force (Manser and Brown, 1980, McElroy and Horney, 1981). An alternative theory of "male backlash" is prominent in the sociological literature which posits that an increase in financial independence of women should increase the incidences of violence against them (Macmillan and Gartner, 1999).

Empirical studies regarding the relationship between labor market conditions for women, bargaining power, and domestic violence has also found mixed results. (Aizer, 2010) finds evidence of decrease in domestic violence with improvement in the gender wage ratio for women. On the contrary, evidence of worsening household status of women with improvement in labor market conditions is also found (Heath, 2014, Kagy, 2014). A potential explanation for the contradicting findings is made in the context of the base household bargaining power of women. It can be argued that in countries where the initial bargaining power of women is high, the threat of a women leaving the marriage in the presence of continued violence and improved financial independence is higher than in a country where the initial bargaining power of women is low. In developing countries, there do exist evidences of positive correlation between whether a woman works and domestic violence (Heath, 2014, Naved and Persson, 2005). There are also evidences of improved female autonomy as measured by their decision-making power in the household and increased intimate partner violence (Fakir et al., 2016).

Due to the difficulty in the construction of an exogenous measure of the relative labor market condition as well as data limitations, the causal link between improved overall labor market opportunities for women and domestic violence has received very little empirical attention, especially in the context of a developing country. Studies that previously tried to address this gap in literature had done so by focusing on a specific type of industry and with small sample size (Majlesi, 2016, Kagy, 2014). However, there focus was on household decision making rather than the different types of domestic violence. In this paper, we analyze the effect of the general relative labor market condition for women on their likelihood of experiencing different forms of domestic violence. To estimate the causal effect of the relative labor market condition for women, this paper constructs an earnings ratio as a function of local demand for female and male workers based on (Aizer, 2010). We find that improvements in female to male earnings ratio decreases the incidences of less severe forms of domestic violence. However, we do not find conclusive evidence of an impact in sexual or severe physical domestic violence in the household due to changes in the relative labor market condition.

The remainder of the paper proceeds as follows. Sections 2 provides the theoretical motivation and Section 3 gives an overview of the survey data used in this paper. Section 4 has the empirical strategy. Section 5 shows results, Section 6 provides additional empirical evidence and Section 7 provides a discussion regarding this study. Finally, Section 8 concludes.

4.2 Theoretical Motivation

Theories on domestic violence has been developed across several disciplines, however, research has still not reached a common consensus on the relationship between improvements in the economic prospective of women and domestic violence. A common theory of male backlash in sociology argues improvement in the economic opportunities for women can increase spousal violence. In patriarchal societies, violence can be used as a means to reinstate the male partners' authority within the household which is threatened with greater economic independence of the wife

(Pence et al., 1993, Macmillan and Gartner, 1999). Similarly, spousal tension can be manifested through the husband and the wife occupying atypical roles within the household such as wife being the main earner which may increase the incidences of domestic violence if the husband feels powerless (Hornung et al., 1981). These theories explains spousal violence as a gender oriented issue where it is used as an instrument to maintain control of the victim's resources or behavior. However, according to (Aizer, 2010), these theories ignore the individual rationality constraints of women in abusive relationship where the women has the option of leaving an abusive relationship.

Economic models have mostly relied on non-cooperative bargaining models to explain intra-household domestic violence. The bargaining outcome depends on the woman's outside option which influences their threat point. Specifically, improvements in the financial resources for women outside the marriage increases their threat point and thus decreases the level of spousal violence in equilibrium (Farmer and Tiefenthaler, 1997, Tauchen et al., 1991). In such cases violence is generally treated as expressive (Farmer and Tiefenthaler, 1997) or an instrument for controlling the wife's behavior (Tauchen et al., 1991). On the other hand, domestic violence may also be used as a bargaining instrument to extract resources from the woman or her family (Bloch and Rao, 2002, Bobonis et al., 2013).

In this paper, inspired by (Aizer, 2010) we create an index to measure the relative improvement in the outside option for women. In line with this work, we hypothesize that an improvement in the outside option should increase the threat point of the female partner and thus decrease the level of intimate partner violence towards women. However, we align more with the model by (Tauchen et al., 1991) where the impact of improvement in outside option depends on the relative utility for women outside the marriage (reservation utility) to their utility from the marriage or partnership. A marginal improvement in the outside option for women should decrease the level of abuse they face if their utility from the marriage is equal to her reservation utility. On the other hand, if a woman lives in a community where separation or divorce is stigmatized and resources such as shelters and police protection is scarce, her reservation utility is likely to be low and a marginal improvement in the outside option for these women may result in no change or an increase in the level of violence they face if violence is used as an instrument of controlling the resources of the household by the husband. We argue that the type of violence faced by the wife may be indicative of her reservation utility and this may lead us to see an impact on a certain type of intimate partner violence and not others.

4.3 Data

4.3.1 Data Source

This paper benefits from a rich, comprehensive individual level nationally representative dataset of women in Peru. For our analysis, we use three datasets which contains data for a period of nine years from the year 2007 to 2015.

The National Census of 2007 and the National Household Survey on Conditions of Living and Poverty (ENAHO) would be used to construct the district level earnings ratio. The ENAHO collects information for the measurement of living conditions and poverty. It is used to measure the national and regional averages of income for different industries.

We also use the Demographic and Health Survey (ENDES) from 2007 to 2015 for individual-level data on the likelihood of different types of violence women aged 15-49 years face from their partner. It also contains information on individual, household and partners' characteristics. Unfortunately, the ENDES does not follow the same individual or household across the survey years, however, it does observe the same geographic area over several years. The ENDES has a two-stage design and a sampling frame is used at each stage for the selection of the sampling unit. The 2007 National Census is used in the first stage for the random selection of the primary sampling units which are geographic clusters. We define the local labor market for the people living in these clusters as the district in which the cluster is. Within each cluster, private dwellings are randomly selected as the secondary sampling units. On average, the survey collects information from more than 25,000 households on average across the different years.

One of the main advantages of this dataset is the consistent collection of data on domestic violence over a long period of time which is hard to find even in developed economies let alone developing countries. Table 4.2 consists of descriptive statistics for the ENDES data to get a better understanding of the distribution of the individual and partner characteristics. Women were randomly selected from the whole sample of the ENDES data for the domestic violence module. We can see that the sample of women from the domestic violence module are very similar to the whole sample in terms of their characteristics.

4.3.2 Constructing the Economic Variable

It is generally difficult to exogenously measure the relative labor condition for individuals that will allow us to make causal inference on domestic violence. Unobserved characteristics of these individuals that may drive both the relative labor market conditions and domestic violence will result in biased estimates. To address the issue of endogeneity, we construct the relative earnings in spirit of (Aizer, 2010) which should reflect exogenous demand of female and male labor demand in the local labor market. This measure was originally proposed by (Bartik, 1991) and is commonly known as the Bartik instrument in the labor literature. It has subsequently been used in several studies (Blanchard et al., 1992, Bound and Holzer, 2000, Hoynes, 2000, Autor and Duggan, 2003, Schaller, 2016, 2012).

The idea is to construct the average annual earnings for each gender- industry- district cell based on the regional changes in the average earnings of the industry, weighted by district-level industry share of workers for each gender. Measured in this way, difference in the earnings by gender would be driven by the changes in the earnings of the different industries that are dominated by the different gender group. In other words, districts with many workers in industries characterized by large, regional earnings growth will experience larger increase in average earnings than districts with many workers in low earnings growth industries. For example, if these growing industries are female dominated, the relative female to male earnings ratio should increase for districts with many female workers working in the growing female dominated industry. This strategy will only be effective if there exist a gender based segregation in different industries.

Figure 4.1 shows the share of male and female workers employed in each industry as a proportion of the total working population in the economy. It provides evidence for the existence of gender based dominance in different industries in Peru. For example, transportation and construction seems to be immensely dominated by male workers whereas industries such as hospitality and domestic services are heavily influenced by male workers. The average annual earnings for each district for each gender is calculated using the following formula:

$$\omega_{gdy} = \sum_i \gamma_{gid} w_{iry}$$

where g indexes gender, d district, i industry, r region and y year. gid is the proportion of women (or men) workers in industry i in district d. The industries are classified into twenty-one broad industry structure as defined by the International Standard Industrial Classification of All Economic Activities (ISIC). These shares are constructed from the National Census of 2007 and are fixed over the period in our analysis. This is to ensure that any changes in the earnings that we construct does not reflect selective sorting across industries as a response to the changes in the actual earnings overtime. wiry is the annual average income from industry i in region r in year y. Once the average earnings for female and male for each district is constructed, we compute the main explanatory variable which is the earnings ratio as follows:

$$Earnings\ Ratio_{dy} = \frac{\omega_{fdy}}{\omega_{mdy}} = \frac{Average\ female\ earnings\ in\ district\ d\ in\ year\ y}{Average\ male\ earnings\ in\ district\ d\ in\ year\ y}$$

4.3.3 Outcome Variables

The ENDES data contains questions on psychological, emotional, physical and sexual abuse pertaining to the last twelve months preceding the interview. Each category of violence contains multiple questions that would indicate some degree of violence in that category. Responses to each question consist of a “yes = 1” or “no = 0” answer, indicating whether the woman had suffered any violence from her partner in the last 12 months.

Based on the type of questions, we use factor analysis to determine if a set of questions explains a specific category of violence. Figure 4.2 shows the types of questions asked and the category of violence they are in.

For this paper, we have four categories of domestic violence reflecting different types of abuse. These categories are Control, Abuse, Sexual and Severe. Control explains any controlling behavior of the partner resulting in psychological maltreatment. The category of Abuse depicts a combination of more serious forms of emotional abuse however, less severe form of physical abuse. These questions are combined together because these actions are more likely to be used complementarily rather than independently [1]. The last two categories are Sexual and Severe which explains any form of sexual violence and the more severe form of physical violence consecutively.

We also compute two different measures for each category of violence. One should be able to capture the likelihood of facing the specific form of violence and the other should capture the intensity of the types of violence by using the number of the types of act a partner employs to impose the certain category of violent act. In other words, the intensity of a specific category of violence a woman suffers from increases as she answers affirmatively to more questions within each category. A combination of acts may be used by partners to impose a certain type of abuse over their female spouses. The intensity measure should be able to capture this aspect.

The outcome variable for the likelihood measure of a specific category would be equal to one if the woman answers affirmatively to at least one question within the category and zero otherwise. The intensity measure is constructed by taking an average of the response to the question within each category. The measure should be between zero and one where zero suggests that the woman did not suffer from any of the acts of violence within that category and one suggest all the different acts of violence within that category were inflicted on the woman by her partner. To test if our results for intensity is robust to the method used for the aggregating of the variables in each category, we compute the intensity measure for each category as a factor variable using factor analysis[2].

[1] Even in this data, there is a much stronger association between the probability of facing emotional abuse (defined by the first three questions in the Abuse category) and probability of physical abuse (defined by the last four questions in the Abuse category) compared to the three other categories in Figure 4.2.

[2] Table 4.9 has the results obtained using the factor variables as our outcome measures. It is consistent with my findings in Table 4.4

4.4 Empirical Strategy

To estimate the impact of the earnings gap on different forms of domestic violence, the following equation is employed:

$$Y_{idy} = \beta_0 + \beta_1 Earnings\ Ratio_{dy} + \alpha_y +_d + X_{idy} + \varepsilon_{idy} \tag{4.1}$$

where Y_{idy} is the outcome variable of woman we in district d in year y; $Earnings\ Ratio_{dy}$ is the calculated female to male earnings ratio in district d in year y; α_y are the year fixed effects; $\alpha_y = d$ are the district fixed effects; X_{idy} includes time-varying women-district covariates such as age, education level dummy, wealth index dummy, ethnicity and household size and, partners age and educational level; and ε_{idy} is the error term.

The individual level controls are added to identify the impact of relative income separately from the individual level and partner characteristics that should also explain a lot of the variation in domestic violence. Year fixed effects are included to control for any systematic difference in domestic violence across years. District level fixed effect should capture any unobserved fixed differences across districts. To get even more conservative estimates, we include province-level and district-level linear time trend to control for any district specific linear time trend in domestic violence. All regressions are weighted by the sample weights in the ENDES data.

4.5 Results

The results with the likelihood outcome measure of domestic violence from the main regression specification are presented in Table 4.3. For the purpose of comparison, the first column of the table contains no controls or linear time trend. For the dependent variable Control Likelihood[3], the estimates are significant and consistent across the different specifications. The results from my main specification which is equation (3.1) is in column (3). The estimate β_1 (-0.158) implies that an increase in the relative earnings of women decreases the likelihood of controlling behavior

[3] Which is equal to one if the woman answers affirmatively to at least one question within the Control category and zero otherwise.

from their partner. Using the standard deviation of the earnings ratio[4], a one standard deviation increase in the earnings ratio, decreases the likelihood of controlling behavior by five percent from the mean[5]. The β_1 coefficient for the Abuse likelihood is also negative and statistically significant. A one standard deviation increase in the earnings ratio causes about eight percent decrease in the abusive behavior of partner from its mean. Albeit statistically insignificant, earnings ratio does not seem to influence the sexual and more severe form of physical abuse since the magnitude of their coefficient is very close to zero. This result is different from the findings of (Aizer, 2010), however, measurement errors as discussed in the later section may be one possible explanation for not finding an effect on these two categories of domestic violence.

The results for the outcome variables measuring intensity of the different forms of domestic violence is presented in Table 4.4. We only find evidence of change in intensity for the controlling behavior which is explained by the Control Intensity[6] variable. A one standard deviation increase in the earnings ratio can lead to a seven percent decrease in the intensity of controlling behavior from its mean. We do not find any effect of the relative labor market condition for women on the intensity measure for the remaining categories of domestic violence.

For our analysis, in the context of Peru, there is evidence of a decrease in the psychological or controlling and the less severe form of abusive behavior with improvements in the relative labor market condition for women.

4.6 Additional Empirical Evidence

4.6.1 Alternative Categorization of Domestic Violence

Given that there is not one single way of categorizing different types of domestic violence, we base our definition of different types of domestic violence based on the factors determined by factor analysis. However, this does not need to be the most accurate categorization of domestic

[4]Which is equal to 0.21

[5]The mean and standard deviation of each outcome variable can be found in the Figure 4.3

[6]Control Intensity measure is computed by taking the average of the responses to the questions in the Control category. Since each question within a category is a dichotomous variable, the Control Intensity variable can take any value from zero to one, where zero suggests that the respondent suffered none of the controlling behavior in the controlling category and one represents that the respondent was a victim of each of the controlling behavior depicted by the questions in the Control category.

violence and thus we compute different composition of responses to create alternative forms of the domestic violence categories.

The regression analysis in Table 4.5 follows the specification from column (3) in Table 4.3 which is our preferred specification. The dependent variables are dichotomous representing the likelihood of suffering from the different types of domestic violence

4.6.2 Impact on Alcohol Consumption and Bargaining Power

Non- cooperative bargaining model's prediction on domestic violence is consistent with our findings in the previous sections. However, less is known about the motive that triggers these acts.

According to the model, improvement in the bargaining power of women in the household should be the reason for the decrease in the level of domestic violence. Direct decisions have been used as a proxy for bargaining power in the literature (Antman, 2014, Atkin, 2009, Friedberg and Webb, 2006). We distinguish between women with high and low decision-making power within the household based on their responses to questions that asks who has a say on different decisions that must be made within a household. Table 4.6 column (7) and (8) shows the impact of improvement in the relative earnings ratio on a dichotomous dependent variable that is equal to 1 if the women has high decision-making power. We do find some evidence that decision making power improves, however, addition of controls makes our estimate imprecise.

Alcohol abuse and domestic violence can be related to the literature on situational violence consistent with the model of failure to self-control (Loewenstein and O'Donoghue, 2007). In Table 4.6, although not statistically significant, we find suggestive evidence of a decrease in the probability of consuming alcohol with better earnings ratio. However, conditional on drinking, the probability of getting drunk increases with improvements in the earnings ratio. This may lead to increase in situational violence, counteracting the impact that improvement in the bargaining power of women had on reducing domestic violence.

4.6.3 Impact on Women's Characteristics

We also explore if the relative earnings ratio had any impact on any characteristics of the women. In Table 4.8, we estimate the impact of the earning ratio on the woman's years of education, fertility, likelihood of living in a female headed household, household size and the wealth index of the household.

In all cases, we do not find any statistically significant evidence that an improvement in the earnings ratio influenced any outcomes for women that are related to their characteristics.

4.7 Discussion

Our findings are consistent with economic theories of spousal violence which focuses on non-cooperative bargaining models. We find evidence of reduced controlling and abusive behavior by partners with improving earnings ratio for women as well a decrease in the overall physical violence by the spouse. However, the impact on sexual and severe form of violence remains inconclusive.

Contrary to our study, (Aizer, 2010) did find evidence of reduced severe physical violence with improvements in the relative labor market condition. Below we discuss some reasons that may have contributed to that.

4.7.1 Opportunity Cost of Violence

Improvement in the outside option for women should influence the opportunity cost of partners to engage in spousal abuse. According to our interpretation, improvement in the labor market condition for women may not translate into contemporaneous or instantaneous increase in the wealth level, employment status or income of the women, rather it improves their labor market prospective and potential income. Given that the improvement in income of the women may not be realized, it may be ineffective in influencing the opportunity cost of partners engaged in the more severe forms of domestic violence. This coupled with the prior willingness of women to stay in a severely abusive relationship[7] which indicates a low threat point for these women may have dictated our inability to find any effect on severe or sexual form of violence. Some evidence of this can be seem in Table 4.7, where we see the fear of retaliation being stronger for women in severely abusive relationship. This negates their reservation utility outside of marriage and thus the marginal improvement in the outside option is ineffective in increasing the reservation utility at a higher level than the utility in the marriage.

4.7.2 Reporting Bias

Self-reported data on domestic violence generally suffers from the problem of measurement error. Part of the non-random measurement error is suspected to be systemic under-reporting. Under-reporting is widely considered to be a much more common threat to validity in measuring violence compared to over- reporting (Ellsberg et al., 2001). Some of the common reasons for not reporting abuse include victims feeling embarrassed or fear repercussion by her abuser should he find out about it.

Although the degree of under-reporting across the different categories of abuse is not clear, we hypothesize that responses by women are more prone to misclassification if they suffer from a more severe form of abuse. This hypothesis stems from the idea that the cost of repercussion for women in a physically abusive relationship are likely to be larger on average than the cost for women with a controlling spouse with no associated physical abuse. This would lead to our data on severe physical violence to be nosier than other measures of domestic violence, leading to us finding inconclusive evidence on the more severe form of domestic violence.

To shed some light on our theory, we see if women are less likely to seek help for different types of domestic abuse based on their fear of reprisal. As we see in the Table 4.7, women are more likely to not seek help due to fear if they suffer from domestic violence and if the type of abuse is more severe. Table 4.7 depicts how different types of domestic violence deters you from reporting out of fear. As we see, the severe form of violence seems to inflict a stronger fear of

[7] Despite institution of domestic violence laws, help available for battered women such as shelter are inadequate and ineffective. Thus, leaving an abusive relationship may not be the most practical option available for these women.

seeking help for domestic violence. We also test if the fear of reporting is associated with the earnings ratio and as we see in the Table 4.7 (Panel A), that does not seem to be the case.

4.7.3 Caveats

The use of self-reported survey data for the construction of average earnings and domestic comes with the disadvantage of classical measurement error. This should both attenuate the estimates towards zero as well as inflate the standard errors. Self-reporting to sensitive questions also suffers from the issue of systematic under-reporting. In the case of domestic violence this problem remains (Ellsberg et al., 2001). According to (Blattman et al., 2016) this should also bias the estimated effect towards the null.

There is also a possibility that the changes in the labor market condition can affect women's propensity of reporting. This would pose a more serious threat on the reliability of my estimates. However, if there is evidence that the improvements in the relative labor market condition for women is causing them to report more in circumstances where they face abuse, my results should again be attenuated towards the null.

To argue that this the case, we show in Table 4.10 that women are more likely to seek help from others, which can be considered a proxy for reporting, if their labor market conditions are improving and they are suffering from abuse. In all the cases of measurement error this data may suffer from, it seems to be more probable that we have an underestimate of the effect of the labor market conditions on different forms of domestic violence rather than an overestimate. Another possible concern in our analysis is the use of earnings instead of wages in the construction of the relative labor market index. In the context of Peru, even in the absence of data limitation, the wage data would not be able to capture the labor market dynamics of the whole population. More than 45% of the total working population in Peru are employed in the informal sector (Jaramillo, 2013). The employer survey data that would be used to collect information will fail to capture almost half of the economy. Monthly hours worked also does not seem to have significantly changed over the years (Jaramillo, 2013). Thus, any changes in earnings is less likely to be a result from changes in labor productivity/supply rather than changes in labor demand.

Over the period in our analysis, there were no major structural or economic reform other than some changes in the trade policy through liberalization. Most of the structural changes took place in the 1990s and thus out study covers a more stable period. This has created more jobs and employment opportunities for the Peruvians. Although unemployment rates for both women and men fell over the years, employment may not be a good indicator of relative labor market condition in the context of Peru since there exist a distinction between formal and informal sector. Men seems to be less likely overtime in our study period to be employed in the low paying informal sector than the women and thus the gap in the earnings has widened overtime despite improvement in the overall employment of women. Therefore, average earning is arguably a better indicator of the relative position of women compared to men in the labor market.

4.8 Conclusion

This paper examines the effect of improvement in the relative labor market position of women on their likelihood of suffering from different forms of domestic violence. This paper fills the gap in the literature by analyzing the effect that economic conditions may have on the more neglected and least studied forms of violence. We find evidence of a decrease in the likelihood of controlling behavior and overall abuse on women with the improvement in the relative earnings ratio. However, unlike (Aizer, 2010), We do not find any evidence of impact on the more severe form of physical violence on women. This may be due to the issue of under- reporting which is likely to be more severe for the more sensitive questions. We also find evidence of a reduction in the intensity of controlling behavior by partners with increasing earnings ratio.

Contrary to many of the findings in the domestic violence and labor market literature in developing countries, our results seem to be aligned with the findings of (Aizer, 2010) albeit providing a more complete picture of the effect of labor market condition on domestic violence and reiterating that the predictions from the bargaining model with domestic violence holds.

Figure 4.1: Proportion of Population Employed in Each Industry by Gender

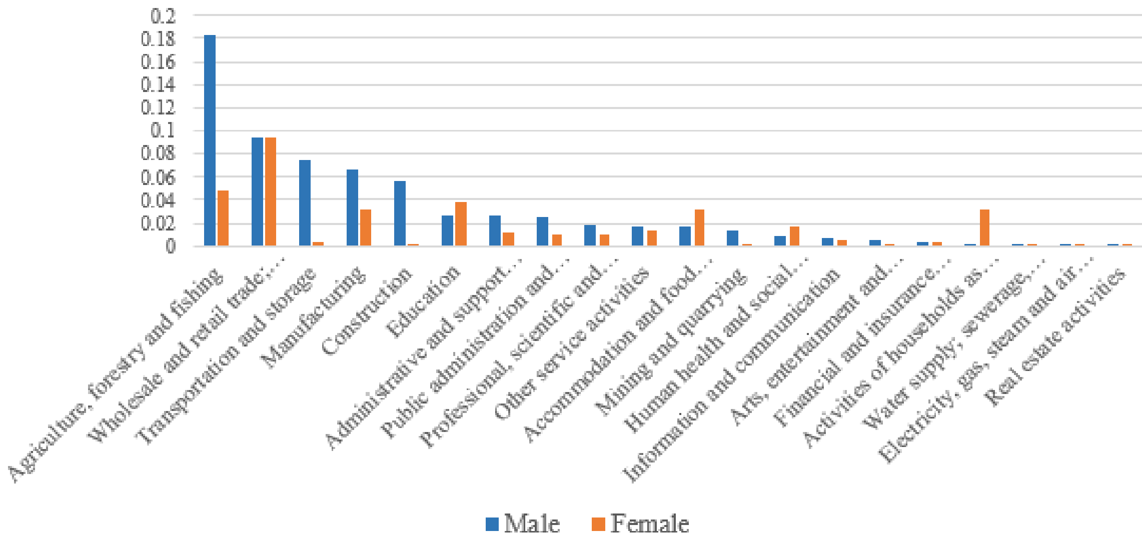


Figure 4.2: Categories of Domestic Violence and the Tests of Reliability

Category	Sub-Category	Variable label	Average Inter-item correlation	Cronbach's alpha	Average item-rest correlation	Kuder-Richarson 20 (KR-20)
Control		Does your husband get jealous or upset if you talk to another man?	0.3534	0.7663	0.5045	0.7535
		Does he frequently accuse you of being unfaithful?				
		Does he prevent you from visiting your friends?				
		Does he try to limit your visits or contacts to your family?				
		Does he always insist on knowing all the places she goes to?				
		Does he distrust you with money?				
Abuse	Emotional Abuse	Has he told you or done things to humiliate you in front of others?	0.4672	0.8599	0.626	0.8541
		Has he threatened to harm you or someone close to you?				
		Has he threatened to leave the house, take away children or stop the financial aid?				
	Any Physical Abuse	Did he push, shove or throw anything at you?				
		Did he slap you?				
		Did he hit you with his fist or something that might have hurt you?				
		Did he kicked or dragged you?				
Severe		Did he try to strangle or burn you?	0.3925	0.6597	0.4568	0.628
		Did he threaten you with a knife, pistol or other weapon?				
		Did he attack or assault you with a knife, pistol or other weapon?				
Sexual		Did he use force to have sex even if you did not want to?	0.5818	0.7356	0.5818	0.7224
		Did he forced you to perform sexual act that you do not approve?				

Note: The table contains information of the different types of questions that are divided into categories to form the outcome variables. Since there are a lot of variables with information on different form of violence, We try to condense the variables into groups for ease of interpretation. To determine which question is appropriate for each group or category, WE use exploratory factor analysis that selects variables for each category based on the correlation of each question and the factor that explains the category. Cronbach's alpha and the KR-20 is a measure of the internal consistency that shows how closely related the set of questions in each category are. A score of greater than 0.65 to cautiously say that the aggregates measures are reliable. A score of 0.15 to 0.5 for the Inter-item correlation is usually considered acceptable to researcher to say that the items in a category are adequately related.

Table 4.1: Summary Statistics of the Outcome Variables

	Likelihood			Intensity		
Variable	Obs	Mean	Std. Dev	Obs	Mean	Std. Dev
Control	132749	0.6721	0.4695	132749	0.2667	0.2799
Abuse	134236	0.2068	0.4050	134236	0.0802	0.1975
Sexual	134262	0.0354	0.1849	134262	0.0249	0.1376
Severe	134266	0.0183	0.1342	134266	0.0086	0.0698

Table 4.2: Descriptive Statistics for the ENDES Data

	Whole Sample	Domestic Violence Sample
Covariates	Mean	Mean
Panel A: Women Characteristics		
Age	30.42	31.17
Ethnicity (Spanish)	0.89	0.88
Household size	5.09	4.68
Sex of Household Head:		
Male	0.77	0.78
Female	0.23	0.22
Education level:		
No education	0.03	0.03
Incomplete primary	0.17	0.18
Complete primary	0.1	0.11
Incomplete secondary	0.21	0.2
Complete secondary	0.22	0.23
Higher	0.27	0.26
Wealth Quintiles:		
Poorest	0.19	0.21
Poorer	0.24	0.25
Middle	0.23	0.22
Richer	0.19	0.18
Richest	0.15	0.13
Panel B: Partner Characteristics		
Partner's Age	37.74	37.35
Partner's Educational level:		
No education	0.01	0.01
Incomplete primary	0.17	0.16
Complete primary	0.09	0.09
Incomplete secondary	0.25	0.25
Complete secondary	0.33	0.33
Higher	0.15	0.15

Note:The table above show the distribution of the different characteristics for the women and their partner in the Demographic and Health Survey (ENDES) data.

Table 4.3: Estimates of the Impact of Female to Male Earnings Ratio on the Likelihood of Different Violence

Dependent Variables	(1)	(2)	(3)	(4)	(5)
Control Likelihood	-0.149***	-0.152***	-0.158***	-0.150**	-0.116*
	(0.0377)	(0.0383)	(0.0413)	(0.0459)	(0.0545)
Observations	132296	132296	115497	115497	115497
R-Squared	0.033	0.038	0.044	0.048	0.061
Abuse Likelihood	-0.0604*	-0.0667*	-0.0801*	-0.0587+	-0.0520
	(0.0302)	(0.0306)	(0.0317)	(0.0333)	(0.0387)
Observations	133778	133778	116809	116809	116809
R-Squared	0.031	0.040	0.045	0.048	0.059
Sexual Likelihood	-0.000487	-0.00115	-0.00738	-0.00557	-0.00905
	(0.0135)	(0.0135)	(0.0137)	(0.0150)	(0.0170)
Observations	133804	133804	116794	116794	116794
R-Squared	0.021	0.023	0.027	0.030	0.042
Severe Likelihood	0.00570	0.00607	0.00914	0.00791	-0.000122
	(0.00888)	(0.00887)	(0.00906)	(0.00969)	(0.0116)
Observations	133808	133808	116796	116796	116796
R-Squared	0.019	0.020	0.025	0.028	0.040
Year Fixed Effect	Y	Y	Y	Y	Y
District Fixed Effect	Y	Y	Y	Y	Y
Individual Controls		Y	Y	Y	Y
Partner Controls			Y	Y	Y
Province Time Trend				Y	
District Time Trend					Y

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The individual level controls include age, educational level dummies, wealth quintile dummies, household size. The partner controls include partner's educational level and age. The outcome variables for each category in this table is a dichotomous variable corresponding to the likelihood of any of the event within the specific category taking place. The dependent variable for a category equals to one if the woman answers affirmatively to at least one question within the category and zero otherwise. Each estimate in the table is the coefficient of the explanatory variable earnings-ratio which is constructed by dividing the average female earnings for each district- year by the average male earnings for that the same district- year.

Table 4.4: Estimates of the Impact of Female to Male Earnings Ratio on the Intensity of Different Violence

Dependent Variables	(1)	(2)	(3)	(4)	(5)
Control Intensity	-0.0865***	-0.0871***	-0.0885***	-0.0636**	-0.0457+
	(0.0218)	(0.0224)	(0.0202)	(0.0227)	(0.0271)
Observations	132296	132296	115469	115469	115469
R-Squared	0.036	0.048	0.058	0.061	0.074
Abuse Intensity	-0.0217	-0.0241	-0.0236	-0.0182	-0.0184
	(0.0153)	(0.0154)	(0.0153)	(0.0164)	(0.0196)
Observations	133778	133778	116781	116781	116781
R-Squared	0.032	0.041	0.047	0.050	0.062
Sexual Intensity	-0.00285	-0.00330	-0.00928	-0.00606	-0.00892
	(0.0100)	(0.0100)	(0.00468)	(0.0108)	(0.0120)
Observations	133804	133804	116794	116794	116794
R-Squared	0.021	0.023	0.027	0.030	0.042
Severe Intensity	0.00147	0.00175	0.00463	0.00484	0.000146
	(0.00472)	(0.00472)	(0.00468)	(0.00524)	(0.00626)
Observations	133808	133808	116796	116796	116796
R-Squared	0.020	0.021	0.026	0.029	0.042
Year Fixed Effect	Y	Y	Y	Y	Y
District Fixed Effect	Y	Y	Y	Y	Y
Individual Controls		Y	Y	Y	Y
Partner Controls			Y	Y	Y
Province Time Trend				Y	
District Time Trend					Y

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The individual level controls include age, educational level dummies, wealth quintile dummies, household size. The partner controls include partner's educational level and age. The outcome variables for each category measuring intensity in this table, is an average of all the responses to the questions within that category. Refer to Figure 4.2 to see the questions in each category. Since the response to each question in a category is a dichotomous variable, dependent variable can take any value from zero to one, where zero suggests that the respondent suffered none of the form of violence mentioned in that category and one represents that the respondent was a victim of each act of violence depicted by the questions in the category. Each estimate in the table is the coefficient of the explanatory variable earnings-ratio which is constructed by dividing the average female earnings for each district- year by the average male earnings for that the same district- year.

Table 4.5: Estimates of the Impact of Female to Male Earnings Ratio on the Likelihood of Different Violence Defined Differently

Dependent Variable height	Coefficient	Mean/ (Std. Dev.)
	(1)	(2)
Domestic violence(control or abuse or severe or sexual violence)	-0.160***	0.6914
	(0.0400)	(0.4619)
Domestic violence (abuse or severe or sexual violence)	-0.0810*	0.2112
	(0.0324)	(0.4081)
Psychological abuse (control or emotional abuse)	-0.156***	0.6828
	(0.0404)	(0.4654)
Emotional Abuse	-0.0501+	0.1633
	(0.0290)	(0.3697)
Psychological Abuse with no physical abuse	-0.112**	0.5562
	(0.0419)	(0.4968)
Any Physical Abuse (Physical abuse or Severe abuse)	-0.0497+	0.1318
	(0.0260)	(0.3383)
Any Physical or Sexual abuse (Any Physical abuse or sexual abuse)	-0.0440+	0.1404
	(0.0253)	(0.3474)
Severe physical violence (broader definition)	-0.000149	0.0778
	(0.0204)	(0.2679)
Year Fixed Effect	Y	
District Fixed Effect	Y	
Individual Controls	Y	
Partner Controls	Y	

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The individual level controls include age, educational level dummies, wealth quintile dummies, household size. The partner controls include partner's educational level and age. The dependent variables are dichotomous Each estimate in the table on column (1) is the coefficient of the explanatory variable earnings-ratio which is constructed by dividing the average female earnings for each district- year by the average male earnings for that the same district- year. Column (2) contains the mean and the standard deviation of the dependent variable. The categorization these variables can be found in Figure 4.3.

Table 4.6: Estimates of the Impact of Female to Male Earnings Ratio on the Likelihood of Other Factors

	Partner consumes alcohol		Partner gets drunk		Partner gets drunk conditional on drinking		Wife's decision-making power improves	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earnings Ratio	-0.0285	-0.0376	0.105*	0.0920+	0.3312***	0.3370***	0.0835*	-0.0550
	(0.0380)	(0.0401)	(0.0454)	(0.0469)	(0.0817)	(0.0855)	(0.0397)	(0.0335)
N	133809	117082	133798	117073	101302	87456	166889	141525
R-sq	0.044	0.054	0.062	0.074	0.0694	0.0833	0.119	0.105
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual Controls		Y		Y		Y		Y
Partner Controls		Y		Y		Y		Y

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The individual level controls include age, educational level dummies, wealth quintile dummies, household size. The partner controls include partner's educational level and age. The dependent variables are dichotomous Each estimate in the table is the coefficient of the explanatory variable earnings-ratio which is constructed by dividing the average female earnings for each district- year by the average male earnings for that the same district- year.

Table 4.7: Estimates of the Impact of Female to Male Earnings Ratio and Different Types of Abuse on the Unwillingness to Seek Help for Violence Out of Fear.

Panel A: Labor market response

Explanatory Variable:	Earnings ratio
Coefficient	0.0803
	(0.0673)
N	34192
R-sq	0.089

Panel B: Psychological Abuse

Explanatory Variable:	Control	Psychological	Emotional Abuse
Coefficient	0.0596***	0.0664***	0.0939***
	(0.00724)	(0.00686)	(0.00854)
N	33791	34200	34195
R-sq	0.094	0.094	0.1

Panel C: Physical or Sexual Abuse

Explanatory Variable:	Abuse	Any Physical Abuse	Physical or sexual abuse
Coefficient	0.0688***	0.0691***	0.0682***
	(0.00739)	(0.00729)	(0.0068)
N	34189	34198	34200
R-sq	0.096	0.095	0.095

Panel D: Severe Physical or Sexual Abuse

Explanatory Variable:	Sexual	Severe	Severe Physical Violence (broad def.)
Coefficient	0.145***	0.152***	0.247***
	(0.0144)	(0.0112)	(0.0259)
N	34196	34200	34198
R-sq	0.096	0.105	0.097

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The individual level controls include age, educational level dummies, wealth quintile dummies, household size. The partner controls include partner's educational level and age. (1) Earnings-ratio: constructed by dividing the average female earnings for each district- year by the average male earnings for that the same district- year (2) Control (Refer to Figure 4.3) (3) Psychological abuse (control or emotional abuse) (Refer to Figure 4.3) (4) Emotional Abuse (Refer to Figure 4.3) (5) Abuse (Refer to Figure 4.3) (6) Any Physical Abuse (Physical abuse or Severe abuse) (Refer to Figure 4.3) (7) Any Physical or Sexual abuse (Any Physical abuse or sexual abuse) (Refer to Figure 4.3) (8) Sexual (Refer to Figure 4.3) (9) Severe physical violence (broader definition) (Refer to Figure 4.3) (10) Severe (Refer to Figure 4.3)

Table 4.8: Estimates of the Impact of Earnings Ratio Women Characteristics

	(1) Education	(2) Total Children	(3) Female HH Head	(4) Household Size	(5) Wealth Index
Earnings Ratio	-0.0698	-0.0341	0.0570	0.0960	0.482+
	(0.118)	(0.0732)	(0.0897)	(0.102)	(0.286)
N	194828	194829	194829	194829	191827
R-sq	0.270	0.080	0.026	0.046	0.360
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The regression is estimated only controlling for the age of the women and the analysis includes all women in the data set between 2007 to 2015 in the DHS. The estimates are presented in terms of the standard deviation of the respective outcome variable.

Figure 4.3: Questions Constituting Each of the Domestic Violence Categories

<table>
<tr><th>Variable label</th><th colspan="6">Categories</th></tr>
<tr><td>Does your husband get jealous or upset if you talk to another man?</td><td rowspan="6">Control</td><td rowspan="18">Domestic violence (control or abuse or severe or sexual violence)</td><td rowspan="6">Control</td><td rowspan="9">Psychological abuse (control or emotional abuse)</td><td rowspan="11"></td><td rowspan="6">Control</td></tr>
<tr><td>Does he frequently accuse you of being unfaithful?</td></tr>
<tr><td>Does he prevent you from visiting your friends?</td></tr>
<tr><td>Does he try to limit your visits or contacts to your family?</td></tr>
<tr><td>Does he always insist on knowing all the places she goes to?</td></tr>
<tr><td>Does he distrust you with money?</td></tr>
<tr><td>Has he told you or done things to humiliate you in front of others?</td><td rowspan="7">Abuse</td><td rowspan="12">Domestic violence (abuse or severe or sexual violence)</td><td rowspan="3">Emotional Abuse</td></tr>
<tr><td>Has he threatened to harm you or someone close to you?</td></tr>
<tr><td>Has he threatened to leave the house, take away children or stop the financial aid?</td></tr>
<tr><td>Did he push, shove or throw anything at you?</td><td rowspan="9">Any Physical or Sexual abuse (Any Physical abuse or sexual abuse)</td><td rowspan="7">Any Physical Abuse</td></tr>
<tr><td>Did he slap you?</td></tr>
<tr><td>Did he hit you with his fist or something that might have hurt you?</td><td rowspan="5">Severe physical violence (broader definition)</td></tr>
<tr><td>Did he kicked or dragged you?</td></tr>
<tr><td>Did he try to strangle or burn you?</td><td rowspan="3">Severe</td></tr>
<tr><td>Did he threaten you with a knife, pistol or other weapon?</td></tr>
<tr><td>Did he attack or assault you with a knife, pistol or other weapon?</td></tr>
<tr><td>Did he use force to have sex even if you did not want to?</td><td rowspan="2">Sexual</td><td rowspan="2"></td><td rowspan="2">Sexual Abuse</td></tr>
<tr><td>Did he forced you to perform sexual act that you do not approve?</td></tr>
</table>

Table 4.9: OLS Estimates of the Impact of Female to Male Earnings Ratio on the Intensity (Factor) of Different Violence

Dependent Variables	(1)	(2)	(3)	(4)	(5)
Control Intensity	-0.190***	-0.190***	-0.191***	-0.133*	-0.0959
	(0.0534)	(0.0547)	(0.0483)	(0.0534)	(0.0639)
Observations	132296	132296	115744	115744	115744
R-Squared	0.036	0.050	0.060	0.063	0.076
Abuse Intensity	-0.0773	-0.0856	-0.0791	-0.0589	-0.0624
	(0.0598)	(0.0602)	(0.0596)	(0.0642)	(0.0768)
Observations	133778	133778	117063	117063	117063
R-Squared	0.032	0.041	0.047	0.050	0.062
Severe Intensity	0.0118	0.0144	0.0340	0.0327	0.00584
	(0.0338)	(0.0337)	(0.0341)	(0.0388)	(0.0464)
Observations	133808	133808	117078	117078	117078
R-Squared	0.020	0.021	0.025	0.028	0.042
Year Fixed Effect	Y	Y	Y	Y	Y
District Fixed Effect	Y	Y	Y	Y	Y
Individual Controls		Y	Y	Y	Y
Partner Controls			Y	Y	Y
Province Time Trend				Y	
District Time Trend					Y

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The individual level controls include age, educational level dummies, wealth quintile dummies, household size. The partner controls include partner's educational level and age. The outcome variables for each category measuring intensity in this table, is a factor variable weighted based on the correlation of the items in each category. This is to check if my results are robust to the way in which we aggregate my outcome variable. The findings in this table corroborates the finding in Table 4.4. Each estimate in the table is the coefficient of the explanatory variable earnings-ratio which is constructed by dividing the average female earnings for each district- year by the average male earnings for that the same district- year.

Table 4.10: Looking at the Effect on the Probability of Seeking Help from Someone Other than Your Own Husband

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control	Abuse	Severe	Sexual	Severe (Broad def.)	Any Domestic violence	Any Physical	Emotional
Type of domestic violence	0.111***	0.154***	0.241**	0.221***	0.242***	0.127***	0.183***	0.156***
	(0.0160)	(0.0265)	(0.0803)	(0.0532)	(0.0402)	(0.0156)	(0.0319)	(0.0272)
Earnings ratio	-0.0137	-0.0334	-0.0458	-0.0397	-0.0445	-0.00811	-0.0374	-0.0362
	(0.0294)	(0.0271)	(0.0286)	(0.0284)	(0.0278)	(0.0284)	(0.0271)	(0.0275)
Interaction term	-0.0202	0.0561*	0.0848	0.0299	0.0433	-0.0214	0.0749*	0.0450+
	(0.0147)	(0.0250)	(0.0738)	(0.0493)	(0.0378)	(0.0144)	(0.0307)	(0.0261)
N	115744	117063	117078	117076	117074	117083	117073	117072
R-sq	0.071	0.109	0.067	0.071	0.097	0.075	0.111	0.097

Note: Standard errors in parentheses. + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Each outcome variable corresponds to the probability of seeking help from the group mentions in the second row for each column. The dependent variable is equal to 1 if the respondent sought help from the any member of the group mentioned in the second row for each column and zero otherwise. All the specification has district, year fixed effect and individual and partner characteristics as covariates. To explain what the regression results mean let consider column (7) and column (11). The group others is 1 if the respondent asked for help from her neighbors or anyone other than her friends or family. The coefficient for the earnings ratio suggests that in the even they do not face controlling behavior from their partner, they are less likely to seek help from others. This is consistent with my results since controlling behavior should reduce with earnings ratio improvement. The interaction term of earnings ratio with control likelihood is interpreted as follows: if the husband of respondent tries to impose controlling behavior on the respondent, they are more likely to seek help with increasing earnings ratio. In other words, respondents are more likely to report with increasing earnings ratio. Same is true for column (8) with abuse likelihood. Column (11) and (12) equal 1 if they do not seek any help. The results are also consistent. The interaction terms suggest that they are less likely to keep abuse to themselves with improvement in the labor market conditions.

5. SUMMARY AND CONCLUSIONS

The three essays in this dissertations explores the long term impact of conflict on criminal behavior and how policies and market conditions that could potentially improve the bargaining power of women impacts women and children outcomes in the context of Peru.

First, I show that exposure to conflict during childhood is associated with an increase in the probability of getting incarcerated in the long run. Particularly, I find evidence that primary school aged children are more susceptible to conflict in determining their criminal career as adults. Second, we show how a government instituted policy for victims of domestic violence improves the health and cognitive outcomes of children under the age of five. Specifically, the paper finds evidence that close proximity to the Women Emergency Centers is associated with improvements in the weight-for-age, wasting and underweight measures of children under five. We also find evidence of improvement in the symbolic function of children under five. Finally, we show that improvement in the relative labor market condition for women decreases the probability of suffering from intimate partner violence.

Civil conflict and domestic violence are persistent and pervasive issues that especially plagues the developing countries. The findings in this dissertation are particularly relevant to the experiences in the developing countries, more specifically the Latin American countries, as all the studies are based in Peru. The first essay betters our understanding of the negative consequences of the civil conflict and identifies the areas that should be taken into account during post conflict recovery. The last two essays are pertinent to the issue of domestic violence that has being relentlessly coexisting with the patriarchal culture in Latin America and other developing societies. The essays provide evidences of policies that are effective in tackling this deep-rooted problem and could lead to the social progress of women and their children.

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