The Effect of Civil Conflict on Child Abuse: Evidence from Peru

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Abstract

This paper explores the inter-generational effects of civil conflict. I use the temporal and spatial variation in the Peruvian civil conflict that occurred between 1980 and 2000 to identify the effect of civil conflict exposure on the use of physical punishment as a child discipline method. The main result suggests a mother exposed to an additional one hundred violent conflict-related events in her district during her lifetime is 3.6 percentage points less likely to abuse her children, with exposure to conflict after early childhood driving the result. This effect is nearly ten times as large as the effect of an additional year of schooling on the probability of using physical child abuse. The main result may be explained by conflict's short-term effect on intra-household violence. I also explore whether conflict's long-term impact on community-level resources may have affected women's use of abusive discipline methods.

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I. Introduction

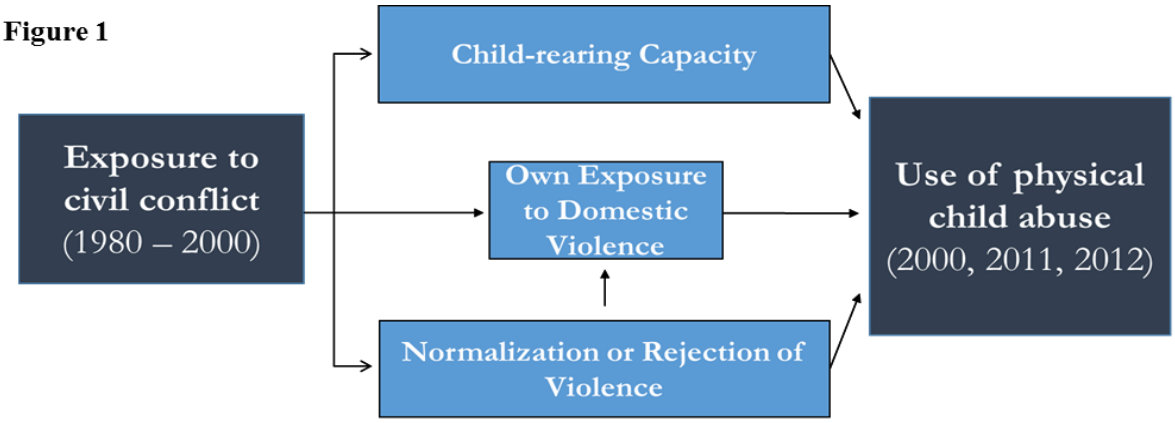
Civil conflict adversely affects the level of economic development through destruction of physical and human capital, the latter being particularly susceptible to investments at an early age. Although early life human capital investment is usually measured through education and health, an important determinant of both could be parenting quality. In earlier work, we find physical child abuse (PCA), which is inherently linked to parenting quality, negatively impacts early childhood nutritional outcomes (Morales & Singh, 2013). In this paper, I investigate whether violence occurring outside the confines of a home can alter intrahousehold violence. Using the Peruvian civil conflict that occurred between 1980 and 2000, this paper is the first to explore whether exposure to violence from an armed conflict affects the later use of physical punishment as a child discipline method.

Civil conflict can have long-term effects on the use of PCA through various channels. Conflict can normalize and desensitize violent behavior, but it could also lead to positive psychological growth—witnessing the terrors of conflict may lead to the rejection of abusive punishment. In this regard, conflict may have a direct or inter-generational impact on parental discipline methods. Intimate partner violence (IPV) or physical child abuse may increase *during* civil conflict, thus possibly marking the beginning to a cycle since childhood exposure to domestic violence is strongly correlated to PCA-use as a parent.[1] There could also be indirect effects. Conflict could influence an individual's child-rearing capacity through disruptions in schooling, income, or post-conflict changes in social services. Figure 1 presents how these channels might relate a history of civil conflict exposure to subsequent changes in PCA-use.

This paper's identification strategy relies on the spatial and temporal variation of Peru's internal civil conflict. I use a fixed effects model to identify a causal relationship between exposure to conflict and the later use of physical punishment as reported in three cross-sections

[1] This paper places both "physical child abuse" and "intimate partner violence" (the latter referring more specifically to intimate partner physical violence) under the more general "domestic violence" term.

Figure 1



of the Peruvian Demographic and Health Surveys (DHS). The extensive use of fixed effects allows me to obtain estimates within districts and within birth year cohorts. The main result suggests a mother exposed to an additional one hundred violent conflict-related events in her district during her lifetime is 3.4-3.8 percentage points less likely to abuse her children, with respect to survey year, birth year cohort, and district averages as well as regional time trends. This effect is equal to the impact of an additional 4-10 years of education (depending on the model).The main finding is consistent to a series of robustness checks, including the use of propensity scores to restrict the analysis to more comparable districts.

I explore the mechanisms driving the results. There is evidence the conflict could have increased parenting knowledge and support. Communities that experienced higher levels of conflict violence saw greater increases in social spending and had more health resources in the post-conflict period, and women's conflict exposure is associated with a higher likelihood of accessing these resources. I also find women previously affected by greater conflict intensity between the ages of 9 and 16 years were less likely to be abused by their parents, which could explain the future decrease in their own use of PCA.

I check the external validity of my results using recent armed violence in Colombia, Peru's northern neighbor. While the Peru context shows long-term effects, in the Colombia analysis the time gap between the exposure to civil conflict and the reporting of child discipline strategies is much shorter, therefore allowing for the identification of short-term effects on physical

punishment. Replicating the analysis from Peru, I also find women's conflict exposure in Colombia is associated with a decrease in abusive child discipline.

This paper joins the literature studying child welfare and the consequences of conflict. Peru offers fertile ground for investigating the nexus of physical child abuse and armed political violence. First, child abuse and maltreatment is commonplace in Peru, where a recent Gallup study found 29% of those polled knew of "a child who was beaten or physically mistreated by his or her parents, guardians, or any extended family in the past 30 days" (English & Godoy, 2010). Recognizing the widespread presence of violence in the home, the past decade has seen the Peruvian government take various legislative measures and raise public awareness around the issue (Ombudsman Against Physical and Humiliating Punishment of Children and Adolescents, 2009) Second, the daily lives of Peruvian citizens were tangibly altered by the internal conflict. The Truth and Reconciliation Commission (TRC), created to investigate the impact of the violence, estimates over 69,000 persons died as a result of the 20-year long conflict, and previous academic research shows its repercussions on a wide-range of development-related outcomes.[2]

Research on the consequences of armed conflict has boomed in the past decade. The recent interest is well deserved since a third of all nations have experienced civil conflicts since 1960 (Marshall & Gurr, 2005). Previous conflict research on child welfare finds youth's education, health, and labor outcomes are negatively affected across a wide range of settings.[3] Child welfare has been a focus of earlier work on Peru's civil conflict. Sánchez (2010) uses the differences in

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[2] Grimard & Laszlo (2010) argue *in utero* exposure to civil conflict negatively affected adult women's height and anemia status. Galdo (2010) finds a one standard deviation increase in civil conflict exposure is associated with a four percent decrease in adult monthly earnings as reported in 2006 and 2007 household surveys. Gallegos (2012) estimates urban women's exposure to civil conflict increased their probability of being employed by 8%. Other Peru-focused research is discussed later in the Introduction.

[3] Studying the 25-year long conflict in Timor Leste, Justino et al. (2011) find conflict negatively affects primary school completion. Singh & Shemyakina (2013) study the 1981-1993 Punjab insurgency and find girls' exposure to violence between the ages of 6-16 decreased their educational attainment. Minoiu & Shemyakina (2012) research the 2002-2007 conflict in Côte d'Ivoire and Akresh et al. (2012) the 1998-2000 Eritrea-Ethiopia war; both find children exposed to violence had lower height-for-age z-scores, a measure of long-term health. Blattman & Annan (2010) investigate the effects of child soldiering in Uganda—where they estimate abducted youth are half as likely to participate in skilled work and earn only a third in comparison to their nonabducted peers.

siblings' conflict exposure to study its effect on infant mortality and short-term nutritional outcomes. He finds the latter is adversely impacted for those born during times of higher regional conflict intensity. León (2012) finds negative long-term effects on human capital accumulation. He estimates the average child affected by the conflict prior to starting school accumulated around 0.21 fewer years of education. The child welfare subsection of the conflict literature has focused on short-term effects and on children's own exposure to violence, however, the impact of conflict is likely persistent and inter-generational. Accordingly, this paper adds to the literature by exploring an outcome affecting the generation of children born to parents who lived through an era of armed violence.

Parenting greatly determines child welfare. The parental behavior this paper focuses on, physical punishment, is a public health concern that deeply shapes the lives of its victims. Its negative effects are well-documented across the medical, psychology, and public health literature. Maltreated children have worse physical and mental health as adults (Springer et al., 2007; Felitti et al., 1998). They also have a higher risk of attempted suicide and drug abuse (Dube et al., 2001). Moreover, physical abuse in childhood is associated with delinquent and violent behavior in adolescence and adulthood (Gilbert et al., 2009). The majority of PCA research focuses on its determinants or post-facto outcomes, but little is known about how child abuse rates change in response to macro-level shocks.[4]

Given the main finding, this paper joins the small subsection of the conflict literature that highlights the positive effects that can occur post-conflict.[5] In addition, the main finding

[4] To my knowledge, only economic shocks have been researched. Using hospital records, Wood et al. (2012) show economic downturns are correlated with greater pediatric admissions for abuse related injuries. Lindo et al. (2013) use California Department of Justice data to study how the Great Recession affected the prevalence of child abuse. They find male layoffs increase abuse while female layoffs have the opposite effect. Similarly, Markowitz & Grossman (2000) find increases in beer taxes reduce maltreatment by females but not by males.

[5] Tilly (1975) argues wars have promoted nation and state formation in Europe. Bellows & Miguel (2009) find an increase in political engagement for individuals that were most affected by the 1991-2002 Sierra Leone civil war. Blattman (2009) finds conflict in Uganda increased political engagement among former combatants. Gil-Alana & Singh (2013) determine longer civil conflicts tend to have faster economic recoveries. Buvinic et al. (2013) argue post-conflict political transitions have increased women's participation in civil and political life.

addresses the research that shows conflict is positively associated with domestic violence. Gutierrez & Gallegos (2011), Noe & Rieckmann (2013), and La Mattina (2013) find women's conflict exposure is linked to an increased probability of IPV-victimhood in Peru, Colombia, and Rwanda, respectively. Previous work appears to contradict my main result; however, this is the first conflict paper on domestic violence to explore violence directed at children rather than adults and perpetrated by women rather than men.[6] In any case, I show our outcomes are not mutually inconsistent by presenting evidence for the normalization of violence, the theory employed by Gutierrez & Gallegos (2011) to explain their results.

The rest of the paper is organized as follows. In the next section, I provide a background on the civil conflict. Section III describes the data. Section IV delineates the conceptual framework and the main empirical strategy. Section V presents the main results and a series of robustness checks, and Section VI tests for the possible channels behind the main results. Section VII focuses on an external validity check using civil conflict in Colombia. Section VIII concludes.

II. History of Peru's Civil Conflict

Peru's civil conflict cost the lives of an estimated 69,280 people (TRC, 2004). The violence traces back to a small town in Peru's Ayacucho region, where ballot boxes were burned during the 1980 presidential elections. The event symbolized the beginning of what the Shining Path (*Sendero Luminoso*), a Maoist rebel group, called the "popular war" against the state (TRC, 2004). Another smaller armed rebel group, the Túpac Amaru Revolutionary Movement (*Movimiento Revolucionario Túpac Amaru*; MRTA), also gained traction in the early 1980s. These groups arose from radical left-wing ideologies that grew in pockets of Peru's southern Andes as a result of the persistent political and economic marginalization of rural communities combined with the instability following nearly a decade of military governments in the 1970s (TRC, 2004).

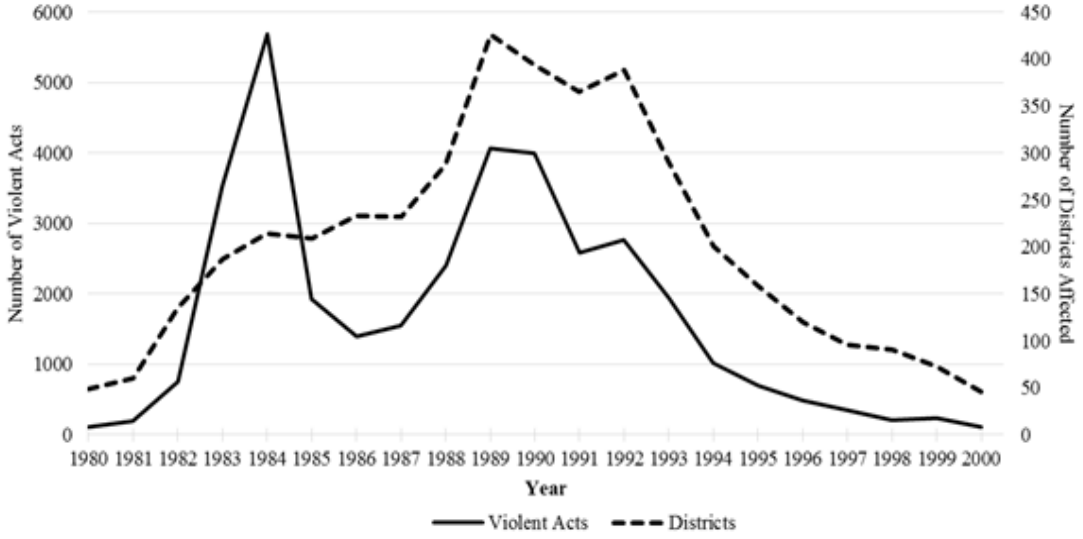
[6] This paper focuses on PCA-use by mothers because the data stem from private interviews with women. Male PCA-use was not recorded as often, even in households where the husband was present. Moreover, the data do not make a clear distinction between husbands, non-spouse intimate partners, and the fathers of the respondents' children (due to these data limitations, this paper assumes the three are synonymous). Nonetheless, a section on general household and fathers' PCA-use is available in the Appendix.

The Shining Path terrorized communities by selectively killing local government officials and police, disrupting elections, and bombing police stations, bank agencies, and town halls (Sánchez, 2010). The Shining Path's strategy, inspired by the Chinese revolution, involved proceeding from rural areas to cities (León, 2012). Affected communities were forced to join or support the Shining Path, although many resisted by creating their own paramilitary forces. The central government underestimated the Shining Path's influence and didn't send the National Army to affected areas until 1983; as the rebel forces retreated, the violence spread to the Amazonian region in the eastern part of the country and across the Andes.

The army was ill-prepared to fight the insurgency. Guerilla tactics overwhelmed soldiers, who were mostly unfamiliar with indigenous languages and the geographic layout of the areas where they fought. This lead to the indiscriminate use of violence against civilians by the army and police, thus further fueling the conflict's expansion (TRC, 2004). Violence intensity escalated in the late 1980s and early 1990s when the Shining Path again spread geographically and advanced to major cities. After Alberto Fujimori's vexed auto-coup in 1992, the fight against the Shining Path took a dramatic turn. He enacted a curfew system and limited civil liberties with new anti-terrorism laws. The conflict's intensity began its steady decline soon after the arrest of Abimael Guzman, the Shining Path's top leader, in September of 1992. Many abandoned guerilla groups after the capture of other high-ranking leaders and the government's subsequent campaign touting the success of its anti-terrorism agenda (Sánchez, 2010).

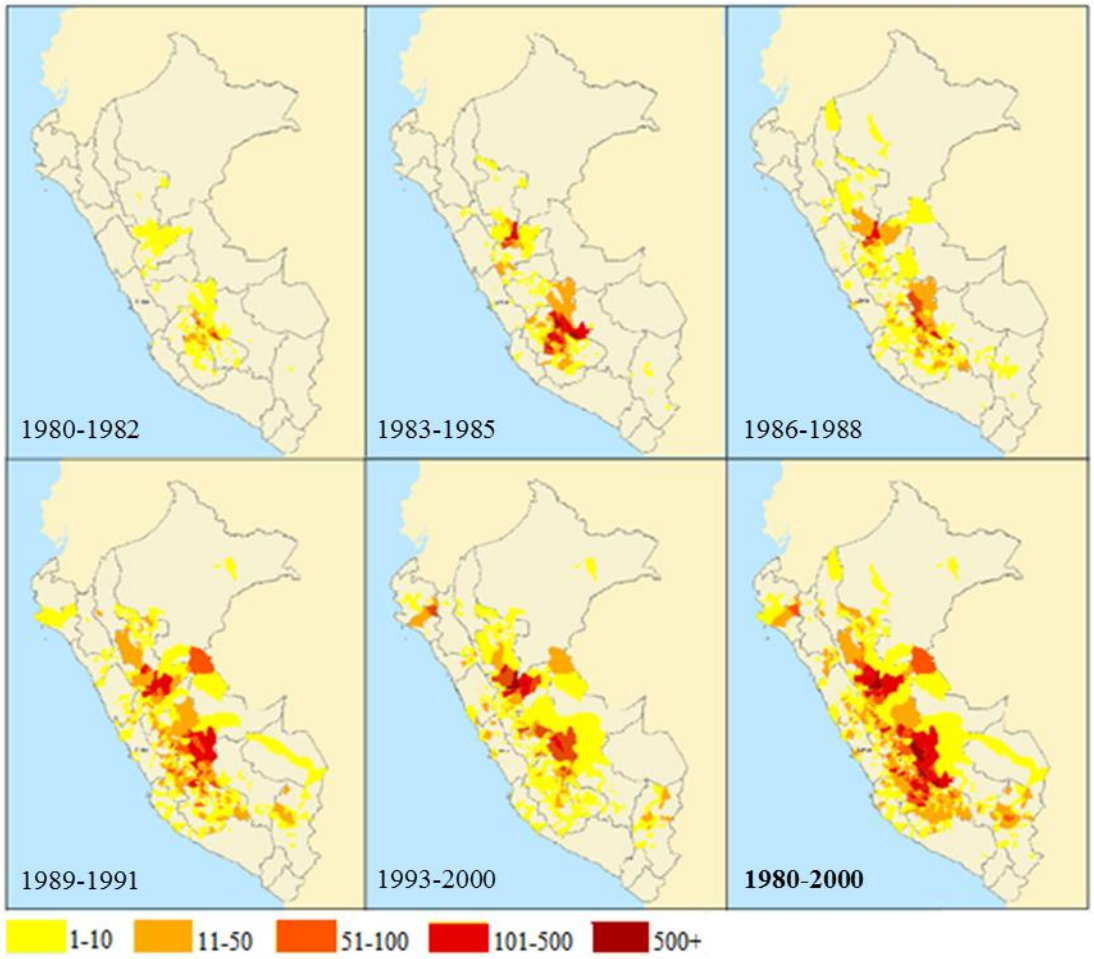
Figures 2 and 3 illustrate the conflict's temporal and spatial progression. Although the fighting concentrated in the Andes Mountains and Amazon jungle, each one of Peru's 25 regions was affected by conflict. The violence peaked in 1984 and again between 1989-1992, which was followed by a steady decline until 2000. This paper's identification strategy uses the conflict's non-monotonic variation over time and space, which is rarely observed in conflict data, to identify the causal impact of conflict exposure on children's physical punishment.

Figure 2: Number of Violent Acts and Number of Districts Affected, by Year



Source: Authors' calculation from TRC Dataset

Figure 3: Spatial Expansion of the Civil Conflict – Number of Deaths, by district



Source: Authors' calculation from TRC Dataset

III. Data

Criticisms of the government's extralegal use of violence against civilians in the fight against the Shining Path increased after President Fujimori fled Peru in 2000.[7] Consequently, the Truth and Reconciliation Commission (TRC) was established in 2001 to assess the impact of the civil conflict and the extent of the previous government's abuses.[8] The TRC gathered data on human rights violations that occurred between 1980 and 2000. Eleven offices were installed in different parts of the country to receive and actively collect testimonies (Sánchez, 2010). Between September 2001 and 2003, well-advertised public hearings took place in 530 districts from 137 provinces to collect testimonies from victims, relatives, and witnesses. The data received were evaluated against six other datasets from non-profit human rights organizations and the government. The resulting dataset contains detailed information (perpetrator's affiliation, victim's characteristics, location, date, etc.) on violent conflict-related events: murders, kidnappings, forced recruitments, forced disappearances, tortures, rapes, and injuries.

I merge the civil conflict dataset with three cross-sections of the Peruvian Demographic and Health Surveys (DHS) from 2000, 2011, and 2012. DHS are nationally representative, randomly sampled, and standardized.[9] They contain data from private in-home interviews with 74,248 women ranging from 15 to 49 years old, and almost 40,000 of those interviews included the full domestic violence and child discipline module. The child discipline module asks respondents how they and their partners (if applicable) discipline their children. The binary variables 'PCA-use' and 'Partner PCA-use' takes a value of one if the respondent mentions "beatings/physical punishment" (as interpreted by the interviewer) as a way she or her partner punish their children.

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[7] For more on the controversial series of events leading to President Fujimori's fall from office, see Perry (2005).

[8] León (2012) gives more context to the TRC: "The [TRC] was a flagship program of the transition government, and it was declared one of its priorities. It was well resourced, with a total budget of about US$19 million over two years of operation, provided by the government and aid agencies. Apart from designating reputable commissioners, the [TRC] also recruited top academics and young professionals for the two years it operated" (999).

[9] The use of a random sample differentiates this paper from most domestic violence research. Yount et al. (2011) highlight family violence research is constrained by the use of data from small clinical samples or purposive samples from nonrandom populations.

Forty percent of respondents and 39% of their partners use PCA, and PCA-use is present in 48% of all two-parent households in the sample. Although these percentages might appear implausible, Latin American cultural norms should be kept in mind when looking at these figures. Moreover, these estimates are consistent with other government and UNICEF statistics from Peru (Ombudsman for Children and Adolescents, 2009).

The measure of PCA-use does not account for the intensity or frequency of abuse, yet it is considered a reliable proxy for the presence of PCA in a household.[10] Testing for the general use of physical punishment minimizes the concern that the most abusive parents are more likely to hide their maltreatment; however, misreporting may still be a concern. The share of women who use PCA decreased from 43% in 2000 to 38% in 2011-2012. This decrease may stem from a combination of evolving norms, a younger generation of parents, and new laws against domestic violence. It is also possible the change in laws decreased the reporting of PCA rather than its actual use. Nonetheless, these concerns can be addressed by analyzing a less incriminating question that may also reflect changing views on the use of abusive child discipline. Respondents were asked whether physical punishment is a necessary part of children's education: 36% agreed in 2000 and only 20% in 2011-2012. This suggests a change in attitudes towards physical child abuse over time.

Civil conflict exposure variables are determined by DHS survey respondents' district of residence and date of birth. The main explanatory variable used throughout the paper, 'Lifetime Conflict Exposure', sums the number of violent civil-conflict-related events that took place in a respondent's district after her birth date. Conflict intensity or exposure is also alternatively measured by the following variables: '0-8', '9-16', and '17+'. These count the number of violent civil conflict events that occurred in the respondent's district according to the age interval in which she was exposed to them. The specific age intervals were chosen for three reasons. First,

[10] Gage & Silvestre (2010) use a similar measure of physical child abuse. Moreover, the child discipline module is widely used around the world to estimate the prevalence of physical punishment (UNICEF, 2010).

Table 3.1: Descriptive Statistics

	Full Sample			Exposed to Civil Conflict (46%)			Never Exposed to Civil Conflict (54%)		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Household Characteristics									
Years of schooling	37342	8.22	4.51	24741	8.45	4.58	12601	7.76	4.32
Partner's years of schooling	35601	9.03	3.82	23579	9.24	3.89	12022	8.61	3.64
Age	37342	34.24	7.82	24741	34.35	7.79	12601	34.02	7.87
Partner's age	31402	38.25	9.06	20627	38.32	9.07	10775	38.13	9.04
Wealth quintile	37342	2.70	1.32	24741	2.79	1.34	12601	2.51	1.25
Number of sons	37342	1.32	1.10	24741	1.31	1.09	12601	1.34	1.11
Number of daughters	37342	1.15	1.03	24741	1.15	1.02	12601	1.16	1.03
Number of household members	37342	5.16	1.99	24741	5.19	2.01	12601	5.10	1.96
Number of children < 5 years	37342	0.84	0.84	24741	0.83	0.83	12601	0.87	0.85
Domestic Violence Exposure									
PCA-use	37342	0.40	0.49	24741	0.41	0.49	12601	0.39	0.49
Partner PCA-use	24683	0.39	0.49	16178	0.40	0.49	8505	0.37	0.48
Exposed to IPV	35845	0.25	0.43	23722	0.26	0.44	12123	0.23	0.42
Childhood history of PCA	37317	0.69	0.46	24722	0.70	0.46	12595	0.68	0.47
Civil Conflict Exposure (in hundreds)									
Lifetime Conflict Exposure	37342	0.27	0.79	24741	0.41	0.94	-	-	-
Between 0-8 years	37342	0.04	0.27	24741	0.07	0.33	-	-	-
Between 9-16 years	37342	0.07	0.35	24741	0.11	0.42	-	-	-
After 17 years	37342	0.15	0.55	24741	0.23	0.66	-	-	-

Sources and Notes: DHS Peru 2000, 2011, and 2012 and TRC (2004). A respondent who lived in a district that was affected by at least one conflict-related event in her lifetime is considered exposed to civil conflict. **'Years of schooling'** and **'Partner's years of schooling'** are continuous variables counting years of education completed by the respondent or her partner. **'Age'** and **'Partner's age'** are continuous variables representing the age in years of the respondent or her partner. **'Wealth quintile'** is a discrete variable based on a wealth index. The wealth index is created by the DHS from survey questions regarding ownership of durable goods (car, refrigerator, TV, etc.), access to electricity, and materials used in home's structure. **'Number of Sons'** and **'Number of daughters'** refer to children who live in the respondent's home. **'Number of household members'** counts the persons living in the respondent's home. **'Number of children <5 years'** counts the children under the age of five years living the in the respondent's home. **'PCA-use'** is a binary variable that identifies whether the respondent uses "beatings/physical punishment" to discipline her children. **'Partner PCA-use'** is a binary variable that indicates whether the respondent's partner uses "beatings/physical punishment" to discipline his children. **'Exposed to IPV'** is a binary variable that takes a value of one if the respondent has ever been "pushed, shaken, or attacked by her partner." **'Childhood history of PCA'** is a binary variable that indicates whether the respondent was physically punished by her parents in her childhood or adolescence. The **"Civil Conflict Exposure"** variables are continuous variables that count the number of civil-conflict-related events (in hundreds) that occurred in the respondent's district of residence during the noted age intervals, as reported in TRC (2004).

they are consistent with the related literature (Gutierrez & Gallegos, 2011). Second, they roughly align with Peru's educational system (schooling is compulsory for children 6-16 years old). Finally, these intervals allow me to test for heterogeneous results.[11]

Table 3.1 presents the descriptive statistics. Forty-six percent of women lived in a district where at least one conflict-related event occurred in their lifetime; they were exposed to an average of seven events between the ages of 0-8, 11 events between the ages of 9-16, and 23 events after turning 17 years old. On average, women exposed to conflict violence in their district have more schooling and wealth than those never affected. They are also more likely to use PCA, to be victims of IPV, and to have histories of childhood abuse.

IV. Conceptual Framework and Empirical Strategy

In relation to prior civil conflict exposure, the probability a parent uses physical punishment can be modelled by the following risk factors: exposure to civil conflict (*C*), domestic violence history (*D*), child-rearing capacity (*E*), and other factors (*Φ*) not affected by civil conflict. The model takes form:

(1) **Pr(PCA)** = $f(C, D(C), E(C), \Phi)$, assuming $\frac{\partial \Pr(\text{PCA})}{\partial D} > 0$ and $\frac{\partial \Pr(\text{PCA})}{\partial E} < 0$

i. <u>Direct impact of civil conflict:</u> Considering the timespan between the end of the conflict and the DHS data collection, the direct impact of exposure to civil conflict on the probability of PCA-use is a long-term effect that is difficult to isolate. On average, the last time a respondent was exposed to a conflict-related event in her district was 1996, which is four, 15, and 16 years prior to the survey for women interviewed in 2000, 2011, and 2012, respectively. Any direct effect conflict has on PCA-use could diminish over these long intervals.

Nonetheless, *C* may still be a relevant channel. Living through an armed conflict could

[11] I hypothesize the effect of childhood exposure to conflict on PCA-use greatly differs from the effect of exposure during adolescence or adulthood. For example, referring to the causal channels established in Figure 1, an individual's education or childhood history of PCA may not be impacted by conflict if it occurs once the individual is older, has completed her education, and is no longer disciplined by her parents. Accordingly, the use of three age intervals for conflict exposure provides a useful placebo check for the results in the Channels section.

permanently alter one's attitudes towards the use of violence. The normalization theory posits aggressive behavior can stem from the desensitization of violence, which is attributed to a history of witnessing it (Fowler et al., 2009). As mentioned in the Introduction, Gutierrez & Gallegos (2011) employ this theory to explain why teenager's exposure to civil conflict increases their likelihood of IPV-victimhood as adults. In contrast, the post-traumatic growth theory considers the positive psychological development that can occur after significant trauma. The theory would suggest that individuals who lived through the conflict may realize the horrors of using aggression as a negotiation tool and subsequently be less likely to punish their children. Post-traumatic growth has been explored in the armed conflict context. Powell et al. (2003) find evidence for psychological growth among former refugees and displaced persons affected by war in former Yugoslavia, and Carmil & Breznitz (1991) argue Israeli Holocaust survivors are more religious and optimistic.[12]

ii. Domestic Violence History: There is a strong relationship between being a victim of physical punishment as a child and using it as a parent (Gage & Silvestre, 2010; Newcomb & Locke 2001; Berlin et al. 2011; Walker, 2009). Witnessing IPV as a child is also correlated to PCA-use as a parent (Cunningham & Baker, 2007). This is known as the inter-generational persistence of domestic violence, and it implies that any shocks to domestic violence may have long-term effects.

Civil conflict could affect domestic violence in the short-run through the abovementioned normalization of violence and post-traumatic growth theories.[13] Other channels may be relevant as well. Domestic violence could rise during conflicts due to increased stress levels, budgetary constraints, and insecurity. Conversely, it could decrease if the conflict affects time allocated for child-rearing (see Lindo et al., 2013). Parents might spend more time with their children due to

[12] See also Maguen et al. (2007); Pietrzak et al. (2010); Feder et al. (2008).
[13] In exploring conflict's effect on individuals' domestic violence histories, I focus on their exposure to physical child abuse as children because no data are available on whether respondents witnessed IPV while growing up. Nonetheless, conflict's short-term effect on IPV likely runs through similar channels as the effect on PCA.

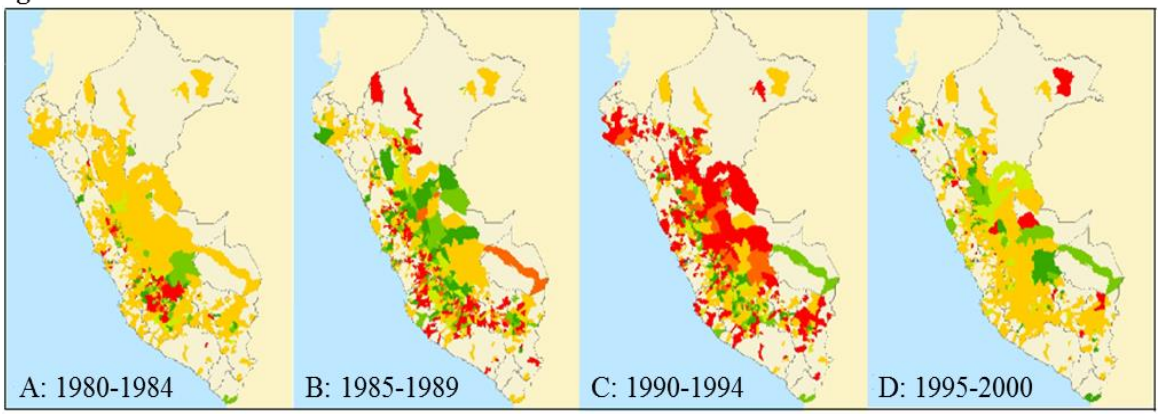
conflict's impact on the labor market, curfew laws, and violence and instability on the streets.

iii. Child-rearing Capacity: Research shows physical punishment is not an effective means of discipline (Gershoff, 2002). As a result, I assume PCA-use is a sub-optimal behavior that occurs when there is a lack of child-rearing capacity, meaning there is a shortage of resources allocated to proper child discipline. Through its effects on income, socio-economic status, and child-rearing knowledge, education increases the amount of resources available for proper child discipline. Community-level factors that decrease the amount of resources parents have to spend on their children's healthcare, schooling, or nutrition can also increase child-rearing capacity. In colloquial terms, community-level resources lessen the burdens associated with parenting.

The long-term effect of conflict on child-rearing capacity can go in either direction. In terms of education, fearful or poverty-stricken parents may delay or interrupt their children's schooling. Parents' reallocation of resources towards boys and away from girls could also affect education outcomes (Singh & Shemyakina, 2012). There may be supply side shocks as well; Léon (2012) finds attacks on teachers partly drive his observed decrease in schooling in the aftermath of Peru's civil conflict. Nevertheless, education could actually increase if conflict's negative effect on available wages lowers the opportunity cost of schooling (Arcand & Wouabe, 2009). Conflict can also have a long-term effect on the level of community resources available to parents. On the supply side, post-conflict reconstruction policies could influence the level of social and health services in communities affected by conflict (Grimard & Laszlo, 2013). On the demand side, through conflict's effects on income and health, parents may need to access resources such as hospitals and social programs more frequently. On the other hand, the destruction of capital associated with armed violence can diminish a community's stock of social services.

iv. Empirical Strategy: I estimate the net effect of past exposure to civil conflict on the probability of PCA-use years after the violence has ceased. Figure 3 suggests individuals born in the same year will have varied conflict exposures depending on their district of birth. In addition,

Figure 4: Within district Variation of Peru's Civil Conflict



Maps show the absolute percentage difference between a district's yearly average of conflict-related events from 1980-2000 and the yearly average from 1980-1984 in grid A, 1985-1989 in grid B, 1990-1994 in grid C, and 1995-2000 in grid D.

25% 50% 75% 100% 150% 150+%

Source: Authors' calculation from TRC Dataset

Figure 4 highlights that even individuals born in the same district but in different years will be affected by different levels of conflict intensity, and this within district variation is observed throughout the country. Consequently, I use a fixed effects (FEs) model, which mimics a Difference-in-Difference-in-Difference (D-I-D-I-D) estimation, to isolate the impact of conflict that cannot be predicted by fixed factors within a district and time-varying factors across the country.[14] In addition to fixed effects, I further control for omitted variable bias with crucial covariates like wealth, education, household characteristics, and childhood history of PCA. The following specification compares an individual's PCA-use against the average of those born in the same district and those born in the same year by using variation in conflict exposure stemming from the changing intensity of conflict violence in a specific location and the timing of respondents' birth (the latter assumed to be exogenous). From Equation (1), I estimate the following reduced-form model:

$$(2)\quad \mathbf{Y}_{icsjrt} = \beta_0 + \beta\,(\mathit{Violence\ Exposure}_{cj}) + \gamma\, X_i + \eta_j + \nu_c + \omega_t + \delta_r(s) + \varepsilon_{icsjrt}$$

where i indexes survey respondents, c represents cohorts based on birth years, s indexes five-year birth cohorts, j denotes districts in regions r, and t marks survey year. Y_{icsjrt} is a binary variable

[14] The model mimics a D-I-D-I-D estimation in that it controls for differences across space with district fixed effects and controls for differences across time with birth year cohort and survey fixed effects. Moreover, the common trend assumption is partly relaxed with the inclusion of regional time trends.

identifying whether the survey respondent uses physical punishment to discipline her children. The 'Violence Exposure' variable takes one of two forms, as described at the end of Section III. X_i is a vector of individual and household-level controls. η represents district FEs, which control for the average differences across districts in observed or unobserved predictors. Similarly, ν denotes birth year cohort FEs and controls for the variation purely due to year of birth. For example, individuals born in earlier decades might be more likely to use physical punishment. Birth year cohort FEs also control for the average country level changes in correlates of conflict (such as nationwide changes in government services, military capacities, and political and economic institutions). ω allow for survey year dummies, which help account for the differences in the reported use of PCA across the three DHS surveys. The model also includes flexible region-specific trends for each five-year birth cohort, as denoted by $\delta_r(s)$. These trends account for any differences across five-year birth cohorts in each region; they capture trending effects such as the difference in each region's development over time.

The identification strategy assumes there are no preexisting trends affecting PCA-use in districts that experienced civil conflict violence. I use data on whether respondents were themselves disciplined with PCA to check the validity of this assumption, albeit in an imperfect manner. Table 4.1 reveals PCA-use over time might have actually decreased by more in districts that were not exposed to civil conflict in comparison to districts that were. Consider Column (1). Accounting for district fixed effects, respondents born in the 1960s, 1970s, 1980s, and 1990s in districts that never experienced violence were respectively 3.5, 3.6, 10.1, and 27.5 percentage points less likely to have a childhood history of PCA in comparison to those born in the 1950s. Meanwhile, the changes were either insignificant or not as large for those born in districts that experienced at least one violent conflict event. The conclusions are similar if I compare the changes in PCA-use over time for districts that experienced less than the median level of violence against districts that experienced the median level of violence or above.

Table 4.1: Previous Trends in PCA-use, by Presence of Civil Conflict Violence in District

	Childhood History of PCA			
	(1)	(2)	(1)	(2)
District Sample Restrictions:	Never exposed to violence	Exposed to at least one violent event	Less than median level of violence	Median level of violence or above
Birth Decade (1950s omitted)				
1960s	-0.035**	0.009	-0.012	0.006
	(-2.45)	-0.93	(-1.04)	-0.43
1970s	-0.036***	-0.006	-0.013	-0.019
	(-2.59)	(-0.63)	(-1.20)	(-1.57)
1980s	-0.101***	-0.061***	-0.078***	-0.068***
	(-6.64)	(-5.34)	(-6.67)	(-4.39)
1990s	-0.275***	-0.204***	-0.242***	-0.204***
	(-14.60)	(-16.12)	(-16.73)	(-12.49)
Fixed Effects:	**X**	**X**	**X**	**X**
Observations	32601	29156	32601	29156
R-squared	0.090	0.080	0.090	0.080

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters). Column (1) restricts t the sample to individuals living in districts that were not affected by civil conflict violence, Column (2) to individuals living in districts that experienced at least one conflict-related event, Column (3) to individuals living in districts that experienced less than nine conflict-related events (the median for districts that were exposed to conflict violence), and Column (4) to individuals living in districts that experienced nine or more conflict-related events. Fixed effects include district (1082) and survey year (3) dummy variables. The dependent variable takes a value of one if the survey respondent was physically abused by her parents while growing up.

The marked difference across districts within a region is the pivotal reason behind using district FEs. Districts are the smallest geographic subdivisions in Peru and regions the largest. In a country as geographically and culturally diverse as Peru, it is difficult to compare persons living within the same region but in different districts. Estimations without district FEs are biased because there are likely district-level omitted variables that are correlated with civil conflict intensity and PCA. As La Mattina (2013) notes for Rwanda's genocide, weak social networks or poor law and policing structures at the local level could be related to both domestic violence and civil conflict. Consequently, district-level FEs (1083 units) are preferred to region or province-level FEs (25 and 183 units, respectively) although some variance in the explanatory variable is compromised in exchange for improved control against bias. Since the large set of fixed effects eliminates considerable variation in the main explanatory variable, the estimates presented are conservative. Additionally, the unobservables in the error term could be serially correlated for individuals within the same district both within and across survey periods. To allow for this serial correlation, the standard errors in all regressions are clustered at the district level.[15]

[15] The main results are robust to clustering at "lower" or "higher" levels (survey cluster and province or region level, respectively).

Table 5.1: Effect of Civil Conflict Exposure On Use of Physical Child Abuse

	Linear Probability Model			Probit (marginal effects)		
	PCA-use			**PCA-use**		
	(1)	(2)	(3)	(4)	(5)	(6)
Lifetime Conflict Exposure	-0.033***	-0.026**	-0.034***	-0.039***	-0.032***	-0.038***
(in hundreds)	(-2.71)	(-2.55)	(-2.96)	(-2.93)	(-2.70)	(-2.75)
Wealth Quintile (lowest omitted)						
2nd		0.005	0.002		0.020	0.011
		(0.52)	(0.19)		(0.73)	(0.39)
3rd		-0.009	-0.014		-0.018	-0.031
		(-0.76)	(-1.14)		(-0.51)	(-0.87)
4th		-0.060***	-0.064***		-0.165***	-0.179***
		(-4.30)	(-4.57)		(-4.08)	(-4.39)
5th		-0.073***	-0.077***		-0.217***	-0.232***
		(-4.58)	(-4.82)		(-4.62)	(-4.88)
Number of Sons		0.057***	0.048***		0.168***	0.143***
		(15.44)	(12.97)		(15.65)	(12.98)
Number of Daughters		0.047***	0.036***		0.139***	0.108***
		(12.59)	(9.48)		(12.66)	(9.44)
Childhood History of PCA		0.173***	0.173***		0.530***	0.538***
		(26.92)	(26.67)		(26.85)	(26.64)
Number of Household Members		-0.003	0.000		-0.010*	-0.002
		(-1.60)	(-0.09)		(-1.76)	(-0.26)
Number of Children < 5 Years		0.001	0.005		0.006	0.016
		(0.34)	(1.11)		(0.44)	(1.29)
Years of Schooling		-0.002*	-0.003***		-0.006*	-0.009***
		(-1.81)	(-2.74)		(-1.85)	(-2.76)
Partner's Years of Schooling		-0.003***	-0.003***		-0.009***	-0.010***
		(-3.25)	(-3.28)		(-3.22)	(-3.30)
Partner's Age		0.000	0.000		0.000	0.000
		(-0.10)	(0.12)		(-0.01)	(0.23)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	37342	31250	31250	37018	30919	30884
R-squared (pseudo for probit)	0.120	0.176	0.186	0.088	0.134	0.143

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Fixed effects include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).

V. Results

A. Main Results

Table 5.1, Columns (1) – (3) display the results for the main specification using a linear probability model (LPM) and Columns (4) – (6) show the marginal effects at the mean from a probit model. The fixed effects influence how the results should be interpreted. All results presented denote the marginal effect of civil conflict violence exposure with respect to district, birth year, and survey year averages (first two levels of controls) and also with respect to regional

trends (third level of controls).[16]

The 'Lifetime Conflict Exposure' marginal effects are consistent across the two models in Table 5.1. At the most strict level of controls, the linear probability model can be interpreted as follows: exposure to an additional hundred conflict-related events decreases the probability that a mother will use PCA by 3.4 percentage points, with respect to birth year cohort, district, and survey year averages as well as regional time trends. In Column (6), the marginal effects at the mean predict a 3.8 percentage point decrease. These estimates imply a one standard deviation increase in lifetime exposure to violence would decrease the probability of using PCA by anywhere from 2.7-3.0 percentage points. Both estimates are significant at the 1% level.

Other coefficients contextualize the size of civil conflict's effect on PCA. The biggest predictor of PCA-use is whether the respondent was physically abused in childhood; all else equals, women who were abused as children were 17.3 percentage points more likely to use PCA. This effect highlights the importance of the inter-generational continuity of domestic violence. The effect of wealth is notable as well; women in the highest wealth quintile are 7.7 percentage points less likely to use PCA relative to those in the lowest quintile. Assuming a strictly linear relationship between years of education and PCA-use, the 'Lifetime Conflict Exposure' coefficient is more than 10 times the size of the effect of an additional year of schooling for women or their husbands. The rest of the covariates affect PCA in expected ways.[17]

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[16] Léon (2012) highlights the fundamental difference in showing the effects at the district level instead of the national level. Consider Table 5.1. At the first level of controls, the 'Lifetime Conflict Exposure' coefficients are positive and significant (1% level) when omitting all spatial FEs, negative and insignificant when using region FEs, and negative and significant (1% level) when using province FEs (results not shown). The closer the FEs "zoom in," the effect of conflict on PCA-use becomes more negative.

[17] The covariates affect PCA in the direction the child maltreatment literature would predict; this reinforces the dependent variable's validity as an indicator of physical abuse. Partner's age, number of household members, and number of children under the age of five are not significantly associated with PCA-use (although they might be highly correlated with other significant predictors). The effect of respondents' age (not included as a control in the regressions) on the use of PCA is unclear because 99.7% of its variation is absorbed by the birth and survey year FEs (which control for age indirectly).

Table 5.2: Effect of Civil Conflict Exposure On Use of Physical Child Abuse, by Age Intervals

	Linear Probability Model			Probit (marginal effects)		
	PCA-use			**PCA-use**		
Violence Exposure, by age: (in hundreds)	(1)	(2)	(3)	(4)	(5)	(6)
0 - 8	-0.035	-0.033*	-0.026	-0.041	-0.038*	-0.028
	(-1.47)	(-1.65)	(-1.32)	(-1.63)	(-1.72)	(-1.24)
9 - 16	-0.037***	-0.027**	-0.034***	-0.043***	-0.032**	-0.037**
	(-3.26)	(-2.43)	(-2.77)	(-3.53)	(-2.55)	(-2.57)
17+	-0.031*	-0.028**	-0.037**	-0.037**	-0.033**	-0.039**
	(-1.94)	(-2.19)	(-2.38)	(-2.13)	(-2.23)	(-2.22)
Wealth Quintile						
2nd		0.005	0.002		0.008	0.004
		(0.52)	(0.19)		(0.73)	(0.39)
3rd		-0.010	-0.014		-0.007	-0.012
		(-0.77)	(-1.13)		(-0.51)	(-0.88)
4th		-0.060***	-0.064***		-0.063***	-0.068***
		(-4.30)	(-4.57)		(-4.16)	(-4.49)
5th		-0.073***	-0.077***		-0.082***	-0.087***
		(-4.58)	(-4.82)		(-4.78)	(-5.06)
Number of Sons		0.057***	0.048***		0.065***	0.055***
		(15.42)	(12.98)		(15.65)	(12.98)
Number of Daughters		0.047***	0.036***		0.054***	0.042***
		(12.58)	(9.49)		(12.66)	(9.44)
Childhood History of PCA		0.173***	0.173***		0.197***	0.199***
		(26.92)	(26.67)		(28.63)	(28.45)
Number of Household Members		-0.003	0.000		-0.004*	-0.001
		(-1.60)	(-0.08)		(-1.76)	(-0.26)
Number of Children < 5 Years		0.001	0.005		0.002	0.006
		(0.34)	(1.12)		(0.44)	(1.29)
Years of Schooling		-0.002*	-0.003***		-0.002*	-0.003***
		(-1.80)	(-2.74)		(-1.85)	(-2.76)
Partner's Years of Schooling		-0.003***	-0.003***		-0.004***	-0.004***
		(-3.25)	(-3.27)		(-3.22)	(-3.30)
Partner's Age		0.000	0.000		0.000	0.000
		(-0.10)	(0.13)		(-0.01)	(0.23)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	37342	31250	31250	37018	30919	30884
R-squared (pseudo for probit)	0.120	0.176	0.186	0.088	0.134	0.143

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).

Table 5.2 shows how additional exposure to violence during different life stages affects the later use of physical punishment.[18] The marginal effects of exposure to civil conflict between the ages of 0 and 8 years are negative but statistically indifferent from zero. In Column (3) in Table 5.2, the coefficients for the '9-16' and '17+' exposure variables are -0.034 and -0.037,

[18] The results in Table 5.2 are consistent with Table 5.A.2 (available in Online Tables Appendix), which uses alternate age intervals (0-4, 5-9, 10-14, 15-19, 20+) to measure exposure to civil conflict. Exposure to conflict after childhood continues to drive the decrease in the probability of PCA-use as a parent.

respectively. Although similar in magnitude, '9-16' is more significant than '17+'. Similar to Table 5.1, the marginal effects for the three violence exposure variables are consistent across LPM and probit specifications.[19]

Table 5.A.1 (available in the Online Tables Appendix) attempts to estimate the effects at varying levels of conflict exposure. In contrast to the previous two tables, these results focus on estimating average marginal effects (AMEs).[20] The coefficients for the AMEs at varying levels are consistent with both the LPM and probit outcomes.[21] The rest of this paper will apply the linear probability model to the main specification for the following two reasons. First, as shown by Table 5.A.1, there is no empirical evidence for non-linear effects of civil conflict exposure on PCA-use. Second, LPM and probit regressions produce comparable results.[22]

B. Robustness Check: Migration

Failing to account for migration could bias the estimates. Given data limitations, it is impossible to determine the districts where respondents were born or where they lived during their childhood or teenage years. Consequently, individuals who ever lived in another district are assigned incorrect civil conflict exposure values. I compare the descriptive statistics for migrants and non-migrants using a DHS data variable noting the number of years a respondent has lived

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[19] Gutierrez & Gallegos (2011) note using separate age intervals assumes the conflict exposure variables are additive separable. For example, the results from Table 5.2 assume the exposure to civil conflict during a given age interval is independent from the exposure during another interval. Table 5.A.3 in the Online Tables Appendix shows only the coefficient for '17+' remains negative and significant when the additive separable assumption is relaxed by interacting the three violence exposure variables with each other. Table 5.A.3, Column (3) suggests an additional 100 events of civil conflict exposure after the age of 17 years is associated with a 3.1 percentage point decrease in the probability of PCA-use, conditional to having never experienced violence prior to turning 17.

[20] The marginal effects at the mean in Tables 5.1 and 5.2 represent the marginal effects for the average observation (the average observation is assigned the mean values of the covariates in the regression). The average marginal effects in Table 5.A.1, calculated using Stata's *margins* command, are useful in this scenario since the "average observation" is difficult to conceptualize, especially given the large set of fixed effects. The AME is calculated by averaging the marginal effect estimated independently for each individual.

[21] The estimations' similarity across varying margins is an unexpected result, and it is not clear why the response to low conflict intensity should be the same as high intensity. Consequently, Table 5.A.1 should be interpreted with caution because it may expose the shortcomings of the *margins* command in Stata. Consider the commands' AMEs estimations for 'Years of schooling' and 'Partner's years of schooling'. They also do not change across varying margins—which intuitively appears highly unlikely.

[22] Probit results shown are robust to using logit. Logit follows standard logistic distribution, while probit follows the standard normal distribution. Logit's distribution has a lower peak and fatter tails relative to probit models.

Table 5.3: Effect of Civil Conflict Exposure On Use of Physical Child Abuse
Robustness Check: Migration [a]

	Panel A:			Panel B:		
	PCA-use			**PCA-use**		
	(1)	(2)	(3)	(1)	(2)	(3)
Lifetime Conflict Exposure	-0.006	0.002	-0.030	-0.032***	-0.027***	-0.035***
(in hundreds)	(-0.33)	(0.11)	(-1.18)	(-2.63)	(-2.64)	(-3.01)
Migrant				0.027***	0.013**	0.012**
				(4.96)	(2.15)	(2.03)
Wealth Quintile						
2nd		-0.008	-0.009		0.002	-0.001
		(-0.52)	(-0.63)		(0.26)	(-0.12)
3rd		-0.028	-0.030*		-0.013	-0.018
		(-1.59)	(-1.68)		(-1.04)	(-1.47)
4th		-0.077***	-0.081***		-0.063***	-0.068***
		(-3.54)	(-3.69)		(-4.53)	(-4.89)
5th		-0.100***	-0.102***		-0.077***	-0.083***
		(-4.23)	(-4.32)		(-4.84)	(-5.18)
Number of Sons		0.050***	0.042***		0.057***	0.049***
		(9.93)	(8.06)		(15.78)	(13.41)
Number of Daughters		0.045***	0.036***		0.047***	0.037***
		(8.98)	(6.92)		(12.70)	(9.70)
Childhood History of PCA		0.173***	0.173***		0.172***	0.173***
		(20.55)	(20.17)		(26.82)	(26.58)
Number of Household Members		-0.001	0.002		-0.003	0.000
		(-0.37)	(0.66)		(-1.55)	(-0.06)
Number of Children < 5 Years		0.001	0.003		0.001	0.005
		(0.09)	(0.51)		(0.32)	(1.07)
Years of Schooling		-0.002	-0.003*		-0.002	-0.002*
		(-1.08)	(-1.69)		(-1.09)	(-1.68)
Partner's Years of Schooling		-0.002*	-0.002*		-0.002*	-0.002*
		(-1.71)	(-1.68)		(-1.70)	(-1.67)
Partner's Age		0.000	0.000		0.000	0.000
		(-0.32)	(-0.16)		(-0.31)	(-0.12)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	19498	15832	15832	37342	31350	31350
R-squared	0.159	0.220	0.232	0.121	0.176	0.186

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).
[a] Panel A restricts the sample to non-migrants. Panel B includes a dummy variable indicating whether the respondent is a migrant.
A migrant is defined as a respondent who has always lived in her current home or lived there prior to the start of the conflict.

in her current home. On average, migrant women have higher rates of childhood history of PCA and PCA-use.[23] Moreover, migration and civil conflict exposure are positively correlated; an estimated 435 communities were abandoned as a result of Peru's civil conflict (Gallegos, 2012). Consequently, if the bias resulting from the inclusion of migrants in the analysis is accounted for, then the estimates on Tables 5.1 and 5.2 will be even more negative.

Table 5.3, Panel A restricts the analyses from Table 5.1 to include only non-migrants.

[23] A respondent who has always lived in her current home or lived there prior to the start of the civil conflict in 1980 is considered a non-migrant. Descriptive statistics for migrants and non-migrants are available in Table 5.B.1 (available in the Online Tables Appendix). Notably, migrant women are less educated and have lower wealth index scores on average.

Column (3) reveals the magnitude of the coefficient for 'Lifetime Conflict Exposure' is consistent with the main findings, yet it is insignificant. Since the magnitude of the coefficients does not change drastically, the loss of significance likely stems from the near fifty percent decrease in sample size. All in all, given the near-zero coefficients for the main explanatory variable in Columns (1) and (2), the main result is not robust to the exclusion of migrants. Panel B takes another approach to account for the bias from migrants. It includes a migration dummy in the analysis. As predicted above, the effect of conflict on PCA-use is greater in magnitude when migration is accounted for (although the increase is negligible) and the results remain highly significant.

It is worth highlighting that the distinction between migrants and non-migrants is problematic. First, the classification of migrants is severely flawed; due to data limitations, respondents who ever moved to different homes yet stayed within their birth district are erroneously considered migrants. Second, migrants who were displaced by the conflict were likely the ones most affected by it, so overlooking their outcomes distorts the true relationship between conflict exposure and physical child abuse.

C. Robustness Check: Propensity Scores

Propensity scores are often used in the medical literature to assess the effectiveness of a treatment. The underlying idea of propensity scores is that outcomes of similar individuals should be compared, with the important difference being one received the treatment and the other didn't. This subsection uses propensity scores to complement the main fixed effects analysis. Unlike a randomized experiment, the treatment (exposure to civil conflict) analyzed in this paper is not random. The best an observational study can hope for is a plausibly exogenous treatment. As motivated in Section IV, exposure to conflict is plausibly exogenous since it is determined by a respondent's district and date of birth. This subsection strives to justify this assumption of exogeneity and improve the composition of the sample used in the main analysis.

Table 5.4.1: District Probability of Experiencing Median Level of Violence
Robustness Check: District Propensity Scores

	Probit Model **Treatment: Median Level of Violence or Above**			
	(1)	(2)	(3)	(4)
GDP growth from 1975 to 1980	-0.347**	-0.127	-0.343**	0.251
(% change)	(-2.02)	(-0.20)	(-2.02)	(0.39)
GDP per capita in 1980 (in thousands)	-0.007***	-0.005**	-0.006***	-0.002
	(-2.97)	(-2.01)	(-2.77)	(-0.96)
Altitude Quintile (highest altitude omitted)				
1st	-0.480***	-0.121	-0.776***	-0.459*
	(-2.71)	(-0.50)	(-3.89)	(-1.87)
2nd	-0.130	0.170	-0.233	-0.018
	(-0.75)	(0.86)	(-1.30)	(-0.09)
3rd	-0.170	-0.221	-0.195	-0.246
	(-0.90)	(-1.20)	(-1.02)	(-1.30)
4th	0.121	-0.048	0.127	-0.049
	(0.75)	(-0.32)	(0.77)	(-0.33)
Proxy for pre-treatment variables:[a]				
Population per district			-0.001	-0.002
(in thousands)			(-0.42)	(-0.59)
Number of Schools			0.004	0.008***
			(1.48)	(2.81)
Number of Health Posts			0.013	0.009
			(0.62)	(0.41)
Region Fixed Effects:		**X**		**X**
Observations	822	819	822	819
Pseudo R-squared	0.062	0.169	0.062	0.169

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the province level (202 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Dependent variable takes a value of one if district was exposed to at least nine conflict-related events (the median for districts that were exposed to conflict violence). 'GDP growth from 1975 to 1980' and 'GDP per capita in 1980' are estimated using region-level GDP figures and district-level population estimates from a 1993 census. The propensity scores used in Table 5.4.2 are estimated from the probit model in Column (4).
[a] 'Population per district,' 'Number of schools,' and 'Number of Health Posts' come from a 1993 census, however they should approximately proxy for pre-treatment values.

Figure 3 shows certain areas near Peru's borders were not affected by violence and the most affected districts were located in the Andes region east of Lima (Peru's capital). There exists large economic and cultural differences between Peru's coastal and Andean regions, and these differences might be correlated both with the pervasiveness of PCA-use and a region's civil conflict history. The use of district fixed effects alleviates this concern. Nevertheless, propensity scores can further improve the analysis by restricting the sample to more comparable individuals and thus reduce the amount of extrapolation involved in the LPM (Stuart, 2010).

Propensity scores estimate the probability that any given district will be exposed to a certain level of violence (the treatment), conditional on a set of covariates. Unlike the main analysis, here I use a dataset of districts rather than individuals. I define the treatment group to include

districts exposed to at least nine conflict-related events from 1980-2000 (the median for districts affected by violence); the remaining districts are part of the control group. I use districts' characteristics to estimate the conditional probability (propensity score) of an individual district receiving treatment. Table 5.4.1 shows the probit model used to obtain the propensity scores.[24]

Surprisingly, economic measures such as GDP growth from 1975 to 1980 and GDP per capita in 1980 are not significant once region fixed effects are introduced in Column (4). Altitude quintile dummies are also not significant with the exception of the lowest quintile. The number of schools in a district is a highly significant predictor of the treatment, which may be explained by the Shining Path's connection to teachers unions (León, 2012). The above results support the exogeneity assumption since they suggest poorer districts within regions were not necessarily more likely to be affected by the median level of violence.

I use the district-level estimated probabilities of treatment in two ways. First, I use them to restrict the main specification to individuals living in districts that are within the "common support" of propensity scores. This implies the regressions include only observations whose propensity scores belong to the overlap of the propensity scores for the treatment and control districts.[25] These results are presented in Table 5.4.2, Panel A. Although the number of observations decreases by over 25%, 'Lifetime Conflict Exposure' remains significant at the 1% level and is larger than previous estimates. Second, I use the propensity scores to replicate Table 5.1 with a weighted least squares (WLS) specification, which downweights individuals who live

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[24] I assume the treatment meets the conditional independence assumption: after controlling for the covariates included in the probit model, the treatment is independent of the outcomes. Stuart (2010) explains the assumption is not as strict as it might sound, "controlling for the observed covariates also matches on or controls for the unobserved covariates, in so much as they are correlated with those that are observed" (3). I also assume the conditional probabilities of receiving the treatment given the covariates is strictly between zero and one.

The covariates included in the model should be measured before 1980 so that they are unaffected by the treatment. However, in Table 5.4.1, Columns (3) – (4), I include the variables 'Population per district,' 'Number of schools,' and 'Number of Health Posts' from a 1993 census. Since district data are unavailable before 1980, these variables are used to proxy for pre-treatment values. Léon (2012) explains schools were not targeted by the Shining Path, therefore it is unlikely 'Number of schools' is an outcome variable.

[25] The goal behind the common support restriction is to include in the LPM only control districts that are similar enough based on propensity scores to be matched to treated districts, and vice versa (Bryson et al., 2002).

$$\text{Weights} = \sqrt{\frac{M(V)}{p(V_i)} + \frac{1 - M(V)}{1 - p(c_i)}}$$

Table 5.4.2: Effect of Civil Conflict Exposure On Use of Physical Child Abuse
Robustness Check: Model Restricted to Common Support and Weighted with District-Level Propensity Scores

	Panel A: Restricting Sample to Common Support			**Panel B:** Weighted Least Squares[a]		
	PCA-use			**PCA-use**		
	(1)	(2)	(3)	(1)	(2)	(3)
Lifetime Conflict Exposure (in hundreds)	-0.037***	-0.032***	-0.044***	-0.033***	-0.028**	-0.038***
	(-2.84)	(-2.64)	(-3.14)	(-2.60)	(-2.27)	(-2.62)
Wealth Quintile						
2nd		-0.005	-0.007		-0.001	-0.005
		(-0.44)	(-0.71)		(-0.05)	(-0.37)
3rd		-0.016	-0.021		-0.022	-0.029
		(-1.09)	(-1.41)		(-1.17)	(-1.57)
4th		-0.071***	-0.074***		-0.079***	-0.084***
		(-4.13)	(-4.31)		(-3.87)	(-4.01)
5th		-0.075***	-0.078***		-0.080***	-0.087***
		(-3.68)	(-3.80)		(-3.28)	(-3.48)
Number of Sons		0.056***	0.047***		0.059***	0.049***
		(13.50)	(11.47)		(11.62)	(9.95)
Number of Daughters		0.047***	0.036***		0.050***	0.039***
		(11.06)	(8.53)		(9.66)	(7.50)
Childhood History of PCA		0.182***	0.182***		0.182***	0.181***
		(23.97)	(23.64)		(19.95)	(19.67)
Number of Household Members		-0.003	0.000		-0.004	-0.001
		(-1.31)	(-0.03)		(-1.29)	(-0.26)
Number of Children < 5 Years		-0.003	0.000		0.000	0.004
		(-0.51)	(0.02)		(0.05)	(0.57)
Years of Schooling		-0.003**	-0.003***		-0.002	-0.003**
		(-2.10)	(-2.92)		(-1.45)	(-2.23)
Partner's Years of Schooling		-0.003**	-0.003**		-0.002	-0.002
		(-2.43)	(-2.50)		(-1.41)	(-1.46)
Partner's Age		0.000	0.000		0.000	0.000
		(0.15)	(0.34)		(-0.08)	(0.02)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	26761	22527	22527	18901	15791	15791
R-squared	0.134	0.192	0.202	0.126	0.185	0.199

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).
Sample includes only observations whose propensity scores belong to the overlap of the propensity scores for treatment and control districts.
[a] The weights are constructed according to the following formula: $Weights = \sqrt{\frac{MVi}{p(Xi)} + \frac{1 - MVi}{1 - p(Xi)}}$
where MVi identifies whether observation i lives in a district that experienced at least nine civil-conflict-related events and $p(Xi)$ is the propensity score (Inbens, 2004).

in districts that were very likely to receive treatment and upweights those who live in districts that were unlikely to receive treatment based on the covariates used in the probit model (Bjerk, 2009).[26] Using propensity score weights in the regression makes the treated and untreated groups more comparable in terms of the distribution of covariates that are correlated with being treated

[26] The formula used to create weights is shown in the Notes of Table 5.4.2. Weighting by propensity scores creates a pseudo-population where there is no confounding (Cole & Hernán, 2008). The combination of weighting and regression leads to a model that is "doubly robust"—meaning the estimator is consistent if either the propensity scores probit model or the least squares model is correctly specified (Inbens, 2004).

(ibid.). Table 5.4.2, Panel B shows the results for the weighted model. These results are also consistent with the main finding, but they should be interpreted with caution. The propensity scores estimate the probability of a binary treatment for districts, yet the LPM uses a continuous conflict exposure variable as the treatment for individuals. Therefore, it is difficult to determine whether the weighted regressions accomplish the goal of analyzing more comparable women.

VI. Channels

A. Acceptance or Rejection of Domestic Violence: Normalization vs. Post-traumatic Growth

In this subsection, I test whether Peru's civil conflict had long-lasting effects on the acceptance or rejection of the use of violence in the home. As discussed in Section IV, post-traumatic psychological growth could explain the main result. Conflict exposure could reveal the perils of using violence as a negotiation method, therefore decreasing affected women's use of physical punishment. Table 6.1 contests this hypothesis by presenting the relationship between civil conflict and attitudes towards domestic violence. Despite this paper's main result, Column (3) in Table 6.1 suggests women affected by higher conflict intensity are more likely to agree physical punishment is a necessary part of children's education. More evidence for the normalization of violence is observed by employing tests similar to ones used by Gutierrez & Gallegos (2011): Columns (4) – (6) show mothers exposed to higher levels of civil conflict might be more likely to accept justifications for intimate partner violence (IPV), although this relationship is not significant at the strictest level of controls. Additionally, out of the women abused by their partners, those exposed to higher conflict intensity were less likely to report the abuse or seek help. Column (9) suggests women exposed to an additional hundred conflict-related events are three percentage points less likely to report IPV; this result is significant at the 5% level.

Table 6.1: Effect of Civil Conflict on Domestic Violence Attitudes

	Punishment Necessary[a]			IPV Justification[b]			Reported IPV[c]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lifetime Conflict Exposure	0.013	0.023***	0.033***	0.009***	0.009**	0.008	-0.013	-0.030**	-0.031**
(in hundreds)	(1.42)	(2.69)	(2.83)	(3.90)	(2.39)	(1.35)	(-1.08)	(-2.10)	(-2.23)
Wealth Quintile									
2nd		-0.008	-0.009		-0.006	-0.006		0.005	0.003
		(-0.95)	(-1.00)		(-1.14)	(-1.15)		(0.45)	(0.29)
3rd		-0.01	-0.011		-0.007	-0.007		0.009	0.004
		(-0.85)	(-0.99)		(-0.99)	(-1.03)		(0.66)	(0.32)
4th		-0.006	-0.007		-0.011	-0.011		-0.019	-0.022
		(-0.44)	(-0.52)		(-1.48)	(-1.47)		(-1.18)	(-1.35)
5th		0.019	0.018		-0.018**	-0.016*		-0.045**	-0.047**
		(1.35)	(1.24)		(-2.08)	(-1.92)		(-2.42)	(-2.52)
Number of Sons		0.029***	0.027***		-0.001	0.000		0.011***	0.008**
		(10.85)	(10.05)		(-0.27)	(0.02)		(3.15)	(2.24)
Number of Daughters		0.026***	0.022***		-0.003	-0.003		0.014***	0.011***
		(9.42)	(7.87)		(-1.47)	(-1.23)		(3.89)	(2.80)
Childhood History of PCA		0.102***	0.102***		0.000	0.000		-0.022**	-0.022**
		(20.89)	(20.80)		(-0.15)	(-0.13)		(-2.52)	(-2.50)
Number of Household Members		-0.002	-0.001		0.001	0.000		-0.006***	-0.005**
		(-1.39)	(-0.79)		(0.58)	(0.32)		(-2.70)	(-2.27)
Number of Children < 5 Years		0.012***	0.012***		0.000	0.000		-0.006	-0.006
		(3.52)	(3.41)		(0.20)	(0.08)		(-1.23)	(-1.18)
Years of Schooling		-0.004***	-0.004***		-0.003***	-0.003***		0.000	0.000
		(-4.10)	(-4.39)		(-6.36)	(-6.24)		(0.29)	(-0.05)
Partner's Years of Schooling		-0.003***	-0.003***		-0.001	-0.001		-0.001	-0.001
		(-3.73)	(-3.84)		(-1.56)	(-1.41)		(-1.14)	(-1.12)
Partner's Age		0.000	0.000		0.000	0.000		0.000	0.000
		(-0.09)	(-0.08)		(0.69)	(0.69)		(0.27)	(0.29)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**			**X**
Observations	61727	38752	38752	46077	22767	22767	27156	18956	18956
R-squared	0.111	0.149	0.153	0.049	0.072	0.078	0.088	0.089	0.096

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).
[a] Respondents were asked whether physical punishment is a necessary part of children's education. The variable 'Punishment Necessary' takes a value of zero if they answered with "no/never" and a value of 1 otherwise.
[b] Respondents were asked five questions on scenarios where it would be justified for a husband to beat his wife. The 'IPV Justification' variable that takes a value of one if the respondents agree wife beating is justified in any of the following scenarios: wife 1) goes out without telling [the partner], 2) neglects the children, 3) argues with [the partner], 4) refuses to have sex with [the partner], or 5) burns the food.
[c] Respondents were asked whether they have ever sought help from an institution (police, courts, ministry of women, public defender, etc.) after experiencing IPV. The 'Reported IPV' variables takes a value of one if the respondent sought help from an institution, zero otherwise.

Table 6.1 indicates conflict exposure might increase the acceptance of domestic violence, thereby supporting the normalization of violence theory. This hints that previous exposure to conflict may increase the likelihood that women are victims of IPV—which is what Gutierrez & Gallegos (2011) find.[27] On the surface, these findings contradict the main result. However, it is imperative to distinguish between the two ways the normalization of violence could work. It could normalize the *use* of violence, or it could normalize being a victim of violence. Table 6.1 lends more support to the latter. Therefore, I cannot definitively conclude post-traumatic growth, with respect to child abuse, does not occur for women affected by conflict violence.

B. Domestic Violence History

There is overwhelming evidence that parents who were abused while growing up are more likely to physically abuse their children (Gage & Silvestre, 2010; Newcomb & Locke 2001; Pears & Capaldi, 2001; Berlin et al., 2011). Hence, the main finding could be driven in part by civil conflict's effect on women's childhood histories of PCA. Table 6.2 explores this relationship. Columns (1) – (3) show cumulative violence exposure is not significantly associated with a history of PCA, and Columns (4) – (6) further reveal exposure during early childhood or adulthood is also not significant. In any case, as discussed in Section III, exposure in adulthood should not affect whether a respondent was maltreated by her parents. More importantly, Column (6) suggests women exposed to an additional 100 conflict-related events between the ages of 9-16 were 2.2 percentage points less likely to have been physically abused by their parents (with respect to controls, fixed effects, and trends). The effect's magnitude is large considering the coefficients of other predictors. These results are critical. They suggest civil conflict had short-term effects on child discipline strategies, therefore likely affecting future PCA-use due to the inter-generational persistence of domestic violence.

[27] Despite the results from Table 6.1, I cannot support Gutierrez & Gallegos (2011) directly. I do not find civil conflict exposure has a statistically significant relationship with IPV-victimhood (results not shown).

Table 6.2: Civil Conflict Exposure and Childhood History of PCA

	Childhood History of PCA			Childhood History of PCA		
	(1)	(2)	(3)	(4)	(5)	(6)
Lifetime Conflict Exposure	-0.014	-0.015	-0.019			
(in hundreds)	(-1.25)	(-1.18)	(-1.57)			
Violence Exposure, by age:						
(in hundreds)						
0 - 8				-0.018	-0.016	-0.020
				(-1.14)	(-0.94)	(-1.18)
9 - 16				-0.018*	-0.018	-0.022**
				(-1.81)	(-1.60)	(-2.03)
17+				-0.011	-0.012	-0.015
				(-0.83)	(-0.87)	(-1.13)
Wealth Quintile						
2nd		0.029***	0.030***		0.029***	0.030***
		(3.77)	(3.85)		(3.77)	(3.85)
3rd		0.010	0.010		0.010	0.010
		(1.07)	(1.13)		(1.07)	(1.13)
4th		-0.027***	-0.025**		-0.027***	-0.025**
		(-2.61)	(-2.46)		(-2.61)	(-2.46)
5th		-0.084***	-0.082***		-0.084***	-0.082***
		(-7.07)	(-6.95)		(-7.07)	(-6.95)
Number of Sons		0.023***	0.023***		0.023***	0.023***
		(9.91)	(9.87)		(9.92)	(9.86)
Number of Daughters		0.018***	0.018***		0.018***	0.018***
		(6.81)	(6.60)		(6.82)	(6.60)
Number of Household Members		-0.004***	-0.004***		-0.004***	-0.004***
		(-3.73)	(-3.34)		(-3.73)	(-3.34)
Number of Children < 5 Years		0.020***	0.019***		0.020***	0.019***
		(7.20)	(6.72)		(7.21)	(6.72)
Years of Schooling		-0.022***	-0.023***		-0.022***	-0.023***
		(-6.19)	(-6.36)		(-6.19)	(-6.36)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	61757	61757	61757	61757	61757	61757
R-squared	0.086	0.098	0.101	0.086	0.098	0.101

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).
'Childhood History of PCA' takes a value of one if the respondent was physically abused by her parents, zero otherwise.

Table 6.3 complements the above analysis by exploring the heterogeneous effects of conflict with respect to previous exposure to domestic violence. 'Lifetime Conflict Exposure' is interacted with an indicator variable identifying a childhood history of PCA in Columns (1) – (3) and an indicator variable for IPV-victimhood in Columns (4) – (6). The focus is on Columns (7) – (9) since they include both interaction variables. All else equals, Column (9) shows the probability of PCA-use increases by 16.6 percentage points if the respondent has a childhood history of PCA and by 7.9 percentage points if she has been exposed to IPV. These figures highlight the extent of the inter-generational continuity of domestic violence.

The positive coefficient for 'Lifetime Conflict Exposure*Childhood History of PCA' implies the effect of conflict on PCA-use is smaller in magnitude for women who were physically abused in childhood. Conversely, the interaction term 'Lifetime Conflict Exposure*Exposed to IPV' has a negative coefficient. Conditional to a one hundred conflict-related event increase in exposure for both groups, women who are victims of IPV are 2.8 percentage points less likely to use PCA than women who have never been affected by IPV. The effect on the probability of PCA-use associated with a hundred event increase in conflict exposure for victims of IPV—a 6.4 percentage point decrease (-3.6 + -2.8)—is nearly as large as the effect of being in the 4th quintile of the wealth index relative to the lowest—a 6.5 percentage point decrease. It appears different types of domestic violence histories have differing interactive effects on PCA, one reinforces the effect (childhood history of PCA) and the other subtracts from it (IPV). This complicates how the abovementioned evidence for the normalization of violence should be assessed. If conflict-affected women were normalized to the use of violence, then it is not clear why we observe a negative coefficient for 'Lifetime Conflict Exposure*Exposure to IPV'.

C. Child-rearing Capacity

As motived in Section IV, the effect of civil conflict on PCA-use could run through changes in parent's child-rearing capacities, which are partly determined by parent's education and access to community resources that support parents. Table 6.4 shows civil conflict's effect on educational achievement. I use three variables of exposure to better illustrate the age interval in which conflict could affect education. Column (2) shows additional exposure to a hundred conflict-related events between the ages of 9-16 is associated with an average of 0.29 fewer years of schooling for women (significant at 1% level). The effect represents a 3.5% decrease for the average respondent. However, once trends are accounted for, there is no significant relationship between conflict intensity and educational achievements for either respondents or their partners. Overall, I cannot conclude civil conflict impacted parental schooling, a key predictor of PCA-use (Berger & Waldfogel, 2011).

Table 6.3: Heterogeneous Effects of Civil Conflict, by Previous Exposure to Domestic Violence

	PCA-use			PCA-use			PCA-use		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lifetime Conflict Exposure (in hundreds)	-0.019	-0.025**	-0.033***	-0.037***	-0.035***	-0.042***	-0.024**	-0.030**	-0.036***
	(-1.56)	(-2.21)	(-2.63)	(-3.18)	(-3.06)	(-3.10)	(-2.15)	(-2.58)	(-2.67)
Childhood History of PCA				0.174***	0.170***	0.171***	0.169***	0.165***	0.166***
				(27.66)	(24.95)	(24.80)	(26.12)	(24.17)	(24.02)
Lifetime Conflict Exposure* Childhood History of PCA				0.012*	0.012*	0.011*	0.012*	0.012*	0.011*
				(1.93)	(1.73)	(1.69)	(1.89)	(1.79)	(1.75)
Exposed to IPV	0.107***	0.095***	0.094***				0.092***	0.081***	0.079***
	(15.44)	(12.93)	(12.98)				(13.48)	(11.19)	(11.19)
Lifetime Conflict Exposure* Exposed to IPV	-0.034***	-0.030**	-0.028**				-0.034***	-0.030**	-0.028**
	(-2.70)	(-2.28)	(-2.26)				(-2.60)	(-2.22)	(-2.18)
Wealth Quintile									
2nd		0.008	0.005		0.005	0.002		0.002	-0.001
		(0.77)	(0.49)		(0.51)	(0.18)		(0.24)	(-0.08)
3rd		-0.010	-0.014		-0.010	-0.014		-0.013	-0.017
		(-0.81)	(-1.14)		(-0.77)	(-1.14)		(-1.03)	(-1.39)
4th		-0.061***	-0.065***		-0.060***	-0.064***		-0.061***	-0.065***
		(-4.29)	(-4.56)		(-4.30)	(-4.57)		(-4.43)	(-4.70)
5th		-0.079***	-0.084***		-0.073***	-0.077***		-0.071***	-0.076***
		(-4.91)	(-5.20)		(-4.57)	(-4.82)		(-4.49)	(-4.75)
Number of Household Members		0.057***	0.049***		0.057***	0.048***		0.056***	0.047***
		(15.40)	(13.07)		(15.44)	(12.97)		(15.34)	(12.87)
Number of Children < 5 Years		0.046***	0.036***		0.047***	0.036***		0.046***	0.035***
		(12.16)	(9.22)		(12.59)	(9.49)		(12.30)	(9.24)
Years of Schooling		-0.004*	-0.001		-0.003	0.000		-0.003	0.000
		(-1.78)	(-0.30)		(-1.59)	(-0.08)		(-1.56)	(-0.03)
Partner's Years of Schooling		0.004	0.007		0.001	0.005		0.003	0.006
		(0.92)	(1.61)		(0.34)	(1.11)		(0.72)	(1.47)
Number of Children < 5 Years		-0.002*	-0.003***		-0.002*	-0.003***		-0.002	-0.003***
		(-1.91)	(-2.81)		(-1.81)	(-2.74)		(-1.64)	(-2.58)
Years of Schooling		-0.003***	-0.003***		-0.003***	-0.003***		-0.003***	-0.003***
		(-3.34)	(-3.36)		(-3.26)	(-3.29)		(-2.92)	(-2.96)
Partner's Years of Schooling		0.001	0.001		0.000	0.000		0.000	0.000
		(1.21)	(1.40)		(-0.08)	(0.14)		(0.26)	(0.46)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**			**X**
Observations	35845	31267	31267	37317	31250	31250	35822	31248	31248
R-squared	0.128	0.157	0.167	0.146	0.176	0.186	0.152	0.180	0.189

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters). *** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies. Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region). 'Childhood History of PCA' takes a value of one if the respondent was physically abused by her parents, zero otherwise. 'Exposed to IPV' is a binary variable that takes a value of one if the respondent has ever been pushed, shaken, or attacked by her intimate partner.

Table 6.4: Effect of Civil Conflict on Parent's Educational Attainment

	Years of Schooling			Partner's Years of Schooling		
Violence Exposure, by age: (in hundreds)	(1)	(2)	(3)	(4)	(5)	(6)
0 - 8	-0.136	-0.175	-0.069	-0.001**	0.000	0.000
	(-1.14)	(-1.64)	(-0.73)	(-2.05)	(-0.21)	(-0.67)
9 - 16	-0.192	-0.288***	-0.143	-0.001***	-0.001	-0.001
	(-1.22)	(-2.60)	(-1.27)	(-3.38)	(-1.24)	(-1.62)
17+	-0.161	-0.161*	-0.021	-0.001***	0.000	0.000
	(-1.28)	(-1.72)	(-0.23)	(-4.52)	(-0.38)	(-0.63)
Wealth Quintile						
2nd		1.060***	1.024***		0.863***	0.865***
		(18.48)	(17.86)		(15.52)	(15.48)
3rd		2.515***	2.448***		1.543***	1.554***
		(31.85)	(31.06)		(22.78)	(22.74)
4th		3.837***	3.758***		2.065***	2.063***
		(39.51)	(39.12)		(24.71)	(24.54)
5th		5.080***	4.983***		2.947***	2.926***
		(47.90)	(46.08)		(27.83)	(27.82)
Number of Sons		-0.347***	-0.438***		-0.069***	-0.076***
		(-19.31)	(-23.44)		(-3.78)	(-3.92)
Number of Daughters		-0.323***	-0.431***		-0.072***	-0.081***
		(-17.23)	(-22.01)		(-4.04)	(-4.34)
Childhood History of PCA		-0.089**	-0.089**		-0.104***	-0.104***
		(-2.55)	(-2.54)		(-3.37)	(-3.32)
Number of Household Members		-0.062***	-0.028**		-0.030***	-0.027***
		(-5.52)	(-2.42)		(-2.91)	(-2.62)
Number of Children < 5 Years		0.190***	0.205***		0.016	0.018
		(7.78)	(8.40)		(0.69)	(0.77)
Partner's Age		-0.056***	-0.054***		0.002	0.001
		(-17.89)	(-17.56)		(0.48)	(0.45)
Respondent's Years of Schooling					0.394***	0.392***
					(70.15)	(68.23)
Partner's Years of Schooling		0.428***	0.420***			
		(65.95)	(64.22)			
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	74248	38784	38784	50650	38784	38784
R-squared	0.354	0.610	0.617	0.261	0.501	0.504

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).
'Years of Schooling' and 'Partner's Years of Schooling' count the years of education completed by the respondent and her partner.

Formal education is related to PCA-use in part through schooling's impact on the level of resources a parent can use on proper child discipline. A better educated parent may afford healthcare services, may have better child-rearing knowledge, and may have more time to spend with her children. Community resources and social spending can lead to the same outcomes from the supply side. Take healthcare. Parents in a community with greater access to healthcare will in effect "outsource" some of their child-rearing to medical professionals that are better trained to an ill-child. A health visit might also have spillover effects on child-rearing knowledge. Although I use healthcare as an illustrative example, the same can apply to any community

resource that can support parents in their child-rearing. Accordingly, I hypothesize the main result could be driven by an increase in community resources available for mothers in the districts that were most affected by civil conflict. Moreover, since the main result relies on comparing individuals in the same district, I predict individuals affected by higher conflict intensity are more likely to access these community resources.

Table 6.5.1 tests if civil conflict affects community resources.[28] The 'Healthcare personnel (logged)' variable is the natural log of the sum of public medical personnel in each district in 2012. Columns (1) – (3) show a positive relationship between civil conflict intensity and higher levels of government-provided healthcare. After controlling for districts' characteristics and region fixed effects, Column (3) estimates districts that experienced an additional 100 civil-conflict events had on average 30.3% more public health personnel in 2012. This increase implies an additional 125 healthcare workers. These results are based on a stock variable measured in 2012 and thus only show a one dimensional picture. It is possible districts that experienced greater conflict intensity had more government health resources even before the conflict began. Grimard & Laszlo (2013) address this concern with their use of longitudinal data. The authors investigate the changes in health services following the Peruvian civil conflict and find the number of public and private health centers increased during recovery periods from 1992-1996 in districts that were affected by civil conflict violence. Notably, they find there is a greater increase in public health centers.

I utilize yearly district revenue data from 1998 to 2008 to explore civil conflict's impact on the growth of social spending, which may affect the level of community resources available to mothers. Table 6.5.1, Columns (4) – (6) focus on transfers to district governments from the Municipal Compensation Fund (FONCOMUN). These transfers are equalization grants

[28] Data used in Table 6.5.1 come from the Ministry of Health, a 1999 census, and from a panel dataset of Peruvian districts' revenue and expenditure records from 2001–2007, as stated on official annual reports prepared by district governments.

Table 6.5.1: Effect of Conflict on District-Level Health Resources and Social Spending

	Healthcare personnel (logged)[a]			% Change FONCOMUN[b]			% Change Vaso de Leche[b]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Total Violence (in hundreds)	0.245	0.264	0.303	0.054	0.062	0.051	0.62	0.909	0.827
	(3.60)***	(5.51)***	(5.21)***	(1.86)*	(2.15)**	(1.22)	(2.22)**	(3.21)***	(2.39)**
	(4.45)***	(7.36)***	(6.50)***	(1.73)*	(1.93)*	(1.27)	(1.58)	(2.25)**	(2.04)**
Population (in thousands)		0.015	0.014		-0.001	-0.001		-0.015	-0.012
		(3.89)***	(4.17)***		(-2.04)*	(-2.29)**		(-1.92)*	(-2.17)**
		(3.62)***	(3.84)***		(-1.75)*	(-1.88)*		(-2.05)**	(-2.00)**
Poverty Rate		-0.041	-0.051		0.002	0.008		-0.012	-0.102
		(-4.06)***	(-3.86)***		(0.56)	(1.74)*		(-0.09)	(-0.54)
		(-5.47)***	(-6.27)***		(0.46)	(1.59)		(-0.08)	(-0.53)
Percentage Without Piped Water		0.003	0.002		0.002	0.001		0.046	0.076
		(1.37)	(1.28)		(1.26)	(0.91)		(1.36)	(1.58)
		(2.21)**	(1.77)*		(1.27)	(0.93)		(1.35)	(1.48)
Percentage Without Piped Sewage/Drains		0.005	0.003		-0.002	-0.002		-0.048	-0.042
		(2.26)**	(1.71)		(-1.15)	(-1.02)		(-1.08)	(-0.98)
		(2.05)**	(1.46)		(-1.09)	(-1.10)		(-0.94)	(-0.86)
Percentage Without Electricity		0.001	0.000		0.002	0.002		-0.008	-0.011
		(0.30)	(0.06)		(1.50)	(2.49)**		(-0.26)	(-0.25)
		(0.42)	(0.08)		(1.62)	(1.76)*		(-0.25)	(-0.24)
Total Budget Growth from 1998-2008		-0.008	0.001		-0.002	-0.003		0.053	0.034
		(-2.58)**	(0.18)		(-0.82)	(-1.00)		(0.85)	(0.46)
		(-1.96)*	(0.17)		(-0.90)	(-1.10)		(0.80)	(0.44)
Total Budget Used 2008 (in hundred thousands)		0.004	0.003		-0.001	-0.001		0.000	0.004
		(2.61)**	(2.58)**		(-3.20)***	(-2.72)**		(0.05)	(0.79)
		(2.77)***	(2.77)***		(-3.48)***	(-3.05)***		(0.05)	(0.78)
Region Fixed Effects:			**X**			**X**			**X**
Observations	1819	1049	1049	1053	1052	1052	1057	1056	1056
R-squared	0.027	0.417	0.495	0.003	0.023	0.072	0.001	0.008	0.032

Notes: First row of parentheses show t-statistics estimated from robust standard errors at the region level (25 clusters). Second row of parentheses show t-statistics estimated from robust standard errors at the province level (202 clusters). *** p<0.01, ** p<0.05, * p<0.1. The district-level covariates were obtained from a 1999 census and from a panel dataset of Peruvian districts' revenue and expenditure records from 2001–2007, as stated on official annual reports prepared by district governments. 'Total Violence' refers to the number of civil-conflict-related events occurred in a district between 1980-2000.

[a] The *'Healthcare personnel (logged)'* variable is the natural log of the sum of the public medical personnel in each district in 2012. The medical personnel are hired by the Ministry of Health (Ministerio de Salud, MINSA) or regional governments. Medical personnel is defined as the number of doctors, nurses, dentists, OBs, psychologists, nutritionists, pharmacists, medical technologists, and healthcare administrative personnel. The variables *'% Change FONCOMUN'* and *'% Change Vaso de Leche'* measure

[b] The variables '% Change FONCOMUN' and '% Change Vaso de Leche' measure the percentage change in FONCOMUN and Vaso de Leche transfers from 1998 to 2008. Alternatively, one could measure the percentage change from the 1998-2000 average to the 2006-2008 average. The FONCOMUN results presented here are robust to the alternative measure. Due to data limitations, only 829 districts have values for the '% Change Vaso de Leche' variable when using the alternate definition, compared to the 1060 districts that have values using the preferred growth measure used in this table.

distributed to each district and have the objective of promoting the development of the most marginalized communities. Columns (4) – (5) show the total violence a district experienced is significantly correlated with higher growth in FONCUM transfers. The magnitude of the effect remains consistent and the significance drops just under the 10% level once region fixed effects are introduced. However, 'Total Violence' coefficient *Vaso de Leche*, a nationwide nutritional assistance program that distributes foods through public kitchens and mothers' clubs, is unlike FONCOMUN in that it specifically targets mothers and children (Tanaka & Trivelli, 2002). *Vaso de Leche* is of special interest because its development over time in districts that were most affected could suggest an increase in social networks and support for mothers. Column (9) suggests districts exposed to an additional 100 conflict-related events saw an 82.7 percentage point increase in the growth of *Vaso de Leche* transfers from 1998-2008 (the average growth over the time period is 214%). Since *Vaso de Lehe* is intended to target poor districts, this result is remarkable because districts' poverty indicators, property tax revenue growth, and size of total fiscal budget are controlled for. It is difficult to compare conflict's effect on the different transfers programs, however, the findings support the hypothesis that there was an increase in social spending in districts that were most affected by conflict.

One direct way to check whether increases in social spending (FONCOMUN and *Vaso de Leche*) drive the main results is to run separate regressions for high and low social spending growth districts.[29] Replicating the main analysis, Table 6.5.2 restricts the sample to high growth districts in Panel A and low growth districts in Panel B. Although the high growth sample is slightly smaller than the low growth sample, the 'Lifetime Conflict Exposure' coefficients are negative and significant only for individuals in high growth districts. It appears the PCA-use of mothers in low growth districts was not affected by conflict intensity. However, the 'Lifetime

[29] "High social spending growth" (high growth) districts are those who have the median or higher growth in FONCONUM and *Vaso de Leche* transfers from 1998-2008, and "low social spending growth" (low growth) districts are below the median.

Table 6.5.2: Effect of Civil Conflict Exposure on PCA, by Post-Conflict District Social Spending Growth

	Panel A: Median or Above Spending Growth			**Panel B:** Below Median Spending Growth		
	PCA-use			**PCA-use**		
	(1)	(2)	(3)	(1)	(2)	(3)
Lifetime Conflict Exposure	-0.029*	-0.028**	-0.051***	-0.084	-0.054	-0.053
(in hundreds)	(-1.95)	(-2.17)	(-3.33)	(-1.07)	(-0.69)	(-0.80)
Wealth Quintile						
2nd		0.015	0.010		-0.020	-0.021
		(0.89)	(0.60)		(-1.13)	(-1.14)
3rd		-0.013	-0.023		-0.031	-0.034
		(-0.70)	(-1.28)		(-1.34)	(-1.46)
4th		-0.069***	-0.077***		-0.077***	-0.079***
		(-3.42)	(-3.77)		(-3.10)	(-3.18)
5th		-0.064***	-0.069***		-0.107***	-0.111***
		(-2.86)	(-3.00)		(-4.03)	(-4.18)
Number of Sons		0.056***	0.045***		0.061***	0.053***
		(8.60)	(7.14)		(10.56)	(9.13)
Number of Daughters		0.046***	0.034***		0.051***	0.041***
		(6.74)	(4.94)		(8.80)	(6.70)
Childhood History of PCA		0.180***	0.178***		0.164***	0.165***
		(16.18)	(16.32)		(15.99)	(15.65)
Number of Household Members		-0.005	-0.002		-0.003	0.000
		(-1.28)	(-0.56)		(-1.14)	(-0.10)
Number of Children < 5 Years		0.005	0.007		-0.004	-0.002
		(0.66)	(1.07)		(-0.57)	(-0.21)
Years of Schooling		-0.003*	-0.004**		-0.003	-0.004**
		(-1.84)	(-2.28)		(-1.43)	(-2.08)
Partner's Years of Schooling		-0.001	-0.002		-0.002	-0.002
		(-0.83)	(-1.10)		(-1.30)	(-1.32)
Partner's Age		0.001	0.001		0.000	0.000
		(1.13)	(0.99)		(-0.36)	(-0.36)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	12988	10788	10788	14375	11950	11950
R-squared	0.109	0.170	0.190	0.120	0.176	0.189

Notes: T-statistics estimated from robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Standard errors adjusted at the district level (1082 clusters). Fixed effects include district (1083), survey year (3), and birth year cohort (48) dummies. Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region). Panel A restricts the sample to individuals living in districts that had the median or higher growth in FONCONUM and *Vaso de Leche* transfers from 1998-2008, and Panel B restricts the sample to individuals living in districts that had lower than the median growth.

Conflict Exposure' coefficients for low growth districts, although not significant, are even larger in magnitude than for high growth districts. Overall, I cannot definitively conclude the main findings are driven by individuals in districts with higher post-conflict social spending growth, but there is suggestive evidence to believe that this channel is active. Future work can further explore this channel by including in the main specification different social spending measures and their interaction with civil conflict exposure.

I test the hypothesis that women exposed to greater conflict intensity are more likely to access community resources in Table 6.6.[30] The DHS data contain a few variables that can be used, although imperfectly, to address this hypothesis. Columns (1) – (3) give some evidence that mothers affected by higher conflict intensity are more likely to access healthcare resources (although this relationship is not significant once trends are introduced).[31] Then, Columns (4) – (6) test whether conflict exposure is associated with increased access to nutritional assistance programs.[32] Column (6) suggests mothers exposed to an additional one hundred conflict-related events are 3.9 percentage points more likely to access nutrition assistance programs with respect to fixed effects and trends averages. This is a notable effect since only 15% of the sample reported using these programs.

Overall, the main results could be explained by increases in parents' child-rearing capacity through changes in social services. Although I cannot definitively confirm exposure to violence is associated with an increased propensity to access healthcare, I find women affected by violence may increase their use of social services, particularly services connected to mothers' clubs like the *Vaso de Leche*. This increased use of social services may be related to conflict's impact on income, but it can also be explained by the higher post-conflict growth in social spending observed in districts affected by higher levels of violence.

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[30] The table's footer details the dependent variables' definitions. It is important to highlight that Table 6.6 adds the variables 'District's Health Resources Per Capita' and 'District's Social Spending Transfers' as controls in addition to the set of controls previously used.

[31] Although I use the 'Health Visit Last Year' variable to proxy for respondents' access to and use of healthcare, the variable might indicate the demand for health services. It is possible women exposed to higher conflict intensity need to visit health facilities more often.

[32] In the 2000 survey, respondents were asked how many times in the previous week they fed their children "mashed potatoes from social programs." In the 2011 and 2012 surveys, respondents were asked whether they gave their children "porridge from social programs." The 'Used Nutrition Social Programs' variable takes a value of one if the respondents gave a non-zero response for the 2000 survey or an affirmative answer for the latter surveys. Although these questions are restrictively specific, they are the only ones available that can be used to deduce nutritional program utilization. The social programs the data refer to are likely *Vaso de Leche* or one of its spinoffs since the former is the largest and oldest food program in Peru (Valdivia, 2004).

VII. External Validity Check: Evidence from Colombia

Through a series of robustness checks and an exploration of possible channels, I have motivated the internal validity of the main finding. In this section, I explore its external validity since the found relationship between conflict and child punishment may be idiosyncratic to Peru and not generalizable to other settings.

There are two reasons in particular why Colombia is a useful case study. First, Colombia's civil conflict is similar to Peru's in many aspects. Second, it complements the Peru case in that we can observe the short-term effects of exposure to conflict since the child punishment data were collected either during the conflict or just two years later, whereas in Peru the individual-level data were collected up to 12 years after the civil conflict ceased nation-wide.

Guerilla groups in Colombia have their roots in the *La Violencia* ("The Violence"), a civil war from 1948-1958 between the conservative and liberal political parties. These guerilla groups, *Fuerzas Armadas Revolucinarias de Colombia* (FARC) and *Ejército de Liberación Nacional* (ELN), were founded on similar Marxist and radical left-wing ideologies as the Shining Path in Peru. They were made up of almost exclusively poor rural citizens. The FARC and ELN did not gain prominence until the early 1980s—when their military capacities increased thanks to financing from kidnappings, extortion, and drug trade (Steele, 2007). Peasant groups formed their own paramilitary forces in order to defend themselves against the FARC and the ELN. Consequently, Colombia has for decades been constantly plagued by fighting between guerrilla groups, paramilitary forces, and the military.[33]

The Colombian government, through a special "Presidential Program for Human Rights and International Humanitarian Law" program, records homicides, massacres, kidnappings, and other violent events related to the civil conflict. Using available data on the number of armed confrontations (fighting between government and non-governmental armed groups) at the

[33] For more detailed histories of the Colombian civil conflict, see Steele (2007) and Ruiz (2001).

Table 7.1: The Effect of Civil Conflict Exposure on Child Punishment
External Validity Check – Colombia

	Spanking				Hits with an object			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Exposure to Conflict	-0.177***	-0.196***	-0.194***	-0.254***	0.086	0.036	0.044	0.060
(in hundreds)	(-3.46)	(-3.52)	(-3.48)	(-4.04)	(1.52)	(0.57)	(0.71)	(0.86)
Wealth Quintile								
2nd		0.017**	0.017**	0.015*		0.010	0.012	0.010
		(2.42)	(2.43)	(1.76)		(1.20)	(1.52)	(1.12)
3rd		0.024***	0.024***	0.025**		0.004	0.008	0.005
		(2.71)	(2.77)	(2.48)		(0.40)	(0.89)	(0.51)
4th		0.014	0.014	0.009		-0.012	-0.009	-0.01
		(1.40)	(1.43)	(0.78)		(-1.09)	(-0.83)	(-0.77)
5th		0.020*	0.017	0.015		-0.047***	-0.046***	-0.047***
		(1.67)	(1.49)	(1.16)		(-3.69)	(-3.65)	(-3.30)
Number of Sons		-0.010***	-0.009***	-0.007**		0.030***	0.030***	0.029***
		(-4.12)	(-3.48)	(-2.37)		(10.90)	(10.75)	(9.25)
Number of Daughters		-0.004	-0.004	-0.005		0.003	0.006	0.006
		(-1.12)	(-1.19)	(-1.26)		(0.71)	(1.42)	(1.27)
Childhood History of PCA		0.284***	0.285***	0.292***		0.058***	0.059***	0.060***
		(40.23)	(40.13)	(36.75)		(8.46)	(8.52)	(7.75)
Hit with Objects by Parents		0.021***	0.021***	0.016***		0.203***	0.201***	0.200***
		(4.03)	(3.97)	(2.62)		(35.23)	(34.90)	(30.82)
Number of Household Members		-0.011***	-0.011***	-0.009***		0.001	0.001	0.000
		(-7.69)	(-7.51)	(-5.53)		(0.83)	(0.92)	(0.23)
Number of Children < 5 Years		0.066***	0.064***	0.062***		0.012***	0.011***	0.012***
		(18.42)	(17.72)	(14.90)		(3.12)	(2.82)	(2.67)
Years of Schooling		0.003***	0.003***	0.003***		-0.009***	-0.009***	-0.009***
		(4.11)	(4.25)	(4.03)		(-11.49)	(-11.51)	(-9.63)
Partner's Years of Schooling		0.001	0.001*	0.001**		-0.002***	-0.002***	-0.002***
		(1.58)	(1.72)	(2.02)		(-3.63)	(-3.70)	(-3.26)
Partner's Age		-0.001**	-0.001***	-0.001*		0.001	0.001	0.000
		(-2.45)	(-2.58)	(-1.77)		(1.50)	(1.43)	(0.51)
Fixed Effects:	X	X	X	X	X	X	X	X
Trends:			X	X			X	X
Restrict to Non-Migrants:				X				X
Observations	51237	36318	36318	28684	51237	36318	36318	28684
R-squared	0.080	0.147	0.155	0.156	0.092	0.146	0.153	0.159

Sources and Notes: Colombia DHS 2005 and 2010. Parentheses show t-statistics estimated from robust standard errors adjusted at the survey cluster level (4991 clusters). *** p<0.01, ** p<0.05, * p<0.1. Regressions include municipality (358), survey year (2), and birth year cohort (44) dummies. Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region). The restriction to non-migrants includes in the sample only individuals who had lived in their current residence since 2002. 'Spanks' takes a value of one if the respondent uses spanking to punish her children. 'Hits with an object' takes a value of one if the respondent punishes her children by hitting them with an object. 'Exposure to Violence' refers to the total number of conflict-related events that occurred in an individual's municipality from 2003-2008. Women surveyed in 2004 are assigned all of the 2004 civil conflict violence even though the interview could have occurred before some of the armed confrontations (this concern is partly mitigated by the fact that the surveys in 2004 took place only from October to December). The assignment of civil conflict exposure is more difficult for those in the 2005 survey since interviews took place from January to July. For the results presented here, I assign all of the 2005 civil conflict violence to those interviewed in 2005, although the results are robust to using the alternate assignment (not shown).

municipal level for the years 2003-2008, I create an 'Exposure to Violence' variable for respondents from Colombian DHS surveys from 2004, 2005, 2009, and 2010.[34] The Colombian conflict was widely spread geographically, and this variation is utilized by the identification strategy employed to estimate the effect of conflict on child punishment.[35]

Although the DHS surveys are mostly standardized, the identification of PCA in Colombia is less clear. While the Peruvian dataset contains one variable indicating the use of physical punishment, the Colombian data include several vague variables that could constitute as PCA. Punishment strategies in the Colombian data included *palmadas* (which can be translated to spanking or slaps) or hitting with objects. Thirty and 47 percent of women in the sample use spanking and hitting with objects, respectively, as punishment methods.

I use the same fixed effects LPM specified in Equation (2). Thanks to the standardization of DHS data across countries, I am able to use the exact same controls as in the Peruvian analysis. The main results for Colombia can be found in Table 7.1. Columns (1) – (4) show the results for 'Spanks' and Columns (5) – (8) for 'Hits with an Object'. The final columns in each panel restrict the sample to respondents who have lived in their current residences since 2002.[36] The results suggest mothers who are exposed to higher conflict intensity are not more likely to hit their children with an object as punishment but are more likely to use spanking.[37] The *'Exposure to Violence'* coefficient in Column (4), the preferred specification, suggests mothers who are exposed to an extra 20 conflict-related events (one standard deviation in *'Exposure to Violence'*)

[34] The individual-level data come from phase V (2005) and VI (2010) of Colombian DHS surveys. The combined sample has a total of 94,865 observations representing every region in the country and 358 municipalities.

[35] I also partly rely on the within-district variation of exposure to violence. For example, respondents in 2009 and 2010 will have, on average, higher levels of exposure to violence than those interviewed in 2004-2005 because the violence data are from 2003 to 2008. One important concern for the identification strategy is that civil conflict data are yearly, thus I am forced to make assumptions about the actual yearly conflict exposure. See Notes on Table 7.1.

[36] Although this helps assure respondents are not assigned an incorrect conflict exposure, this sample restriction is problematic, as it was in the Peru case, because it omits respondents who have simply moved within their same municipality. Nonetheless, given the short time period in between the civil conflict violence and the time of the surveys, the restriction to a non-migrant sample here is much more reliable than in the Peru analysis.

[37] The results for whether the father uses 'spanks' or 'hitting with an object' as punishment methods suggest no decrease for 'spanks' and a slightly significant increase in 'hitting with an object' (although there is no relationship when the sample is restricted to non-migrants). These results are not reported in this version.

are 5.08 percentage points less likely to spank their children, in respect to municipality, birth year, and survey year averages and after controlling for regional trends flexibly. This effect is greater than the one observed in Peru. Notably, the R-squared values across the specifications (0.08-0.16) are consistent with those observed in Peru (0.12-0.19).

The above findings are complicated by data restrictions. The civil conflict in Colombia did not necessarily end in 2008 (the last year I have data for), so respondents interviewed in 2009 and 2010 could have been exposed to additional armed confrontations that were unaccounted for. Perhaps even more problematic, the exposure to armed confrontations for all respondents before 2003 is unknown. Nevertheless, the observed results for Colombia suggest there is external validity to the main finding. The results here specifically corroborate those from Table 6.2; conflict exposure may have short-term effects on abusive discipline strategies. Future research should explore the channels behind this relationship in Colombia, as they might differ greatly from those identified in the Peru case.

VIII. Discussion and Conclusion

Using three large cross-sections of nationally representative data, I find earlier civil conflict exposure is associated with a decrease in the use of physical punishment as a discipline method. After controlling for known predictors of domestic violence, I find mothers exposed to an additional hundred conflict-related events in their lifetime are 3.4 percentage points less likely to use physical punishment, with respect to district, birth year cohort, and survey year fixed effects and regional time trends. This effect appears to be driven by exposure to conflict after early childhood.

I explore the channels driving the main finding. I find civil conflict may decrease physical punishment in the short-run. Women who were affected by greater conflict intensity between the ages of 9-16 years were less likely to have been physically maltreated by their parents. This finding is reinforced by analyzing civil conflict in Colombia, where recent conflict exposure is

also associated with decreased use of abusive punishment against children. Given the inter-generational continuity of domestic violence, negative shocks to PCA-use are likely to persist. Therefore, a decrease in children's physical punishment contemporaneous to armed conflict may explain why PCA decreases long after conflict exposure.

It remains unclear what drives the decrease in physical punishment either in the long or short-term. It appears women who were more affected by conflict are actually more likely to accept or tolerate the use of violence in the home. However, the decrease in PCA-use is larger for women who are physically abused by their partners. Given these findings, the main results are not mutually inconsistent with previous work showing conflict exposure increases intimate partner violence victimhood in Peru and Colombia (Gutierrez & Gallegos, 2011; Noe & Rieckmann, 2013).

A parent's ability to parent might be affected by civil conflict—the main results could be explained by conflict's impact on parents' child-rearing capacity. I show women impacted by conflict may be more likely to access health and social programs. I also find districts that were more affected by conflict saw higher social spending growth in the post-conflict period, and I present preliminary evidence showing these districts drive the main result. Using a case study from a shanty town in Lima largely occupied by migrants from regions deeply affected by armed violence, Isla (1997) writes, "Both the [community kitchens] and *Vaso de Leche* have helped women to move out of the private household to a public and communal sphere. In these committees, women discuss issues of survival, social, and communal conflicts as well as personal and gender problems such as violence in their home" (82). The author highlights the child-rearing capacity that may be built up through social services. Social services can directly support parents through the provision of resources, but they may lead to child-rearing knowledge spillover effects.

If political violence brings fear and isolation, then conflict-affected women's higher

propensity to use social programs can be leveraged to rebuild social networks and trust. Programs that support child-rearing and integrate community and women's clubs, like *Vaso de Leche*, may be particularly effective. Accordingly, future reconstruction policies should aim to empower women—they may help recovery by promoting safe and healthy childhood development within a family.

Follow-up research calls for the investigation of the long timespan between conflict exposure and its effect on physical punishment. Knowing how long the effect persists would further shed light on the mechanisms that drive it, and this may incentivize policies and research to consider the possible inter-generational effects of conflict.[38] Along these lines, future research may address other welfare outcomes that could impact children born to parents that lived through armed conflict. This is particularly important given the recent emphasis on greater protection for war-impacted children (UNICEF, 2009).

This paper's main contribution is that conflict exposure does not affect intrahousehold violence in a homogenous way. While previous work suggests conflict increases IPV, I find conflict-affected women reduce their use of physical punishment. This effect is greater for those who have been physically abused by their intimate partners, perhaps suggesting the more violence women are exposed to in adulthood, the less likely they are to inflict it upon their children. Trying times can expose resiliency. In the face of inter-generational continuity of domestic violence, it is encouraging to observe that those affected by civil conflict may help curb the use of physical child abuse in the future.

[38] Table 8.1 (available in the Online Tables Appendix) presents a preliminary exploration of how long the observed decrease in PCA-use lasts after conflict exposure.

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Appendix

V. Results: *D. Robustness Check: Spatial Identification*

In this subsection, I run robustness checks that consider spatial identification problems.[39] Respondents could have been unaware of, or unaffected by, conflict-related acts that occurred within their district but very far from where they lived. Therefore, the conflict exposure variables for larger districts may overinflate actual exposure. Similarly, residents from smaller districts are likely affected by conflict events that occur in nearby districts.[40] In this case, the violence exposure variables for smaller districts may underestimate actual exposure.

In order to address the above concerns, I exclude respondents from districts whose areas are less than the 20th percentile of all districts (smaller than 6613.45 km squared), and in separate regressions, I exclude respondents from districts whose areas are more than the 80th percentile of all districts (greater than 101764.7 km squared). Table 5.D.1 (available in the Online Tables Appendix) presents these results. For the regressions excluding the smaller districts, the effect of 'Lifetime Conflict Exposure' on women's PCA-use is smaller than the effect for the unrestricted sample. Meanwhile, for the regressions excluding the largest districts, the 'Lifetime Conflict Exposure' coefficients are larger. Both results remain significant at the 5% or 1% level across the levels of controls. These results suggest civil conflict exposure is overinflated for larger districts and underestimated for smaller ones.

V. Results: *E. Fertility Check*

Another possible identification issue could be the sample composition. A common concern in the conflict literature is that estimated effects are biased due to conflict-related deaths: the behavior or outcome of the most affected is not observed. This is not a concern in this paper since the number of deaths is relatively small compared to the whole population. The total sample size of the combined DHS data (74,248) is greater than the total estimated number of deaths for the civil conflict (69,290).

The sample could still be biased, however, if women who were most affected by the conflict are less likely to have ever had children.[41] This concern is addressed in Table 5.E.1 (available in the Online Tables Appendix). The table reveals women exposed to greater conflict intensity are not more or less likely to have ever given birth. There is slight evidence affected women had more children, although the result is only significant at the 10% level and not robust to the inclusion of regional trends.

V. Results: *F. Household Use of Physical Punishment*

The related conflict literature on domestic violence has overlooked men's conflict exposure (Gutierrez & Gallegos, 2011; Noe & Rieckmann, 2013). However, the conceptual framework for conflict's effect on PCA-use also applies to men. Columns (1) – (3) in Table 5.F.1 suggest men's exposure to conflict is not significantly associated with their PCA-use. This result should be interpreted with caution. Only the husband's age at the time of the interview is known, so the exposure to violence is not as precisely measured as it is for women. Nonetheless, the results suggest that men do not respond to conflict exposure in the same way women do. This may explain why previous research finds conflict exposure is associated with increases in IPV-victimhood while I find it reduces physical child abuse.

To better explore the heterogeneous effects of conflict on PCA-use by gender, Panel B

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[39] These tests were inspired by similar ones run by Noe & Rieckmann (2013).

[40] As Noe & Rieckmann (2013) note, the civil conflict violence "does not sharply stop at a district border, which is why conflict intensity measures for neighbouring districts are anyway correlated" (15).

[41] For a review of armed conflict's effect on fertility, see Abu-Musa (2008).

Table 5.F.1: Effect of Civil Conflict Exposure On Use of Physical Child Abuse
Intimate Partner and General Household PCA-use

	Panel A:			Panel B:		
	Partner PCA-use			**Household PCA-use**		
	(1)	(2)	(3)	(4)	(5)	(6)
Lifetime Violence Exposure (in hundreds)				-0.033*	-0.036	-0.057*
				(-1.74)	(-1.13)	(-1.69)
Partner Lifetime Violence Exposure (in hundreds)	0.001	-0.018	-0.025	0.045***	-0.031**	-0.037**
	(0.08)	(-0.86)	(-0.97)	(5.34)	(-2.21)	(-2.49)
Lifetime Violence Exposure * Partner Lifetime Violence Exposure				-0.000**	0.000	0.000
				(-2.53)	(0.94)	(1.49)
Wealth Quintile						
2nd		-0.005	-0.007		0.007	0.004
		(-0.45)	(-0.65)		(0.76)	(0.41)
3rd		-0.034**	-0.038***		-0.015	-0.02
		(-2.45)	(-2.65)		(-1.24)	(-1.64)
4th		-0.075***	-0.078***		-0.070***	-0.075***
		(-4.76)	(-4.91)		(-5.17)	(-5.45)
5th		-0.107***	-0.112***		-0.093***	-0.099***
		(-6.54)	(-6.67)		(-6.01)	(-6.32)
Number of Sons		0.071***	0.063***		0.074***	0.065***
		(20.18)	(17.75)		(22.22)	(19.23)
Number of Daughters		0.045***	0.036***		0.056***	0.045***
		(11.85)	(9.23)		(15.76)	(12.10)
Childhood History of PCA		0.133***	0.134***		0.174***	0.174***
		(20.17)	(20.21)		(27.54)	(27.41)
Number of Household Members		-0.006***	-0.003		-0.004**	-0.001
		(-2.61)	(-1.37)		(-2.10)	(-0.51)
Number of Children < 5 Years		-0.005	-0.003		-0.005	-0.001
		(-1.07)	(-0.58)		(-1.18)	(-0.24)
Years of Schooling		-0.007***	-0.008***		-0.004***	-0.005***
		(-6.82)	(-7.43)		(-4.33)	(-5.32)
Partner's Years of Schooling		-0.004***	-0.004***		-0.003***	-0.004***
		(-3.59)	(-3.52)		(-3.79)	(-3.84)
Partner's Age		-0.001*	-0.001		-0.001	-0.001
		(-1.88)	(-1.61)		(-1.30)	(-1.06)
Fixed Effects:	**X**	**X**	**X**	**X**	**X**	**X**
Trends:			**X**			**X**
Observations	25976	25714	25714	37936	31828	31828
R-squared	0.153	0.208	0.216	0.137	0.205	0.215

Notes: Parentheses show t-statistics estimated from robust standard errors adjusted at the district level (1082 clusters).
*** p<0.01, ** p<0.05, * p<0.1. Regressions include district (1083), survey year (3), and birth year cohort (48) dummies.
Trends refers to region-specific time trends (a dummy is created for survey five-year birth cohorts from each region).
'Partner PCA-use' takes a value of one if the respondent's husband physically punishes their children. 'Household PCA-use' takes a value of 1 if either the respondent or her partner use PCA, zero otherwise.

analyzes both parents. The dependent variable tests for the general presence of physical punishment in a household: 'Household PCA-use' takes a value of one if either the respondent or her husband physically punish their children. Panel B includes both the respondents' and their partners' conflict exposure variables along with their interaction. In contrast to Panel A, column (6) shows both the respondents' and their male partners' conflict exposures have an effect on child physical punishment. A one hundred event increase in a women's lifetime exposure to conflict is associated with a 5.7 percentage point decrease in the probability either her or her partner use PCA. The same conflict intensity increase for a husband is associated with a 3.7 percentage point decrease. Due to the interaction of conflict exposures, the results for women are conditional to their husbands having never experienced violence, and vice versa. Overall, the findings suggest the general presence of PCA-use is more sensitive to the conflict exposure of mothers than the exposure of fathers.