Long Term Effects of Civil Conflict on Women's Health Outcomes in Peru

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Comments welcome.

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Abstract

We investigate the long term effects of Peru's internal conflict on women's outcomes. According to Peru's Truth and Reconciliation Commission (CVR), the conflict was responsible for over 69,000 deaths and disappearances from 1980 to 2000 and between 500,000 to 1 million internally displaced persons. This conflict affected households' ability to generate income because of the death or disappearance of income earners and loss of productive assets. Using data from Peru's Demographic and Health Surveys and district-level conflict data published by the CVR, we find long-term effects of the conflict on some indicators of women's health, particularly on height and anemia.

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1. Introduction

Recent literature in economics and epidemiology has documented important and long-lasting effects of shocks experienced in early life, either *in utero* or in early infancy. The usual story is that shocks to nutrition in a critical stage of human development can have permanent deleterious effects on an individual's health over her life-course because they have permanent effects on her physiology (Barker, 1998). A number of recent papers have documented long-lasting effects of such shocks on adult height (Case and Paxson, 2010), on adult socio-economic outcomes (Almond, Chay and Lee, 2005; Currie, 2009), on adult self-reported health, on child development (Currie, 2009) and on educational outcomes (Cutler and Lleras-Muney, 2008). Evidence has been gathered in both developed countries (Almond, 2006; Currie and Hyson, 1999; Currie and Moretti, 2007; Oreopoulos et al., 2008) and developing countries (Maccini and Yang, 2009), from childhood (Paxson and Schady, 2006) to old age (Case, Fertig and Paxson, 2005; Grimard, Laszlo and Lim, 2010).

Making the case for this critical period programming hypothesis is complicated by two major problems of identification. First, the effects of shocks *in utero* or in early infancy on outcomes later in life will be confounded by their indirect effects though the acquisition of human capital and the determination of adult socio-economic factors. Put differently, if nutritional shocks in a critical period have effects on cognitive ability (Walker et al., 2005), then educational attainment will also be affected. Meanwhile, a vast literature in labour economics documents important returns to education, which influence earnings, both of which influence adult health through health information or investment in health inputs. And past investment in education may affect risk and time preferences in ways that influence adult behaviour and hence outcomes ranging from the socio-economic to the psycho-social, all of which affect the individual's quality of life. These life-course or pathways channels are qualitatively important because they can help reverse some of the negative effects experienced by shocks in the critical period. They are also policy relevant because they imply that pro-human capital investments can counter the long-term effects of these shocks.

The second identification challenge is to find a credible source of exogenous variation in outcomes during the critical period which can affect nutrition. Early generations of studies of this issue pointed to maternal socioeconomic outcomes (Barker, 1998; Currie, 2009). The notion here is that more able, educated and wealthy mothers will be better fed during pregnancy and breastfeeding and so infant outcomes will be better. The problem with this strategy is that such variation is not exogenous if we consider the likely genetic transmission of, say, cognitive ability. One influential study links exposure to the 1918 influenza pandemic *in utero* and adult economic outcomes (Almond, 2006). In this case, the swiftness and virulence of the flu through the US population generated a credible source of exogenous variation and the children of pregnant women exposed during the pandemic fared much worse than their counterparts with mothers who escaped the flu in their pregnancy months. Similarly, Maccini and Yang (2009) exploit regional variation in rainfall at birth to explain a host of adult socio-economic outcomes (especially height), and in doing so finding evidence of the critical period programming.

In this paper, we consider the effects of the Peruvian civil conflict of the 1980s and 1990s on women's health and psychosocial outcomes. We focus on Peru because it provides a unique opportunity to seek for evidence of the critical period programming hypothesis due to the extensive geographic and time variation in the intensity and extent of conflict. It is possible to do so thanks to the work published by the Truth and Reconciliation Commission, which investigated and reported detailed events data at the district level covering a 20 year period. The conflict is believed to have caused significant economic losses to affected households due to theft and destruction of homes, agricultural plots and other farm assets. In extreme cases, people lost loved ones, who are also income earners. They also abandoned their land or dwelling to seek shelter to avoid being caught in the fighting, and such displacement can lead to susceptibility to disease. In addition, prenatal stress induced by the conflict likely affected individuals' cognitive development (Entringer et al. (2009); Laplante et al. (2008), King and Laplante, (2005)). The literature cited in Entringer et al. (2009) suggests neurological factors in causally linking prenatal stress to cognitive development. There are thus three possible mechanisms through which exposure to conflict in early life can lead to lower health status later in life, independently of their effects on determining socio-economic status: shocks to nutrition resulting from the death of income earners and the loss or theft of assets, shocks to health because of unsanitary environments during displacement, and prenatal psychosocial stress shocks.

We focus on women because it is believed that women tend to suffer disproportionately from adverse shocks. The development literature has documented a great deal of evidence that in times of economic hardship, girls bear the brunt of intra-household re-allocation in the face of negative shocks to disposable income (Escobal, 2007; United Nations, 2008). While Peru ranks 17th out of 102 non-OECD countries in the OECD's Social Institutions and Gender Index, a rating indicative of low gender discrimination, Peruvian women nonetheless experience higher poverty and unemployment rates than men, and domestic violence and psychological and sexual abuse against Peruvian women is unfortunately a common reality.[1] Given these facts and the central stage that women hold in the Millennium Development Goals, our focus on considering the long term effects of civil conflict on women's outcomes is justified.

We utilize two datasets to conduct our analysis. Our outcome variables come from the 5th round of the Peru Demographic and Health Survey, which is a continuous survey from 2004 to 2008 of over 41,000 women aged 15 to 49. From this data set, we extract information on women's health and psycho-social outcomes: height, BMI, anemia, and domestic abuse. This dataset also provides us with important socio-economic controls such as wealth, education, and

[1] Gender Index, OECD. http://genderindex.org/country/peru, last accessed May 18, 2010.

mother tongue, the latter which allows us to control for whether the woman is indigenous. The main explanatory variable that allows us to measure stress in early infancy comes from Peru's Truth and Reconciliation Commission report (CVR, 2003). The CVR provides detailed data at the district level of violence over the 1980-2000 period. We construct from these data dummy variables on whether a woman's birth district experienced deaths and/or disappearances in her year of birth and adjacent years. The CVR estimates that over 69,000 Peruvians died or disappeared over the course of the conflict at the hands of the Shining Path or government forces. These variables, which exhibit considerable space and time variation, provide us with the basis for the empirical strategy we use to determine whether the negative shocks in early life have long-lived effects. To capture long-term effects, we consider only women over the age of 18, so we restrict our sample to include only women born up-to and including 1990.

Our paper thus contributes to several literatures. First, it contributes to the literature on critical period programming (Barker, 1998; Case, Fertig and Paxson, 2005; Case and Paxson, 2010). Second, it contributes to an important emerging literature on the micro-economic effects of armed conflict. Richard Akresh and colleagues have investigated the effects of armed conflict on children's health outcomes in the contexts of the Burundian civil war and Rwandan genocide (Akresh and De Walque, 2010; Akresh et al., 2009 and forthcoming). Christopher Blattman has investigated the socio-economic and psycho-social effects of abduction and child soldiering in the context of the Lord's Resistance Army in Uganda (Blattman et al., 2009; Blattman and Annan, *forthcoming*). León (2010) and Laszlo and Santor (2009) have both utilized data from the CVR to explain socio-economic outcomes: the former finds short- and long-term effects of violence on educational attainment, while the latter use CVR data to instrument for migration patterns in analyzing migrants' access to credit.

The latest draft by León (2010), which has only just recently been available, also includes health outcome variables, namely child and mother anthropometrics.[2] Our analysis is complementary to but different than his. First, our analysis focuses exclusively on the long-term adult health outcomes of women who were born during the conflict, while Leon (2010) focuses on both girls and boys and their human capital accumulation and especially education. Second, our analysis focuses on the critical period programming and life-course model of long-term effects of shocks *in utero*, while Leon (2010) provides a story regarding human capital accumulation. Third, we appeal to a different data set, the 2004-2008 continuous DHS, while Leon (2010) uses the 2007 census for education and the 1992 DHS for health. This distinction on health is important: since our objective is to assess the critical period programming and life-course mechanisms, the 2004-2008 DHS is appropriate for our study because it allows for us to speak to the long-term health impacts. Meanwhile, though he looks at long-term educational outcomes with the 2007 census, León (2010) only considers short-term health outcomes in 1992

We find that civil conflict events during the year preceding birth have measurable deleterious effects on women's height and anemia, even controlling for observable adult socio-economic status such as education and wealth. Events at older ages have either no effect, or weaker effects on these variables. However, we find only moderate evidence of the effects of conflict in early life on a number of psycho-social indicators such as domestic abuse. That we find such little persistence in effects over the life course suggests a number of encouraging underlying explanations. First, it could be that Peruvian women are resilient. Such resilience has been documented in Blattman et al. (2010) and Blattman and Annan (2009) who show that former child soldiers and abductees are able to reintegrate into society, limiting the long-term effects of abduction on socio-economic and psychosocial outcomes. Second, though related, they suggest

[2] We have only just recently learned of Leon's latest draft, which was posted only recently on the BREAD website.

that positive events can reverse the deleterious effects of negative shocks *in utero* and during early infancy. Indeed, the 1990s saw a tremendous increase of publicly funded social programs in Peru, and especially in poor rural regions that would have been particularly hard hit by the conflict.[3]

2. Conceptual Framework

2.1. Long-term effects of shocks *in utero* and at birth

The conceptual framework for this analysis follows closely that in Grimard et al. (2010), which is inspired by the work by Grossman (1972), Maccini and Yang (2009). Specifically, health (h) at any given point in time t is a function health in all previous periods, human capital investments (E) in all previous periods, wealth (Y) and initial health (h_0). In addition, local community programs and local environments (C) in all periods are also believed to affect health. As outlined in Grimard et al. (2010), we can summarize this process in the following health production function:

$$h_t = H\left(h_0, h_1, \ldots h_{t-1}, E_1, \ldots E_t, Y_0, \ldots Y_t, C_0, \ldots C_t, X\right) \quad (1)$$

where X represents time-invariant individual and regional characteristics. Since health in any given period is itself a recursive function of health, education and wealth in previous periods, we consider the following reduced form health production function:

$$h_t = h\left(h_0, E_1, \ldots E_t, Y_0, \ldots Y_t, C_0, \ldots C_t, X\right) \quad (2)$$

[3] We will investigate these effects later in a subsequent version of the paper.

Since we are particularly interested in critical period programming effects, a key variable in (2) is initial health h_0, which we assume to be determined by unobserved genetic factors (*G*) and community level characteristics (C_0), which in the present case is the district level conflict shock:

$$h_0 = g\left(C_0, G, X\right) \tag{3}$$

The system comprised of equations (2) and (3) can be expressed as a linear approximation in a reduced form relationship between current health and initial shocks (*in utero* or in early infancy) as follows:

$$h_{ti} = \beta_0 + \beta_1 C_{0i} + \beta_E E_i + \beta_Y Y_i + \beta_c C_i + \boldsymbol{\beta}_X \mathbf{X}_i + \varepsilon_i \tag{4}$$

An identification issue arises because critical period programming mechanisms may be at play alongside life-course mechanisms. Both mechanisms are models from the epidemiological and social determinants of health literatures that link conditions in early life to outcomes in later life. The critical period programming model, based on Barker (1998) and (Barker et al., 2002), posits that negative shocks *in utero* and early infancy affect fetal development in ways which have permanent effects on the individual's health. In other words, these early shocks can cause a permanent downward shift in an individual's health profile over their life. Conversely, the life-course models (Kuh and Wadsworth, 1993; Ben-Shlomo and Kuh, 2002) propound that conditions in early-life affect the determination of human capital acquisition, which in turn affects adult socio-economic conditions and hence health outcomes in later life. That is, adverse shocks *in utero* and early infancy will influence an individual's ability to invest in education. Since education is critical in generating income, low education leads to low income, which leads to poor adult outcomes. Because both mechanisms are likely to exist, the effect of *in utero* or early infancy exposure to adverse shocks on adult health is confounded by a direct (critical period programming) effect via physiological sequelae and by an indirect (life-course) effect via their effects on adult socio-economic status.

Because both mechanisms can exist simultaneously and are not mutually exclusive, our identification strategy consists of testing for the presence of critical period programming effects and their persistence once we control for adult socio-economic status. This is the strategy employed in Grimard et al. (2010) which we follow here. Specifically, we first consider an empirical specification similar to equation (3) that omits adult socio-economic factors (namely education and wealth), then sequentially add adult socio-economic conditions. If the effects of shocks at birth persist when controlling for adult socio-economic conditions, then we uncover evidence of critical period programming. If the effects of these shocks weaken when controlling for adult socio-economic conditions, then we also provide evidence of life-course effects.

2.2. Peru's Civil Conflict and shocks at birth.

In this sub-section, we attempt to establish the possible mechanisms through which the Peruvian internal conflict could be associated with worse infant outcomes. The existing economics literature on the micro-economic effects of conflict have identified two important channels through which conflict could worsen childhood outcomes. First, Akresh and De Walque (2010) and Akresh et al. (2009 and forthcoming) argue, in the contexts of the conflicts in Burundi and Rwanda, that conflict created unanticipated shocks to income (theft or destruction of assets including livestock and grain stocks, death or abduction of income earner). Since these shocks are unanticipated, affected households are unable to adjust in the short term, and so already poor households are likely to face food shortages and hence an inability to provide adequate nutrition.[4] There is little reason to believe that the Peruvian internal conflict is any different than the Burundian or Rwandan contexts in this case. Indeed, Tome VIII of the CVR report (CVR, 2003)

[4] Food shortages during the Peruvian Internal conflict were made all the more worse by disruption of transportation routes. Since these shocks are likely felt fairly uniformly within districts, we do not consider these here as our empirical strategy will include district fixed effects.

estimates that the average peasant household suffered material losses worth over US$5,000 (through dwelling, tools, cookware, ploughs, harvesters, livestock and potato seeds). This amount is considerable for poor peasant households. In addition, land was often abandoned because of conflict (CVR, 2003). It is estimated that nationally, over 30,000 hectares were rendered unusable because of terrorism (CVR, 2003). In the department of Junín alone, one of Peru's major bread baskets, over 13,000 hectares were rendered unusable due to terrorism (CVR, 2003). If we include labour shortages, the report identifies over 134,000 hectares rendered unusable. There is little question that the extent of theft and destruction severely impacted peasant households' ability to feed themselves. Barker's critical period programming model builds on nutritional deficiencies in early life, deficiencies which are likely to have affected individuals *in utero* or in early life during the Peruvian conflict.

Second, in the contexts of the Burundian and Rwandan conflicts, Akresh and De Walque (2010) and Akresh et al. (2009 and forthcoming) also argue that displacement during conflict can have negative effects on child outcomes. The mechanism at play here would predominantly affect child outcomes through unsanitary environments and hence disease. In Peru, the internal conflict forced between 500,000 to one million individuals to become displaced (Internal Displacement Monitoring Centre, 2007 and 2009). The long term effects of the conflict on the internally displaced peoples (IDP) continue to be felt today, as IDPs continue to be socially and economically excluded (IDMC, 2007 and 2009). Nearly 50% of the IDPs, according to the CVR report (CVR, 2003) and the IDMC (IDMC, 2007 and 2009), eventually returned to their original location (the remaining half stayed in their new locations, mostly slums in urban Lima). Indeed, Laszlo and Santor (2009) document the role that the internal conflict played in determining migration patterns.

A third important channel may also be in operation. Beyond deleterious physical effects of conflicts, the internal conflict in Peru is associated with severe psychological effects. Indeed, the

CVR documents numerous psychosocial effects of the conflict: fear and distrust, disintegration of kinship and community ties (orphan-hood, loss of loved ones, breakdown of the family, insecurity, community stigma, disruption to normal mourning rituals, loneliness…) and harm to personal identity (sexual assault, torture, humiliation, desperation, emotional distress…) (CVR, 2003). The medical literature provides evidence towards noxious neurological effects of prenatal psychosocial stress (Marmot, 1997; King and Laplante, 2005; Laplante et al., 2008; Entringer et al., 2009). This literature finds evidence that maternal psychosocial stress during pregnancy leads to lower fetal development, which in turn leads to lower cognitive ability and worse mental health outcomes. Meanwhile, Camacho (2009) finds evidence of the effect of prenatal stress on low birth weights in the context of the terrorist attacks in the US.

In summary, whether through malnutrition, disease exposure during forced displacement, or prenatal psychosocial stress, the effects of the Peruvian internal conflict has a reasonable chance to inflict negative consequences to early life development. Through either physiological or neurological effects, there is good reason to suspect that exposure to the conflict *in utero* or early infancy would have direct effects on adult health outcomes. In other words, the brutality of the conflict provides an environment in which the critical period programming mechanism might take effect.

That said, there are also important life-course mechanisms present in conflict situations, including in Peru's internal conflict. Neurological effects of prenatal exposure to stress in various settings have been found to include reductions in cognitive and linguistic functioning and memory (King and Laplante, 2005; Laplante et al., 2008; Entringer et al., 2009). The consequences of these effects might very well translate into lower educational attainment. León (2010) looks at the effects of the Peruvian internal conflict on educational attainment. His results show that violent events in the individuals' birth district have a negative effect on years of schooling. These effects are largely driven by events in early childhood (which he defines as 2 years before birth to age 3) and in pre-school years (ages 4 to 6). His empirical specifications for education do not report what happens in the year of birth. Rather León combines ages -2 to 3 into one category due to concerns of recall error in the CVR and measurement error in year of birth. From León (2010), it is therefore difficult to attribute the effects in his definition of early childhood to *in utero* effects which tend to have permanent effects on fetal development from life-course events. Indeed, the internal conflict also disrupted the provision of educational services: schools were often destroyed, teachers killed, and parents were reluctant to send their children to school for fears of insecurity (CVR, 2003; León, 2010).

2.3. Empirical Strategy

We follow the empirical strategy in Grimard et al. (2010) to evaluate the long term effects of the internal conflict in Peru on adult health outcomes. We are particularly interested in whether the effects are long-lived and the degree to which adult socio-economic conditions reduce or eliminate the effect of exposure *in utero* or in early infancy. In other words, we are seeking evidence on the relative importance of critical period programming and life-course mechanisms. If life-course mechanisms are strong enough, then there is a positive message to this paper: it would suggest that investments in human capital accumulation (education, income, nutrition) can reverse the deleterious effects of conflict. From a policy perspective, such a finding would justify policy interventions in hardest hit localities.[5]

We begin by estimating a reduced form version of equation (4), which excludes adult socio-economic conditions (such as education and wealth), where X_d^c are district fixed effects, X_{id}^b are birth year fixed effects, and $PROVTREND_{id}$ is a birth-year-province trend. District fixed effects

[5] We will come back to this point in the interpretation of the results and FONCODES and other programs in a subsequent version of the paper.

are included to account for district level heterogeneity (this could include district level health or nutrition programs, level of district economic development, etc…). Birth year fixed effects are included for two reasons. First, they measure age – some health outcomes can be age-dependent – and they allow capturing changes in the macroeconomic environment over time. Second, because they capture changes in the macroeconomic environment over time, they allow us to track the effects of economic crises which overlapped with the political crises. Paxson and Schady (2005) document a strong link between deep economic shocks in the 1980s and early 1990s and child health outcomes (infant mortality and birth weight, for instance). Since these are macroeconomic shocks, birth year effects should be therefore rather strong, especially given the link between the shocks and child health outcomes found in Paxson and Schady (2005). In fact, these birth year effects also speak to the research question at hand – are these macro shocks long lived? Province-birth-year effects are included to control for province specific changes in the economic environment over the lifecourse.[6] We also include other individual characteristics (such as native mother tongue, current location is urban, and marital status), captured in $\mathbf{X_{id}^{i}}$. The shocks vector $\mathbf{C_{id}}$ includes a variable for conflict exposure *in utero*, at birth, and in subsequent years:

$$h_{tid} = \beta_0 + \boldsymbol{\beta_1}\mathbf{C_{id}} + \boldsymbol{\beta_2}\mathbf{X_{id}^{i}} + X_d^c + X_{id}^b + PROVTREND_{id} + \varepsilon_{id} \quad (5)$$

The main hypothesis we test in equation (5) is whether $\mathbf{C_{id}}$ has a predictive and negative effect on health outcomes h_{tid}. Specifically, we are interested whether conflict exposure at birth and/or in the year preceding birth negatively affect h_{tid}. Recall that such a find would be only preliminary evidence of a critical period programming effect – if the $\mathbf{C_{id}}$ at in the birth year or in

[6] In this sense, our specification is similar to León (2010), though he includes a cubic province level time trend.

the year preceding birth are significant, it could be that they are important determinants of adult socio-economic status.

In order to check the extent to which the effect of conflict *in utero* or at birth on health outcomes via their life-course effects on adult socio-economic status, we sequentially add education (E_i) and wealth variables (Y_i) to equation (5). Education and wealth are believed to be extremely strong determinants of adult socio-economic status.[7] Thus, in addition to equation (5), we estimate:

$$h_{tid} = \beta_0 + \boldsymbol{\beta}_1 \mathbf{C}_{id} + \boldsymbol{\beta}_2 \mathbf{X}^{i}_{id} + \beta_3 E_{id} + X^{c}_{d} + X^{b}_{id} + PROVTREND_{id} + \varepsilon_{id} \quad (6)$$

$$h_{tid} = \beta_0 + \boldsymbol{\beta}_1 \mathbf{C}_{id} + \boldsymbol{\beta}_2 \mathbf{X}^{i}_{id} + \beta_3 E_{id} + \beta_4 Y_{id} + X^{c}_{d} + X^{b}_{id} + PROVTREND_{id} + \varepsilon_{id} \quad (7)$$

In summary, our empirical strategy is to test whether the district-level conflict shock *in utero* or in early infancy has long lasting effects on women's health outcomes. If we find that the effects of the shocks are significant, and that they persist when controlling for adult socio-economic factors, then our results would suggest the presence critical period programming effects. If we find that the effects weaken when controlling for adult socio-economic status, then the results would suggest that life-course mechanisms are also in effect.

3. Data

3.1. DHS

We employ the 5th wave of Peru Demographic and Health Survey (DHS), which is a continuous survey with five yearly cycles from 2004 to 2008. The total sample size is 46,073

[7] In Grimard et al. (2010), we show in the context of the elderly in Mexico that these do a decent job at capturing adult socio-economic status.

households, and is nationally representative. We utilize the individual recode for women between the ages of 15 to 49, which covers a sample of 41,648 women. The individual recode files include detailed information on women's health (mostly reproductive), their children's health, as well as numerous socio-economic characteristics. Information about the DHS can be found on their website, http://www.measuredhs.com/start.cfm. While the DHS for Peru includes earlier waves (1986, 1991-1992, 1996, 2000), including waves used to measure the effects of shocks on child health outcomes in Paxson and Schady (2005), we chose the 2004-2008 wave because many of the women born during the conflict would have reached adulthood by 2004. We are thus only now able to look at the long-term health effects (if any) of the conflict. In addition, the sample size is large, which will be important given the sample restrictions we are forced to make.

Indeed, a major limitation of the DHS is that to match events *in utero* or in early infancy, we require location codes (in Peru, this is the "ubigeo" which uniquely identifies districts). The DHS only provides current district. This means that we can only observe birth district for non-migrants. While 53.49% of the individual recode sample reported never moving, there is a concern that this leads to a selection bias because of importance of IDPs during the internal conflict (IDMC, 2007 and 2009). In fact, Laszlo and Santor (2009) show that the conflict was an important determinant of migration patterns during this period. Nevertheless, León (2010) who uses the Peruvian census, shows that the effect of the conflict did not have important differential effects across migrants and non-migrants in terms of their years of education (while the magnitudes of his point estimates are different, they lie within one standard error).

Another sample restriction that we will make is by cutting off age ranges. Since we are interested in adult outcomes, we restrict our sample to those born in or before 1990. We also wish to remove older women who would not have been affected by the conflict. One logical option would be to cut the sample of at 1980, which is the year in which the internal conflict began. Another option would be to extend the sample to several years before the conflict started.[8] Our base sample will thus include women born between 1980 and 1990, but we will also consider wider sample by extending the earlier cutoff to 1975 and 1990.

We will consider a number of dependent variables to capture physical and psychosocial health. Specifically, we will consider height, weight and anemia for physical health. Height is an important health indicator of long term health (Elo and Preston, 1992; Case et al., 2005; Almond, 2006; Strauss and Thomas, 2008 and Maccini and Yang, 2009), and according to research cited in Elo and Preston (1992) is pre-determined in early childhood. Height is thus an adequate measure of health to capture long-lasting effects of conditions very early in life. The availability of weight data allows us to construct a BMI index, which is also indicative of overall health (O'Donnell et al., 2008). The medical literature provides hormonal explanations linking prenatal stress and long-term effects on adult BMI (Welberg and Seckl, 2001; Dahlgren et al, 2001; Mueller and Bale, 2006).

A third indicator of women's health that we extract from the DHS is anemia. Anemia is a condition characterized by an insufficiency of red blood cells. This means that the body does not receive enough oxygen, which in severe cases can seriously harm human organs (such as the brain and the heart) and can even cause death (World Health Organization, 2008). Medical literature links iron deficiency in early life with anemia (Oski, 1993) and prenatal stress has been shown to lead to offspring iron deficiencies among non-human primates (Coe et al.; 2007)

In addition we consider psychosocial variables. First, the DHS reports data on whether individuals were ever submitted to any emotional domestic abuse. It also asks if individuals experienced any domestic violence. The World Health Organization produced a report on

[8] León (2010) extends to 1975 to use individuals born between 1975 and 1980 as a control group. While this paper came out after we began working on our paper, we include specifications which includes the León year cut-offs and do robustness checks.

violence and health in 2002, outlining the relationship between domestic abuse and its long-term consequences: mental health disorders (including depression), chronic pain, gastrointestinal disorders, reproductive health consequences in addition to the physical injuries sustained during the abuse (World Health Organization, 2002).

Table 1 presents the mean and standard deviations for two samples of the DHS data set. The first one comprises of women born between 1980 and 1990 whereas the second adds women born between 1975 and 1980. Two columns show, besides the number of observations and the age of the individuals, the summary statistics appear to be similar across both samples. Women have an average height of 1.5 meters, and about 26 percent are reported to be affected with anemia. More than 60% of women appear to have a BMI that is outside the normal range. These women have on average 10 years of education and do not appear to be in the poorest quintile of the wealth distribution, as measured by the DHS. Indeed only 10% are reported to be in the poorest quintile whereas the other women appear about equally likely to be in the other quintiles (with a slight increase, 24% for the richest quintile). Finally, in terms of domestic violence, around 30% of women in both samples have reported experiencing emotional of physical domestic abuse. It is possible that individuals who were born and lived in a civil conflict environment might be more susceptible to experience domestic violence in one way or another. It could be that people who lived in a violent environment during their formative years have difficulties escaping other forms of violence in their adult years, either because it was relatively difficult to find partners who did not resort to violence or because they live in locations where violence is more tolerated than elsewhere and these individuals have not migrated. The rates are slightly lower in (and not statistically different from) the 1980-1990 sample.

3.2. CVR

In 2003, Peru's Truth and Reconciliation Commission (*Comisión de Verdad y Reconciliación,* CVR) produced a 9 tome final report on their investigations of the Peruvian internal conflict between 1980 and 2000. This final report (CVR, 2003) collected detailed information about violent events that took place where the perpetrators where either from the *Sendero Luminoso,* a Mao-inspired rebel movement whose goal was to eliminate the drastic economic and social inequalities experienced in Peru, the *Movimiento Revolucionar Tupac Amarú* (MRTA, infamous for its' siege of the Japanese Embassy in 1997), government forces, and local *rondas campesinas*.[9] The report outlines the time-space progression of the conflict, which originated in 1980s mostly with the *Sendero Luminoso's* subversive activities in the rural Sierra (especially in the Ayacucho department) and by the capture of its leader Abimael Guzmán in 1992 had reached urban Lima. With Guzmán's capture, the *Sendero Luminoso* activities were drastically hampered, which translated in a tremendous drop in violent activity. Over the two decades, it is estimated that almost 70,000 Peruvians died or disappeared.

Figure 1 (figure 23 from Tome I of the CVR, 2003) shows the country-wide total number of deaths and disappearances between 1980 and 2000. We observe a severe peak in 1984 and another in 1989, and we observe a drastic fall following Guzmán's capture in 1992. Since our identification strategy relies on birth district conflict, Figure 2 (figure 24 from Tome I of the CVR) provides a useful picture. We see that, especially up to the early 1990's, there is significant heterogeneity in the location of the conflict (measured here as the number of deaths and disappearances).

We use district level number of deaths and disappearances from the CVR. The only two other economic studies which we are aware of that also used CVR data (Laszlo and Santor, 2009;

[9] The full report and data can be freely accessed on line at: http://www.cverdad.org.pe/ifinal/index.php

León, 2010) also use birth district-level total deaths and disappearances. While the CVR also publishes district level data on other forms of violence (assaults, sexual assaults, kidnappings, etc…), we focus on the most extreme cases of deaths and disappearances. To construct our main independent variable, we aggregate all deaths and disappearances at the district level for each year. We then match this district-year value to the birth district – birth year of the individual in the DHS. We include in our regressions this value annually for each of the two years before birth, at birth and for first five years of life.

Table 1 reports the summary statistics for these indicator variables. For instance, the first column shows that for 15% of women in the 1980-1990 sample were born in a district which experienced conflict related deaths and disappearances in the year of their birth. Similarly, 17% of women were born in a district which experienced conflict related deaths and disappearances a year after their birth. At age 5 over 20% of the sample lived in districts which experienced deaths and disappearances. Our identification strategy then relies on the shocks that these women faced compared to those women who resided in districts that did not directly experience conflict related deaths and disappearances.

3.3. Public Health Infrastructure Census

We also appeal to census data on health infrastructure, provided by Peru's Ministry of Health, *Censo de Infraestructura Sanitaria*.[10] The census, which is intended to capture the universe of public and private health facilities, was conducted in three years: 1992, 1996 and 1999. While there is a wealth of information available, we use district-level public health infrastructure data

[10] We thank Dr. Loyola Garcia-Frias from Peru's *Ministero de Salud* (Oficina General de Estadística e Informática) for giving us access to the Health Infrastructure Census.

from 1992, 1996 and 1999. This data will help us assess the role that public health plays in determining women's health outcomes.

4. Results

4.1. Main results

In this section, we present the results of estimating equations (5), (6) and (7), for a series of dependent variables described in Section 3. The tables only show the estimated coefficients on the conflict variables, years of education and wealth dummies. While we do not report the results of other controls, all specifications include birth year dummies, district fixed effects, the province-birth year trend, and controls for whether the respondent resides in an urban area, whether she is married, and whether her mother tongue is indigenous (Quechua, Aymara or other indigenous).

We consider two samples: one from 1980 to 1990 and another from 1975 to 1990. We interpret effects of birth district conflict related deaths and disappearances at given years as relative to a "control" group which is made up of individuals who did not experience conflict in their birth districts. In the 1975-1980 sample, the "control" group also includes individuals born before the conflict. Since we are seeking evidence of the critical period programming hypothesis, this is a reasonable "control".

Tables 2 to 5 present the results from estimating equations (5) to (7) for each dependent variable: height, normal BMI, anemia, victim of emotional domestic abuse and victim of physical domestic abuse. Each table presents the results corresponding to the 1980-1990 sample in the first three columns, and those corresponding to the 1975-1990 sample in the last three columns.

For each sample, the first column does not include any adult socio-economics, the second adds education, and the third adds wealth quintiles.

The results on height (table 2) show a pattern similar to what has been found in other contexts: shocks at birth are important determinants of height. The results show that birth district deaths and disappearances in the year preceding birth have a negative effect on a woman's stature. Conflict at other ages is not significantly estimated. Not conditioning for education and wealth, the effect of conflict at birth is smaller and more weakly statistically significant in the 1980-1990 sample compared to the 1975-1990 sample. However, the effect becomes insignificant once we control for education and wealth. Moving across columns in the 1975-1990 sample, however, reveals a pattern which is consistent with the critical period programming hypothesis: the effect is robust to including education and wealth. In fact, the coefficients on birth district deaths and disappearances in the year preceding birth reduce only slightly in magnitude when adding education and wealth (which themselves have predictive power), though they are not statistically different from each other. The implication of these results is that effects of shocks *in utero* or at birth on height are irreversible, and is consistent with the literature on the determinants of height, which suggest that height is predetermined by an early age (Elo and Preston, 1992) and that the uterine environment is particularly important. Why would the results be different when we restrict the sample only to women who were born during the internal conflict? It suggests that the effect in the first three columns is really being picked up by the pre-conflict "control" group. Intuitively, the results across samples can be interpreted to mean that the conflict had an effect even on women born in districts that did not suffer deaths and disappearances in their birth year or *in utero*.

This interpretation is reasonable since the conflict had more widespread effects: while one district might have suffered deaths or disappearances, the conflict also disrupted food distribution and labour movements, which would have severely affected a household's ability to nourish itself. Since nutritional deficiencies are at the core of Barker's hypothesis, our results suggest that the effect shock of conflict was widespread and that extreme events in one's birth district had little additional effect on height.

The results on BMI (table 3) are different. Here the dependent variable is a dummy variable for whether the woman has a normal BMI, and the regression was estimated as a linear probability model with fixed effects. The results show no effect in the 1975-1990 sample, while the 1980-1990 sample show that birth district deaths and disappearances at birth and at age 1 predict that a woman will be less likely to have a normal BMI. These effects are persistent as we add education and wealth variables, and so are not inconsistent with the critical period programming hypothesis.

Table 4 provides the results on whether the woman is anemic. District level deaths and disappearances only appear to increase the probability that a woman is anemic if they occur at age 2. This result is robust to including education and wealth variables and across both samples. Puzzlingly, however, the conflict decreases the probability of anemia if at age 3 and age 5 in the 1975-1990 sample only, though the significance level is only at the 10% level. Clearly, this suggests that anemia is influenced by life-course events and that more needs to be done here to understand how various events can affect how women suffer from anemia, including the availability of feeding programs, clinical and health services that they may receive throughout their years.

Tables 5 and 6 show the results on two variables used for measuring psychosocial health: whether the respondent is a victim of emotional or physical domestic abuse. Table 5 shows that conflict in the birth district in early ages have no effect on whether women report experiencing emotional domestic abuse. In table 6, however, we find a negative correlation between deaths and disappearances in the year before birth on the probability women report experiencing physical domestic abuse. While this finding may seem counterintuitive, the conflict may cause women to be less trusting and more fearful, and so may make more careful relationship choices.[11]

4.2. The Role of Public Health

Our main results point to some evidence of long-term effects of the civil conflict *in utero* and in early childhood, but that this result is somewhat weakened by including adult socio-economic status (education and wealth). In other words, our results point to both critical period programming and life course mechanisms being present. Given the potential of life-course mechanisms in our sample, the next step is to investigate whether public health interventions have been able to mitigate some of the negative effects of the conflict on women's health. This is a particularly important avenue of investigation from a policy perspective. Indeed, the 1990s saw significant health reforms, largely beginning in 1994 (Cotlear, 2000; Valdivia, 2002; and Rousseau, 2007). These reforms came on the heels of a near "complete collapse" of public health services (Rousseau, 2007) in the 1980s as a result of civil conflict and the deep economic crisis. The 1990s, under President Alberto Fujimori, the Peruvian government invested tremendously in public health and public health infrastructure (Cotlear, 2000; Valdivia, 2002; and Rousseau, 2007), including through the Peruvian Social Fund – FONCODES (Van Der Gaag, 1995).

We thus augment our regression in equation (7) with data from the Health Infrastructure Census. Specifically, we include the total number of public health facilities at the district level in 1992, 1996 and 1999. We have three hypotheses here. First, the availability of health facilities should be associated with better health outcomes. Second, the effect of civil conflict should be smaller in regions where there were public health facilities. Two identification problems immediately arise in that the first observation for health infrastructure (1992) occurs i) after the

[11] A result we will explore in more depth in a subsequent revision.

women in our sample were born and ii) once the civil conflict is greatly reduced. Third, since public health investments increased in the 1990s, we expect the health effects to be more widespread in 1992 and 1999.

Table 7 presents the results of equation (7) augmented with the health infrastructure data for the case of height. We report the results on height because it is our preferred measure of women's health, which is an indicator of long-term health (results on the other dependent variables are available upon request). Because we are including district-level variables in this analysis, we cannot include district fixed effects. Table 7 thus reports the same 3 columns as in Table 2 for comparison, and repeats column (3) without fixed effects in column (4) and in column (5) adds the health infrastructure variables.

We first observe the sensitivity of our results to the exclusion of district fixed effects. Indeed, the results excluding district fixed effects suggest that the critical period programming effect may still be in effect in the 1980-1990 sample (as opposed to what the fixed effects results showed in Table 3 for this sample).

Second, we observe that our first hypothesis holds: Total public health centers in 1992 have a positive and significant effect on height: the availability of public health services improves health outcomes.

Third, the second hypothesis posited that the availability of public health services should mitigate the negative effects of the conflict. The way to see whether this is true here is to compare the results on birth district level deaths and disappearances across columns (4) and (5). While it is true that the coefficient on birth district level deaths and disappearances 1 year before death falls in magnitude when controlling for district level health facilities, the coefficients are not statistically significantly different from each other.

Finally, the third hypothesis suggested that health effects of public health investments should become more widespread over time. We find that only the public health facilities in 1992 are statistically significant. One possible interpretation is that the closer in time to the conflict, the more likely health facilities can mitigate the negative effects on height. Another possible interpretation is that public health became so widespread by 1996 and 1999 that the variation in these variables falls to the extent that they no longer explain variation in health status. This interpretation has support given that Valdivia (2002) finds that the expansion in facilities did not lead to an increase in the equitability in health services utilization.

5. Conclusions

In this paper, we investigate the long term health implications for women of strong negative shocks at birth, namely the Peruvian internal conflict during the 1980s. Using data from the Peru Demographic and Health Survey on women's health and psycho-social outcomes (anthropometrics, anemia, and domestic abuse), and data from Peru's Truth and Reconciliation Committee Report on deaths and disappearances in birth districts, we investigate the importance of critical period programming and life-course mechanisms of the socio-economic determinants of health. Specifically, we find that the effects of the shock at birth are long-lived, especially for height, even controlling for adult socio-economic conditions such as education and wealth. Meanwhile, we find only moderate effects on domestic abuse, suggesting that women may be particularly (psychosocially) resilient.

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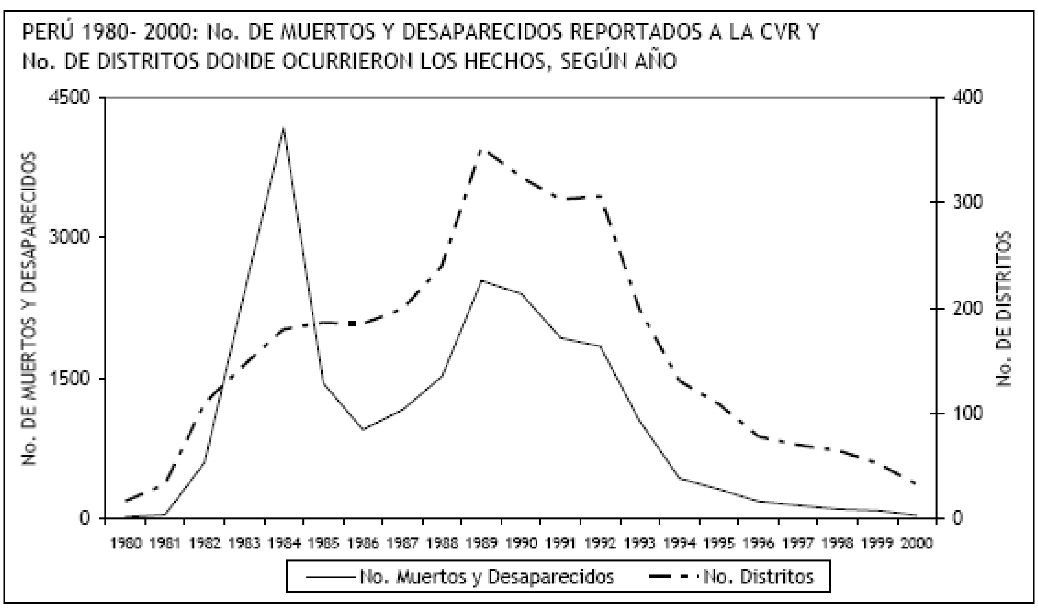
Figure 1 - total deaths and disappearances

Source: CVR (2003), Tome I, Figure 24, page 133.

Figure 2 - District distribution of Deaths and Disappearances



Source: CVR (2003), Tome I, Figure 24, page 133.

Table 1 - Descriptive Statistics

	1980-1990 sample	1975-1990 sample
height (cm)	1522.79	1522.18
	(56.790)	(56.960)
anemia	0.2600	0.2600
	(0.440)	(0.440)
bmi indicator	0.66	0.61
	(0.470)	(0.490)
emotial domestic abuse indicator	0.26	0.28
	(0.440)	(0.450)
Some domestic abuse indicator	0.32	0.34
	(0.470)	(0.470)
age (years)	21.01	23.13
	(3.420)	(4.810)
Birth district deaths and disappearances 2 years before birth	0.09	0.07
	(0.300)	(0.260)
Birth district deaths and disappearances 1 year before birth	0.13	0.1
	(0.340)	(0.300)
Birth district deaths and disappearances at birth	0.15	0.11
	(0.340)	(0.310)
Birth district deaths and disappearances at age 1	0.17	0.13
	(0.370)	(0.330)
Birth district deaths and disappearances at age 2	0.2	0.15
	(0.390)	(0.360)
Birth district deaths and disappearances at age 3	0.22	0.17
	(0.410)	(0.380)
Birth district deaths and disappearances at age 4	0.22	0.176
	(0.414)	(0.381)
Birth district deaths and disappearances at age 5	0.205	0.1722
	(0.404)	(0.377)
Years of Education	10.15	10.07
	(3.350)	(3.680)
Poorest wealth quintile	0.09	0.1
	(0.290)	(0.300)
Second wealth poorest quintile	0.22	0.22
	(0.420)	(0.420)
Middle wealth quintile	0.24	0.23
	(0.420)	(0.420)
Second richest wealth quintile	0.21	0.2
	(0.410)	(0.400)
Richest wealth quintile	0.24	0.24
	(0.430)	(0.430)
	(5750 obs.)	**(7722 obs.)**

Table 2: Regressions on Height

	(1)	(2)	(3)	(4)	(5)	(6)
		1980-1990 sample			1975-1990 sample	
Birth district deaths and disappearances 2 years before birth	4.666	5.343	4.867	4.834	5.466	4.895
	(3.209)	(3.202)*	(3.221)	(3.074)	(3.040)*	(3.039)
Birth district deaths and disappearances 1 year before birth	**-5.641**	**-4.670**	**-4.431**	**-5.895**	**-5.342**	**-5.130**
	(2.917)*	**(3.063)**	**(3.161)**	**(2.818)****	**(2.910)***	**(3.002)***
Birth district deaths and disappearances at birth	2.785	2.546	2.991	1.673	1.272	1.655
	(2.944)	(2.912)	(2.962)	(2.814)	(2.783)	(2.820)
Birth district deaths and disappearances at age 1	0.143	0.415	0.62	-0.45	-0.233	0.034
	(2.999)	(2.993)	(2.975)	(2.943)	(2.928)	(2.940)
Birth district deaths and disappearances at age 2	3.029	2.406	2.386	3.083	2.233	2.135
	(2.537)	(2.406)	(2.392)	(2.318)	(2.234)	(2.246)
Birth district deaths and disappearances at age 3	-3.285	-3.273	-3.412	-3.224	-3.273	-3.501
	(2.500)	(2.428)	(2.409)	(2.276)	(2.250)	(2.237)
Birth district deaths and disappearances at age 4	-1.377	0.195	-0.101	-2.577	-1.295	-1.402
	(3.091)	(3.020)	(3.060)	(2.529)	(2.477)	(2.529)
Birth district deaths and disappearances at age 5	-0.282	0.215	0.261	1.956	2.306	2.503
	(2.490)	(2.472)	(2.460)	(2.351)	(2.304)	(2.286)
Years of Education		3.78	3.113		3.509	2.823
		(0.343)***	(0.335)***		(0.277)***	(0.276)***
Poorest wealth quintile			--			--
			--			--
Second wealth poorest quintile			-5.821			-2.013
			(3.357)*			(2.757)
Middle wealth quintile			0.968			4.715
			(3.928)			(3.411)
Second richest wealth quintile			6.701			10.408
			(4.214)			(3.691)***
Richest wealth quintile			17.706			21.859
			(4.382)***			(3.767)***
Constant	631.392	179.675	409.003	957.622	543.739	710.059
	(842.713)	(857.789)	(851.705)	(512.262)*	(513.059)	(506.442)
Observations	5750	5750	5750	7722	7722	7722
Number of ubidist	586	586	586	600	600	600
R-squared	0.01	0.04	0.05	0.01	0.03	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses are clustered at the district level. All specifications include controls for marital status, mother's native tongue is indigenous, current location is urban, birth year dummies, district fixed effects, and province-birth year effects.

Table 3: Regressions on Normal BMI

	(1)	(2)	(3)	(4)	(5)	(6)
		1980-1990 sample			1975-1990 sample	
Birth district deaths and disappearances 2 years before birth	0.002	0.004	0.003	-0.001	0	-0.002
	(0.023)	(0.024)	(0.024)	(0.023)	(0.023)	(0.023)
Birth district deaths and disappearances 1 year before birth	0.023	0.026	0.027	0.026	0.027	0.027
	(0.021)	(0.021)	(0.021)	(0.020)	(0.020)	(0.020)
Birth district deaths and disappearances at birth	-0.031	-0.031	-0.030	-0.026	-0.026	-0.025
	(0.018)*	(0.018)*	(0.018)	(0.017)	(0.017)	(0.017)
Birth district deaths and disappearances at age 1	-0.033	-0.031	-0.028	-0.027	-0.026	-0.024
	(0.019)*	(0.019)	(0.019)	(0.020)	(0.020)	(0.020)
Birth district deaths and disappearances at age 2	-0.023	-0.023	-0.024	-0.013	-0.013	-0.014
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Birth district deaths and disappearances at age 3	0.024	0.024	0.023	0.02	0.021	0.02
	(0.020)	(0.020)	(0.020)	(0.018)	(0.018)	(0.018)
Birth district deaths and disappearances at age 4	-0.028	-0.024	-0.026	-0.01	-0.007	-0.008
	(0.016)*	(0.016)	(0.016)	(0.014)	(0.014)	(0.014)
Birth district deaths and disappearances at age 5	0.000	0.000	0.000	0.006	0.006	0.007
	(0.017)	(0.017)	(0.017)	(0.015)	(0.016)	(0.016)
Years of Education		0.02	0.017		0.017	0.013
		(0.002)***	(0.002)***		(0.002)***	(0.002)***
Poorest wealth quintile			--			--
			--			--
Second wealth poorest quintile			0.014			-0.006
			(0.027)			(0.022)
Middle wealth quintile			0.024			0.011
			(0.032)			(0.027)
Second richest wealth quintile			0.028			0.018
			(0.034)			(0.029)
Richest wealth quintile			0.133			0.112
			(0.039)***			(0.032)***
Constant	-5.608	-8.61	-7.944	-5.01	-7.425	-6.636
	(7.511)	(7.664)	(7.445)	(4.635)	(4.698)	(4.639)
Observations	9006	9006	9006	12055	12055	12055
Number of ubidist	744	744	744	761	761	761
R-squared	0.02	0.03	0.04	0.03	0.03	0.04

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses are clustered at the district level. All specifications include controls for marital status, mother's native tongue is indigenous, current location is urban, birth year dummies, district fixed effects, and province-birth year effects.

Table 4: Regressions on Anemia

	(1)	(2)	(3)	(4)	(5)	(6)
		1980-1990 sample			1975-1990 sample	
Birth district deaths and disappearances 2 years before birth	-0.019	-0.018	-0.019	-0.025	-0.025	-0.025
	(0.027)	(0.027)	(0.027)	(0.025)	(0.025)	(0.025)
Birth district deaths and disappearances 1 year before birth	0.000	0.000	0.001	0.001	0.001	0.001
	(0.025)	(0.025)	(0.025)	(0.023)	(0.023)	(0.023)
Birth district deaths and disappearances at birth	-0.001	-0.001	0.000	0.001	0.001	0.001
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Birth district deaths and disappearances at age 1	-0.018	-0.018	-0.019	-0.018	-0.018	-0.019
	(0.025)	(0.025)	(0.025)	(0.023)	(0.023)	(0.023)
Birth district deaths and disappearances at age 2	0.051	0.051	0.051	0.054	0.054	0.054
	(0.026)**	(0.026)**	(0.026)**	(0.024)**	(0.024)**	(0.024)**
Birth district deaths and disappearances at age 3	-0.038	-0.038	-0.038	-0.03	-0.03	-0.03
	(0.022)*	(0.022)*	(0.022)*	(0.022)	(0.022)	(0.022)
Birth district deaths and disappearances at age 4	0.007	0.008	0.008	0.009	0.009	0.009
	(0.026)	(0.026)	(0.026)	(0.023)	(0.023)	(0.023)
Birth district deaths and disappearances at age 5	-0.043	-0.043	-0.042	-0.031	-0.031	-0.031
	(0.022)*	(0.022)*	(0.022)*	(0.021)	(0.021)	(0.021)
Years of Education		0.001	0.002		0.001	0.002
		(0.003)	(0.003)		(0.002)	(0.002)
Poorest wealth quintile			0.069			0.061
			(0.044)			(0.037)*
Second wealth poorest quintile			-0.004			0.014
			(0.029)			(0.027)
Middle wealth quintile			0.034			0.041
			(0.020)			(0.020)**
Second richest wealth quintile			0.032			0.03
			(0.019)*			(0.019)
Richest wealth quintile			0			0
			0			0
Constant	-12.125	-12.216	-12.321	-4.505	-4.578	-4.943
	-8.478	-8.497	-8.567	(4.946)	(4.948)	(4.913)
Observations	5232	5232	5232	7011	7011	7011
Number of ubidist	577	577	577	596	596	596
R-squared	0.01	0.01	0.01	0.01	0.01	0.01

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses are clustered at the district level. All specifications include controls for marital status, mother's native tongue is indigenous, current location is urban, birth year dummies, district fixed effects, and province-birth year effects.

Table 5: Results on Victim of Emotional Domestic Abuse

	(1)	(2)	(3)	(4)	(5)	(6)
		1980-1990 sample			1975-1990 sample	
Birth district deaths and disappearances 2 years before birth	0.003	0.001	0.006	-0.02	-0.021	-0.016
	(0.057)	(0.057)	(0.058)	(0.051)	(0.052)	(0.052)
Birth district deaths and disappearances 1 year before birth	-0.041	-0.044	-0.052	-0.042	-0.043	-0.050
	(0.049)	(0.049)	(0.049)	(0.042)	(0.041)	(0.041)
Birth district deaths and disappearances at birth	-0.086	-0.084	-0.086	-0.048	-0.045	-0.046
	(0.043)**	(0.043)*	(0.044)*	(0.041)	(0.041)	(0.041)
Birth district deaths and disappearances at age 1	0.031	0.026	0.024	0.034	0.029	0.029
	(0.040)	(0.041)	(0.041)	(0.035)	(0.035)	(0.035)
Birth district deaths and disappearances at age 2	0.000	-0.001	0.002	-0.001	0.000	0.003
	(0.038)	(0.038)	(0.038)	(0.033)	(0.033)	(0.033)
Birth district deaths and disappearances at age 3	0.014	0.015	0.012	-0.003	-0.002	-0.003
	(0.040)	(0.040)	(0.040)	(0.032)	(0.032)	(0.032)
Birth district deaths and disappearances at age 4	-0.015	-0.018	-0.014	-0.031	-0.036	-0.033
	(0.043)	(0.043)	(0.043)	(0.031)	(0.032)	(0.032)
Birth district deaths and disappearances at age 5	-0.011	-0.012	-0.007	0.011	0.010	0.011
	(0.041)	(0.041)	(0.041)	(0.030)	(0.029)	(0.029)
Years of Education		-0.008	-0.006		-0.011	-0.009
		(0.004)**	(0.004)		(0.003)***	(0.003)***
Poorest wealth quintile			0.053			0.049
			(0.059)			(0.040)
Second wealth poorest quintile			0.084			0.084
			(0.051)*			(0.035)**
Middle wealth quintile			0.090			0.101
			(0.039)**			(0.030)***
Second richest wealth quintile			0.010			0.036
			(0.041)			(0.026)
Richest wealth quintile			--			--
			--			--
Constant	-8.497	-6.995	-7.163	3.822	5.006	4.545
	(14.180)	(14.217)	(14.163)	(5.501)	(5.446)	(5.420)
Observations	2355	2355	2355	4382	4382	4382
Number of ubidist	597	597	597	699	699	699
R-squared	0.08	0.08	0.09	0.07	0.08	0.08

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses are clustered at the district level. All specifications include controls for marital status, mother's native tongue is indigenous, current location is urban, birth year dummies, district fixed effects, and province-birth year effects.

Table 6: Results on Victim of Physical Domestic Abuse

	(1)	(2)	(3)	(4)	(5)	(6)
		1980-1990 sample			1975-1990 sample	
Birth district deaths and disappearances 2 years before birth	0.035	0.033	0.036	0.022	0.022	0.026
	(0.060)	(0.059)	(0.059)	(0.055)	(0.054)	(0.054)
Birth district deaths and disappearances 1 year before birth	-0.071	-0.075	-0.082	-0.095	-0.096	-0.102
	(0.048)	(0.048)	(0.050)*	(0.044)**	(0.044)**	(0.045)**
Birth district deaths and disappearances at birth	0.028	0.031	0.029	0.025	0.028	0.026
	(0.042)	(0.042)	(0.042)	(0.040)	(0.040)	(0.040)
Birth district deaths and disappearances at age 1	0.031	0.025	0.024	0.006	0.001	0.003
	(0.041)	(0.041)	(0.041)	(0.037)	(0.037)	(0.037)
Birth district deaths and disappearances at age 2	-0.048	-0.049	-0.048	-0.015	-0.013	-0.013
	(0.039)	(0.039)	(0.039)	(0.035)	(0.035)	(0.035)
Birth district deaths and disappearances at age 3	0.013	0.014	0.014	-0.006	-0.004	-0.003
	(0.039)	(0.040)	(0.039)	(0.030)	(0.030)	(0.030)
Birth district deaths and disappearances at age 4	-0.011	-0.015	-0.011	-0.024	-0.029	-0.027
	(0.034)	(0.033)	(0.032)	(0.026)	(0.026)	(0.026)
Birth district deaths and disappearances at age 5	-0.005	-0.006	-0.004	-0.023	-0.025	-0.025
	(0.047)	(0.047)	(0.048)	(0.032)	(0.031)	(0.032)
Years of Education		-0.011	-0.008		-0.012	-0.009
		(0.004)**	(0.005)		(0.003)***	(0.003)***
Poorest wealth quintile			0.135			0.119
			(0.063)**			(0.045)***
Second wealth poorest quintile			0.121			0.133
			(0.052)**			(0.037)***
Middle wealth quintile			0.083			0.113
			(0.042)*			(0.029)***
Second richest wealth quintile			0.018			0.072
			(0.039)			(0.024)***
Richest wealth quintile			--			--
			--			--
Constant	-5.621	-3.715	-4.087	8.083	9.393	8.727
	(14.309)	(14.338)	(14.371)	(6.475)	(6.424)	(6.374)
Observations	2355	2355	2355	4382	4382	4382
Number of ubidist	597	597	597	699	699	699
R-squared	0.04	0.04	0.05	0.04	0.05	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses are clustered at the district level. All specifications include controls for marital status, mother's native tongue is indigenous, current location is urban, birth year dummies, district fixed effects, and province-birth year effects.

Table 7: Results on Height, including Public Health Facilities in 1992, 1996 and 1999.

	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	1980-1990 sample					1975-1990 sample				
Birth district deaths and disappearances 2 years before birth	4.666	5.343	4.867	1.535	1.697	4.834	5.466	4.895	1.44	1.521
	(3.209)	(3.202)*	(3.221)	(3.009)	(3.104)	(3.074)	(3.040)*	(3.039)	(2.983)	(3.072)
Birth district deaths and disappearances 1 year before birth	**-5.641**	-4.67	-4.431	-6.355	-6.087	**-5.895**	**-5.342**	**-5.130**	**-6.676**	**-6.435**
	(2.917)*	(3.063)	(3.161)	(2.956)**	(2.940)**	**(2.818)****	**(2.910)***	**(3.002)***	**(2.932)****	**(2.905)****
Birth district deaths and disappearances at birth	2.785	2.546	2.991	-0.617	-0.688	1.673	1.272	1.655	-0.84	-0.922
	(2.944)	(2.912)	(2.962)	(2.784)	(2.754)	(2.814)	(2.783)	(2.820)	(2.799)	(2.779)
Birth district deaths and disappearances at age 1	0.143	0.415	0.62	0.165	-0.494	-0.45	-0.233	0.034	0.077	-0.448
	(2.999)	(2.993)	(2.975)	(2.852)	(2.864)	(2.943)	(2.928)	(2.940)	(2.846)	(2.875)
Birth district deaths and disappearances at age 2	3.029	2.406	2.386	1.707	1.585	3.083	2.233	2.135	1.338	1.207
	(2.537)	(2.406)	(2.392)	(2.332)	(2.288)	(2.318)	(2.234)	(2.246)	(2.286)	(2.254)
Birth district deaths and disappearances at age 3	-3.285	-3.273	-3.412	-4.103	-4.679	-3.224	-3.273	-3.501	-4.292	-4.633
	(2.500)	(2.428)	(2.409)	(2.372)*	(2.368)**	(2.276)	(2.250)	(2.237)	(2.242)*	(2.239)**
Birth district deaths and disappearances at age 4	-1.377	0.195	-0.101	-0.335	-0.961	-2.577	-1.295	-1.402	-1.55	-1.982
	(3.091)	(3.020)	(3.060)	(2.756)	(2.855)	(2.529)	(2.477)	(2.529)	(2.474)	(2.535)
Birth district deaths and disappearances at age 5	-0.282	0.215	0.261	-1.539	-1.958	1.956	2.306	2.503	1.324	0.94
	(2.490)	(2.472)	(2.460)	(2.328)	(2.291)	(2.351)	(2.304)	(2.286)	(2.176)	(2.138)
Years of Education		3.78	3.113	3.093	3.082		3.509	2.823	2.647	2.617
		(0.343)***	(0.335)***	(0.294)***	(0.294)***		(0.277)***	(0.276)***	(0.251)***	(0.253)***
Poorest wealth quintile			-5.821	-3.154	-3.945			-2.013	1.983	1.442
			(3.357)*	(2.902)	(2.958)			(2.757)	(2.406)	(2.417)
Second wealth poorest quintile			0.968	2.069	1.317			4.715	8.202	7.625
			(3.928)	(3.289)	(3.370)			(3.411)	(2.898)***	(2.947)***
Middle wealth quintile			6.701	10.745	9.227			10.408	16.42	15.366
			(4.214)	(3.349)***	(3.491)***			(3.691)***	(2.933)***	(3.016)***
Second richest wealth quintile			17.706	25.915	23.666			21.859	32.114	30.407
			(4.382)***	(3.700)***	(3.731)***			(3.767)***	(3.190)***	(3.159)***
Total public health centres in district (1992)					0.447					0.362
					(0.195)**					(0.170)**
Total public health centres in district (1996)					0.324					0.275
					(0.480)					(0.425)
Total public health centres in district (1999)					-0.581					-0.502
					(0.446)					(0.399)
Constant	631.392	179.675	409.003	1476.1	1477.206	957.622	543.739	710.059	1476.128	1476.886
	(842.713)	(857.789)	(851.705)	(4.999)***	(4.881)***	(512.262)*	(513.059)	(506.442)	(4.748)***	(4.743)***
Observations	5750	5750	5750	5750	5695	7722	7722	7722	7722	7651
Number of ubidist	586	586	586			600	600	600		
R-squared	0.01	0.04	0.05	0.12	0.12	0.01	0.03	0.05	0.12	0.12
District Fixed Effects	Y	Y	Y	N	N	Y	Y	Y	N	N

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors in parentheses are clustered at the district level. All specifications include controls for marital status, mother's native tongue is indigenous, current location is urban, birth year dummies, district fixed effects, and province-birth year effects.