Long-Term Consequences of Early Life Health Shocks: Evidence from the 1980s Peruvian Crisis

Federico H. Gutierrez[*†‡]

Vanderbilt University

July 2013

first version: October, 2012

Abstract

During the Peruvian economic crisis of the late 1980s, infant mortality significantly increased. This paper investigates the long-term consequences on health and education for infants who survived this period. Because no longitudinal data are available, the estimation of causal effects is performed combining two sets of difference-in-differences estimators. Results indicate that the detrimental health conditions associated with an additional percentage point in infant mortality makes children who survived the crisis 2.36 percentage points more likely to suffer a chronic illness and 2 percentage points less likely to complete primary education by age 15. A partial identification approach suggests that the attenuation bias due to selection for survival is relatively small.

Keywords: crisis, Perú, long-term consequences, early childhood, disease, nutrition

[*]Dep. of Economics, 421 Calhoun Hall

[†]e-mail: federico.h.gutierrez@Vanderbilt.Edu

[‡]I thank Armand Sim for valuable research assistance. I also thank comments and suggestions from seminar participants at Clemson University, Vanderbilt University and the PAC-DEC 2013 conference.

1 Introduction

Gestation and infancy constitute a crucial period in life. The body and the brain grow and develop at an exceptionally fast rate. This is also a period when a person is particularly vulnerable to health shocks. Insufficient nutrition and disease tend to increase the probability of death during the early years of life and, if the infant survives, may have lasting implications in adulthood. This problem is particularly relevant in developing countries and potentially exacerbated during economic downturns. This paper explores the long-term consequences of health shocks experienced in early childhood by those born in Perú during a severe economic crisis in the late 1980s.

Medical studies indicate a positive correlation between early childhood health and chronic illnesses later in life (Ericksson et al. (2000); Eriksson et al. (2001), Rich-Edwards et al. (1999), Singhal et al. (2001), Walker et al. (2001), WHO (2003)). However, most of these studies fail to establish a causal connection because they do not control for genetic and family background determinants. It is possible that the same genetic traits that make a person less healthy during childhood, make him more likely to suffer from chronic illness later in life. The correlation between health and unobservable family background characteristics is similarly problematic. A child who receives better care during the first year of life, probably because the mother has good knowledge of disease prevention, is more likely to have better health care during the rest of his life. For these reasons, the simple correlation between child health and chronic illness does little to answer the policy-relevant question of whether improving health during childhood improves health outcomes later in life.

This paper makes use of an exceptionally severe economic downturn to study the long-term consequences of early childhood health shocks. As a consequence of heterodox policies and previous mismanagement of the debt crisis (Glewwe and Hall (1994)), Perú experienced a sharp economic contraction in late 1980's. Between 1987 and 1990, the Peruvian GDP fell 30%, real wages in Lima decreased 80%, and inflation reached four digits. Previous evidence indicates that a consequence of the severe crisis was an increase in infant mortality resulting in 17,000 excess deaths (Paxson and Schady (2005)). Although the exact causes of the increased infant mortality cannot be clearly identified due to a lack of data, the evidence suggests that a decline in private and public health expenditure played an important role (Paxson and Schady (2005)).

In this paper, I focus on individuals who were born during the crisis but survived childhood. The same adverse conditions that increased infant mortality may have permanently affected the health of survivors and the outcomes that depend on health, such as education and employment. Combining information from eight repeated cross-sections (ENAHO survey from 2004 to 2011) with health information from the economic downturn (obtained retrospectively from DHS 1996 and DHS 2000), I exploit the within-cohort heterogeneous impact of the crisis and the between differences in cohort exposure observed at different points in time to determine if health shocks in infancy had any consequence on health and education in adolescence.

The intensity of the health shock depends on the year of birth (i.e., during or after the crisis) and the level of education of the mother. I estimate the contemporaneous impact on health by computing the differential probabilities of dying in the first year of life across these two dimensions using a standard difference-in-differences estimator. For the long-term effects, I follow a similar strategy but using a different sample representative of the same individuals obtained when they were 15 to 18 years old. In this case, the difference-in-differences is used to estimate the differential probabilities of suffering a chronic illness and the differential probabilities of completing primary school by ages 15 to 18. The ratio of the difference-in-differences estimator that measures the long-term effect of the crisis to the difference-in-differences estimator that measures the contemporaneous impact of the crisis is a particular case of the two-sample instrumental variable approach (TSIV) (Angrist and Krueger (1992)). It identifies the long-term causal effect of early life health shocks, measured as an increased probability of dying during the first year of life, on health and educational outcomes in teenage years. This is a novel strategy that combines a difference-in-differences estimator with a two-sample instrumental variables approach to estimate long-term effect when panel data are not available.

There are two common econometric problems when estimating long-term effects of early life health shocks: i) measurement error in the intensity of the exposure, and ii) endogeneity due to confounding elements (Meng and Qian (2009)). The instrumental variable approach embedded in this empirical strategy solves these two problems if the standard conditions for a difference-in-differences estimator holds.

The results indicate that those whose health were most affected by the crisis during their infancy were more likely to report a chronic illness and less likely to attend secondary school in their teenage years. The detrimental health conditions during the crisis associated with a 1% increase in the probability of dying cause the children who survived the crisis to be 2.36 percentage points more likely to suffer a chronic illness and 2 percentage points less likely to complete at least one grade in secondary school by ages 15 to 18. There is no statistically significant difference in the adolescent employment rate of those born during the crisis and those born afterwards.

The above-mentioned results are lower bound effects of the crisis because they may be downwardly biased by a selection effect. Those who died during the crisis may be infants who naturally had below-average health and consequently could not tolerate the health shock. To account for this problem, I use a partial identification approach that provides the lower bound and the upper bound effects of early life health shocks. The strategy consists of making two extreme assumptions about the health of infants who died during the crisis, had they survived and reached adolescence. The first of these extreme assumptions is that there was no selection effect. Assuming that the infants who died were naturally as healthy or unhealthy than those who survived provides the lower bound health impact of the crisis. To estimate the upper bound, the assumption is that there was a '*complete*' selection effect. In this case, I assume that all infants who died during the crisis would have suffered a chronic illness in teenage years, had they survived. Few papers in this literature deal with selection for survival effect (exceptions are Meng and Qian (2009) and Bozzoli et al. (2009)), and they generally impose additional assumptions. This is the first paper on this topic that uses a partial identification approach.

This paper contributes to the growing literature on the long-term consequences of fetal and infant health. Behrman and Rosenzweig (2004) estimate the returns to birthweight. They eliminate the influence of genetic endowments and family background by using a sample of identical twins from the U.S. They find a strong impact of birth weight on school attendance and adult height. Behrman and Rosenzweig (2004) identify the effects of birthweight assuming that in-the-womb nutritional differences between monozygotic twins are random. Black et al. (2007) use a similar strategy to estimate the short-term and long-term effects of low birth weight. Using a dataset from Norway, they find a significant impact of birth weight on IQ, earnings and education. In contrast to Behrman and Rosenzweig (2004) and Black et al. (2007), I use the heterogeneous impact of the crisis as an exogenous shock to fetal and infant health to identify its long-term consequences. In a cross-country study, Bozzoli et al. (2009) show that post-neonatal mortality correlates with adult height. However, in contrast to my paper, they do not show if post-neonatal mortality affects other dimensions of health as well as other outcomes that depend on health, such as education.

There are other papers in the literature that study how economic downturns affect child health (e.g., Paxson and Schady (2005) on infant mortality in Perú, and Bozzoli and Quintana-Domeque (2010) and Cruces et al. (2011) on low birth weight in Argentina). Nonetheless, none of these studies provide clear evidence on the long-term consequences of economic downturns. An exception is Hidrobo (2011) who examines how the Ecuador crisis affected the cognitive ability of children. She finds that five year-olds exposed to the crisis during their first three years of life had significantly lower vocabulary test scores. Nonetheless, she cannot identify how this reduction in cognitive skills affects education and labor market outcomes.

In section 2, I briefly discuss the background of the Peruvian crisis in the late 1980s. In section 3, I present the identification and estimation strategy. I use is a difference-in-differences estimator embedded in a two-sample instrumental variable approach. In section 4, I describe the different surveys used, and present descriptive statistics. In section 5, I analyze the heterogenous impact of the Peruvian crisis on infant health. These results constitute the first stage of the TSIV. In section 6, I use pooled cross sections to show if those who experience severe health socks during the crisis were more likely to suffer from chronic illness and less likely to complete primary education in teenage years. These results correspond to the second stage reduced form of the TSIV. In section 7, I use results from the previous sections to compute a two-sample instrumental variable approach. In section 8, I estimate the bias caused by a selection for survival effect using a partial identification approach. In section 9, I perform a robustness analysis to show that changes in the composition of mothers during the crisis does not threaten the identification strategy. In section 10, I eliminate the possibility that my results are driven by episodes of civil violence that occurred during the period. Finally in section 11, I conclude.

2 The Peruvian crisis

The 1980s were a difficult period for Latin America, and Perú was no exception. At the beginning of the decade, the *debt crisis* created strong external pressures on the economy. The increase in international interest rates made the already onerous debt service difficult to afford and took a larger portion of public sector revenues. Concurrently, the value of export goods declined, which hurt the balance of payments even more. In response to this situation, Perú implemented a series of heterodox stabilization policies in 1985. The most notable policy was the suspension of foreign debt payments and the use of these resources to stimulate the economy. As Glewwe and Hall (1994) indicate, the plan was successful in the short run and boosted consumer demand, but unsustainable due to strong inflationary pressures and severe fiscal deficits. In September 1988, the government was not able to continue on that path and announced a series of new policies that inevitably involved a sharp contraction of government expenditure. Figure 1 shows real expenditures of the Peruvian central government from 1980 to 2000. After the 1987 peak, government expenditures fell 43% in four years. This fiscal policy generated a substantial decline in the level of economic activity. Figure 2 shows real GDP per capita (PPP) for the same years. The figure shows a dramatic 28% contraction from 1987 to 1990.

The impoverishment of the population during the crisis had a negative impact on the health of children. Figure 3 shows infant mortality from 1984 to 1998. It is evident that the percentage of children dying in their first year of life significantly increased during the economic downturn. In 1990, infant mortality reached its peak at 7.8%. A year later it fell to 4.9%. The exact reasons of this spike in infant mortality are difficult to ascertain due to data limitations. Paxson and Schady (2005) suggest that a collapse in private and public expenditures on health could be an important cause of the increase in infant mortality. The same authors find no significant evidence of changes in food consumption, changes in the composition of women giving birth, or outbreaks of infectious diseases that can otherwise explain the behavior of infant mortality during the crisis.

Although the economic downturn affected the entire economy, the negative impact on the population was not homogeneous. Generally, the poor and less educated are more vulnerable to economic crises. This is because their ability to maintain consumption levels is constrained. Their incomes are low, sometimes close to subsistence. Consequently, they have little or no savings to cope with a negative income shock. They also lack good access to credit markets, and have a limited ability to smooth consumption by borrowing from friends and relatives in the face of aggregate economic shocks.[1]

Glewwe and Hall (1994) present evidence that the Peruvian crisis affected less educated people more severely. The total expenditure of households with household heads who completed primary education or less decreased by almost 60%, in contrast to a 52% decline among

[1]There is a vast literature in development economics about the importance of family-and-friends networks in smoothing consumption among the poor in developing countries. Theoretically and empirically, it is clear that these networks are particularly relevant in idiosyncratic shocks, but not aggregate shocks. See Townsend (1994)

households with more educated household heads. Accounting for the fact that less educated people tend to have lower incomes in normal times, a sharp decline during the crisis makes them more likely to cut back on basic needs in comparison to educated people. This may involve food and health services for children. In this paper, the heterogeneity in the way the crisis affected infants is used to identify the long-term consequences of early life health shocks in a context where no panel data are available.

3 Identification

In the context of the Peruvian crisis, the severity of nutritional deficiencies in the womb and the exposure to diseases during infancy depend on the date of birth of the child (during or after the crisis) and the level of education of the mother. Children born to less-educated mothers suffered the most during the crisis. A low level of education is associated with low income and possibly insufficient mechanisms to smooth consumption. As mentioned in the previous section (Glewwe and Hall (1994)), there is evidence that total expenditure fell more in households where the household head completed only primary education or less. Also, the decline in public health spending that occurred during the crisis (Paxson and Schady (2005)) more likely affected children from poor and less-educated households since the rich could afford private health care. In addition, highly educated mothers may have a better understanding of disease prevention and provide better care of their infants.

Identification relies on the plausible exogeneity of the interaction between the mother's education and the year the person was born. Table 1 illustrates this idea. Panel A shows the probability that a child dies in the first year of life if she is born during the crisis (years 1988-1990) or after the crisis (years 1991-1993)[2]. In both cases, this probability is computed

[2]The comparison is made with a cohort born after the crisis and not before to guarantee that its members were not exposed to the crisis at any age. Additionally, in 1986 there was a smaller crisis that may have affected younger cohorts.

conditional on the level of education of the mother. Classifying what years correspond to a crisis is arbitrary. In Perú, it seems clear that the crisis began in 1988. What is less clear is the exact date when it finished. I define the crisis as the years for which infant mortality sharply increased. These years correspond to the period when GDP abruptly declined.[3]

Table 1 panel A shows no significant change in the probability of death among children born to high-educated mothers. The change in 0.3 percentage points is small and statistically not different from zero. On the other hand, children born to low-educated mothers had higher probabilities of dying during the crisis. Among mothers with primary education or less, the probability of death was 1.9 percentage points higher for children born during the crisis than those born after the crisis. In the absence of a crisis, the gap between low-educated and high-educated mothers was 3.2 percentage points. Assuming that this gap would have prevailed in the years 1988-1990 had the crisis not occurred, the 1.6 percentage points corresponding to the double difference identifies the increased probability of death caused by the crisis among children born to low-educated mothers.

The key identification assumption that the gap in the probability of death between children born to low-educated and children born to high-educated mothers would have not changed in the absence of the crisis cannot be taken for granted. Secular improvements in health tend to benefit more those who are more exposed to diseases, in this case children born to low-educated mothers. For this reason, in table 1 panel B, I repeat the exercise performed in panel A, but I compare children born in the years 1991-1993 to children born in the years 1994-1996. Because no child in either group was born during a crisis, the difference-in-differences should be zero in this case. The results corroborates this hypothesis. The double difference in the absence of a crisis is not statistically different from zero. Following Duflo (2001), I call panel A the experiment of interest and panel B the control experiment.

[3]Paxson and Schady (2005) p.206 also identify the years 1988-1990 as those when child health was affected.

3.1 Combining two difference-in-differences estimators

From table 1, it should be clear that the economic crisis created especially adverse conditions for children born to low-educated mothers. These adverse conditions may have affected the health of survivors permanently. If there are long-term effects, the increase in the health gap generated during the crisis between children born to low-educated mothers in relation to those born to high-educated mothers should persist over time. Computing a difference-in-differences as in table 1 but using health measures at ages 15 to 18 provides information about the long-term impact of early childhood health shocks. Because no panel data are available, this difference-in-differences is computed with a different sample. However, the sample used to estimate table 1, and the sample used to estimate health and outcomes affected by health in adolescence are both representative of the same population. The estimates from these two samples can be combined to obtain long-term effects of early life health shocks as follows:

$$y_{ib} = \delta(b) + \gamma_1 h^0_{ib} + \gamma_2 E^m_{ib} + \epsilon_{ib} \tag{1}$$

Equation (1) is the relationship of interest. The coefficient γ_1 measures the impact of early childhood health (h^0_{ib}) on outcome y_{ib} for person i born in year b when he reaches teenage years. This outcome alternates between chronic illness, education, and employment. Equation (1) includes birth cohort fixed effects ($\delta(b)$) that capture any time trend in the variable of interest. It also includes characteristics of the person and her family (E^m_{ib}). To illustrate the methodology, I assume E^m_{ib} contains only one characteristic, the level of education of the mother.

Equation (1) cannot be directly estimated; there are no panel data in Perú that contains outcomes in adolescence and measures of infant health for the same person. Nontheless, even if a sample had all the variables required to estimate (1), a simple OLS regression would yield biased results because the error term contains genetic traits and unobserved family background characteristics correlated with h^0_{it}. However, γ_1 can be consistently estimated in a two-step procedure. The first step uses a sample that contains information about infant health during and immediately after the crisis to estimate the following equation:

$$h^0_{ib} = \zeta(b) + \alpha_1 E^m_{ib} + \alpha_2(r_b * E^m_{ib}) + \epsilon^1_{ib} \tag{2}$$

where r_b is a dummy variable that takes the value one if the person was born during the crisis and zero otherwise. The coefficient α_2 in (2) is the double difference in table 1 panel A with the caveat that a set of birth year dummy variables is included to better control for cohort fixed effects $\zeta(b)$[4]. The coefficient α_2 is an indicator of early life health shocks measured as the increased probability of dying in infancy irrespectively of whether the person died or not.

The second step uses a different sample carried out many years later when the cohorts included in (2) reached their teen years. Because of data limitations, equation (1) cannot be estimated, but a reduced form substituting (2) in (1) is possible if r_b and E^m_{ib} are observed.

$$y_{ib} = \psi(b) + \beta_1 E^m_{ib} + \beta_2(r_b * E^m_{ib}) + \epsilon^2_{ib} \tag{3}$$

Equation (3) contains valuable information. The coefficient β_2 is a difference-in-differences estimator similar to equation (2), but in this case it indicates whether there is a long-term effect of early life health shocks on human capital outcomes observed in adolescence.

The reduced form (3) does not have a natural unit that indicates whether the impact is large or small (Meng and Qian (2009)). Nonetheless, this equation can be combined with equation (2) to consistently estimate the relationship in equation (1). From equation (3), $\beta_2 = (\gamma_1 * \alpha_3)$ is identified but not γ. However, the ratio of $\widehat{\beta_2}$ and $\widehat{\alpha_2}$ yields γ_1.

$$\widehat{\gamma_1} = \frac{\widehat{\beta_2}}{\widehat{\alpha_2}} \tag{4}$$

[4]The regression version of table 1 is $h^0_{ib} = \alpha_0 r_b + \alpha_1 E^m_{ib} + \alpha_2(r_b * E^m_{ib}) + \epsilon^1_{ib}$, where E^m_{ib} takes the value 1 if the mother completed primary school or less and zero otherwise.

The numerator and the denominator in (4) are estimated with different samples. The numerator is estimated with the eight ENAHO rounds and the denominator with the two DHS surveys. This is a particular case of the two-sample instrumental variable estimator (Angrist and Krueger (1992)) when the model is just identified.[5] The ratio of two diff-in-diff estimates as a Wald estimator is also used in Duflo (2001). Here, I combine the two empirical strategies.

4 Data and variable definition

This study uses ten surveys, two rounds of the Demographic and Health Survey (*DHS*), and eight rounds of the Encuesta Nacional de Hogares Actualizada (*ENAHO-Actualizada*). I use the *DHS* to analyze the contemporaneous effect of the crisis on infant health, and the *ENAHO-Actualizada* to study its long term impact by tracking those who were born during the crisis in their teenage years.

The *DHS* is a nationally representative survey. It provides basic information of each member in the household, including age, sex, and education. The survey collects extensive information about women aged 15 to 49 years old. Each woman is asked when her children were born, if they are still alive and, for those children who died, how old they were when they passed away. I focus on children born between years 1988 and 1996.

Table 2 presents summary statistics after pooling the DHS-1996 and the DHS-2000. In total, the sample contains 47,757 observations. Each one corresponds to a birth between the years 1988 and 1996. I calculate statistics separately from the cohorts of interest. Following Paxson and Schady (2005), I compute infant mortality as the fraction of children who died at age 12 months or younger. Children who were younger that 13 months of age at the moment of the interview are discarded to eliminate problems with censored data. It is not possible to know if they died in their first year of life. To minimize recall bias, children born more than 12 years before the survey are also discarded. In addition, children born to mothers younger than 15

[5] Dee and Evans (2003) also compute a TSIV as the ratio of two estimates.

years of age and older than 44 are excluded from the sample. For this reason, each observation in DHS-1996 corresponds to a child born between 1988 and 1995, and each observation in 2000 is a child born between 1989 and 1996. To avoid oversampling the births in years where the two rounds of the DHS overlap, I divide the sample weights for children born between 1989 and 1995 by two.

Table 2 shows that infant mortality is clearly higher for the crisis years (6.3% for 1988-1990 in contrast to 4.8% and 4.1% in the non-crisis years). On average 54% of the children in the sample has a mother with primary education or less. During the crisis this number is slightly higher at 59%. The age of the mother at the moment of birth is 26.8 years old. During the crisis mothers were on average half a year younger. There is no difference in the proportion of male and female babies born during and after the crisis.

Similar to the DHS, the ENAHO-Actualizada is also a nationally representative survey. It is regularly conducted by the National Institute of Statistics of Perú (INEI). Since 2004, approximately 90,000 individuals were interviewed each year. Each new round of the ENAHO-Actualizada is comparable to previous rounds. The sample design is the same each year and the framing of questions used in this study have not changed since 2004. Before 2004, the ENAHO had a different design with a smaller set of questions. This survey is named ENAHO-Anterior. To avoid comparability problems due to methodological changes, the ENAHO-Anterior is not used in this study.

The ENAHO-Actualizada covers a wide range of topics such as education, consumption, dwelling characteristics, employment, and income. The health section is almost entirely focused on the use of health services and not on the health conditions of the people. Nonetheless, one important question about chronic illnesses is included in the survey.

Table 3 presents summary statistics for the relevant variables obtained from the ENAHO survey. Panel A contains a sample consisting of people born between 1988 and 1996, the same birth cohorts used in the DHS (table 2), but this time when all the members of these cohorts are 15 years old. To compare people of different cohorts at the same age, I pool all the available

ENAHO-Actualizada rounds. Since the first round was in 2004 and the most recent round was carried out in 2011, fifteen is the only common age for which all the cohorts are observed. Panel B contains a sample of people born between 1988 and 1993 when members of these birth cohorts were 15 to 18 years old. The advantage of this sample is the larger number of observations it contains, which increases the accuracy of the estimations. The disadvantage of the panel B sample is the fact that it is not possible to compute a control experiment for this age group. Some of the 1994-1996 cohorts used for the control experiment are not observed when they were 16 years or older in any of the ENAHO rounds. I will use panel A and panel B samples in the regressions. The panel B sample is used to obtain more accurate results and panel A sample to contrast the experiment of interest with a control experiment to make the empirical strategy more credible.

The first four columns of table 3 present summary statistics for the same time-invariant characteristics observed in the DHS sample (table 2). The education of the mother, the age of the mother at the moment of birth and the sex of the child are very similar in the ENAHO sample and in the DHS sample.

The last four columns of table 3 present summary statistics for the health and education outcomes of interest. They show important differences across the cohorts. People born later are more likely to be enrolled in school and have achieved education. This may correspond to secular improvements in education. A counter-intuitive trend is for chronic illness. At the same age, cohorts born later within the sample have a higher prevalence of chronic illness. Since the type of illness is not reported, it is possible that some of them are associated with improvement in standards of living such as obesity, hypertension and type II diabetes[6].

[6]The question the survey asks is: do you suffer a chronic illness or chronic ailment?. There is not information about the type of chronic illness

5 First stage: The crisis and the health of infants

This section shows the negative and heterogeneous impact that the Peruvian crisis had on infant health. This relationship is the first step of the two-step procedure to identify the long-term consequences of early childhood health shocks.

There are a variety of channels through which the economic downturn may affect infant health. Some of these channels are expected to have a negative impact and others a positive impact on health. During a crisis, the decline in income may decrease the quantity or quality of food consumed in the household. If this is the case and a child is malnourished, her body's ability to fight diseases decreases, and her growth and development are retarded (Duggan et al. (2008)).

On the other hand, nutrition may actually improve during a crisis. If the mother spends more time at home, possibly due to unemployment or involuntarily reduced working hours, she can breastfeed her infant longer. There is evidence that breastfeeding not only improves the child's nutrition but also fortifies the baby's immune system (M'Rabet et al. (2008)). Moreover, if the economic crisis negatively impacts the nutrition of the mother, her ability to breastfeed is not affected (Frigerio et al. (1991),Spring et al. (1985)) Changes in the time allocation of the mother induced by the crisis may also affect the health of children through other means. When the mother is at home, she can more closely care for the baby.

Another way through which the economic downturn may impact child health is the increase in exposure to disease. If an adult in the household gets sick then the child is more likely to get sick even if she is well nourished. The immune system of a newborn is weaker; a disease that only displays mild symptoms in an adult may have serious consequences for young children (M'Rabet et al. (2008)). In addition, if the quality of health services declines, the prevalence of disease may significantly increase. This seems to be the most important determinant for Perú. As mentioned before, Paxson and Schady (2005) point out that private and public health expenditures collapsed during the crisis.

5.1 Rationale for the double difference and results on the crisis and infant mortality

The rationale for the first step (equation (2)) of the methodology is as follows. The health of child i, born in year b, during the first year of life can be decomposed into two terms: potential health h_i and detrimental elements of health v_{ib}.

$$h_{ib}^0 = h_i - v_{ib} \tag{5}$$

The first term h_i is the health of the child based on her genetic traits at the moment of conception. The second term, v_{ib}, is a compound of elements that prevent the child from reaching his potential health. v_{ib} is a function of all the elements that affect the health of the child, including but not restricted to exposure to disease, which transform (5) in a standard health function (Rosenzweig and Schultz (1983), Strauss and Thomas (1998)).

$$v_{ib} = v(C_{ib}(r_b), l_{ib}^m(r_b), B_{it}(r_b), D_b(r_b), E_i^m) \tag{6}$$

More specifically, v_{ib} depends on the quantity and quality of nutrients (C_{ib}), the amount of time the mother spends with the baby (l_{ib}^m), exposure to disease (D_b), availability of health services (B_{ib}), and the level of education of the mother (E_i^m). For the reasons explained above, all these variables except the mother's education may depend on whether the child was born during the crisis or not (r_b). Then, the reduced form of (5) and (6) is:

$$h_{ib}^0 = h_i - v(r_b, E_i^m) \tag{7}$$

Since r_b and E_i^m are both binary variables, the functional form of $v(.)$ is not a concern. Including both variables in levels and their interaction saturates the model. The estimating equation is:

$$h_{it}^0 = \alpha_0 + \alpha_1 r_b + \alpha_2 E_i^m + \alpha_3 (r_b * E_i^m) + h_i \tag{8}$$

Coefficient α_3 is the double difference reported in table 1. Replacing r_b with a series of dummy variables for the year of birth yields equation (2). The health of the child (the dependent variable) is an indicator of whether the child died during her first year of life. The coefficient of interest, α_3, measures the excess probability of death for a child born to a low-educated mother during the crisis.

Table 4 presents results from estimating (8) together with alternative specifications for the pooled DHS data. Column 1 is the regression version of the two-by-two matrix in table 1, panel A. The gap between children born to low-educated and high-educated mothers increased 1.6 percentage points as a consequence of the crisis. In columns 2 to 4, I replace the variable 'crisis' with a series of year of birth dummies to better control for cohort effects. In column 3, I include the age of the mother when the child was born and the sex of the child as additional regressors. As expected, there is a U-shape relation between infant mortality and the mother's age. The probability that a child dies in his first year of life is relatively high when the mother is a teenager, it reaches its minimum when the mother is in her twenties and increases when she is in her thirties and early forties.

The probability of dying in the first year of life in Perú is one percentage point higher for boys (table 4, column 3). This result is consistent with others in the literature (see Pham et al. (2012) for Vietnam). More importantly, the interaction of the gender variable and the variable 'crisis' is not statistically different from zero (column 4), which suggests that during the economic downturn there was no (additional) intra-household sex discrimination. Resources in the household were allocated independently of the child's sex. Also in column 4, the interaction of the variable 'crisis' and the age of the mother is not statistically different from zero, which suggests that the economic downturn affected mothers of all ages equally.

Table 5 is the control experiment. The sample contains children born between 1991 and 1996. None of the children in this sample were born during the crisis. But, I label those born in years 1991-1993 as 'placebo crisis' and estimate equation (8). The interaction of mother's education and 'placebo crisis' is not statistically different from zero. The contrasts in the results in tables 4 and 5 suggest that the increase in infant mortality during the crisis was particularly severe for children born to low-educated mothers. The gap in the probability of death between children born to low-educated mothers and children born to high-educated mothers increased by 1.6 percentage points (table 4).

6 Reduced form: health, education, and employment in teenage years

6.1 Chronic illnesses

The World Health Organization (WHO (2003)) enumerates several different types of associations between fetal and infant health, and chronic conditions that appear later in life. For example, intrauterine growth retardation is associated with an increased risk of diabetes, heart disease and raised blood pressure (Godfrey and Barker (2000), Rich-Edwards et al. (1999)). Insufficient growth during the first year of life is associated with coronary diseases. This association is independent of the birth weight (Barker et al. (1989), Eriksson et al. (2001)). Although the evidence suggests that there is a link between fetal and infant health, and chronic diseases, these studies only capture correlations but not causation.

In this section, I study if those who experienced severe health shocks during the Peruvian crisis suffered any permanent effect as a result. Since the crisis had a particularly detrimental effect on the health of children born to low-educated mothers during the crisis, then the prevalence of chronic illness should be higher for them if health shocks during early childhood have a long-lasting impact on health. Nonetheless, there is the possibility of a "positive" effect of early childhood disease on teenagers' health if the selection effect, and probably also a compensation effect, dominates the scarring effect. The following equation helps illustrate these three effects:

$$h_{it}^{T} = \phi h_{ib}^{0} + (1-\phi)h_i - g_{ib} \tag{9}$$

Equation (9) indicates that the health h_{it}^T of teenager i, born in year b, is a weighted average of her health when she was a child h_{ib}^0 (equation (5)) and her potential health given by her genetic traits h_i. The coefficient ϕ indicates how persistent the early life health shock is. If $\phi = 0$, it indicates that the child fully recovers from illness, i.e., no sequela. If $\phi > 0$, it indicates that an early childhood health shock affects health permanently. g_{it} is a compound of elements that affect the health of the teenager contemporaneously. It includes variables that the person controls, such as diet, and others the person does not control, such as air pollution. The conditional expectation of (9) yields:

$$E[h_{ib}^T|h_{ib}^0] = \phi h_{ib}^0 + (1-\phi)E[h_i|h_{ib}^0 > z] - E[g_{ib}|h_{ib}^0] \tag{10}$$

The first term in (10) is the scarring effect. It indicates the persistence of early childhood health shocks. The second term is the selection effect. Diseases in the first year of life tend to kill the unhealthiest children (i.e., children whose health was below a threshold z). Consequently, those who reach their teenage years tend to have a better health endowment h_i. Finally, the third term is the set of variables that compensate or exacerbate the scarring effect. For chronic illnesses, it usually entails actions to mitigate symptoms (e.g., appropriate diet for diabetes). But, because of the nature of these medical conditions, these actions do not generally cure or cause these illnesses in the short-run. Since the dependent variable is an indicator of whether the person has a chronic condition, the third term in (10) is expected to be zero. Nonetheless, later in the paper I will study outcomes that are potentially affected by general health conditions. Then, the three components may be important.

To determine if the Peruvian crisis permanently affected the health of those born during that period, I repeat the analysis performed in the previous section but focus on the prevalence of chronic illnesses when those who survived the crisis were teenagers. The Peruvian economic crisis created particularly adverse conditions for the health of children with low-educated mothers (tables 4 and 5). If there is a permanent effect on health, the same pattern should be observed later in life: a higher prevalence of chronic illnesses for those born to low-educated mothers during the crisis. The estimating equation for chronic illnesses is:

$$h_{ib}^{T} = \psi(b) + \beta_2 E_i^m + \beta_3 (r_b * E_i^m) + \epsilon_{ib}^1 \tag{11}$$

Equation (11) is the reduced form second stage (equation (3) in section 3). The specification is identical to (8), but the dependent variable in (11) is an indicator of whether person i born in period b has a chronic illness. The coefficient β_3 measures the excess prevalence of chronic illness of those born during the crisis to low-educated mothers when they were teenagers. It contains the three components in (10): scarring, selection, and compensation.

I estimate equation (11) by pooling eight repeated cross sections (ENAHO 2004-2011). The reason to use multiple surveys is to compare different cohorts at the same age. In table 6 panel A, I present the results of estimating equation (11), including only teenagers that are 15 years old at the moment of the survey. The specifications are identical and the sample represents the same cohorts as the sample used in table 4. The interaction of the mother's education variable and the indicator of whether the person was born during the crisis suggests that the economic downturn increased the prevalence of chronic illnesses at age 15 by 3.4 percentage points of teens born to low-educated mothers as compared to those born to high-educated mothers. The age of the mother when the child was born does not seem to be relevant in explaining chronic illness (columns 3 and 4), but the sex of the children seems to be important. Boys have a 2.6 percentage point lower probability of suffering a chronic illness. Since boys are more likely to die in their first year of life (tables 4 and 5), this negative number is consistent with a strong selection effect. But, we have to be cautious with this interpretation. The crisis had no differential effect on boys (table 4 column 4), so, this hypothesis cannot be tested.

Table 6 panel B shows the same regression results as panel A but for the sample of teenagers that are 15 to 18 years old. Results are almost identical to those in panel A but are more precisely estimated.

Table 7 is the control experiment. For reasons explained in section 4, it can be computed only for the sample of 15 year olds. The effect of the interaction of the 'pseudo crisis' variable and mother's education on chronic health is much lower in magnitude and not statistically different from zero in all the specifications.

6.2 Education and employment

The results in tables 6 and 7 suggest that health shocks in the first year of life had a long-term effect that is evidenced in the increased prevalence of chronic disease. It is also important to analyze if the deterioration of health affected the ability of the person to generate income. It is not obvious that chronic illnesses should impair the normal functioning of a person. Medications and healthy habits may reduce or eliminate the symptoms that could affect work. In other words, the compensation effect in (10) may offset the scarring effect. On the other hand, some health issues that may reduce the work capacities of the person are not associated with chronic illness, such as the normal development of the brain. The medical literature suggests that malnutrition during infancy may harm cognitive ability for life (Georgieff (2007)). During the first three years of life, the brain of a well nourished child grows 300% and reaches 80% of its final weight (Dekaban (1978)). If during this period the brain does not develop normally, the possibilities of later compensation are thought to be limited.

Since a child's progression through formal education depends on health and cognitive ability, the Peruvian crisis may have negatively impacted the education of children that were exposed the most to health shocks during that period. To analyze this possibility, I estimate equation (11) but replace the dependent variable with educational outcomes.

Table 8, columns 1 to 4, shows the results for school enrolment. A child born during the crisis has a 3.7 percentage points lower probability of school enrolment at age 15 if the mother was low-educated. Nonetheless, this result is only statistically different from zero in column 4. Columns 5 to 8 show another dimension of education. The dependent variable takes the value one if the maximum level of education is primary school or less. At age 15 a person should be in secondary school. So, if the child school progression is good, the dependent variable takes the value zero. The interaction of mother's education and 'crisis' indicates that teens born to low-educated mothers during the 1980s crisis are 3 percentage points more likely to have completed only primary school or less by age 15 that other teens.

The last four columns of table 8 show the results for the same econometric specifications, but with employment as the dependent variable. The interaction of the 'mother's education' and the 'crisis' variables suggests that the likelihood of being employed at 15 is not different for those who were born during the late 1980s economic downturn to low-educated mothers.

Table 9 is similar to table 8 but with the larger sample of ages 15 to 18. Results are very similar although more precisely estimated. The interaction of crisis and mother education is significant at 1% when the dependent variable is primary education or less but not when the dependent variable is employment.

Finally, for comparison reasons, table 10 is analogous to table 8 for the control experiment. In this case, the interaction of 'mother's education' and 'placebo crisis' is not statistically different from zero in any of the regressions.

7 Long-term consequences of early childhood disease

The causal impact of early life health shocks on long term outcomes can be estimated by combining the results in sections 5 and 6 as explained in the identification section.

The ratio of β_3 from (11) and α_3 from (8) identifies the causal effect of early life health shocks, measured as the increased probability of dying the first year of life (Bozzoli et al. (2009)) on the prevalence of chronic illnesses, education, and employment of survivors at ages 15 to 18. Table 11 presents results. Standard errors are computed using the Delta method. The first column uses the sample containing only 15-years-olds. This is the same sample I use to compute table 6 panel A. The third column uses the sample with 15 to 18 year-olds (table

6 panel B). The results indicate that the detrimental health conditions associated with each additional percentage point in the probability of dying in the first year of life generated a higher prevalence of chronic illness among survivors in their teenage years in the order of 2.2 to 2.4 percentage points. The accumulation of human capital also seems to be affected; an increase in an additional percentage point in the probability of dying in infancy cause a two percentage point reduction in the probability of primary school completion by ages 15 to 18. The impact on employment is negative but very imprecisely estimated.

8 Correcting for selection effect: upper and lower bounds

The potential presence of a selection effect caused by the eventual survival of naturally healthier infants (section 6.1) implies that previous results may suffer from attenuation bias. A large scarring effect may be masked by measuring health outcomes of teenagers with relatively good health endowments. Excluding the section effect provides the correct counter-factual health outcomes of survivors. However, this is econometrically challenging. Without additional assumptions, it is only possible to obtain partially identification within certain bounds.

Results in table 11 are lower bounds effects of early life health shocks. If the infants who died during the crisis had survived and suffered on average the same prevalence of chronic disease in teenage years than those who actually survived, then the additional 2.36 percentage points in the probability of suffering a chronic illness can be attributed entirely to the scarring and the compensation effects. The assumption required for this value to be the lower bound is that the selection effect is zero. That is, those who died would have been equally healthy/unhealthy than other members of the same cohort and would have taken similar compensatory actions to counteract chronic disease, had they survived.

On the other hand, the upper bound can be estimated assuming the maximum selection effect that is possible in this situation. The first stage provides the fraction of people who passed away conditional on the birth cohort and the education of the mother. Assuming that *all* of them would have suffered a chronic condition had they survived yields the upper bound long-term health effect of early life health shocks (see appendix A for a detailed description of the methodology). Because those born in the years 1988-1990 may have died after they were one year old as a consequence of health complications that emerged during the crisis, I re-estimate the first stage with the dependent variable being 1 if the child died in the first *three* years of life instead on just the first year of life and use this information to estimate the upper bound health effect. Table 13 shows these results. They are very similar to those in table 4. This indicates that the economic crisis had a strong impact on mortality in the first year of life but very little on the subsequent survival rate.

In table 12, I show the lower bound and the upper bound long-term effect of early life health shocks after eliminating the selection effect. The difference between these values is approximately half a percentage point. The selection effect counteracts at most 17% of the total effect of early life health shock on adult chronic illness. Since the assumption made to compute the upper bound is very extreme, it is reasonable to believe that the selection effect is much smaller.

9 Composition of mothers

A threat to the identification strategy is the possibility that changes in infant mortality during the crisis are the result of changes in the composition of mothers instead of the result of health shocks. Unhealthy mothers are more likely to have unhealthy children. If the economic crisis made women delay pregnancy based on their health, then this could have generated part of the increase in infant mortality. Nonetheless, the identification strategy is invalidated only if delaying pregnancy based on the health of the mother was different in relation to the level of education of the mother. Note that the birth cohort fixed effects eliminate any change in the average health of mothers during the crisis (table 4). In addition, the only health dimension that matters is the one that affects the potential health of the child at the moment of conception (genetic traits). Health problems that the mother suffered while pregnant are considered part of early life health shocks.

Paxson and Schady (2005) find no evidence that changes in the composition of mothers can explain a significant part of the increase in infant mortality during the economic crisis. It is even more unlikely that changes in the composition of mothers can explain the differential mortality rate between children born to low-educated mothers in relation to children born to high-educated mothers. Nonetheless, to study the impact of maternal health on child mortality, I include the height of the mother among regressors in the first stage. This is the only measure associated with the health endowment of the mother available in the DHS survey. But, it is expected to be a good indicator. According to WHO (2003) p41, stunted mothers are more likely to have children with health problems. This document suggests that *"young girls who grow poorly become stunted women and are more likely to give birth to low-birth-weight babies who are then likely to continue the cycle by being stunted in adulthood"* also *"low maternal birth weight [which is associated with mother height] is associated with higher blood pressure levels in the offspring, independent of the relation between the offsprings own birth weight and blood pressure"*. In table 14, I re-estimate the first stage (equation (2)) including mother's height as a regressor. Results are as expected, the coefficient on the mother height is negative and statistically significant. But, by comparing columns 1 and 2, including this regressor has no effect on the interaction of the mother's education variable and the crisis variable[7].

Results in table 14 suggest that the composition of mothers is not a problem for the identification strategy. The inclusion of a proxy for the health of the mother does not affect the coefficient of the interaction between the education of the mother and the crisis variable. To corroborate that changes in the composition of mothers do not invalidate the identification strategy, I include in the second stage a variable that takes the value one if the mother suffers a chronic illness and zero otherwise. Certainly, if a mother suffers a chronic illness, it is more

[7]Column 2 in table 14 and column 2 in table 4 are the same specification but different samples. Mother's height is only available for 30,242 observation of the 36,057 included in the first stage.

likely that her children suffer the same condition. This is particularly the case for genetically transmitted diseases. Then, controlling for mother's chronic illness is expected to eliminate most of the health dimensions of the mother that are relevant in explaining the health of the child at the moment of conception.

Table 15 indicates that the inclusion of the mother's chronic illness among regressors does not affect the results, which validates the identification strategy. When the mother suffers a chronic illness, the child is seven to eight percentage points more likely to suffer a chronic illness. But, the inclusion of this regressor does not affect the estimated coefficients, particularly the one corresponding to the interaction of the level of education of the mother and the crisis indicator.

10 Civil Violence

During the 1980s, Perú experienced a period of intense civil violence caused by a series of attacks perpetrated by the rebel group Sendero Luminoso. The increased in infant mortality observed during the last years of 1980s could be the result of these terrorists attacks instead of the poor economic conditions during the crisis. Paxson and Schady (2005) rule out this possibility. Nonetheless, León (2012) in a more recent paper finds that civil conflicts affected the accumulation of human capital in Perú. The strategy of Leon consists of exploiting the regional and time variation of violence. He runs a regression of human capital on measures of civil violence at the district level in a diff-in-diff type estimator.

To eliminate the possibility that my results are driven by terrorists attacks, I estimate the first stage controlling for district fixed effects interacted with crisis years. Although I do not have the measures of civil violence that Leon has, this strategy eliminates any possible effect that these and all other variables at the district-year level may have in my results.

Table 16 shows the results of including district fixed effects in the first stage. Columns 1 and 2 are for the experiment of interest and columns 3 and 4 for the control experiment. Results are almost identical to those in tables 4 and 5 which indicate that episodes of civil conflicts do not threat the identification strategy. This result is consistent with Paxson and Schady (2005) result in relation of the impact of terrorists attacks on infant mortality.

11 Conclusions

The economic history of Latin America is characterized by severe macroeconomic crises, but little is known about their long-term consequences on human capital. This paper investigates the possibility that sharp contractions in economic activity negatively impact the health of children not only temporarily but permanently. In spite of not having panel data, I use two sets of difference-in-differences estimators to combine information from two rounds of the Demographic and Health Survey (DHS 1996, 2000) and eight rounds of the Encuesta Nacional de Hogares (ENAHO 2004-2011).

My results indicate that children more exposed to diseases and nutritional deficiencies during the late 1980s Peruvian crisis are more likely to suffer a chronic health condition and less likely to achieve high levels of formal education in their teenage years. The TSIV results suggest that an increase in adverse health conditions during the crisis associated with each additional percentage point in infant mortality exacerbates on average the prevalence of chronic illnesses by 2.36 percentage points among survivors and reduces by 2 percentage points the probability of completing more than primary education by ages 15 to 18.

The findings have important implications for policy. They suggest that the implementation of safety nets by governments that improve the well-being of the poor during macroeconomic shocks may significantly improve the health of next generations and the accumulation of human capital in the economy.

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A Appendix: Eliminating selection effect: partial identification

β_3 in equation (11) is the difference-in-difference estimator conditional on having survived the economic crisis. To simplify the notation, let $Y_{E1,r1}$ be the outcome (chronic illness at ages 15 to 18) of a person born to a low-educated mother ($E_i^m = 1$) during the economic crisis ($r_b = 1$). Similarly, $Y_{E0,r1}$ is the outcome of a person born to a high-educated ($E_i^m = 0$) mother during the crisis ($r_b = 1$). $Y_{E1,r0}$ and $Y_{E0,r0}$ are outcomes post-crisis. Using this notation, the diff-in-diff estimator can be written as follows:

$$\begin{aligned}\beta_3 =& [E(Y_{E1,r1}|m=0) - E(Y_{E0,r1}|m=0)] - ...\\ & [E(Y_{E1,r0}|m=0) - E(Y_{E0,r0}|m=0)]\end{aligned} \tag{12}$$

where m takes the value 1 if the person died in infancy and zero otherwise. To eliminate the selection effect, this diff-in-diff should be ideally computed for everybody whether the person died or not. Then, using the law of total probability the diff-in-diff with no selection effect would be the following.

$$\begin{aligned}\beta_3^{ns} =& [E(Y_{E1,r1}|m=0)(1-q_{E1,r1}) + E(Y_{E1,r1}|m=1)q_{E1,r1} - ...\\ & E(Y_{E0,r1}|m=0)(1-q_{E0,r1}) + E(Y_{E0,r1}|m=1)q_{E0,r1}] - ...\\ & [E(Y_{E1,r0}|m=0)(1-q_{E1,r0}) + E(Y_{E1,r0}|m=1)q_{E1,r0} - ...\\ & E(Y_{E0,r0}|m=0)(1-q_{E0,r0}) + E(Y_{E0,r0}|m=1)q_{E0,r0}]\end{aligned} \tag{13}$$

where $q_{E1,r1}$ is the probability that a person born to a low-educated mother during the crisis died in infancy. Note that this and the other probabilities are identified in the first stage (equation (8)). The part not identified is the average prevalence of chronic illness in teenage years that those who died in infancy would have suffered had they survived (i.e., $E(Y_{Ei,rj}|m=1) = 1 \; \forall i, j \in \{0,1\}$)).

To partially identify β_3^{ns}, I make two extreme assumptions. To identify the lower bound of β_3^{ns} I assume that those who died in infancy would have suffered the same prevalence of chronic illness as survivors (i.e. $E(Y_{Ei,rj}|m=0) = E(Y_{Ei,rj}|m=1)\ \forall i,j \in \{0,1\}$). Then, equation (13) reduces to equation (12). To identify the upper bound, I assume that those who died in infancy would have suffered chronic illness with a probability of one had they survived (i.e. $E(Y_{Ei,rj}|m=1) = 1\ \forall i,j \in \{0,1\}$). Then, the upper bound of β_3^{ns} is

$$\begin{aligned}
\beta_3^{ns}(upper) =& [E(Y_{E1,r1}|m=0)(1-q_{E1,r1}) + q_{E1,r1} - ... \\
& E(Y_{E0,r1}|m=0)(1-q_{E0,r1}) + q_{E0,r1}] - ... \\
& [E(Y_{E1,r0}|m=0)(1-q_{E1,r0}) + q_{E1,r0} - ... \\
& E(Y_{E0,r0}|m=0)(1-q_{E0,r0}) + q_{E0,r0}] \qquad (14)
\end{aligned}$$

The estimation of $\beta_3^{ns}(upper)$ is performed using sample analogues and the variance is calculated using the Delta method.

B Figures

Figure 1:

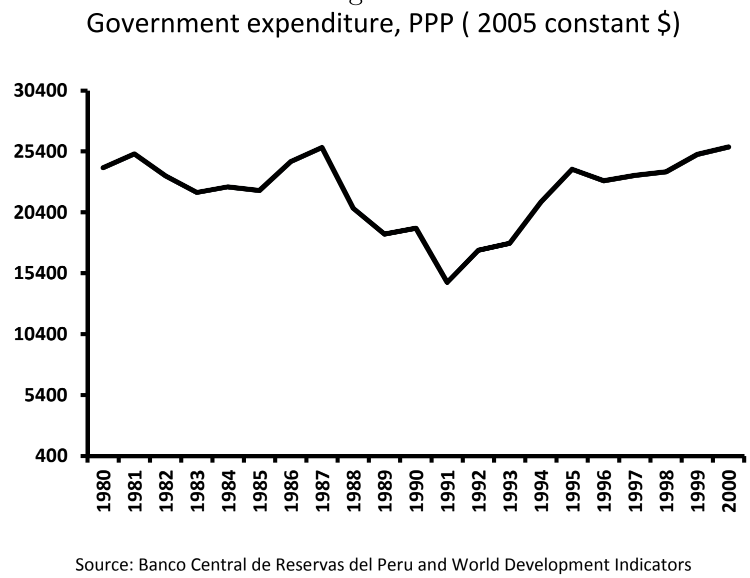


Figure 2:

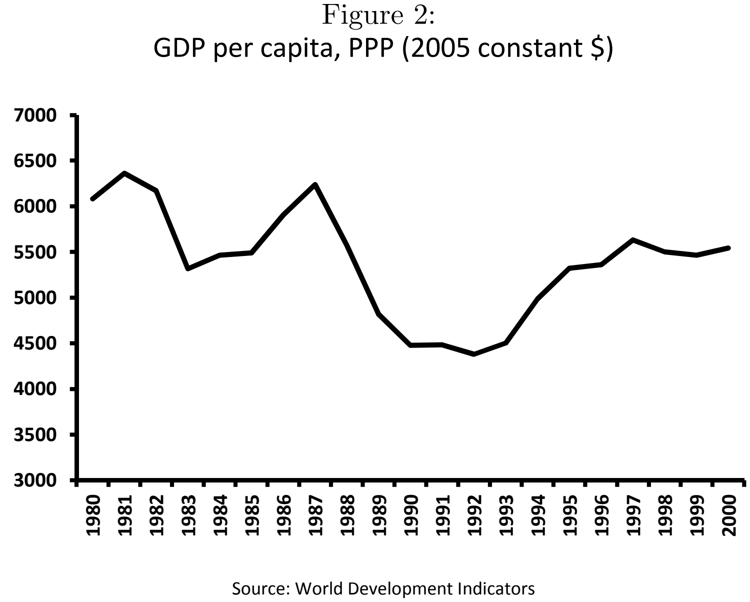
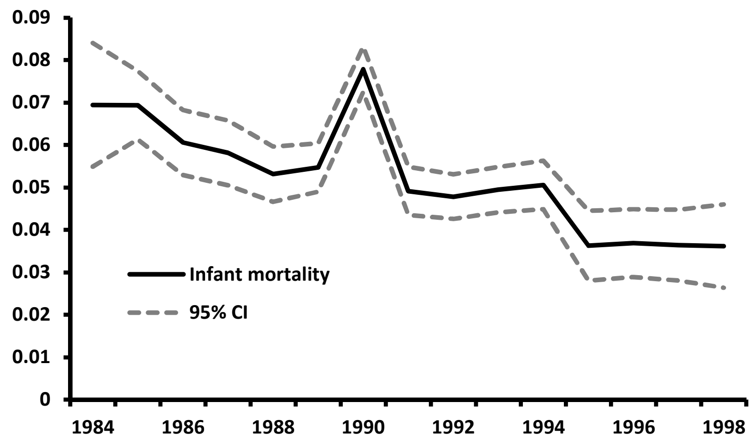


Figure 3:
Infant mortality



Source: DHS-1996 and DHS-2000

Table 1:
Probability of dying in the first year of life by year of birth and level of education of the mother

Panel A: Experiment

	mother's education		
	low	high	difference
born in years 1988-1990 (crisis)	0.082	0.035	0.048
	(0.003)	*(0.002)*	*(0.004)*
born in years 1991-1993 (post-crisis)	0.063	0.031	0.032
	(0.002)	*(0.002)*	*(0.003)*
difference	0.019	0.003	0.016
	(0.003)	*(0.003)*	*(0.005)*

Panel B: Control

	mother's education		
	low	high	difference
born in years 1991-1993	0.063	0.031	0.032
	(0.002)	*(0.002)*	*(0.003)*
born in years 1994-1996	0.054	0.027	0.027
	(0.003)	*(0.002)*	*(0.004)*
difference	0.009	0.004	0.005
	(0.003)	*(0.003)*	*(0.005)*

s.e. in parenthesis

Table 2:
Summary statistics: pooled data (DHS-1996 and DHS-2000)

	year born	obs	Infant mortality	mother primary school or less	mother age	male
Experiment						
	1991-1993	19,643	0.048	0.544	26.747	0.508
			(0.21)	*(0.50)*	*(6.43)*	*(0.50)*
	1988-1990	16,414	0.063	0.590	26.306	0.506
			(0.24)	*(0.49)*	*(6.20)*	*(0.50)*
Control						
	1994-1996	11,700	0.041	0.531	26.807	0.515
			(0.20)	*(0.50)*	*(6.57)*	*(0.50)*
	1991-1993	19,643	0.048	0.544	26.747	0.508
			(0.21)	*(0.50)*	*(6.43)*	*(0.50)*

standard deviations in parenthesis

Table 3:
Summary statistics: pooled data (ENAHO 2004-2011)

	year born	obs	mother primary school or less	mother age	male	chronic illness	school enrollment	primary school or less	employed
Panel A									
Experiment (15 year-olds)									
	1991-1993	5,482	0.527	27.260	0.506	0.127	0.777	0.154	0.485
			(0.50)	*(6.58)*	*(0.50)*	*(0.33)*	*(0.42)*	*(0.36)*	*(0.49)*
	1988-1990	4,168	0.572	27.273	0.502	0.096	0.663	0.181	0.491
			(0.49)	*(6.65)*	*(0.50)*	*(0.29)*	*(0.47)*	*(0.39)*	*(0.49)*
Control (15 year-olds)									
	1994-1996	4,832	0.498	27.633	0.512	0.167	0.817	0.130	0.505
			(0.50)	*(6.73)*	*(0.50)*	*(0.37)*	*(0.39)*	*(0.34)*	*(0.50)*
	1991-1993	5,482	0.527	27.260	0.506	0.127	0.777	0.154	0.485
			(0.50)	*(6.58)*	*(0.50)*	*(0.33)*	*(0.42)*	*(0.36)*	*(0.49)*
Panel B									
Experiment (15 to 18 year-olds)									
	1991-1993	18,884	0.512	27.401	0.524	0.150	0.598	0.111	0.537
			(0.50)	*(6.53)*	*(0.50)*	*(0.36)*	*(0.50)*	*(0.31)*	*(0.50)*
	1988-1990	17,396	0.551	27.176	0.523	0.115	0.539	0.131	0.523
			(0.50)	*(6.51)*	*(0.50)*	*(0.32)*	*(0.50)*	*(0.34)*	*(0.50)*

standard deviations in parenthesis

Table 4:
Impact of Peruvian crisis on the probability of dying in the first year of life

EXPERIMENT
Sample: children born in years 1988-1993
Dep. Variable: child died in his/her first year of life

VARIABLES				
	(1)	(2)	(3)	(4)
mother educ.	0.032***	0.032***	0.031***	0.032***
	(0.0035)	*(0.0035)*	*(0.0036)*	*(0.0036)*
mother educ. * crisis	0.016***	0.016***	0.016***	0.014**
	(0.0057)	*(0.0057)*	*(0.0057)*	*(0.0058)*
mother age			-0.0055***	-0.0055**
			(0.0020)	*(0.0023)*
mother age sqr			0.00010***	0.000094**
			(0.000036)	*(0.000041)*
male			0.011***	0.014***
			(0.0029)	*(0.0036)*
mother age * crisis				-0.00057
				(0.0041)
mother age sqr * crisis				0.000028
				(0.000075)
male * crisis				-0.0068
				(0.0059)
crisis	0.0035			
	(0.0038)			
Constant	0.031***	0.027***	0.092***	0.095**
	(0.0023)	*(0.0048)*	*(0.027)*	*(0.044)*
year of birth fixed effects	NO	YES	YES	YES
Observations	36,057	36,057	36,057	36,057
R-squared	0.009	0.010	0.011	0.011

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5:
Control experiment:

Probability of dying in the first year of life

CONTROL
Sample: children born in years 1991-1996
Dep. Variable: child died in his/her first year of life

VARIABLES				
	(1)	(2)	(3)	(4)
mother educ.	0.027***	0.026***	0.025***	0.025***
	(0.0044)	*(0.0044)*	*(0.0045)*	*(0.0045)*
mother educ. * p-crisis	0.0053	0.0057	0.0060	0.0068
	(0.0056)	*(0.0056)*	*(0.0056)*	*(0.0058)*
mother age			-0.0066***	-0.0076**
			(0.0020)	*(0.0032)*
mother age sqr			0.00011***	0.00014**
			(0.000036)	*(0.000058)*
male			0.0097***	0.0046
			(0.0028)	*(0.0044)*
mother age * p-crisis				0.0021
				(0.0039)
mother age sqr * p-crisis				-0.000042
				(0.000071)
male * p-crisis				0.0097*
				(0.0057)
scrisis	0.0040			
	(0.0038)			
Constant	0.027***	0.031***	0.11***	0.10***
	(0.0031)	*(0.0037)*	*(0.026)*	*(0.032)*
year of birth fixed effect	NO	YES	YES	YES
Observations	31,343	31,343	31,343	31,343
R-squared	0.005	0.006	0.007	0.007

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6:
Impact of Peruvian crisis on chronic illness (15 to 18 years afterwards)

EXPERIMENT
Sample: children born in years 1988-1993
Dep. Variable: chronic illness

	Panel A:				Panel B:			
VARIABLES	**15 year-olds**				**15 to 18 year-olds**			
	(1)	(2)	(3)	(4)	(2)	(3)	(4)	(5)
mother educ.	-0.092***	-0.092***	-0.094***	-0.094***	-0.10***	-0.10***	-0.10***	-0.10***
	(0.012)	(0.012)	(0.013)	(0.013)	(0.0069)	(0.0069)	(0.0070)	(0.0070)
mother educ. * crisis	0.034*	0.034*	0.032*	0.032	0.037***	0.037***	0.036***	0.037***
	(0.019)	(0.019)	(0.019)	(0.020)	(0.0097)	(0.0097)	(0.0097)	(0.0099)
mother age			0.0031	0.0095			0.0011	0.0078*
			(0.0053)	(0.0071)			(0.0029)	(0.0042)
mother age sqr			-0.000026	-0.00013			6.4e-06	-0.000096
			(0.000092)	(0.00012)			(0.000050)	(0.000073)
male			-0.026***	-0.022*			-0.048***	-0.051***
			(0.0090)	(0.012)			(0.0048)	(0.0068)
mother age * crisis				-0.015				-0.014**
				(0.011)				(0.0057)
mother age sqr * crisis				0.00024				0.00021**
				(0.00019)				(0.000100)
male * crisis				-0.0092				0.0054
				(0.018)				(0.0095)
crisis	-0.0013				-0.013			
	(0.027)				(0.011)			
Constant	0.10***	0.11***	0.064	0.18*	0.080*	-0.031	-0.058	0.047
	(0.026)	(0.017)	(0.075)	(0.11)	(0.045)	(0.081)	(0.091)	(0.098)
year of birth fixed effects	NO	YES	YES	YES	NO	YES	YES	YES
year of survey fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Observations	9,641	9,641	9,641	9,641	37,671	37,671	37,671	37,671
R-squared	0.021	0.021	0.024	0.024	0.023	0.023	0.029	0.030

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7:
Control experiment: Impact of Peruvian crisis on chronic illness (15 to 18 years afterwards)

CONTROL
Sample: **15 year-olds** born in years 1991-1996
Dep. Variable: chronic illness

VARIABLES				
	(1)	(2)	(3)	(4)
mother educ.	-0.11***	-0.11***	-0.11***	-0.11***
	(0.015)	(0.015)	(0.015)	(0.015)
mother educ. * p-crisis	0.019	0.019	0.019	0.021
	(0.019)	(0.019)	(0.019)	(0.020)
mother age			0.0085	0.0077
			(0.0054)	(0.0084)
mother age sqr			-0.00011	-0.000084
			(0.000095)	(0.00015)
male			-0.032***	-0.044***
			(0.0094)	(0.015)
mother age * p-crisis				0.0018
				(0.011)
mother age sqr * p-crisis				-0.000049
				(0.00019)
male * p-crisis				0.021
				(0.019)
scrisis	0.010			
	(0.025)			
Constant	0.22***	0.18***	0.047	0.092
	(0.015)	(0.018)	(0.076)	(0.11)
year of birth fixed effects	NO	YES	YES	YES
year of survey fixed effects	YES	YES	YES	YES
Observations	10,292	10,292	10,292	10,292
R-squared	0.026	0.027	0.031	0.031

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8:

Impact of Peruvian crisis on education and employment (15 years afterwards)

EXPERIMENT
Sample: **15 year-olds** born in years 1988-1993

VARIABLES	School enrolment				Max educ.Primary school or less				Employed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
mother educ.	-0.13***	-0.13***	-0.13***	-0.13***	0.21***	0.21***	0.21***	0.21***	0.34***	0.34***	0.34***	0.34***
	(0.014)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)	(0.016)	(0.016)	(0.016)	(0.016)
mother educ. * crisis	-0.037	-0.036	-0.036	-0.038*	0.031*	0.030*	0.030*	0.028	-0.0046	-0.0048	-0.0016	0.0019
	(0.023)	(0.023)	(0.023)	(0.023)	(0.017)	(0.017)	(0.017)	(0.017)	(0.025)	(0.025)	(0.025)	(0.026)
mother age			0.014**	0.0075			-0.0073	-0.0052			-0.016**	-0.030***
			(0.0066)	(0.0084)			(0.0053)	(0.0065)			(0.0074)	(0.0097)
mother age sqr			-0.00022*	-0.00011			0.00012	0.000079			0.00024*	0.00048***
			(0.00012)	(0.00015)			(0.000093)	(0.00011)			(0.00013)	(0.00017)
male			0.0061	0.0042			-0.0021	-0.0026			0.058***	0.055***
			(0.011)	(0.013)			(0.0087)	(0.011)			(0.012)	(0.016)
mother age * crisis				0.016				-0.0046				0.030**
				(0.014)				(0.011)				(0.015)
mother age sqr * crisis				-0.00025				0.000098				-0.00053**
				(0.00024)				(0.00019)				(0.00026)
male * crisis				0.0044				0.0013				0.0078
				(0.023)				(0.018)				(0.025)
crisis	-0.25***				-0.043**				0.0088			
	(0.029)				(0.022)				(0.032)			
Constant	1.04***	0.63***	0.41***	0.27*	0.082***	0.025*	0.13*	0.16	0.31***	0.32***	0.55***	0.32**
	(0.030)	(0.025)	(0.095)	(0.15)	(0.024)	(0.015)	(0.074)	(0.12)	(0.034)	(0.025)	(0.11)	(0.16)
year of birth FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
year of survey FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	9,640	9,640	9,640	9,640	9,640	9,640	9,640	9,640	9,636	9,636	9,636	9,636
R-squared	0.049	0.110	0.111	0.112	0.096	0.099	0.099	0.099	0.111	0.111	0.116	0.116

Robust standard errors in parenthese
*** p<0.01, ** p<0.05, * p<0.1

Table 9:

Impact of Peruvian crisis on education and employment (15 to 18 years afterwards)

EXPERIMENT
Sample: **15 to 18 year-olds** born in years 1988-1993

VARIABLES	School enrolment				Max educ.Primary school or less				Employed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
mother educ.	-0.094***	-0.095***	-0.096***	-0.096***	0.16***	0.16***	0.16***	0.16***	0.33***	0.33***	0.33***	0.33***
	(0.0085)	(0.0084)	(0.0085)	(0.0085)	(0.0049)	(0.0049)	(0.0050)	(0.0050)	(0.0087)	(0.0087)	(0.0087)	(0.0087)
mother educ. * crisis	-0.0014	-0.0034	-0.0038	-0.0050	0.032***	0.032***	0.032***	0.032***	-0.0035	-0.0035	-0.0016	-0.00014
	(0.013)	(0.013)	(0.013)	(0.013)	(0.0073)	(0.0073)	(0.0073)	(0.0074)	(0.013)	(0.013)	(0.013)	(0.013)
mother age			0.0033	0.0027			-0.0017	-0.0031			-0.011***	-0.016***
			(0.0037)	(0.0050)			(0.0024)	(0.0033)			(0.0038)	(0.0052)
mother age sqr			-0.000040	-0.000035			0.000035	0.000055			0.00014**	0.00022**
			(0.000064)	(0.000087)			(0.000042)	(0.000057)			(0.000065)	(0.000091)
male			-0.015**	-0.0074			0.0032	0.0028			0.073***	0.067***
			(0.0062)	(0.0084)			(0.0038)	(0.0050)			(0.0063)	(0.0086)
mother age * crisis				0.0013				0.0029				0.0096
				(0.0074)				(0.0048)				(0.0075)
mother age sqr * crisis				-7.3e-06				-0.000042				-0.00017
				(0.00013)				(0.000085)				(0.00013)
male * crisis				-0.015				0.00097				0.012
				(0.012)				(0.0076)				(0.013)
crisis	-0.19***				-0.039***				0.016			
	(0.014)				(0.0071)				(0.014)			
Constant	1.99***	-0.78***	-0.84***	-0.85***	0.28***	0.016	0.034	0.011	-0.11*	0.047	0.23*	0.16
	(0.061)	(0.11)	(0.12)	(0.13)	(0.036)	(0.065)	(0.073)	(0.081)	(0.060)	(0.11)	(0.12)	(0.13)
year of birth FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
year of survey FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	37,668	37,668	37,668	37,668	37,668	37,668	37,668	37,668	37,653	37,653	37,653	37,653
R-squared	0.086	0.125	0.125	0.125	0.081	0.082	0.082	0.082	0.111	0.111	0.118	0.118

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10:

Control experiment: Impact of Peruvian crisis on education and employment (15 years afterwards)

CONTROL
Sample: **15 year-olds** born in years 1991-1996

VARIABLES	School enrolment				Max educ.Primary school or less				Employed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
mother educ.	-0.11***	-0.11***	-0.11***	-0.11***	0.19***	0.19***	0.19***	0.19***	0.37***	0.37***	0.37***	0.38***
	(0.014)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)	(0.017)	(0.017)	(0.017)	(0.017)
mother educ. * p-crisis	-0.018	-0.020	-0.020	-0.020	0.019	0.019	0.020	0.021	-0.036	-0.035	-0.035	-0.039
	(0.019)	(0.019)	(0.019)	(0.019)	(0.015)	(0.015)	(0.015)	(0.015)	(0.023)	(0.023)	(0.023)	(0.024)
mother age			0.0028	-0.0025			-0.0074	-0.0097			-0.024***	-0.019*
			(0.0057)	(0.0077)			(0.0048)	(0.0070)			(0.0070)	(0.010)
mother age sqr			-0.000037	0.000049			0.00012	0.00016			0.00037***	0.00027
			(0.000099)	(0.00013)			(0.000085)	(0.00013)			(0.00012)	(0.00017)
male			0.0037	0.0032			0.0057	0.015			0.046***	0.036**
			(0.0095)	(0.013)			(0.0076)	(0.011)			(0.012)	(0.017)
mother age * p-crisis				0.010				0.0044				-0.010
				(0.011)				(0.0095)				(0.014)
mother age sqr * p-crisis				-0.00016				-0.000085				0.00020
				(0.00020)				(0.00017)				(0.00024)
male * p-crisis				0.0010				-0.017				0.019
				(0.019)				(0.015)				(0.023)
scrisis	-0.16***				-0.040**				0.018			
	(0.023)				(0.016)				(0.031)			
Constant	0.85***	0.93***	0.88***	0.021	0.042***	0.067***	0.17**	-0.0030	0.32***	0.29***	0.64***	0.76***
	(0.013)	(0.015)	(0.081)	(0.13)	(0.0084)	(0.016)	(0.068)	(0.096)	(0.017)	(0.022)	(0.099)	(0.15)
year of birth FE	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
year of survey FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	10,292	10,292	10,292	10,292	10,292	10,292	10,292	10,292	10,291	10,291	10,291	10,291
R-squared	0.034	0.088	0.088	0.088	0.088	0.090	0.090	0.091	0.125	0.126	0.131	0.131

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11:
Impact of early childhood health on selected variables: TSIV

	age 15		age 15 -18	
	coef.	s.e.	coef.	s.e.
chronic illness	2.19	1.45	2.36	1.06
school enrolment	-2.31	1.67	-0.12	0.81
primary school or less	1.93	1.30	2.04	0.88
employed	-0.31	1.62	-0.20	0.81

Table 12:
Lower and upper bound of scarring effect

	age 15		age 15 -18	
	coef.	s.e.	coef.	s.e.
chronic illness				
lower bound	2.19	1.45	2.36	1.06
upper bound	2.67	1.38	2.84	1.15

Table 13:
Impact of Peruvian crisis on the
probability of dying in the first three years of life

EXPERIMENT
Sample: children born in years 1988-1993
Dep. Variable: child died before age 3

VARIABLES				
	(1)	(2)	(3)	(4)
mother educ.	0.043***	0.043***	0.042***	0.043***
	(0.0044)	*(0.0044)*	*(0.0044)*	*(0.0045)*
mother educ. * crisis	0.019***	0.018***	0.019***	0.016**
	(0.0066)	*(0.0066)*	*(0.0066)*	*(0.0066)*
mother age			-0.0064***	-0.0057*
			(0.0023)	*(0.0029)*
mother age sqr			0.00012***	0.000096*
			(0.000042)	*(0.000052)*
male			0.0088***	0.014***
			(0.0034)	*(0.0045)*
mother age * crisis				-0.0020
				(0.0047)
mother age sqr * crisis				0.000057
				(0.000085)
male * crisis				-0.011
				(0.0068)
crisis	0.0054			
	(0.0043)			
Constant	0.036***	0.032***	0.11***	0.12***
	(0.0029)	*(0.0052)*	*(0.031)*	*(0.047)*
year of birth fixed effects	NO	YES	YES	YES
Observations	32,495	32,495	32,495	32,495
R-squared	0.012	0.013	0.014	0.014

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 14:
Impact of Peruvian crisis on the probability of dying in the first year of life: the role of mother's height

Dep. Variable: child died in his/her first year of life

VARIABLES	Experiment cohorts 1988-1993		Control cohorts 1991-1996	
	(1)	(2)	(3)	(4)
mother educ.	0.028***	0.032***	0.025***	0.027***
	(0.0038)	*(0.0038)*	*(0.0045)*	*(0.0044)*
mother educ. * crisis	0.012*	0.012*		
	(0.0067)	*(0.0067)*		
mother educ. * p-crisis			0.0047	0.0046
			(0.0058)	*(0.0058)*
mother height	-0.0012***		-0.00085**	
	(0.00028)		*(0.00025)*	
Constant	0.22***	0.033***	0.16***	0.036***
	(0.043)	*(0.0067)*	*(0.038)*	*(0.0044)*
year of birth fixed effects	YES	YES	YES	YES
Observations	30,242	30,242	29,902	29,902
R-squared	0.010	0.009	0.006	0.006

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 15:
Impact of Peruvian crisis on chronic illness (15 to 18 years afterwards): role of mother chronic illness

Dep. Variable: child chronic illness

VARIABLES	Experiment cohorts 1988-1993 at age 15		Experiment cohorts 1988-1993 at age 15-18		Control cohorts 1991-1996 at age 15	
	(1)	(2)	(3)	(4)	(5)	(6)
mother educ.	-0.091***	-0.092***	-0.099***	-0.10***	-0.11***	-0.11***
	(0.012)	*(0.012)*	*(0.0069)*	*(0.0069)*	*(0.014)*	*(0.015)*
mother educ. * crisis	0.035*	0.034*	0.036***	0.037***		
	(0.019)	*(0.019)*	*(0.0097)*	*(0.0097)*		
mother educ. * p-crisis					0.015	0.019
					(0.019)	*(0.019)*
mother chronic illness	0.056***		0.075***		0.087***	
	(0.010)		*(0.0052)*		*(0.010)*	
Constant	0.099***	0.11***	-0.027	-0.031	0.15***	0.18***
	(0.017)	*(0.017)*	*(0.081)*	*(0.081)*	*(0.017)*	*(0.018)*
year of birth fixed effects	YES	YES	YES	YES	YES	YES
Observations	9,638	9,641	37,663	37,671	10,292	10,292
R-squared	0.027	0.021	0.034	0.023	0.041	0.027

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 16:
Impact of Peruvian crisis on the probability of dying in the first year of life: district fixed effects to eliminate the incidence of civil conflicts and violence

Sample: children born in years 1988-1993
Dep. Variable: child died in his/her first year of life

VARIABLES	\`Experiment'		\`Control experiment	
	(1)	(2)	(3)	(4)
mother educ.	0.026***	0.026***	0.022***	0.025***
	(0.0037)	*(0.0037)*	*(0.0043)*	*(0.0043)*
mother educ. * crisis	0.016***	0.014**	0.0062	0.00098
	(0.0057)	*(0.0061)*	*(0.0055)*	*(0.0057)*
Constant	0.025***	0.031***	0.032***	0.026***
	(0.0050)	*(0.0041)*	*(0.0040)*	*(0.0053)*
year of birth fixed effects	YES	YES	YES	YES
district fixed effects	YES	YES	YES	YES
district x crisis fixed effects	NO	YES	NO	YES
Observations	36,057	36,057	32,051	32,051
R-squared	0.013	0.014	0.008	0.011

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1