THE PERMANENT EFFECTS OF RECESSIONS ON CHILD HEALTH: EVIDENCE FROM PERU*

Jorge M. Agüero
University of California, Riverside and SALDRU

Martín Valdivia
Grupo de Análisis para el Desarrollo

Resumen: Se explora el efecto permanente de las recesiones macroeconómicas sobre la salud materna e infantil en Perú. Para evitar el problema de auto selección por dar a luz durante la recesiones, se comparan las tasas de mortalidad infantil entre hijos de una misma madre. Así, una caída de 1 por ciento en el PIB per cápita está asociada con un incremento en la tasa de mortalidad infantil entre 0.30 y 0.39 por ciento. El impacto negativo sobre el control prenatal sugiere que los efectos permanentes empiezan cuando el niño está aún en el vientre de la madre.

Abstract: We explore the permanent effects that recessions have on health-related outcomes of mothers and children in Peru. To account for possible self-selection in giving birth during recessions, we compare the infant mortality rates of siblings born in different phases of the economic cycle. A 1 percent decline in GDP per capita is associated with an increase in infant mortality rates between 0.30 and 0.39 percent. We find evidence that recessions also have a negative effect on long-term health measures for surviving children. The additional negative effect found on prenatal care suggests that the permanent effects start while children are in-utero.

Clasificación JEL/JEL Classification:I12, J13, J18

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1. Introduction

Macroeconomic shocks are temporary by definition. In most cases, after a few periods, countries return to their long-run paths. However, the duration of the crisis as well as the limitations of public and private safety nets may lead to permanent effects on the countrys human capital, especially of the most vulnerable groups, which in turn may affect the long-run path of the economy.

Household resources decrease during crises, impacting adversely the nutritional status of children and other household members, especially of the uninsured poor, which increases their epidemiological vulnerability. If the reduction in nutritional intake is severe enough for a child in his first months, a severe health shock might lead to the death of the child. Even if the child manages to survive, severe and prolonged nutritional deprivation can affect the height of the child and the growth of his brain. In both cases, the recovery of macroeconomic indicators and of household income after a crisis will not restore these health outcomes, thus limiting aggregate economic growth and poverty reduction in the long-run. Public early childhood development programs may help isolate poorest households from these permanent effects, but resources for such programs are also reduced during a crisis, limiting their ability to work efficiently as a public safety net.

Understanding the magnitude of the impact of macroeconomic crises on health outcomes is essential to the design of policies that can prevent or at least reduce the long-term effects of these shocks. Thus, this paper explores the role of macroeconomic crises on health-related outcomes in Peru.

Using data from the Peruvian Demographic and Health surveys we estimate the effects on three main health outcomes: infant mortality, child nutrition and prenatal care. Our results show a negative effect of recessions on child health. Recessions increase child mortality terminating any possible future accumulation of human capital for these children. For those who survive, which in Peru is the majority of children, the effect of a recession is so severe that it creates irreversible effects on height. Moreover, the negative effects start before birth as the number of prenatal visits also decreases with recessions.

The rest of the paper is organized as follows: Section 2 briefly documents the macroeconomic environment in Peru since 1980. The theoretical considerations are presented in section 3. Section 4 describes the data used in this paper followed by a discussion of our identification strategy in section 5. The effects of the past crises on health-related outcomes are presented in sections 6 and 7. An important aspect of our paper is the discussion of how our estimates allow us to identify the effect of the current crisis. This aspect is developed in section 8 followed by our conclusions.

2. Macroeconomic Crises in Peru

Since the end of military rule in 1980, Peru has experienced severe macroeconomic crises. In figure 1 we can identify four of these episodes. In 1982 and 1983 Peru suffered from a mixture of domestic, external and climate-related shocks. Increments in international interest rates together with falling international prices for main exports (generated by a recession in the United States) were accompanied by inadequate macroeconomic policies (Hamann and Parades, 1997) and the *El Niño* phenomenon of 1983. This led to a decline in GDP per capita of more than 13 percent in 1983.

During the 1980s another crisis, even more severe in magnitude, took place just a few years later. In 1988 and 1989 the Peruvian economy shrank by more than 10 percent each year followed by an additional seven percent in 1990. This crisis, a domestic one, was also characterized by a period of hyperinflation resulting in an increase of more than 1 million percent in prices between 1985 and 1990. The structural adjustment process started in August of 1990 was followed by two additional years of cumulative negative growth (1991 had positive but minimal growth).

The third crisis worth mentioning between 1980 and 2007 took place in 1998 following the Asian crisis of the previous year. This time, the GDP per capita shrank by two percent in 1998 followed by zero growth in 1999. For Peru, this crisis is probably the closest experience to the current conditions for at least two reasons. First, the crisis was driven by external factors as opposed to domestic ones. Second, the effects on the economy were smaller. While it is too soon to establish what the effects of the current international crisis on the Peruvian economy will be, recent estimates suggest a relatively minor effect compared to other countries. A fourth crisis, of similar origins as the 1998 crisis, took place in 2000. During this crisis, GDP fell by less than 5 percent.

Finally, it is also important to mention that in all these crises the negative effects were spread throughout the country. However, while data exists at the regional level for GDP, we are not able to exploit these variations because the surveys used to measure health outcomes do not allow us to identify the child's birthplace but only the location of the mother at the time of the survey. In the next section we present a conceptual framework to show how these (temporary) macroeconomic crises could leave a permanent mark on child health.

3. Health under Economic Crisis: A Conceptual Framework

Aggregate economic shocks, such as the ones created by recessions and economic crises, have an income and a substitution effect. The income effect appears because the amount of resources available to households changes with the economic cycle. The substitution effect comes from the changes in the wage rate of adults (and children), which in turn affect the opportunity costs of the time spent away from the labor market.

During recessions the income effect will have a negative impact on health as resources become scarce. On the other hand, the substitution effect increases the opportunity for parents to allocate their time away from work and possibly into health-promoting activities. Thus, from a theoretical point of view, the effect of a recession on health outcomes is ambiguous and requires an empirical answer.

Nonetheless, the income effect is amplified when households do not have access to credit markets or insurance and when they lack the assets to buffer their consumption. Procyclical public expenditures on health add to the income effect by failing to provide a safety net when is needed the most. Thus, while in theory the effects are ambiguous, we should expect that in developing countries the income effect dominates the substitution effect and hence, recessions are expected have a negative effect on health. On the other hand, in developed nations recessions might have a positive effect on health.

The recent review of the literature by Ferreira and Schady (2009) documents this pattern. Studies in the US find that infant mortality increases during booms and declines during recessions (*e.g.*, Ruhm, 2000; Chay and Greenstone, 2003; and Dehejia and Muney, 2004). However, evidence from developing countries, including middle-income countries in Latin America suggest a negative effect of recessions on child health (*e.g.*, Alderman, Hoddinott, and Kinsey, 2006; Friedman and Schady, 2009; Baird, Friedman, and Schady, 2007; and Bhalotra, 2010).

Furthermore, within a country the effects are likely to be heterogeneous. As mentioned earlier, access to credit or insurance markets, as well as the level of the initial stock of human and physical capital, will play a role in the transmission mechanism of the effect of recessions. For example, Agüero and Robles (1999) show that rural households in Peru can be classified into three groups: net sellers, net buyers and off-market. During a recession, negative shocks to agricultural prices would have heterogeneous effects depending of the market integration of households. Net sellers will face a negative shock as their unit price decreases, while net buyers will suffer a gain because they have to spend less to acquire the same quantity. Off-market farmers will remain unaffected. Thus, the type and degree of market integration of rural households will also play an important role in the transmission of the effect of recessions.

For children, the lack of good nutrition at an early age has irreversible effects. Failure to provide appropriate nutrition during developmental stages will have a permanent effect on the human capital of children and will affect their productivity as adults (See Maluccio *et al.* (2009); Glewwe, Jacoby, and King (2001); Cunha *et al.* (2006). Thus, while aggregate shocks have a temporary nature, the effects on health could be permanent.

4. Demographic and Health Surveys

To estimate the impact of recessions on health outcomes, we use several rounds of the Peruvian Demographic and Health Surveys. The DHS are nationally representative household surveys conducted in developing countries. The sample is composed by women aged between 15 and 49 at the time of the survey (and their families). Peru has conducted five DHS[1] starting in 1986 and repeated in 1991/1992, 1996, 2000 and a "continuous" survey since 2004 (we refer to the latter as the 2004+ and "1992" for the 1991/92 survey). These surveys will allow us to concentrate on three measures of health: infant mortality, child's height and prenatal care.

Women are asked about their birth history, use of contraceptives and fertility preferences in addition to their socioeconomic background, among other topics. Regarding birth history, for all births, information about date of birth (month and year), child's gender, and whether the child was still alive at the time of the survey is recorded. For children who died, information is collected about the date of death as well as the child's age at death. We will use this information to compute our measures of infant mortality.

[1] DHS are independent cross-sectional surveys and it is not possible to construct a panel of women or of households.

For women with children born in the five years prior to the survey, anthropometric measures are available. Height and weight are collected for the mother and all her children under the age of five (at the time of the survey). Thus, it is possible to compute z-scores for height-for-age of these children (HAZ).[2] We focus on height instead of weight because the former is considered a long-run measure of an individual's health (Behrman and Deolalikar, 1988).

Women were also asked about the care received, if any, while pregnant in the five years prior to the survey. However, this question is not available for all pregnancies. Starting with the 2000 survey, questions about prenatal care are limited to the last birth (within five years). Also, this question is not available for the 1986 survey. This survey is also missing the anthropometric data for mothers and children. Thus, infant mortality rate is the only health indicator for which data is collected in all surveys and we will concentrate most of our analysis on this measure.

Table 1 summarizes the first two moments of these outcomes as well as the main characteristics of mothers for the sample related to infant mortality. Panel A shows that the mortality rate for children under 12 months of age averages 61 deaths per thousand live births for children born between 1980 and 2005. The mortality rate in the first six months is 47 per thousand births and is 35 for mortality in the first month after birth. Several issues concerning these estimates are worth mentioning. First, the birth records and the mortality are obtained from retrospective data. As mentioned above, mothers at the time of the survey are asked about their birth history so measurement error could be a problem. We therefore restrict the sample to births that took place within 12 years prior to the survey. A similar approach has been implemented by Paxson and Schady (2005) and Friedman and Schady (2009). While this restriction is somehow arbitrary, it will allow us to compare our findings to these two previous studies. Second, these births do not necessarily represent the births (or mortality rates) at a particular time. Consider the year of 1991. The births recorded for that year included in our analysis correspond to mothers aged between 14 and 48 in 1991 if they were interviewed in the 1992 DHS, but also from women aged between 10 and 44 in 1988 if they were interviewed in the 1996 DHS and similarly for those

[2] The HAZ z-score (z_{igj}) for child i of gender g that is j years old is calculated as follows: $z_{igj} = \frac{h_{igj} - \mu_{gj}}{\sigma_{gj}}$ where h_{igj} is the actual height of a child and μ_{gj} and σ_{gj} are, respectively, the mean and standard deviation of the height by gender and age.

in the 2000 and 2004+ surveys. Nonetheless, the fact that we have five surveys over 20 years allow diminishes the bias due to a lack of overlap.[3] Third, the women included in the sample are "survivors". The nature of the DHS does not permit the inclusion of births (or mortality rates) of children born to women who died prior to the survey. If the mortality rates are higher for these children then our mortality measures are biased downwards.

Panel A of table 1 shows the average age and years of schooling of the mother. The mothers in the sample accumulated an average of six years of education, the equivalent of completing primary education. There are significant disparities in this indicator as shown by the large standard deviation. Also, most mothers were married at the time a child was born. Panel B describes the outcomes related to child health. On average, a child younger than 5 is 1.3 standard deviations shorter than the World Health Organization (WHO) benchmark. When the *z*-score is -2 or below, a child is considered stunted. In the sample, 30 percent of children under 5 are stunted. Panel C shows the outcomes associated with prenatal visits. Just under 70 percent of the pregnancies in the five years prior to each of the surveys had access to a prenatal visit. The average number of visits is 4 and about half of the pregnancies had four or more visits.[4]

Finally, GDP per capita is obtained from the World Bank's World Development Indicators and it is measured in constant US dollars of 2000. Given the restrictions imposed by the DHS, we use annual GDP from 1980 to 2005. The next section describes the methodology to evaluate the permanent effects of changes in GDP per capita on health outcomes.

5. Methodology

We argued earlier that macroeconomic crises could have a permanent effect on health outcomes. However, the identification of a causal effect represents a challenge if crises are correlated with other (mostly unobserved) variables. Consider the following reduced form equation

[3] For a discussion about how this nonrandom selection could bias our mortality measures see Paxson and Schady (2005).

[4] The one or more and four or more prenatal visits are considered to be the critical thresholds for maternal health. In fact, these are the two indicators used to measure the achievement of the Millennium Development Goal regarding maternal health targeting reproductive health (Target 5B). See http://mdgs.un.org/unsd/mdg/Metadata.aspx?IndicatorId=0&SeriesId=762 for more details.

$$y_{imt} = \alpha + \beta \ln(GDP_t) + g(t) + \theta' x_{imt} + e_{imt} \quad (1)$$

where y_{imt} is a health outcome for child i born to mother m at time t, x_{imt} is a vector containing child characteristics (*e.g.*, sex, month of birth and birth order) and mother characteristics such as her age, marital status and educational attainment. In this paper we explore several functional forms to capture the systematic trends in household outcomes. This is expressed in equation (1) by the function $g(t)$. In particular we consider a linear, a quadratic and a cubic trend. The parameter of interest is β as it captures the effect of (the natural logarithm of) GDP per capita on the health outcomes described in the previous section.

An OLS estimator for the parameter β will be unbiased if we assume that women giving birth during a crisis are a random sample of the population at large. However, this might be incorrect. If agents (mothers or couples) could predict, with some degree of accuracy, the probability of a crisis in the coming periods and believe that it would negatively affect the health of their children then they are most likely to avoid having children during crises. On the other hand, women who are unable or unwilling to control their fertility, fail to predict a crisis, or have different beliefs about the effect of crises on health outcomes could be more likely to give birth during these periods. These characteristics could not only affect the type of children they might give birth to but also the type and amount of resources available to raise their children, which in turn will affect the health outcomes of children. Thus, an OLS estimator is likely to be biased.

We propose a methodology that will allow us to reduce (if not eliminate) the possible bias from OLS estimates. To show that, we rewrite equation (1) as follows:

$$y_{imt} = \alpha + \beta \ln(GDP_t) + g(t) + \theta' x_{imt} + \mu_m + \upsilon_{imt} \quad (2)$$

Equation (2) is obtained from (1) by assuming that disturbance of the latter (e_{imt}) could be decomposed into two elements. The first element is υ_{imt} which is an *iid* disturbance. The second element is μ_m which represents unobserved characteristics of mothers. As described above, if mothers self-select to give birth during particular periods, OLS estimates will be biased. By allowing mother fixed-effects (μ_m) we can control for all time-invariant characteristics.[5]

[5] A similar methodology has been implemented by Bhalotra (2010) in India and by Friedman and Schady (2009) in Sub-Saharan Africa.

Unbiased estimates of β using equation (2) require two important assumptions. We already stated the first one where the unobservable characteristics included in μ_m must be time invariant. The second assumption is that the sample of mothers with two children is not different from the population at large. Unlike the first assumption, the latter is testable. As shown in table 1, Panel A, the characteristics of children born to mothers with two or more children do not differ from the population at large. The columns on the right of the table indicate that the mortality measures are higher in the 2+ sample but not statistically different given the standard deviations. Similarly, the sample of mothers with more than two children have the same average age, years of schooling and marriage rates compared to the population as a whole (Panel A). There are no systematic differences in the 2+ sample; thus estimates with mother fixed-effects are not biasing the main results.[6]

Panels B and C extend the analysis for child health measures and prenatal visits, respectively. There, the differences between the full sample and the 2+ sample are quite salient. Note that there are important reductions in the sample size when using the 2+ sample. For these outcomes, women need to have at least two children within five years prior to the survey. Clearly, these women are unlikely to be a random sample of the universe of women so the results need to be interpreted with caution. For example, as explained in the previous section, the 2000 and 2004+ surveys only ask questions about prenatal care for the last birth in the last five years. Thus, the analysis for these outcomes is coming from the 1992 and 1996 surveys when using mother fixed-effects.

The thought experiment with this methodology compares the outcomes of children born during a crisis to their siblings born outside a crisis. Thus, the identification of the effect of the crisis depends heavily on mothers giving births to two or more children but with at least one of them born outside a crisis period. Strictly speaking, given that we use ln(GDP) as our key variable, all that is required is that siblings be born under different levels of (the log of) GDP per capita. Given that this is a continuous variable, the probability that any two years have exactly the same GDP per capita is zero. Therefore, all that is needed is that the children are born in two different years.[7]

[6] The use of mother fixed-effect is the main reason why we estimate equation (2) using a linear probability model for binary outcomes. Using nonlinear models such as a *probit* or *logit* would limit our ability to obtain the marginal effect of crisis on health outcomes.

[7] This is the main reason why we use log of GDP. per capita as our measure

Nonetheless, we explore the issue of variation in exposure to a crisis among siblings in table 2. In Panel A, column 6, for the sample related to infant mortality, a child who was born during a crisis (any of the crises described in section 2) has 48.5 percent chance of having siblings who were also born during a crisis, compared to 38.2 percent for a child born outside a crisis. This offers a substantial variation when comparing children born to the same mother. Table 2 shows that these percentages are similar when looking at each survey separately, with the exception of the 2004+ survey.

Panel B shows that there is less variation in the sample of child health. At best, only the 1996 and 2000 surveys offer some variation for children born during a crisis. Unfortunately, the rate of siblings born during a crisis is almost zero for children born outside a crisis. This is not surprising. As discussed above, the anthropometric data is available for children born five years prior to the survey so the identification comes from surveys where there was a crisis at some point between d and d-5, where d is the date of the survey. Panel C shows a similar difficulty when the outcome is prenatal care. Thus, our more accurate results come from the analysis of infant mortality rates. The effects of crises on this outcome are discussed in the next section.

6. The Effect on Infant Mortality

6.1. *Trends in Infant Mortality*

Figure 2 shows infant mortality rates from 1980 to 2002 as calculated using the five Peruvian DHSs. We plot mortality rates for children who died within the first month after birth, within the first six months, and within the first 12 months. Two clear features arise. First, all measures exhibit a negative trend. In 1980, the under-12-month mortality rate was around 100 deaths per thousand live births but it was only 20 deaths per thousand by 2002. This remarkable decline is observed in the two other measures as well. It is then important to separate the effect of this "deterministic" trend from the effect

as opposed to a binary value equal to one if the child was born in a recession (or crisis) and zero otherwise. The binary variable will lead to less variation among siblings limiting the accuracy of the estimates. Also, a binary specification does not allow us to compute the elasticity of the outcomes with respect to, for example, a 1 percent decline in GDP.

of GDP per capita on infant mortality rates. Failing to do so might overestimate the role of crises on this health outcome.

The second feature is the clear spike in mortality rates in years where crises occurred. The most dramatic increase is observed around 1990. In their study on mortality rates for the late 1980s crisis, Paxson and Schady (2005) argue that the crisis represented 17 000 "extra" deaths. As described above, it is difficult to establish the extent to which the increased mortality can be attributed to the crisis itself, as some women could self-select to give birth during harsh economic conditions. However, Paxson and Schady (2005) argue against this alternative explanation by showing that the crisis did not alter the fertility rates even when comparing women with different levels of education. In the next subsection, we will further explore this issue, controlling for unobserved characteristics of mothers using the regression framework described in the previous section.

6.2. *Main Results*

In table 3, Panel A shows the results of using a linear trend. Column 1 shows an estimate of β by OLS as in equation (1). A 1 percent reduction in GDP per capita is associated with an increase in infant mortality of 0.18 per thousand.[8] To understand whether this effect is "large" or "small", we can compare it to the findings of Friedman and Schady (2009) where they use a similar specification applied to Sub-Saharan African countries. The authors find that a reduction in GDP of 1 percent is associated with an increase in infant mortality between 0.34 and 0.62 deaths per thousand.

However, this comparison could be misleading since Peru starts with a much lower base of infant mortality rates. An alternative way is to calculate the elasticity of infant mortality rates with respect to GDP. In table 1 we show that the average infant mortality rate is about 61 per thousand. Thus, our OLS estimate implies an elasticity of -0.30. Comparable numbers for Africa are between -0.32 and -0.58 (Friedman and Schady, 2009). The lower elasticity for Peru is consistent with a model where the sensitivity to shocks is inversely related to the level of development.

OLS estimates could be biased if women self-select to give birth during harsh economic conditions. Assuming that the unobserved

[8] The *absolute* change in infant mortality per thousand for a 1 *percent* change in GDP per capita is given by $\beta \times 10$ or $\beta \times \frac{1000}{100}$. The numerator is required to express the rate per thousand of live births and the denominator comes from using a so-called *lin-log* model.

characteristics leading to self-selection are time-invariant, estimates of β using mother fixed-effects should eliminate that source of bias. Implementing a fixed-effect model requires comparing the infant mortality rates of two (or more) children born to the same mother, but where one child was born *during* a recession while the other(s) was (were) born *outside* a recession period. In column 2 of Panel A we show that limiting the sample to children born to mothers with at least two live births does not lead to different estimates of β compared to the population at large. Since most mothers in Peru have at least two children, restricting the sample to the 2+ does not alter the main moments of the distribution. Nonetheless, there is a minor loss in efficiency as the standard error increases from 0.008 to 0.009, as expected.

In column 3 we show the estimates using a mother fixed-effect model. The estimated value for β implies that a 1 percent decrease in GDP per capita is associated with an increase in the mortality rate of 0.27 per thousand live births (in relation to the child's siblings). This number is larger (in absolute value) than the OLS estimates but recall that the sample of women with two or more children has a slightly higher infant mortality rate (70 per thousand). Thus, it is better to compare the corresponding elasticities. The implied elasticity with mother fixed-effects is -0.39. This suggests that the time-invariant unobserved characteristics are important but there is only a small self-selection to give birth during recessions.

Note that modeling the deterministic time trend as quadratic or cubic (panels B and C) does not alter the quantitative or qualitative aspects of our estimates with a linear trend. Overall, our results suggest that the elasticity of infant mortality rates with respect to GDP per capita is somewhere between -0.30 and -0.39. Declines in aggregate economic activity are temporary but do indeed leave a permanent effect as shown by the sensitivity of infant mortality rates to economic crises.

6.3. *Heterogeneous Effects*

Table 4 shows how the effects of recessions on infant mortality vary according to the type of recession, the education of the mother, and the child's gender after controlling for mother fixed-effects (and a linear trend). Section 2 described the four recessions in Peru since 1980. Recessions vary in their severity, their causes and length. Therefore it is important to test for possible differences. However, the results presented so far assumed that a decline in GDP per capita had the same effect in all recessions. Panel A of table 4 relaxes this assumption. In all recessions there was a negative association between GDP per capita and mortality rates. Surprisingly, the effects do not seem to vary with the severity of the crisis. Likewise, the recessions of the 1980s do not show a larger impact than those of the 1990s, either in terms of the length of the crisis or their causes. As documented by Paxson and Schady (2005), the sensitivity of the mortality rates are linked to the specific ways in which private and public expenditures on health react during recessions and such variations could be the reason behind the variation mortality rates among the crises.

We find that the effect on infant mortality varies with mother's education. Panel B shows that a child born to an uneducated mother when GDP per capita declined by 1 percent relative to his siblings'GDP level at birth, will have his mortality rate increased by 0.67 per thousand. The effects are much smaller for those born to mothers with at most primary education (0.17 per thousand live births) or more schooling (0.08 per thousand live births). Thus, the permanent effect of recessions had important distributional effects as the sensitivity of the mortality rates is strongly related to the mother's educational attainment.

Finally, the impact on infant mortality does not vary by the gender of the child. The point estimates in table 4 are not statistically different for boys compared to girls. Overall, our findings suggest that while recessions are temporary they leave a permanent mark by increasing their mortality rates during the first 12 months of life. The effects vary with the type of recession and tend to be higher for children born to uneducated mothers, even after we control for time-invariant unobserved characteristics of the mother. As we will discuss later, given the increases in schooling for women, including in rural areas, it is possible that our estimates serve as an upper bound for the effect of the current crisis. This could be further reinforced if there is an increase in access to information across all educational levels about "good" practices regarding health and nutrition. We will return to this point in section 8.

7. Effects on Child Nutrition and Prenatal Care

7.1. *Child health*

Despite the important effects on infant mortality of an economic crisis, most children survive their first 12 months after birth. Nonetheless, their nutrition could be negatively affected and could have permanent consequences if the effect is severe enough. In this section we explore this issue. The DHS, as explained earlier, allow us to measure the height-for-age *z*-score of children aged less than 60 months at the time of the survey. In figure 3 we show the trends in child nutrition using *z*-score and the stunting rate (i.e., those with a *z*-score less than -2).

Two main patterns arise. First, the *z*-score declines with age. Newborns have a *z*-score that on average is close to the mean of the WHO reference group. At 20 months of age, the average child is 1.75 of a standard deviation shorter than the reference group. This is not a cohort problem. This could be the case if, as in the case of infant mortality, there is a "natural" trend where younger cohorts have better nutrition than older ones. As shown in figure 3a the decline is observed in all surveys rejecting the cohort hypothesis. A similar pattern is found in figure 3b where the proportion of stunted children is displayed.

Second, except for the 1992 survey, it is hard to separate the levels observed across all other surveys. Children in the 1992 survey were born between 1986 and 1992 and were exposed to the most severe recession from birth or at a very young age. They indeed show a much lower level of *z*-score and higher stunting rate. This lower *z*-score is even larger for those with higher exposure to the crisis. Nonetheless, it is going to be very difficult to identify the impact of the most severe recession (1988-1992) since we do not have data on children born before this crisis.

Additionally, the decline in *z*-scores with age will make it difficult to separate this "natural" trend from the effect of GDP per capita when comparing children born to the same mother and even more for those born to different mothers. Unfortunately, compared to the infant mortality estimates, the within-siblings estimates are going to be less precise due to the limited variation in exposure to a recession across siblings as presented in table 2.

Column 1 of Panel A in table 5 shows precisely this problem with an OLS estimation. We observe a counterintuitive relationship. A *decline* in GDP per capita is associated with an *increase* in the height-for-age *z*-score. In Panel B, the *decline* in GDP coincides with a *reduction* in the stunting rate instead of the expected *increase*. Restricting the sample to children born to mothers with two or more children aged 60 months or less, as expected, does not solve the problem.

However, when controlling for mother fixed-effects we observe the expected patterns. A 1 percent decrease in GDP per capita is associated with an increased in the z-score of 0.0033 (relative to the child's siblings.) The corresponding elasticity is estimated around 0.21, thus a 1 percent decline in GDP decreases the child's z-score by 0.21 percent. In terms of the probability of being stunted, the elasticity is -0.10, but the estimated parameter is not statistically different from zero. Similar results are found if we restrict the sample to those included in the 1996 and 2000 surveys where there is slightly more variation in the exposure to a recession across siblings (not shown).

The data structure imposes a severe limitation on the accuracy and precision of the estimates for child nutrition. Nevertheless, if taken at face value, these results extend our previous findings and show a long-lasting effect of recessions on the health status of surviving children.

7.2. *Prenatal Care*

Figure 4 shows the dramatic improvement in prenatal care in the second half of the 1990s. The proportion of women *without* prenatal care fluctuated around 35 percent between 1988 and 1994. Starting in 1995 there is a systematic decline and five years later the corresponding number is less than 10 percent.

As in the case of child nutrition we are again limited to births taking place five years prior to the survey and since the 1986 DHS did not include prenatal questions it is hard to evaluate the effect of that crisis on this health outcome. Nonetheless, it is clear from figure 4 that the proportion of pregnancies without prenatal care increased during the 1998 recession. From last quarter of 1997 to the second quarter of 1998 the proportion of mothers without prenatal care increased by of 11 percent. As discussed in the previous sections, this could hardly be the right measure of the effect of the recession because many other factors could be involved.

In table 6 we show estimates for the effect of a recession on three measures of prenatal care: proportion with access to prenatal care (the complement of the outcome shown in figure 4), the number of prenatal visits and the proportion of women who had four or more visits. In Panel A, column 1 we find that that the OLS estimate suggests a positive association between GDP per capita and the proportion of women with prenatal care. This effect is larger relative to the estimates in column 2 where only women with two or more pregnancies in the five years prior to the survey are included (0.31 versus

0.14). This reduction is driven by the substantial differences in the size of the samples used for columns 1 and 2, as described earlier. Furthermore, when we control for mother fixed-effects (column 3) we obtain an even smaller parameter (0.052). Time-invariant unobserved characteristics of the mother seemed to be overestimating the effect of a recession. In Panel A, we find that a recession does not have a statistically significant effect on the probability of making a prenatal visit.

Nonetheless, panels B and C indicate that the negative effects of a recession are present through the effects of other variables. For example, in Panel B we find that 1 percent decline in GDP per capita reduces the number of visits by 0.008 (with fixed-effects, column 3). Given that the average number of visits (in the 2+ sample) is 2.8, the implied elasticity is 0.28 for a 1 percent increase in GDP per capita.

Similar calculations could be done for the proportion of mothers with 4 or more prenatal visits based on the fixed-effect estimates of Panel C (column 3). A 1 percent decrease in GDP reduces this probability by 0.31 percent.

At face value, these findings suggest that the effect of a recession on child health starts before children are even born. Those *in utero* during a recession will start with an important disadvantage compared to their "not treated" siblings. Thus, part of the effects on mortality and child nutrition (for the surviving children) could be traced back to the inadequate care received *before* they were born. These findings have an important implication in the design of countercyclical policies as they should not just focus on the nutrition of young children but also on pregnant women.

8. Conclusions and Lessons for the Current Crisis

How can we use our findings to draw lessons for the effect of the current crisis on child health? First, it is important to note that in the case of Peru the crisis is expected to be less severe. The most recent estimates for the growth rate in 2009 still predict a positive growth rate of about 2 percent. Nonetheless, the rest of the Andean countries are more likely to experience a recession.

Second, our results show a negative effect of recessions on child health. More importantly, despite the temporary nature of recession they leave a permanent mark on children's human capital. In Peru, we document that recessions increase child mortality, thus eliminating any possible future accumulation of human capital by these children.

For those who survive, which in Peru is the majority of children, the effect of a recession is so severe that it creates irreversible effects on long-run measures such as height. Moreover, the negative effects start before birth.

Third, we found evidence of heterogeneous effects by mother's education, at least in the case of mortality rates.[9] Children born to mothers with lower educational levels are more vulnerable compared to those born to more educated mothers. However, we argue that given the recent improvements in education and information, our estimates could be interpreted as an upper bound.

Consider the case of educational attainment. Figure 5 shows that younger cohorts (male and female) have more education than older cohorts. The proportion of people without education has declined from an average of 30 percent for those born before 1940 to almost zero for those born in 1980. This is accompanied with a clear decline of those with just primary education from just under 60 percent (pre-1940 cohort) to almost 20 percent (1980 cohort). The proportion of people with secondary education or higher is on the rise and it has almost tripled.

These findings are also evident when we focus only on women's schooling. Figure 6 shows that the average years of schooling of a woman born in 1940 is short of four years (i.e., less than primary education). On the other hand, a woman born in 1980 is likely to have 10 years of education, one year short of finishing secondary education. The results are even more pronounced for rural women. Those born in 1980 have seven times more schooling compared to their counterparts born in 1940. As fewer women remain uneducated, we should expect the impact of current recessions to have a mitigated effect.

Furthermore, we argue that even for the same level of education, women in general have more information about "good practices" regarding child nutrition and hygiene. For example, in figure 7 we show that across all education levels, women in the 2000 DHS are more exposed to mass media (top panel) and their children are more likely to be fully vaccinated[10] (bottom panel) compared to those in the 1986 DHS. These two trends combined suggest that our findings should be interpreted as an upper bound for the effect of the current crisis on child health.

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[9] The limited sample size does not allow us to obtain consistent estimates by educational levels for child nutrition or prenatal visits.

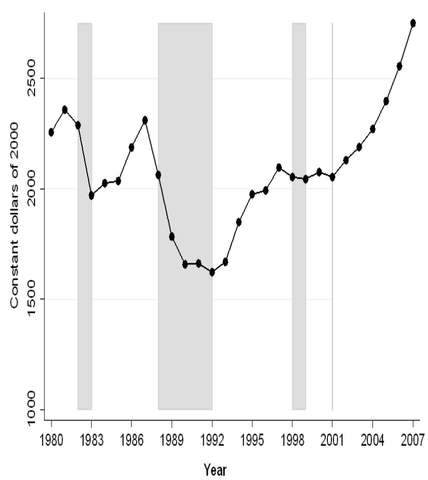
[10] A child is fully vaccinated if she received BCG, measles, and three doses of DPT and polio (excluding polio 0, which is given at birth).

Our findings have two important policy implications. Countercyclical policies should focus on providing a safety net especially for very young children. This would reduce the impact of recession on infant mortality and child nutrition for those who survive. The second policy implication is that the negative effects are found even before children are born, since prenatal visits also decline with a recession. This decline is very likely to be the result of the combination of a reduction in household resources and a contraction in public provision of health services. Finally, these countercyclical policies should be accompanied with a sustained effort to increase the education of girls, which in the medium-term will provide safety net for the next generation as suggested by our results.

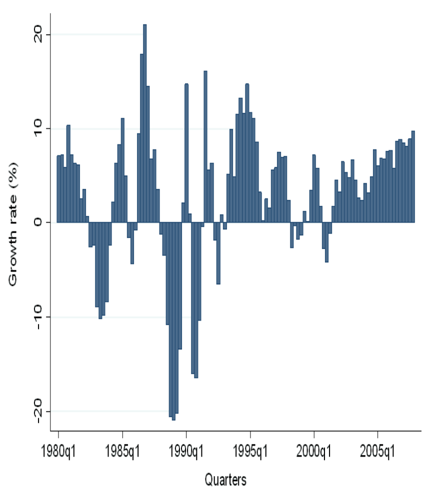
Figure 1

Peru's GDP *Per Capita (1980-2007)*

A. GDP *Per Capita in Constant Dollars*

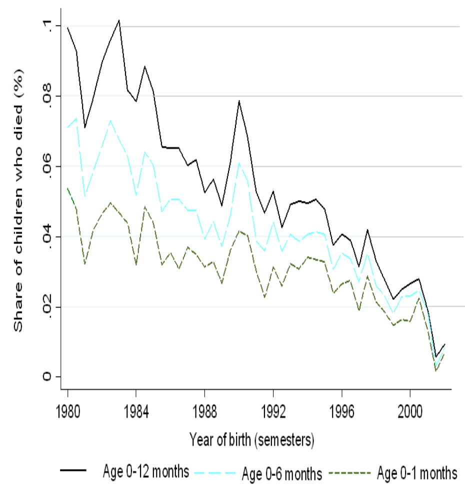


B. Annualized Growth Rate



Source: World Bank's World Development Indicators (levels) and Banco Central de Reserva del Peru (growth rates). In figure A recessions are shaded.

Figure 2
Trends in Infant Mortality Rates



Source: Peruvian DHS 1986, 1992, 1996, 2000 and 2004+.

Figure 3
Child Nutritional Status by Age and Survey
A. Height for Age z-score

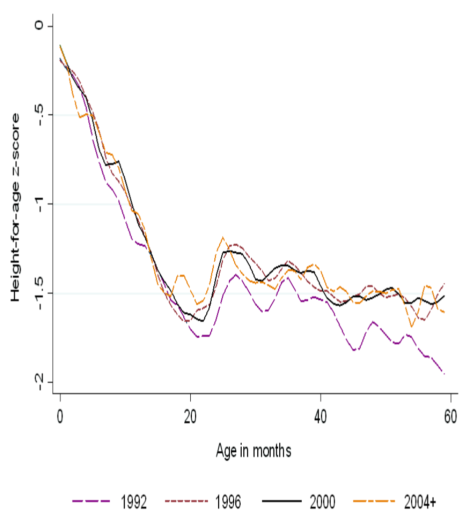
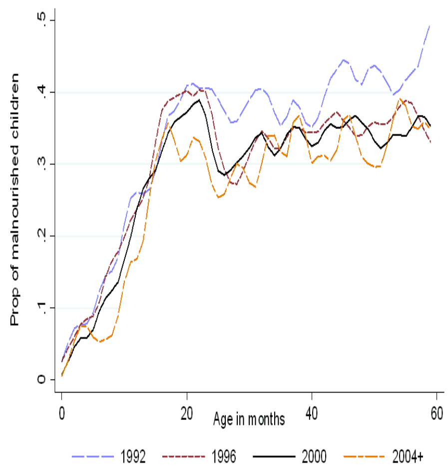
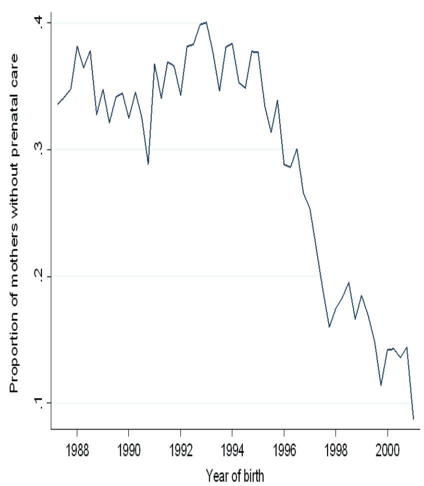


Figure 3
(*continued*)
B. Stunting Rate (z-score ≤ −2)



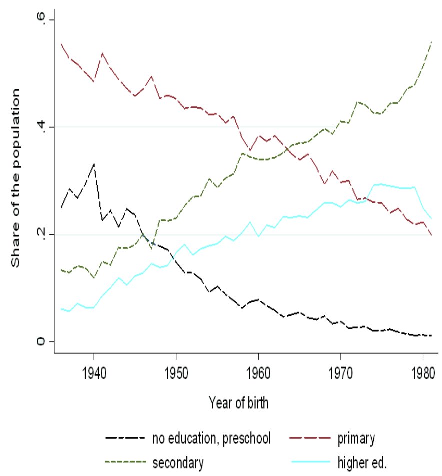
Note: Each variable was smoothed using a kernel regression on age in months. Source: Peruvian DHS 1992, 1996, 2000 and 2004+.

Figure 4
Proportion of Women without Prenatal Care



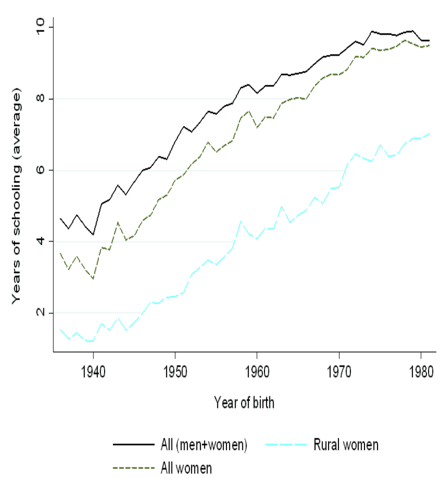
Source: Peruvian DHS 1992, 1996, 2000 and 2004+.

Figure 5
Levels of Education by Cohort (male and female)



Source: Peruvian DHS 2000. The sample includes all household residents.

Figure 6
Years of Schooling by Cohort, Gender and Location

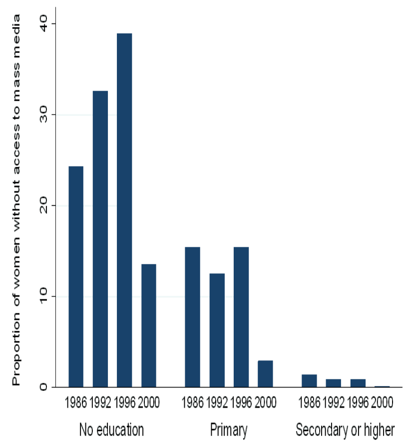


Source: Peruvian DHS 2000. The sample includes all household residents.

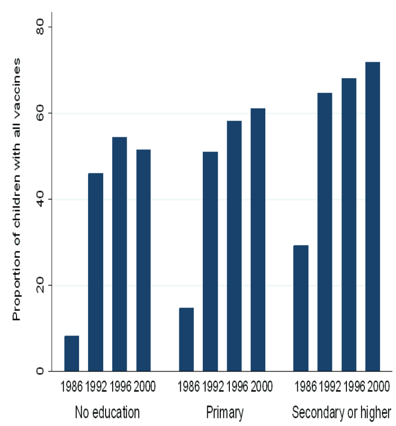
Figure 7

Access to Information and Vaccines by Survey and Mother's Education

A. Lack of Access to Mass Media



B. Access to Vaccination for Children



Note: (Lack of) Exposure to mass media is calculated as the percentage of women who usually read a newspaper at least once a week, watch television at least once a week, or listen to a radio daily (weekly) by selected background characteristics. A child is fully vaccinated if she received BCG, measles, and three doses of DPT and polio (excluding polio 0). Source: Peruvian DHS 1986, 1992, 1996, 2000.

Table 1
Summary Statistics

Variable	Units	Full sample Mean	Full sample Std Dev.	2+ sample Mean	2+ sample Std Dev.
Panel A: Infant mortality (N_{Full} = 102 434 and N_{2+} = 82 943)					
Under 12 months	Binary	0.061	0.239	0.070	0.254
0-6 months	Binary	0.047	0.211	0.053	0.224
Under 1 month	Binary	0.035	0.183	0.039	0.194
Mother's age	Years	32.644	7.047	32.880	6.572
Mother's schooling	Years	5.963	4.359	5.407	4.163
Married	Binary	0.949	0.221	0.966	0.181
Panel B: Child health (N_{Full} = 36 256 and N_{2+} = 16 127)					
Height for age	*z*-score	-1.318	1.367	-1.553	1.380
Stunted	Binary	0.304	0.460	0.378	0.485
Panel C: Prenatal visits (N_{Full} = 29 697 and N_{2+} = 9 542)					
Had prenatal care	Binary	0.692	0.461	0.560	0.496
Visits	Number	4.063	3.884	2.827	3.486
Had 4+ visits	Binary	0.499	0.500	0.338	0.473

Notes: Panel A includes the 1986, 1992, 1996, 2000 and 2004+ DHS. Panels B and C exclude the 1986 survey. N_{Full} and N_{2+} represent, respectively, the sample size for the full and restricted sample (women with 2 or more children).

Table 2
Variation Across Siblings in the Exposure to Recessions at Birth

	Proportion of siblings born during a recession (by survey)					
Born during a recession	*1986 (1)*	*1992 (2)*	*1996 (3)*	*2000 (4)*	*2004+ (5)*	*All surveys (6)*
Panel A: Infant mortality sample						
Yes	0.398	0.493	0.595	0.516	0.176	0.485
	(1 911)	(9 387)	(15 388)	(13 283)	(6 526)	(46 495)
No	0.122	0.345	0.408	0.416	0.133	0.382
	(866)	(7 058)	(15 449)	(11 748)	(1 327)	(36 448)
Panel B: Child health sample						
Yes		0.980	0.340	0.234	0.189	0.344
		(810)	(5 214)	(3 579)	(673)	(10 276)
No		0.738	0.030	0.034	0.000	0.397
		(3 031)	(1 828)	(865)	(127)	(5 851)

Table 2
(*continued*)

	Proportion of siblings born during a recession (by survey)					
Born during a recession	*1986 (1)*	*1992 (2)*	*1996 (3)*	*2000 (4)*	*2004+ (5)*	*All surveys (6)*
Panel C: Prenatal visits sample						
Yes		0.966 (1 045)	0.507 (3 923)			0.603 (4 968)
No		0.594 (2 487)	0.047 (2 087)			0.345 (4 574)

Notes: Number of observations in parenthesis. Panel A includes the 1986, 1992, 1996, 2000 and 2004+ DHS. Panels B and C exclude the 1986 survey.

Table 3
Effect on Infant Mortality Rates

	Full sample (1)	*2+ sample (2)*	*Mother FE[a] (3)*
Panel A: Linear trend			
Ln (GDP)	-0.018 [0.008]**	-0.018 [0.009]**	-0.027 [0.010]***
Panel B: Quadratic trend			
Ln (GDP)	-0.028 [0.010]***	-0.028 [0.011]**	-0.022 [0.012]*
Panel C: Cubic trend			
Ln (GDP)	-0.028 [0.010]***	-0.028 [0.011]**	-0.021 [0.012]*
Observations	102 434	82 943	82 943
Number of mothers			29,341

Notes: [a]Fixed effects. Robust standard errors in brackets,*significant at 10%; **significant at 5%; ***significant at 1%. Regressions include child's birth order, gender, and separate dummies for the child's month of birth and the mother's educational level. Mother's age and her marital status are calculated for the child's year of birth. The 2+ sample refers to mothers with two or more live births.

Table 4

Heterogeneous Effect on Infant Mortality Rates

Categories	*Ln (GDP)*	*t-statistic*
Panel A: By recession		
1982-1983	-0.0368	-2.84
1988-1992	-0.0381	-2.92
1998-1999	-0.0389	-3.07
2001	-0.0385	-3.03
Panel B: By mother's education		
No education	-0.0671	-2.54
Primary	-0.0173	-1.26
Secondary[a]	-0.0082	-0.51
Panel C: By gender of the child		
Boys	-0.0304	-2.12
Girls	-0.0238	-1.82

Notes: Regressions include child's birth order; gender; separate dummies for the child's month of birth and the mother's educational level; and a linear trend. Mother's age and her marital status are calculated for the child's year of birth. The sample is restricted to mothers with two or more live births. T-statistics are calculated with robust standard errors. [a]Secondary education includes higher education.

Table 5

Effect on Child Health (children under 6)

	Full sample (1)	*2+ sample (2)*	*Mother FE[a] (3)*
Panel A: Effect on height-for-age			
Ln (GDP)	-0.355 [0.079]***	-0.372 [0.109]***	0.326 [0.187]*
Panel B: Effect on stunting			
Ln (GDP)	0.074 [0.027]***	0.121 [0.038]***	-0.036 [0.072]

Table 5
(continued)

	Full sample (1)	*2+ sample (2)*	*Mother FE*[a] *(3)*
Observations	36 256	16 127	16 127
Number of mothers			7 676

Notes: [a]Fixed effects. Robust standard errors in brackets,*significant at 10%; **significant at 5%; ***significant at 1%. Regressions include child's birth order, gender, and separate dummies for the child's month of birth and the mother's educational level. Mother's age and her marital status are calculated for the child's year of birth. The 2+ sample refers to mothers with two or more live births.

Table 6
Effect on Prenatal Visits

	Full sample (1)	*2+ sample (2)*	*Mother FE*[a] *(3)*
Panel A: Effect on the probability of a visit			
Ln (GDP)	0.308 [0.033]***	0.141 [0.040]***	0.052 [0.032]
Panel B: Effect on the number of visits			
Ln (GDP)	3.224 [0.241]***	1.302 [0.268]***	0.794 [0.199]***
Panel C: Effect on the probability fo having 4 or more visits			
Ln (GDP)	0.383 [0.031]***	0.149 [0.038]***	0.106 [0.034]***

Notes: [a]Fixed effects. Robust standard errors in brackets,*significant at 10%; **significant at 5%; ***significant at 1%. Regressions include child's birth order; gender; separate dummies for the child's month of birth and the mother's educational level; and a linear trend. Mother's age and her marital status are calculated for the child's year of birth. The sample includes the 1992, 1996, 2000 and 2004+ DHS. The 2+ sample refers to mothers with two or more children born five years prior to the survey which eliminates the 2000 and 2004+ surveys

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