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Aggregate Income Shocks and Infant Mortality in the Developing World

Sarah Baird
Jed Friedman
Norbert Schady

The World Bank
Development Research Group
Poverty Team & Human Development and Public Services Team
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Abstract

The causal role that income plays in determining child health has been discussed and contested in much previous work. The authors revisit this question with an investigation of short-term fluctuations in aggregate income and infant mortality using an unusually large dataset of 59 developing countries, covering over 1.7 million births. The authors show that there is a large, negative association between per capita GDP and infant mortality—on average, a one-unit decrease in log GDP is associated with an increase in mortality of between 18 and 44 infants per 1,000 children born. Given our use of unit data we are able to control for changes in the characteristics of women giving birth, and for various possible confounding factors, including weather shocks, conflict, female education, and the quality of institutions. None of these factors have an appreciable effect on our results. Further, female infant mortality is more sensitive than male infant mortality to economic fluctuations, especially during negative shocks to GDP, suggesting that policies that protect the health status of female infants may be especially important during economic downturns in much of the developing world.

This paper—a product of the Research Division, Development Economics Department—is part of a larger effort in the department to determine the influence of economic policy on health outcomes. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at jfriedman@worldbank.org.

Aggregate Income Shocks and Infant Mortality in the Developing World[♣]

Sarah Baird
University of California at San Diego

Jed Friedman
World Bank

Norbert Schady
World Bank

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1. Introduction

One of the mandates of the Millennium Development Goals is a sharp reduction in infant mortality across the developing world, yet uncertainty remains over how best to achieve this goal. Long-term reductions in mortality are generally believed to be driven mainly by improvements in medical technology (Preston 1980; Acemoglu and Johnson 2006; Cutler et al. 2006). The extent to which changes in income per se affect health status, including the mortality and morbidity of young children, remains controversial. A good deal of this controversy arises from the fact that there are likely to be feedbacks from health status to income—for example, both Gallup and Sachs (2001) and the World Health Organization (2001) argue that improvements in health status would increase rates of economic growth. On the other hand, Pritchett and Summers (1996), who instrument economic growth with investment levels, terms of trade shocks, the black market exchange rate premium, and price level distortions, argue that there is a strong causal relation running from income to health status. However, their identification and conclusions have been challenged by, among others, Jamison et al. (2004) and Deaton (2006), who argue that the co-variation between income and health is likely to be driven primarily by omitted variables such as country-specific differences in institutional quality, female education, and the capacity to absorb medical technology,.

In this paper, we extend the discussion of the interrelationship between health and income with an investigation of GDP and infant mortality for a large set of developing countries between 1975 and 2004. We focus on infant mortality not only because of its pervasive presence in much of the developing world, but also since it is less likely than adult mortality to be affected by reverse causality from health to income. We address the natural covariation of trending series such as GDP and infant mortality in a variety of ways to limit the potential for spurious correlations between income and health; as a result, our focus is on departures of income from trend or year-on-year deviations, and the effect that these have on infant mortality, rather than on the relationship between long-term changes in infant mortality and income.

Given that the paper focuses on the effect of sudden departures of aggregate income from trends on infant mortality, it is closely related to a literature on the effects of "booms" and "busts" in aggregate income. In a recent paper, Dehejia and Lleras-Muney (2004) conclude that infant mortality is generally pro-cyclical in the United States. Various transmission mechanisms from economic recessions to improved child health have been proposed, including reductions in air pollution (Chay and Greenstone 2003), reductions in health-damaging behaviors such as smoking and drinking, and increases in the probability that mothers engage in time-intensive activities such as exercise and prenatal care (Ruhm 2000; Ruhm and Black 2002).

In developing countries, such as those we analyze, economic downturns may result in reduced household consumption of nutritious foods, and lower expenditures on other inputs into child health, including preventive health care or medical attention for children who are ill. If downturns are sufficiently severe, they may also result in contractions in public health expenditures. The empirical evidence from developing countries is mixed. Sharp economic downturns were associated with increases in infant mortality in Mexico (Cutler et al. 2002), and particularly Peru (Paxson and Schady 2005), but not in Argentina (Rucci 2004) or in countries of the former Soviet Union (Brainerd 1998; Brainerd and Cutler 2005; Shkolnikov et al. 1998). Evidence from the 1998 financial crisis in Indonesia is inconclusive, but generally suggests small effects on infant mortality (Rukumnuaykit 2003; Frankenberg et al. 1999). Echoing the results from the United States, Miller and Urdinola (2007) find that regions hit by negative coffee price shocks saw improvements in child survival in Colombia.[1] Finally, there are a handful of cross-country studies of the relationship between income and infant mortality in developing countries that also find mixed results (Pritchett and Summers 1996; Filmer and Pritchett 1999; Jamison et al. 2004; Deaton 2006).

Our paper extends the existing literature in a number of important ways. The sample of countries, fifty-nine, is much larger than that from the country-specific studies. This allows us to estimate the relationship between income and health in a variety of settings across recent decades. In addition, and unlike the cross-country studies discussed above, we use unit data rather than working with bi-decennial country averages of infant mortality. As a result, we focus on higher-frequency changes in infant-mortality than previous research. Furthermore, because the analysis is based on unit data, we can control for the changing composition of women giving birth, and assess how income shocks interact with a variety of characteristics of mothers and children, such as mother's education and the gender of the child. Finally, we include data on a number of potential omitted variables, including rainfall shocks, conflict, and the quality of institutions. None of these controls affects our results in a substantive way.

The next section, Section 2, describes the data for our sample of countries and provides details on the construction of the variables used in the analysis, in particular our measure of infant mortality. In Section 3, we discuss the basic estimation approach and present results. This section also includes a discussion of changes in the composition of women giving birth; omitted variables; the timing of GDP shocks; and heterogeneity of the effects. Section 4 concludes.

[1] Some of the country-specific studies rely on vital statistics data to construct infant mortality. In many developing countries, these data suffer from serious under-reporting—the degree of which may itself be correlated with aggregate economic conditions.

2. Data and construction of variables

The data on per capita GDP used for this paper are taken from World Bank (2007). The values correspond to real per capita GDP in 2000 US dollars, adjusted for differences across countries in purchasing power parity (PPP). The data on births and deaths are based on 123 Demographic and Health Surveys (DHS) covering 59 countries. The surveys include countries in Africa (33 countries, 68 surveys), Latin America (12 countries, 31 surveys), and Asia (14 countries, 27 surveys). The earliest surveys in our sample were carried out in 1986, the latest ones in 2004. Taken together, the surveys we use collect information on approximately 760,000 women and 1.7 million births. However, the sample sizes vary considerably—for example, the 1999 India DHS covers approximately 90,000 women, while the sample size for the 1987 DHS for Trinidad and Tobago is just over 3,800 women. The list of specific surveys and their sample sizes are given in the Data Appendix.

The DHS ask women a set of questions about the date of birth, current vital statistics, and date of death (if deceased) of all children ever born. We use the responses to these questions to construct retrospective birth and death histories, closely following Paxson and Schady (2005). Our measure of infant mortality is an indicator that takes on the value of one if a child died at a reported age of 12 months or younger. We use this measure of infant mortality, rather than the standard definition of mortality for children *younger than* 12 months, because of age heaping in reports of mortality. Further, we discard information for children born within 12 months of the survey when calculating mortality rates to avoid complications with censored data.

Although the DHS are a rich source of data, they also have some limitations for our analysis. We briefly discuss two of these limitations, both of which are related to the use of *retrospective* information in the DHS to construct birth and death histories. First, recall bias may be a concern if women are less likely to accurately remember more distant births and deaths. To minimize recall errors, we do not use information on births that occurred more than 11 years prior to the date of the survey. As we discuss below, our results are robust to different cut-offs for the recall period. Second, any given survey is representative of women ages 15-49 at the time of the survey, but is not representative of all births and child deaths in earlier years. To see this, note that a woman aged 49 in a survey carried out in 2000 would have been 39 in 1990. If no surveys were carried out between 1990 and 2000 in this country, no data would be available on births to women aged 40 or older in 1990. Children born to older women may respond to economic fluctuations differently than those born to younger women. To avoid this problem, we discard from the sample births to women age 40 or older. Our analysis therefore provides meaningful estimates of the relationship between income fluctuations and infant mortality for women aged 15 to 39. (We note, however, that only 1.2 percent of births in our sample of DHS countries occur to women age 40 or older.) This retrospective construction of births and infant deaths to women aged 15-39 results in series of varying lengths and with varying start periods depending on the number and dates of DHS surveys in each country.

The DHS collect a great deal of *current* information on mothers (for example, their education levels, whether they are employed) and children (for example, the gender and birth order and, in the most recent surveys, height and weight). Some DHS also ask respondents about their use of health services—for example prenatal check-ups and the place of delivery. However, these data are not collected in a comparable fashion in every survey, and typically are available only for the last birth. The degree to which we can analyze possible transmission mechanisms from income to infant mortality with our data is therefore limited.

As we show in the Data Appendix, our estimates of aggregate infant mortality are internally consistent and are highly correlated with other sources of data that have been used to assess the relationship between per capita GDP and infant mortality, in particular data from the World Development Indicators (WDI) data base (World Bank 2007). However, the estimates of infant mortality we calculate are likely to be more useful to estimate the relationship between fluctuations in per capita GDP and infant mortality for a variety of reasons. First, we have constructed annual series of infant mortality to look at higher-frequency changes than what can be observed in the bi-decennial WDI series. Data like those in the WDI series will have smoothed some of the year-on-year variation in infant mortality in the DHS. A share of the variation that is smoothed is likely to be measurement error, but the remainder likely reflects genuine annual fluctuations in infant mortality. Second, the data in the WDI series would not allow us to adjust for the changing composition of women giving birth during economic expansions or contractions, or to estimate the heterogeneity of responses to economic fluctuations by characteristics of the mother and child.

3. Econometric specification and results

To estimate the association between infant mortality and per capita GDP in our data we proceed in two ways. Our first approach involves collapsing the unit data to the level of the country-year observation. As we show, the results we obtain with the country-year data are similar regardless of the approach taken to control for country-specific deterministic trends or non-stationary processes in the observed GDP and infant mortality series. We therefore next turn to the unit data in order to control for mother- or child- specific covariates, as well as country-specific deterministic trends, and to inspect possible heterogeneity in the relation between GDP and infant survival.

A. Main results based on country-year averages

To calculate country- and year-specific infant mortality rates we estimate the following:

(1) $$\mathrm{D}_{\mathrm{imtc}} = \mathrm{D}_{\mathrm{tc}} Z_{tc} + \varepsilon_{imtc}$$

where D_{imct} is an indicator variable for whether child *i* born to mother *m* in country *c* in year *t* died; Z_{ct} is a set of country-year fixed effects, and ε_{imct} is the regression error term. For reporting convenience, we scale the coefficients D_{ct} so that they estimate the mean infant mortality rate (expressed in infant deaths per 1000 live births) in country *c* for the various years of observation.[2]

With the estimates of D_{ct} in hand, we then address the possibility of spurious correlation between infant mortality and GDP that can arise when relating two trending series. We adopt several standard approaches to this problem.

The first approach we take is to use first-differenced data. Differencing creates a stationary series if the underlying data series follow a random walk or is integrative of order 1. This approach, common in the growth literature, is shown in equation (2).[3]

(2) $$\mathrm{D}_{\mathrm{ct}} - \mathrm{D}_{\mathrm{ct-1}} = \eta(\log\mathrm{GDP}_{\mathrm{ct}} - \log GDP_{ct-1}) + (\varepsilon_{ct} - \varepsilon_{ct-1})$$

To account for the fact that the estimates of infant mortality are calculated more precisely for some countries and years than others, largely as a result of the much larger sample sizes in some surveys, this regression (as well as all of the other specifications that use the country-year averages) is weighted by the inverse of the variance of the estimated coefficient on the year dummy for a given country-year observation.

[2] All calculations of infant mortality for a particular country-year observation use the weights provided in the survey. In some cases, more than one survey is available for a given country-year observation—for example, the infant mortality rate in Peru in 1990 can be calculated on the basis of the DHS for 2000, 1996, and 1992. In these instances, we first calculate the infant mortality rate for a given year separately for each of the surveys, using the appropriate weights, and then take a simple average of the three estimates.

[3] Nelson and Plosser (1982) present evidence that macroeconomic aggregates such as GNP are better modeled as integrated process of order one (I(1) processes) rather than stationary processes with a deterministic trend.

It is also possible to supplement the first-differenced estimates with lagged values of the levels of per capita GDP and infant mortality in a formal Error-Corrections Model (ECM). This approach estimates the stochastic co-movement of infant mortality and GDP, and relates observed infant mortality to the deviation from long-run equilibrium in the previous period (Engle and Granger 1987). Equation (3) illustrates this method.

$$(3) \qquad \mathrm{D_{ct}} - \mathrm{D_{ct\text{-}1}} = \eta(\mathrm{logGDP_{ct}} - \log GDP_{ct-1}) + \chi(D_{ct-1}) + \lambda(\log GDP_{ct-1}) + (\varepsilon_{ct} - \varepsilon_{ct-1})$$

Additionally, the presence of the lagged dependent variable in the ECM specification generates residuals that closely hew to the properties of classical error terms.[4]

An alternative to these first-differenced specifications is to assume that the series are stationary, but follow deterministic trends. We remove these trends by regressing our country-specific series on flexible formulations of time (equations (4) and (5)):

$$(4) \qquad \mathrm{D_{ct}} = \mathrm{f_c}(t) + \upsilon_{ct}$$

and:

$$(5) \qquad \mathrm{logGDP_{ct}} = \mathrm{g_c}(t) + u_{ct}$$

where $f_c(t)$ and $g_c(t)$ can be linear, quadratic, or cubic functions of time.[5] The residuals from these regressions give de-trended measures of the infant mortality rate and per capita GDP, $\mathrm{D^*_{ct}}$ and $\mathrm{logGDP^*_{ct}}$, where the trends that have been removed are country-specific. Country-specific trends allow, for example, for the adoption of medical technology over time to vary across countries, and relaxing the assumption of a single global time trend has been shown to be

[4] In general, we analyze relatively short time series and thus do not emphasize dynamic considerations that can be explicitly modeled, for example, in a Vector-ECM framework or with Arellano-Bond type instrumental estimators with data series of greater length. It is reassuring, however, that results (not shown) from either a two-lag VECM model or Arellano-Bond estimator are nearly identical to the findings reported in this paper.
[5] When we extend the analysis to include a quartic function of time, we obtain results that are very similar to those from the cubic specification. Therefore we only report results through the cubic formulation.

important in practice (Jamison et al. 2004). Finally, the detrended measure of infant mortality is regressed on the detrended measure of per capita GDP as shown in equation (6):

(6) $$D^*_{ct} = \log GDP^*_{ct} + \varepsilon^*_{ct}$$

We also consider specifications that smooth series with two standard time-series filters—the Hodrick-Prescott filter, and the more recent Baxter-King filter (Hodrick and Prescott 1997; Baxter and King 1999). The Baxter-King filter adopts an approximate medium frequency band pass filter that is constrained to produce stationary outcomes when applied to trending time series.[6] The Hodrick-Prescott filter decomposes a time series into a trend and a cyclical component and then determines the optimal trend component that minimizes a linear combination of deviations from trend and variations in the growth rate of trend.

Our main set of results using the country-year averages is presented in Table 1. Specifications that use the level of infant mortality as the dependent variable are presented in the upper panel of the table, those that use the log of the infant mortality rate, and hence relate proportional changes in infant mortality rather than absolute changes, in the lower panel. Per capita GDP always enters the regression in logs. The sample size in these regressions corresponds to the number of country-year observations for which data on infant mortality can be calculated. Column 1 presents the results from a specification in first differences; in column 2, the first-differenced model is supplemented with lagged levels of per capita GDP and the infant mortality rate; columns 3 through 5 present specifications in which linear, quadratic, or cubic trends are removed from the data; and finally, in columns 6 and 7, the Hodrick-Prescott and Baxter-King filters are used to remove low- and medium-frequencies from the data series.

[6] We adopt the Baxter-King recommendation of a seven year bandwidth around each time point.

The coefficients in Table 1 provide clear, consistent evidence of a negative association between the stationary or detrended components of per capita GDP and the infant mortality rate.[7] The implied effects are substantial: a one-percent increase in per capita GDP is associated with a decrease in infant mortality of between 0.18 and 0.44 deaths per thousand children born. When the dependent variable is defined in logs, the coefficients imply an elasticity of between -0.31 and -0.79.[8] Note also that those methods which are most flexible in accounting for underlying secular trends result in larger (in absolute value) estimates of the association between per capita GDP and infant mortality. Previous studies have generally adjusted only for linear trends (as in Jamison et al. 2004) and hence may underestimate the contemporaneous relationship between detrended GDP and infant mortality.

B. Main results based on unit data

Table 1 suggests that, no matter how we account for underlying trends, there is a robust association between infant mortality and per capita GDP when we look at country-year averages. We therefore now move to analysis of the unit data with the assurance that modeling deterministic trends with a polynomial in time does not yield results that are substantially different from those obtained with other methods. We begin by presenting the results from a one-step regression of the probability that child i born to mother m in country c and year t dies as a

[7] These results are not particularly sensitive to the decision to adopt a 10 year recall period. For example, in the change on change specification in Table 1 we estimate that (based on the 10-year recall), the elasticity of infant mortality with respect to per capita GDP is -0.557 (with a standard error of 0.146); when we do the same analysis with a 15-year recall, the comparable value is -0.518 (with a standard error of 0.132); and when we conduct the analysis with a 5-year recall period, the point estimate is -0.514 (with a standard error of 0.239). A complete set of results with different recall periods is available from the authors upon request.

[8] Deaton (2006) uses the WDI data to show that there is a strong negative relationship between the growth of GDP and the *proportional* change in infant mortality, but no significant relationship between the growth of GDP and the *absolute* reduction in infant mortality. We identify a negative relationship between GDP and infant mortality both when infant mortality enters in logs and when it enters in absolute levels.

function of per capita GDP in that country-year. These regressions include country-specific intercepts, and country-specific linear, quadratic, or cubic time trends as follows:

$$\text{(7)} \qquad D_{imtc} = \alpha_c + \log GDP_{ct} + f_c(t) + \varepsilon_{imtc}$$

Standard errors are adjusted for clustering at the level of the country-year observation; similar results are obtained when clustering at the level of countries or individual surveys.

We would expect the results from this one-step procedure to be very close to those from the two-step procedure that removes trends from the data, although there are some minor differences in estimation.[9] The first row in Table 2 shows that, reassuringly, estimates based on the one-step procedure imply that a one unit-change in log per capita GDP is associated with a -17.22 to -44.61 change in mortality, compared to -18.03 to -40.95 for the corresponding estimates from the two-step procedure in Table 1.

I. Adjusting for changes in the composition of women giving birth

In principle, there are two mechanisms that could account for a negative association between infant mortality and aggregate economic circumstances. First, it is possible that a child born to a woman of given characteristics is more likely to die if economic circumstances are unfavorable. Second, it is possible that the composition of women giving birth changes with economic circumstances. Clearly, these two causes for the countercyclical relationship between GDP and infant mortality—changes in mortality risk for a child born to a given woman or

[9] One difference concerns the weighting of the data. In the two-step procedure in Table 2, observations are weighted by the survey weights provided in the DHS in the first step, and are then weighted by the inverse of the variance of the estimated coefficient on the year dummy in the second step. In the one-step results presented below, observations are weighted only by the survey weights provided in the DHS. Since the one-step procedure directly analyzes the unit data, it implicitly "weights" the estimates for a given country-year cell by the number of observations in this cell, N. This is closely related, but not equivalent to, the inverse of the variance. A second difference is that the two-step procedure removes separate trends from the GDP and infant mortality data, while the one-step regression only includes a single time trend for each country.

compositional changes in the pool of women giving birth—have very different implications. We now turn to this question.

An obvious way to adjust for compositional changes is to include the characteristics of women, children, and births in equation (7), which gives us the following:

$$\text{(8)} \qquad \mathrm{D}^{\mathrm{adj}}_{\mathrm{imtc}} = \alpha_{\mathrm{c}} + \log\mathrm{GDP}_{\mathrm{ct}} + \mathrm{f}_{\mathrm{c}}(t) + \beta X_{im} + \varepsilon_{imtc}$$

where X_{im} is a vector of characteristics of child *i* born to mother *m*. Recall that child births and deaths are calculated on the basis of retrospective questions asked of mothers at the time of the survey, which limits the variables that can be included in (8). In practice, we control for a cubic term for mother's years of education and place of residence (urban or rural) at the time of the survey; birth order, the gender of the child, and whether or not the child was a multiple birth; and a cubic term for maternal age at the time of the birth. All of these variables are highly correlated with the probability of child survival.[10] This approach implicitly assumes that place of residence at the time of the survey is correlated with place of residence at the time of child birth, and that schooling has been completed by age 15; these should be reasonable approximations for most of the countries and years in our sample.

Results from regressions that include the vector of covariates X_{im} are presented in the second row of Table 2. These results show that including these covariates has a negligible effect on estimates of the association between log per capita GDP and infant mortality.

In addition, as an alternative means of controlling for compositional effects, we include a set of mother fixed effects, as well as birth-specific characteristics (child gender and an indicator for multiple births). This approach has the advantage that it controls for all time-invariant mother characteristics, not just education and place of residence, but limits the sample to women who

[10] There is an extensive literature on this topic. See, for example, the review papers by Behrman and Deolalikar (1988); Strauss and Thomas (1998); and Schultz (2002).

have had at least two live births. To put things in context, we therefore first report the results from regressions that do not include mother characteristics or fixed effects for this smaller sample of women. These results, reported in the third row of Table 2, show that the association between per capita GDP and infant mortality is similar in this smaller sample and the full sample of women. Finally, in the last row of the table, we report the results that include the mother fixed effects and birth-specific characteristics. These coefficients tend to be somewhat larger, especially in the specification that only includes a linear trend, suggesting that women with characteristics that are associated with higher mortality are more likely to time their fertility to coincide with good economic times—see Dehejia and Lleras-Muney (2004) for similar findings for the US. However, Table 2 makes clear that the changing composition of women cannot account for the bulk of the association between infant mortality and log per capita GDP that we observe in our data.

II. Omitted variables

Now that we have seen that changes in the composition of mothers giving birth do not drive our results, we discuss whether there are plausible omitted variables that affect both aggregate economic conditions and infant mortality, and hence bias our estimates. Two of the most likely candidates are conflict, including civil war, and weather shocks, including droughts and floods. Conflict may directly result in infant deaths or disrupt the provision of health services. Large-scale droughts or floods may change the health environment and the transmission of communicable disease. Further, conflicts, floods, and droughts may all result in economic contractions—thus raising the possibility that the association between infant mortality and per capita GDP we observe in our data is spurious.

To control for the possible effect of weather shocks, we use data from the Global Precipitation Climatology Project (GPCP) (Miguel et al. 2004; see also Huffman et al. 1995, 1997).[11] These series are not complete—we have rainfall data for 797 of the 907 country-year observations for which we can calculate infant mortality, although there does not appear to be a common pattern of countries and years with missing rainfall data. We take two approaches to assessing whether our basic results are sensitive to the inclusion of data on rainfall. Our first approach identifies as "dry" periods those years in which rainfall was below the 10th percentile of the annual rainfall data for a particular country, and as "wet" periods years in which rainfall was above the 90th percentile of observed rainfall.[12] We create indicator variables for births in wet and dry years and then include them in a specification similar to Equation (7) along with interaction terms between these indicators and log per capita GDP. In our second approach we simply include the continuous precipitation measure as a regressor along with an interaction term with log per capita GDP. The coefficients on log per capita GDP (analogous to the coefficients in Table 2) are presented in the second panel of Table 3. These results make clear that adjusting for rainfall has a negligible effect on the main results.[13]

In the next panel of Table 3, we explore the possible effect of conflict. We consider two definitions of conflict. The first of these is based on Sambanis (2000). We define as a conflict any war event (internal or external) that, over the course of the conflict, killed 0.5 percent or

[11] The GPCP data rely on a combination of actual weather station rainfall gauge measures, as well as satellite information on the density of cold cloud cover (which is closely related to actual precipitation), to derive rainfall estimates, at 2.5 latitude and longitude degree intervals. The units of measurement are in millimeters of rainfall per day and are the average per month. We multiply each monthly average by the number of days in a given month, which gives us an estimate of total monthly rainfall. We then sum all of the total monthly estimates in a given year to generate an estimate of total yearly rainfall. Next, each yearly rainfall estimate per 2.5 latitude / longitude degree node is averaged over all nodes in a given country to produce an estimate of total yearly rainfall per country.

[12] For example, in Bangladesh we have rainfall and infant mortality data between 1983 and 1999. Mean annual rainfall over this period was 1559 milliliters per year. In 1983 and 1984, rainfall was below the 10th percentile for the Bangladesh series (1180 and 1260 milliliters, respectively), and in 1993 and 1998 rainfall was above the 90th percentile for the series (1821 and 1914 milliliters, respectively).

[13] Infant mortality tends to be higher in "wet" years. However, the interaction between the rainfall terms and GDP is small and insignificant.

more of the initial population, regardless of whether these were battle deaths or other casualties. Note that most war events in the data took place over a large number of years—for example, 1981 to 1989 for Nicaragua, or 1983 to the present for Sudan—so the number of deaths in any given conflict year is substantially smaller. The second definition of conflict is based on the UCDP/PRIO Armed Conflict Dataset (Gleditsch et al. 2002). These data identify "internal armed conflicts", "interstate armed conflicts", or "internal internationalized armed conflicts" for every country and year in our data, and also provide measures of the intensity of the conflict. We take the most conservative approach and create an indicator variable for any conflict. In both cases, we then parallel the analysis for rainfall periods, interacting the indicator for conflict with log per capita GDP, and including this interaction and the main effects for log per capita GDP and conflict in the regressions. The results, presented in the third panel of Table 3, show that controlling for possible conflict impact has no appreciable effect on our estimates of the association between infant mortality and log per capita GDP.

Other factors that have been stressed in the literature that could be correlated with both GDP and infant mortality shocks, such as education and the quality of institutions (Deaton 2006), are also unlikely to account for the basic pattern of results we observe. The results in Table 2 show that controlling for mother education has little effect on our estimates. Moreover, our approach focuses on short-term (yearly) changes in per capita GDP and infant mortality, and one would expect factors such as education and the quality of institutions to follow trends or cycles that are considerably longer than single-year changes. Nevertheless, we make various attempts to correct for possible shocks to institutional quality that could be correlated with both the GDP and infant mortality trend deviations that form the basis of our estimates.

The results from these specification checks are included in the bottom panel of Table 3. We first include inflation as an additional covariate in our basic set of regressions with the logic that high inflation may serve as one signal of poor overall quality of governance. Inflation is not correlated with trend deviations in infant mortality, does not significantly interact with GDP, and hence does not alter the basic pattern of results. Second, we include a governance quality measure developed by Marshall and Jaggers (2002). This "polity" measure is derived from a coding of the competitiveness of political participation, the regulation of participation, the openness and competitiveness of executive recruitment, and constraints on the chief executive. The measure ranges from 10 (corresponding to strongly democratic governments) to -10 (strongly autocratic governments). As with inflation, including the measure of polity in the regressions does not substantially change our main results. Third, to account for possible disruptions resulting from changes in government, we identify all country-years where a regime change takes place and create an indicator for such events.[14] Once again, the coefficient on per capita GDP changes very little once we include this indicator and an interaction term with GDP.

In sum, while we cannot account for every possible omitted variable in our analysis, these specification checks suggest that the most obvious candidates have little effect on our results. We therefore posit that the association between short-term fluctuations in per capita GDP and infant mortality is robust and most likely has a causal interpretation.[15]

[14] There are 70 such events in our data.

[15] The AIDS epidemic could bias our results if it results in both large-scale infant deaths and an economic slowdown (see, for example, Bell, Devarajan, and Gersbach 2006, although the focus of their analysis is on the long-term consequences of AIDS). However, the negative association between per capita GDP and infant mortality we report is apparent in every region in the developing world. Indeed, the association tends to be *stronger* in regions where HIV infection rates are relatively low (such as Latin America) than in regions where HIV rates are highest (in particular, Sub-Saharan Africa). (These results are available from the authors upon request.) Given this pattern of effects it seems unlikely that the AIDS epidemic is an important omitted variable in our analysis. Maternal health is another potential omitted variable if it affects both infant health and economic output. However, as we show below, the relationship between per capita GDP and infant mortality is most pronounced in periods in which the economic shock is largest—for example, when per capita GDP deviates by 1.5 standard deviations or more from its long-term

III. Timing of shocks to GDP

So far we have focused on the contemporaneous relationship between GDP and infant death, but have not looked more precisely at dynamic considerations. We next turn to a discussion of how the timing of shocks to per capita GDP affects birth outcomes. We begin by including terms in lagged and lead per capita GDP in our basic regression. The top panel in Table 4 shows that the coefficients on both of these terms are small, and are not significant at conventional levels. Moreover, the coefficient on current GDP is essentially unaffected by the inclusion of these additional terms, suggesting a purely contemporaneous effect of GDP deviations on infant health.

In addition to serving as a specification check, the lag and lead terms in the regression speak to the issue of the timing of the impact of the GDP shock on infant survival. To see this, it is useful to work out what the coefficients on lagged, current, and lead GDP imply for children born at different times in the year. For a child born early in the year (say, in January), the coefficient on lagged GDP mainly reflects conditions before conception and in utero, the coefficient on current GDP reflects conditions in the first year of life, and the coefficient on lead GDP reflects conditions in the second year—beyond the period that is relevant for the calculation of infant mortality. By contrast, for a child born late in the year (say, in December), the coefficient on lagged GDP reflects conditions before conception, the coefficient on current GDP reflects conditions in utero, while the coefficient on lead GDP reflects the conditions after birth. Finally, for a child born at the midpoint of the year, on June 30, lagged GDP reflects conditions before conception and during the first three months in utero, current GDP reflects conditions in

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trend. It seems unlikely to us that changes in economic conditions of this magnitude would be driven primarily by changes in maternal health. Moreover, as shown below, negative economic shocks affect the mortality of girls but not of boys, which also makes it less likely that maternal health is the main reason we observe an association between aggregate economic conditions and infant mortality.

the last six months in utero and the first six months after birth, while lead GDP reflects conditions after the infant is six months of age. Only the coefficient on current GDP in the top panel of Table 4 is significant. This suggest that it is not the economic conditions early in the pregnancy which are most important in determining infant mortality—these conditions are loaded on lagged GDP for most children, and the coefficient on lagged GDP is insignificant. Similarly, it does not appear to be that conditions in the later part of a child's first year in life substantially affect the probability of survival—these conditions are loaded on to lead GDP for most children in our sample, and the coefficient on lead GDP is also insignificant. Rather, it appears that economic conditions in those months shortly before and shortly after birth have the biggest effect on the probability that a child survives.

It is possible to make a further attempt to clarify issues about the window of vulnerability that infants face with regard to GDP shocks. Mothers report the year and month of birth of each child, and we assign the 15th day of the relevant month as the birth date for each child. We then construct birth-month specific exposure windows for economic conditions in utero (which is a weighted average of GDP in the lagged and current years, with the weights determined by the month in which a child was born); economic conditions in the first month of life, which corresponds to current GDP;[16] and economic conditions in the next eleven months of a child's life (which is a weighted average of GDP in the current and lead years, with the weights determined by the month in which a child was born). We then replace the terms for lagged, current, and lead GDP with these terms for economic conditions in utero, in the first month of life, and in the next eleven months. The results from these regressions are presented in the lower panel of Table 4. The coefficients on economic conditions in utero and after the first month of

[16] Exceptions are children born in December, for whom economic conditions are defined as an average of conditions in the current and lead years.

life are both small and insignificant. By contrast, the coefficient on per capita GDP in the first month is large and significant—for example, in the specification that includes a cubic time trend it is -53.93, with a standard error of 20.85. Once again, it appears that economic conditions around birth are most important for determining infant survival.

Our analysis of timing has obvious limitations because we only observe annual GDP data. For example, if instead of focusing on the month of birth we define a variable for the first *two* months after birth, and run our basic regression including the cubic time trend and variables for per capita GDP in utero, and after the second month of life, the coefficient on per capita GDP in the two-month window after birth is -45.81, with a standard error of 17.94. The coefficients on the other two exposure variables are small and insignificant, as before. Similarly, when we define a variable for the last two months in utero, and run our basic regression including the cubic time trend and variables for per capita GDP in the first seven months in utero and the twelve months after birth, the coefficient on per capita GDP in the two-month window before birth is -39.23, with a standard error of 21.30. Once again, the coefficients on the other two exposure variables are small and insignificant. Clearly, with annual GDP data, it is not possible to tease out the relative importance of conditions in narrow windows of exposure. Rather the results suggest that conditions close to the time of birth appear to be more important in determining survival than those in the early in utero period, or in the later part of a child's first year of life.[17]

[17] We also experimented with breakdowns of the in utero period. For example, in a study of the effect of the Chernobyl nuclear disaster, Almond et al. (2007a) show that radiation exposure was particularly damaging during the period between 8 and 25 weeks after conception. The emphasis of our paper is on economic conditions, rather than radiation exposure, but it is conceivable that the period of 8 to 25 weeks post-conception is one in which health insults more generally are particularly damaging. However, in none of the specifications we ran was the coefficient on economic conditions in the period corresponding to 8-25 weeks after conception significant once we controlled for conditions in the last three months of pregnancy and after birth.

Finally, the importance of economic conditions "around" birth for infant survival yields some clues about the likely transmission mechanisms from trend deviations in per capita GDP to infant mortality. Low birthweight is considered an important risk factor in predicting neonatal and infant death (see for example the review by Lawn et al. 2005). However, the fact that the coefficient on economic conditions for much of the in utero period is not significant in Table 4 suggests that this is unlikely to be the main reason for elevated infant mortality during economic downturns.[18] On the other hand, attendance during birth, or health care for children who face infections shortly thereafter, may help explain our findings. Approximately 36 percent of neonatal deaths worldwide are a result of severe infections during birth, and another 23 percent are a result of asphyxia (Lawn et al. 2005). Poor economic conditions around birth could result either in a deterioration in public health services or in a decrease in households' ability to pay for public or private birth assistance (as suggested for example by Paxson and Schady 2005 in their analysis of infant mortality in Peru), both of which could lead to increased mortality in the first year of life.

IV. Heterogeneity

Up to this point, we have implicitly assumed that aggregate income shocks affect all mothers and children equally. Yet, there are a host of reasons why this need not be so. For example, more educated and wealthier mothers may be better able to smooth consumption of critical inputs into child health; there may also be within-household discrimination so that boys are better protected from negative health shocks than girls. We now turn to the question of heterogeneity of impacts, focusing on differences by the gender of a child, the education and age of the mother, place of residence, and birth parity.

[18] Selection may be important if poor economic conditions in utero lead to a higher rate of spontaneous abortions. The sample of children born alive may then have higher health endowments, introducing a downward bias to the association we estimate between economic conditions in utero and infant mortality.

To motivate our results, we first present the mean infant mortality rates in Table 5 for each mother or child characteristic we use in our analysis. The first row of the table shows that girls are almost ten percentage points less likely to die in the first year of life than boys—a well-known finding in the demographic literature.[19] The other coefficients show that children born in rural areas are more likely to die than those born in urban areas; that the mortality of children born to mothers with less than primary schooling is more than twice as high as that of children born to mothers with completed primary schooling or more; that children born to young mothers (age 15-19) and older mothers (age 35-39) are more likely to die than those born to "prime-age" mothers (age 20-34); and that high-parity births (5th birth or higher) are also more likely to die than lower parity births.

We next analyze heterogeneity in the relationship between detrended per capita GDP and infant mortality along these observable dimensions of mothers and children. Our approach is straightforward. In each case, we generate an indicator for the characteristic in question—for example, an indicator for the birth of a girl—and then interact this indicator with the measure of log per capita GDP. Table 6 then reports the coefficients on the main effect for log per capita GDP and on the interaction between log per capita GDP and the given characteristic. We focus on the specifications that include country-specific cubic time trends, as these account for underlying time trends most flexibly.

Table 6 shows that the mortality of infants born to mothers with low education levels is significantly more sensitive to changes in economic conditions than that of children born to mothers with higher education levels. In part, this is likely the result of the higher mortality rates among women with lower education levels, although this cannot fully explain the differences in

[19] For example, the World Health Organization (2006) estimates that the male-to-female ratio in neonatal mortality and in early neonatal mortality in developing countries is 1.3.

the magnitudes we estimate—a one log-unit decrease in per capita GDP results in an increase in mortality of approximately 69.5 percent for low-education mothers, and 34.4 percent for high-education mothers.[20] A similar pattern can be seen in a comparison between rural and urban areas—the increase in mortality during economic downturns is larger in rural areas, but from a higher base. However, the most striking result in the table relates to differences by gender. Although the average mortality among boys is higher than among girls, Table 6 shows that the mortality of girls is much more sensitive to changes in economic circumstances than that of boys: A one log-unit change in per capita GDP changes the mortality of boys by approximately 28 percent, and that of girls by approximately 77 percent—a remarkable difference by any standard.

V. Magnitude and sign of shocks to per capita GDP

A logical extension of our inquiry is to consider differences by the sign (positive or negative) and the magnitude of the GDP shock. We begin by showing a nonparametric regression of the likelihood that an infant dies (scaled to standard units of the infant mortality rate) as a function of the income shock, after removing country intercepts and country-specific trends from the data.[21] The results are shown in Figure 1, without controlling for mother and child observables (solid line), comparable to the first row in Table 2, and after controlling for mother and child observables (dashed line), comparable to the second row of Table 2. The figure shows a downward sloping relationship between infant mortality and economic conditions

[20] A one log-unit decrease in per capita GDP would increase the infant mortality rate of children born to low education women from 104 to 160.7, and that of children born to high education women from 51.7 to 69.5.

[21] Our approach is closely related to the two-stage procedure described earlier in the paper. Specifically, we regress the dummy for infant death on country dummies and a country-specific cubic in time and predict the residual from this regression. This regression can also include characteristics of mothers, births, or children—see the discussion in section 3.B.II above. We also regress GDP on country dummies and a country-specific cubic in time, and predict the residuals from this regression. Finally, we use locally weighted least squares to depict the relation between the residual from the infant death regression and the residual from the GDP regression. For presentational purposes, the figure is trimmed at the 1st and 99th percentiles of GDP deviation from cubic trend.

throughout the distribution. Controlling for the composition of mothers giving birth makes little difference. However, the association between per capita GDP and infant mortality appears to be strongest when there are large, negative shocks to per capita GDP—the slope of the line is steepest at the left-hand side of the graph.

We next extend this analysis to consider differences by gender, again focusing on the sign and magnitude of the shock to per capita GDP. Ex ante it is not clear whether we would expect negative shocks to have a larger or smaller effect on the mortality of girls than boys. On the one hand, there is a long-standing literature that suggests that parents in many settings favor boys over girls during lean times—see, for example, Das Gupta (1987), Behrman and Deolalikar (1990) and Rose (1999), all of which focus on gender differences in India. On the other hand, a more recent literature has used insights from the Trivers-Willard (1973) hypothesis in evolutionary biology, which argues that natural selection should skew the sex ratio towards female offspring as maternal condition declines.[22]

Figure 2 presents the results from gender-specific nonparametric regressions. The figure suggests that boys and girls benefit from positive shocks to per capita GDP in a similar way. On the other hand, negative shocks are much more harmful to girls than to boys.

We analyze the same question parametrically using gender-specific continuous spline regressions in Table 7. The first (top) row for both panels presents the results from a spline regression with a knot at zero, which allows for different slopes for positive and negative changes in GDP. We then turn to spline regressions with two knots. In the second row these

[22] Empirical applications of Trivers-Willard include Almond and Edlund (2007), who find that infant deaths in the US are more likely to be male if the mother was uneducated and young (and therefore more likely to be poor); Hopcroft (2005) who argues that sons of high-status fathers attain more education than daughters in the US, while the opposite is true for offspring to low-status fathers; and Almond et al. (2007b), who argue that the dramatic economic downturn that followed the Great Leap Forward in China reduced the sex ratio (males to females), a finding they attribute to heightened male mortality.

knots are fixed at -1σ and 1σ (where σ stands for standard deviations of GDP trend deviations in our sample), in the third row they are fixed at -1.5σ and 1.5σ, and in the fourth (bottom) row the knots are fixed at -2σ and 2σ; these specifications therefore allow for the slope to differ between GDP shocks of different signs and magnitudes, with the cut-off between “large” and small” shocks varying across specifications. All of the regressions are based on the unit data, and all allow for country-specific cubic trends in time.

Table 7 presents a similar pattern to that observed in Figure 2. Positive shocks to per capita GDP affect girls and boys in a similar fashion. On the other hand, negative shocks have larger effects on the mortality of girls than boys. For example, for negative shocks of -1.5σ or larger, a one unit decrease in log per capita GDP is associated with an increase in girl infant mortality of -124.95 (with a standard error of 58.13), but an increase in boy infant mortality of only -26.00 (with a standard error of 35.88). Figure 2 and Table 7 therefore appear to be consistent either with girls being more fragile in their first year of life than boys, which seems unlikely, or with families protecting boys more than girls during economic downturns. Put differently, this suggests that household behavioral responses to negative shocks play an important role in determining infant survival. Finally, these results underscore the argument that the main results are not due to omitted variables, as these omitted variables would have to interact with both the gender of the child and the direction of the income shock. It is hard to imagine what such an omitted variable would be.

We conclude this section by providing a sense of the magnitude of the estimated effects. The average year-on-year decrease in infant mortality in our sample is 2.5 per 1000 live births. On average, countries with a negative shock to per capita GDP of 1.5σ or larger had a contraction of 5.9 percent. (There are 122 such events in our data.) The average increase in girl infant mortality during these negative shocks to aggregate income is 7.4 deaths per 1000, that of boy mortality is 1.5 per 1,000. These simple back of the envelope calculations suggest that the magnitude of the effects of income volatility on infant mortality is large by any standard.

4. Conclusion

Macroeconomic volatility is a fact for most developing countries. In recent decades, the standard deviation of income over time has been approximately twice as large in developing as developed countries (Aguiar and Gopinath 2007). A recent review stresses the welfare costs of volatility for developing countries in terms of their inability to smooth consumption (Loayza et al. 2007). In this paper we document another way in which aggregate economic fluctuations can have dramatic welfare consequences. In developing countries, infants, in particular girls, are more likely to die when there is a negative economic shock.

Our analysis adds to the existing literature on economic volatility and child health in several important ways. One strand of this literature has used unit data for a single country to investigate the effects of income shocks (for example, Paxson and Schady 2005; Miller and Urdinola 2007); the findings from these papers are mixed. Another strand has used available bi-decennial aggregates of infant mortality and income (for example, Pritchett and Summers 1996; Jamison et al. 2004); with these data it is hard to convincingly control for changes in the composition of women giving birth, or to investigate whether the estimated effects vary with the characteristics of mothers and children. Moreover, bi-decennial averages are likely to have smoothed out some genuine variation in infant mortality. In this paper, we use the unit data for a large number of developing countries, fifty-nine, covering approximately 1.7 million births. This allows us to combine some attractive features of the country-specific approach—notably, to control for compositional changes and to explore heterogeneity—as well as those from the cross-country approach—notably, to test whether our results are robust to different methods of accounting for deterministic trends or non-stationary processes in the data. We also show that the association between per capita GDP and mortality we observe is not a result of omitted variables that have received attention in the literature—including female education, droughts or floods, conflicts, or the quality of institutions.

We conclude by discussing two areas where our data impose limitations on the possible analysis we can conduct. The first of these is the timing of the GDP shocks. Our results suggests that it is macroeconomic conditions around birth, rather than in the early in utero period or in the later half of a child's first year of life, which matter most for a child's survival in her first year. However, because our data on GDP is annual, our results on the timing of the shock to aggregate income should be viewed as suggestive rather than definitive. Second, because we construct birth and death histories retrospectively, we do not have data on the utilization of health services before, during and after birth for the majority of births (and deaths) we observe. Further, the DHS data we use do not include information on other potential inputs into child health, such as the consumption of nutritious foods. We are therefore unable to explore in a satisfactory manner the transmission mechanisms from income shocks to infant mortality. Nevertheless, our results clearly indicate that short-term fluctuations in aggregate income can have important consequences for the likelihood that a child survives her first year of life. Earlier research has shown that volatility in aggregate income has implications for adult health and mortality (for example, van den Berg et al. 2006; Maccini and Yang 2007). Our research suggests that the benefits of policies that reduce the volatility of per capita GDP in developing countries, or that protect health status during sudden economic downturns, may have significant short term benefits for child survival as well, especially that of girls.

Data Appendix

The data used for this paper are derived from all of the Demographic and Health Surveys publically available by end-2005. Table A1 provides details on the countries, years, and sample sizes of these surveys. (More information on individual surveys can be found at www.measuredhs.com.)

To give a broad overview of the data we first calculate infant mortality for individual country-year cells, and present a scatter plot of infant mortality and log per capita GDP in Figure A1. Infant mortality varies considerably in our sample, and there is a clear negative association between infant mortality and GDP. On average, (with these "averages" imputed from a nonparametric regression) countries at the 10th percentile of log GDP have an infant mortality rate that is more than twice as high as those at the 90th percentile of log GDP (130 deaths per thousand, compared to 56 deaths per thousand).

We next compare the estimates of infant mortality we construct with estimates of infant mortality in the World Development Indicators (WDI) data base (World Bank 2007). Aggregate data like the WDI have been used frequently for research on the relationship between income and infant mortality in the developing world (for example, Pritchett and Summers 1996; Jamison et al. 2004; Deaton 2006). The WDI data are based on data from both the United Nations Statistical Division and UNICEF. These two bodies collate data from administrative records (civil registration, population registration), population and housing censuses, and social and demographic surveys. Demographic smoothing processes are then used to generate bi-decennial estimates of infant mortality (World Bank 2007b). Given the difference in methodologies and data sources used to construct the infant mortality series in the WDI series and in our own, we would not expect the estimates for any given country-year to be identical across both data sets but we would hope there to be sufficient overlap and similarity. Figure A2 plots the estimates of infant mortality we construct from the DHS against the WDI estimates, for all years of overlap. Estimates of infant mortality from the DHS and WDI tend to be very highly correlated with each other. The average infant mortality rate in the WDI series for countries and periods that also have DHS is 82.8, that estimated exclusively from the DHS is 85.3. The R-squared in a regression of the WDI infant mortality on the DHS infant mortality is 0.81.

As a final check on our measures of infant mortality we exploit the fact that, for countries with more than one survey, we can construct more than one estimate of infant mortality for a given year. There are 346 country-year observations with more than one estimate of infant mortality in our data. (In our main results, including in Figures A1 and A2 above, we take the average of these estimates.) When we regress the infant mortality rate for a given country-year calculated from the later survey on that calculated from the earlier survey, the coefficient is 0.90 (with a standard error of 0.03). This result is consistent with a close relationship between the two measures of infant mortality in the presence of some attenuation bias from measurement error.

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Table 1. Change in (or detrended) Infant Mortality Rate (IMR) on change in (or detrended) Log Per Capita GDP, by various methods of trend accounting

Dependent variable	Δ on Δ	Error correction model	Linear	Quadratic	Cubic	Hodrick-Prescott filter	Baxter-King filter
IMR	-32.18	-43.56	-18.03	-30.72	-40.95	-40.17	-44.41
	[7.59]***	[8.40]***	[6.34]***	[6.03]***	[7.74]***	[8.97]***	[10.70]***
Log IMR	-0.557	-0.667	-0.311	-0.434	-0.590	-0.646	-0.785
	[0.146]***	[0.169]***	[0.126]**	[0.095]***	[0.124]***	[0.166]***	[0.218]***
N	840	839	900	900	900	876	533

Note: All regressions are weighted. Robust standard errors, depicted in brackets, are clustered at the country level. N refers to the number of country-year cells used in the estimation. GDP is measured in year 2000 international (PPP) dollars.
* p<.10, ** p<.05, *** p<.01

Table 2. One stage estimates of relation between IMR and GDP, by various trend accountings

Dependent variable	Linear	Quadratic	Cubic
		Unadjusted	
IMR	-17.22	-33.22	-44.61
	[6.95]**	[8.04]***	[9.13]***
		Controlling for mother and birth characteristics	
IMR	-17.32	-30.61	-42.22
	[6.65]***	[7.74]***	[8.77]***
		Unadjusted, restricted to mothers with multiple births	
IMR	-20.09	-30.91	-43.21
	[7.96]**	[8.70]***	[10.16]***
		Mothers' fixed effects	
IMR	-33.32	-34.03	-47.99
	[5.91]***	[7.39]***	[8.81]***

Number of observed births equals 1,634,360 in first two panels, 1,356,738 in bottom two panels. Mother and birth characteristics include indicators for rural location, gender of child, and multiple birth, and cubic terms for mothers' age, years of education, and infant birth order. All regressions are weighted. Robust standard errors are clustered at the country-year level - there are 900 country-year cells. GDP is measured in year 2000 international (PPP) dollars.
* p<.10, ** p<.05, *** p<.01

Table 3. One stage estimates of IMR and GDP, controlling for potential omitted variables

Additional controls	Linear	Quadratic	Cubic
Unadjusted for potential omitted variables (from Table 2)			
No additional controls	-17.22	-33.22	-44.61
	[6.95]**	[8.04]***	[9.13]***
Rainfall			
Extreme rainfall periods	-18.04	-35.12	-45.28
	[7.01]**	[8.04]***	[9.16]***
Continuous rainfall measure	-17.62	-29.43	-46.58
	[9.12]*	[8.94]***	[10.92]***
Conflict			
Sambanis conflict definition	-17.45	-36.48	-47.21
	[6.96]**	[8.20]***	[9.10]***
UCDP conflict definition	-17.51	-33.70	-44.75
	[7.05]**	[8.17]***	[9.18]***
Governance			
Government change and durability	-15.58	-33.72	-45.77
	[7.60]**	[8.86]***	[10.17]***
Polity measure	-14.96	-33.19	-42.04
	[7.19]**	[8.26]***	[9.47]***
Log inflation	-13.72	-30.16	-46.87
	[7.97]*	[8.85]***	[10.16]***

Note:In addition to log percapita GDP, regressions in each panel also include the controls listed and full interaction terms between GDP and controls (coefficients not shown). Robust standard errors clustered at the country-year level. GDP is measured in year 2000 international (PPP) dollars. Conflict periods refer to country-year cells indicated as conflict-affected by the Sambanis (2000) data or the UCDP/PRIO Armed Conflict Dataset. Extreme rainfall periods refer to country-year observations either above the 90th percentile or below the 10th percentile of the country specific annual rainfall data series given by the GPC Project data. Government change and durability indicators are constructed from the Marshall and Jaggers (2002) data. Government change takes a value of one for country-years that experience a regime change and government durability is an indicator for governments that remained at least 7 years in power. The polity score is a continuous measure from the Polity IV dataset by Marshall and Jaggers (2002) and ranges in value from +10 (strongly democratic) to -10 (strongly autocratic). Log inflation refers to inflation from the WDI dataset of the World Bank and is defined as GDP deflator, annual %.
* p<.10, ** p<.05, *** p<.01

Table 4. One stage estimates of IMR and GDP, including possible lead and lag effects

Independent variable	Linear	Quadratic	Cubic
Lagged, Current, and Lead GDP			
Lagged GDP	1.65	-7.63	-8.07
	[11.37]	[10.31]	[11.51]
GDP	-27.99	-37.96	-40.07
	[11.91]**	[11.18]***	[11.30]***
Lead GDP	10.39	2.03	-1.73
	[8.83]	[8.70]	[9.44]
GDP Series Reweighted to Approximate Exposure over Course of In Utero Development and First Year			
In Utero	16.56	7.47	8.38
	[19.67]	[18.51]	[18.91]
First Month	-50.36	-54.2	-53.93
	[22.74]**	[21.66]**	[20.85]***
Next 11 Months	19.99	9.93	5.98
	[15.46]	[15.60]	[16.63]

All regressions are weighted. Robust standard errors are clustered at the country-year level - there are 1,549,745 observations distributed across 840 country-year cells. GDP is measured in year 2000 international (PPP) dollars.
* p<.10, ** p<.05, *** p<.01

Table 5. Estimated IMR by mother and child characteristics

Characteristic	Estimated IMR		
Child gender	Boys		Girls
	92.8		83.1
N	833,545		800,814
Mothers' education	Less than primary		Primary or greater
	104.0		51.7
N	1,093,757		540,603
Mother location	Urban		Rural
	61.1		100.4
N	555,742		1,078,618
Mothers' age	15-19	20-34	35-39
	110.3	81.7	92.7
N	296,461	1,193,847	144,052
Birth order	1st	2nd-4th	5th or greater
	83.4	80.2	106.0
N	381,176	804,593	448,591

Note: Mean estimates derived from Demographic and Health Survey (DHS) data pooled from 123 surveys covering 59 countries.

Table 6. Heterogeneity in IMR and GDP relation by mother or birth characteristics, cubic trend

Characteristic	GDP	Interaction (GDP*characteristic)
Female infant	-25.67	-38.13
	[12.58]**	[15.37]***
Low mother's education	-17.77	-38.97
	[11.30]	[15.35]**
Rural location	-18.18	-38.17
	[10.48]	[13.15]***
Young mother (<20)	-49.15	22.20
	[10.67]***	[20.83]
Older mother (>34)	-40.28	-3.95
	[9.77]***	[20.59]
First births	-54.53	44.61
	[7.23]***	[14.99]***
High birth order (>4)	-32.06	-41.61
	[10.35]***	[16.36]**

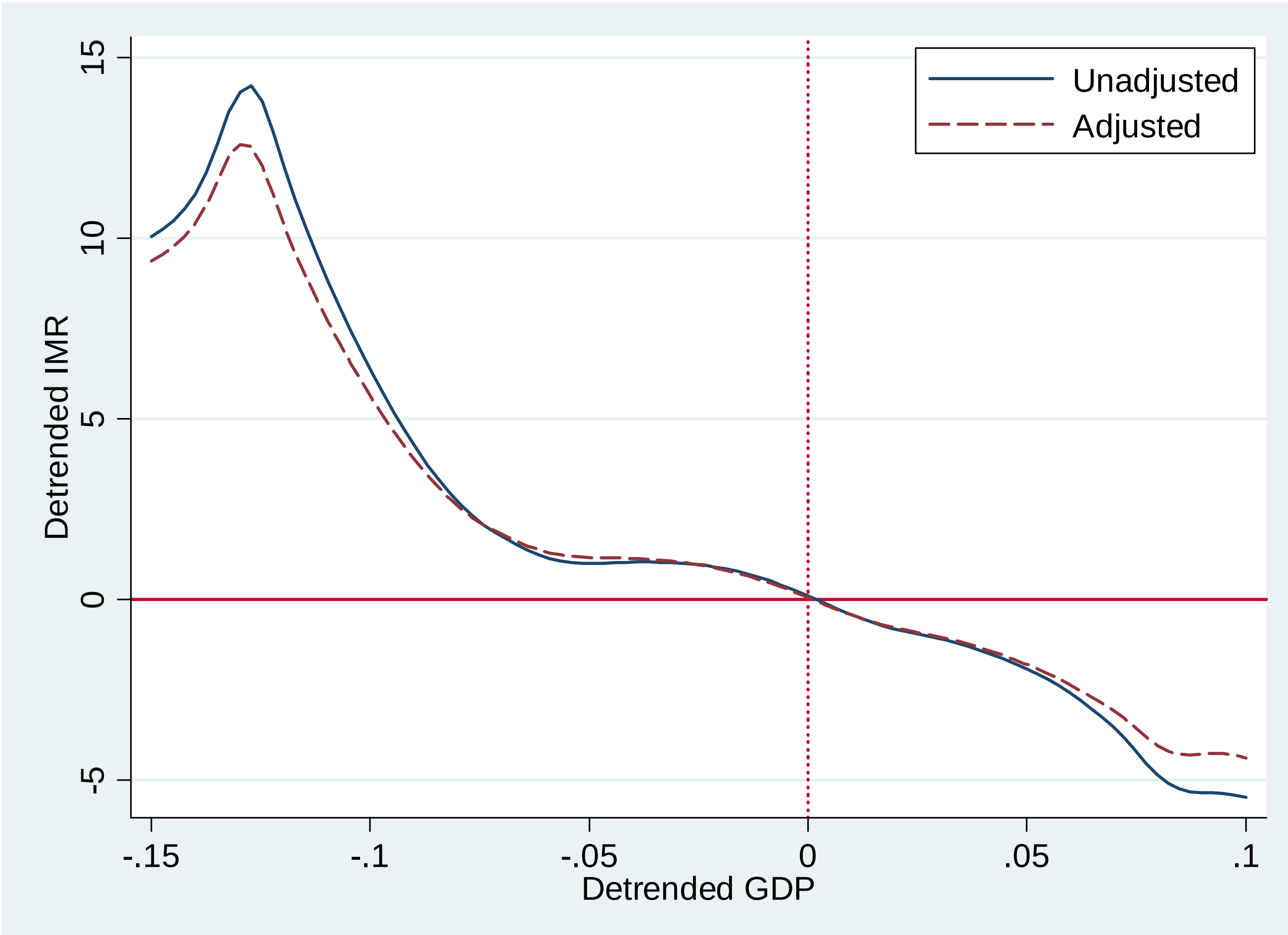
Low mother's education is defined as less than primary attainment. All regressions are weighted. Robust standard errors are clustered at the country-year level. GDP is measured in year 2000 international (PPP) dollars.
* p<.10, ** p<.05, *** p<.01

Table 7. Heterogeneity in IMR and GDP relation by size of GDP deviation from cubic trend, separate for male and female children

Dependent variable	Magnitude and direction of GDP deviation		
IMR for Boys (N=833,545)	*<= 0*		*> 0*
	-11.80		-39.27
	[18.73]		[20.16]*
	<= -1 sd	*> -1 sd & <= 1 sd*	*> 1 sd*
	-9.16	-26.63	-39.28
	[28.84]	[23.69]	[34.54]
	<= -1.5 sd	*> -1.5 sd & <= 1.5 sd*	*> 1.5 sd*
	-26.00	-16.71	-82.57
	[35.88]	[17.49]	[40.03]**
	<= -2 sd	*> -2 sd & <= 2 sd*	*> 2 sd*
	-20.73	-21.13	-88.67
	[44.42]	[14.74]	[49.50]*
Dependent variable	Magnitude and direction of GDP deviation		
IMR for Girls (N=800,814)	*<= 0*		*> 0*
	-69.05		-55.52
	[24.02]***		[18.20]***
	<= -1 sd	*> -1 sd & <= 1 sd*	*> 1 sd*
	-101.62	-39.86	-84.68
	[44.24]**	[21.43]*	[28.08]***
	<= -1.5 sd	*> -1.5 sd & <= 1.5 sd*	*> 1.5 sd*
	-124.95	-46.36	-93.18
	[58.13]**	[15.32]***	[33.73]***
	<= -2 sd	*> -2 sd & <= 2 sd*	*> 2 sd*
	-165.21	-50.66	-78.43
	[77.37]**	[12.99]***	[34.57]**

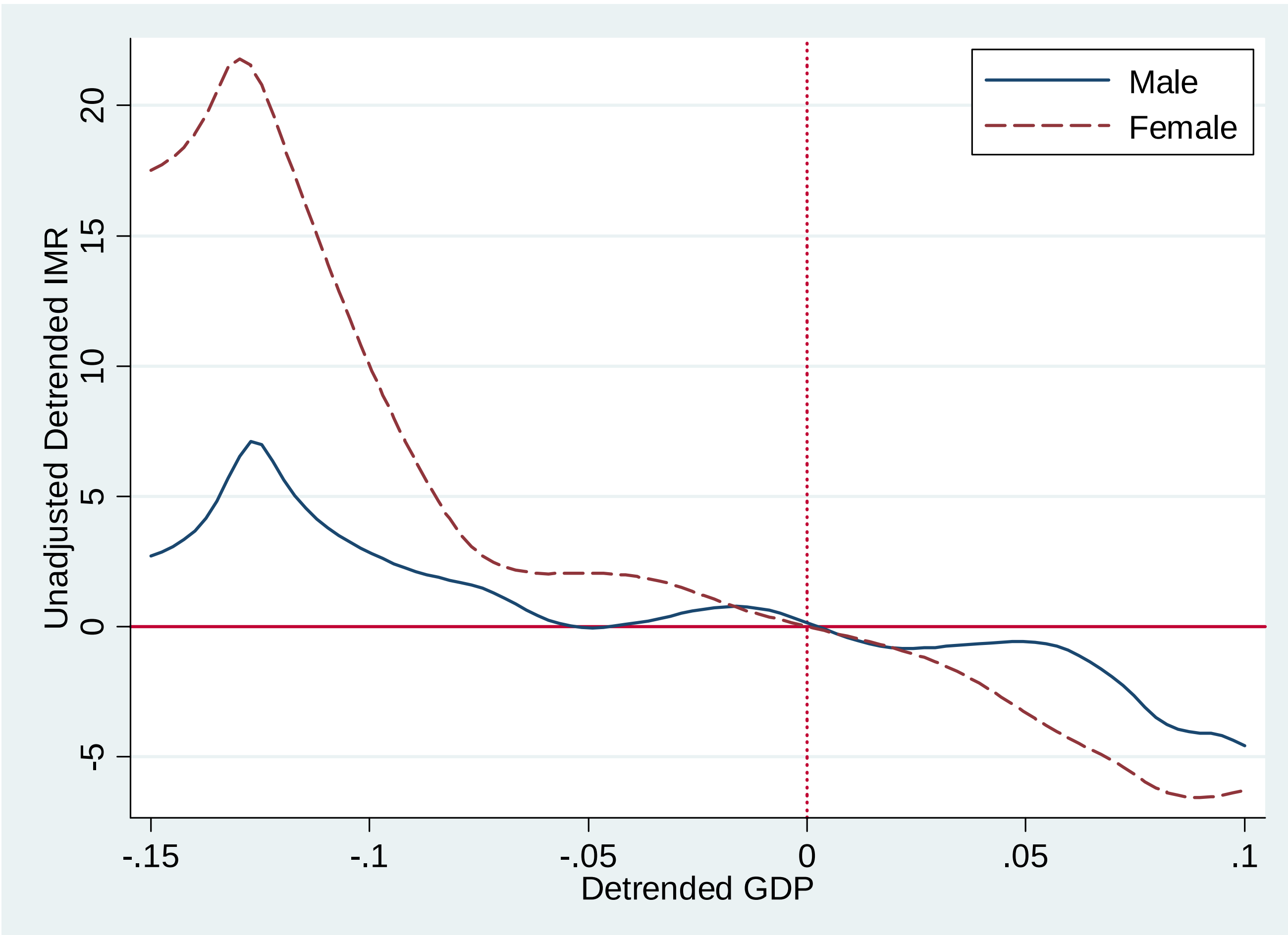
Note: Slope coefficients are estimated from a continuous spline specification. All regressions are weighted and robust standard errors are clustered at the country-year level. GDP is measured in year 2000 international (PPP) dollars.
* p<.10, ** p<.05, *** p<.01

Figure 1. Relation between detrended (cubic) IMR and detrended (cubic) log per capita GDP, both unadjusted and adjusted for observable birth and mother characteristics



Note: Estimated with locally weighted least squares. GDP is measured in year 2000 international (PPP) dollars. Observable characteristics include: mother's years of education, mother's age at birth, rural/urban location, birth order of infant, infant gender, and an indicator for a multiple birth.

Figure 2. Relation between detrended (cubic) IMR and detrended (cubic) log per capita GDP, separately for male and female children

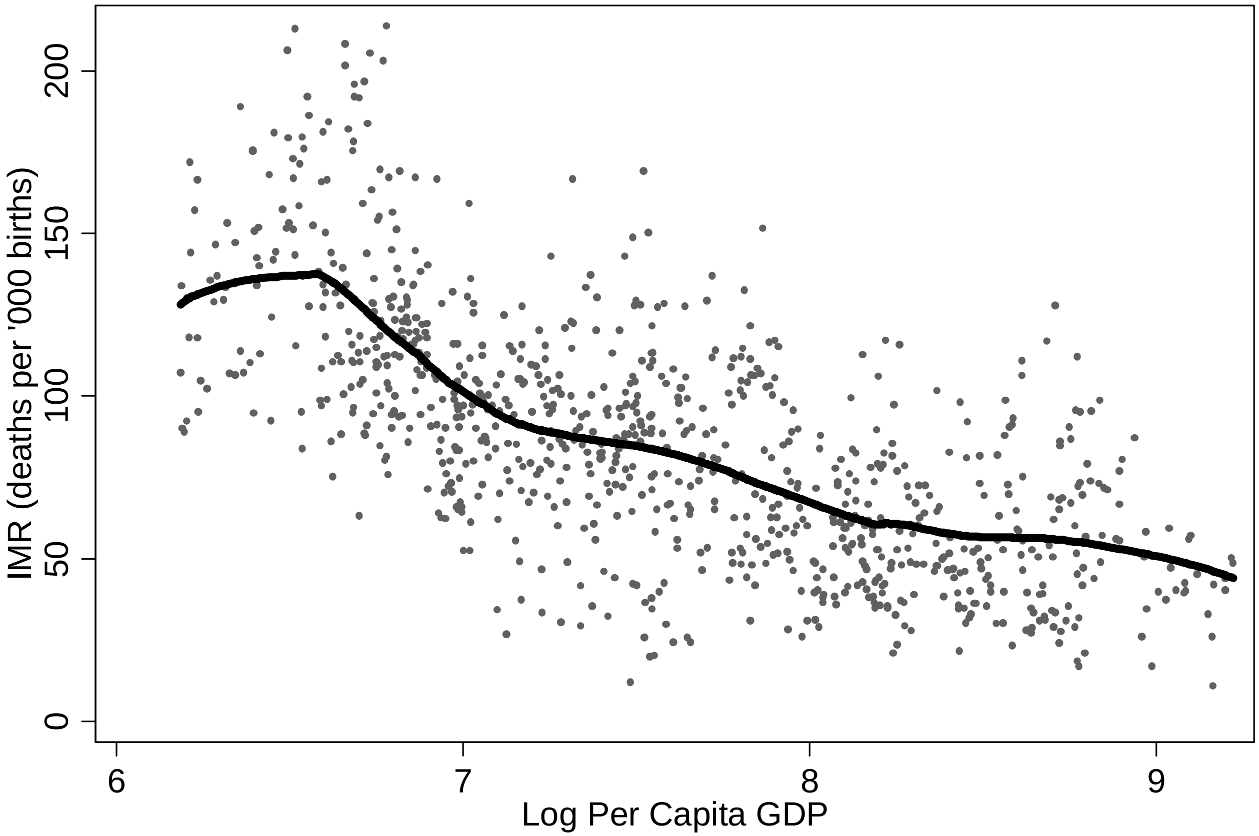


Note: Estimated with locally weighted least squares. GDP is measured in year 2000 international (PPP) dollars.

Appendix Table 1. List of DHS datasets used in the analysis, including information on country, year of survey, number of mothers, and number of births

Country	Survey Years	Total Mothers	Total Births
Armenia	2000	2446	4234
Bangladesh	1994, 1997, 1999, 2004	26313	51071
Benin	1996, 2001	7515	18891
Bolivia	1989, 1994, 1998,2004	24574	54474
Brazil	1986, 1992, 1996	11672	23590
Burkina Faso	1993, 1999, 2003	16362	39410
Burundi	1987	2416	6464
Central African Republic	1995	3373	7962
Cameroon	1991, 1998, 2004	11444	27350
Chad	1997	4655	11829
Colombia	1986, 1990, 1995, 2000	17149	31010
Comoros	1996	1405	3838
Cote d'Ivoire	1994, 1999	6660	15993
Dominican Republic	1986, 1991, 1996, 1999, 2002	23486	48458
Ecuador	1987	2536	5817
Egypt	1993, 1996, 2000, 2003	33988	73378
Ethiopia	2000	8436	20484
Gabon	2001	3371	7084
Ghana	1988, 1994, 1999, 2003	11841	25675
Guatemala	1987, 1995, 1999	13496	33832
Guinea	1999	4549	11224
Haiti	1995, 2000	7764	18283
India	1993, 1999	103669	208690
Indonesia	1987, 1991, 1994, 1997, 2003	81673	153661
Kazakhstan	1995, 1999	3971	6624
Kenya	1989, 1993, 1998, 2003	18457	44289
Kyrgyz Republic	1997	2131	4100
Liberia	1986	3419	8669
Madagascar	1992, 1997	7592	19195
Malawi	1992, 2000	11368	27292
Mali	1987, 1996, 2001	17915	47710
Mexico	1987	4528	10177
Morocco	1987, 1992, 2004	14775	33052
Mozambique	1997	5535	12468
Namibia	1992, 2000	6674	13550
Nepal	1996, 2001	12058	27569
Nicaragua	1998, 2001	14098	29598
Niger	1992, 1998	9468	26714
Nigeria	1990, 1999, 2003	14333	36543
Pakistan	1991	4874	13255
Paraguay	1990	3208	7752
Peru	1986, 1992, 1996, 2000	40330	84225
Philippines	1993, 1998, 2003	20621	46551
Rwanda	1992, 2000	9317	23607
Senegal	1986, 1993, 1997	11881	30636
South Africa	1998	6017	9970
Sri Lanka	1987	4121	8250
Sudan	1990	4242	11314
Tanzania	1992, 1996, 1999	12826	29743
Thailand	1987	4294	7516
Togo	1988, 1998	7611	18582
Trinidad and Tobago	1987	1786	3588
Tunisia	1988	3224	8318
Turkey	1993, 1998	7897	15306
Uganda	1989, 1995, 2001	11883	30062
Uzbekistan	1996	2315	4744
Vietnam	1997, 2002	7643	13012
Zambia	1992, 1997, 2002	13776	32044
Zimbabwe	1989, 1994, 1999	9346	19913
Total:		764327	1668640

Appendix Figure 1. Scatter plot of IMR and Log Per Capita GDP with fitted non-parametric regression line



Note: Annual country level observations of Log Per Capita GDP (2000 international (PPP) dollars) and IMR, a total of 900 observations from 59 countries.

Appendix Figure 2. Scatter plot of bi-decennial IMR estimates from WDI data and corresponding DHS-based author estimates