# Mortality Expectations and Fertility Decisions

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## Abstract

Recent papers suggest that health improvements have little potential to act as a catalyst for economic growth. The hypothesis underlying these papers is that a reduction in mortality does not lead to a corresponding decrease in fertility, bringing about an increase in population and declining income per capita. We directly investigate the link between mortality and fertility using data from 165 DHS surveys and find that the empirical underpinnings do not support this bleak outlook: reductions in mortality are typically followed by a one-for-one reduction in fertility, at least in countries far along in the demographic transition. Most existing empirical studies conduct their analysis at the individual level and hence can only capture the direct, i.e. ex-post, replacement of child deaths. We argue that in order to analyze the total impact of mortality on fertility, the analysis has to be conducted at higher –regional or country - levels to take into account insurance effects based on mortality expectations. In order to account for the potential endogeneity of fertility and mortality, we present an instrument based on the global vaccination rate.

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## 1. Introduction

There has been a resurgence of papers investigating the impact of increases in life expectancy as a measure of health improvements on economic growth. While some papers such as Bloom and Canning (2004) argue for a positive influence of improvements in life expectancy or reductions in child mortality on GDP per capita, other papers have come to a more pessimistic conclusion. Acemoglu and Johnson (2006) state that "…there is no evidence that the large exogenous increase in life expectancy led to a significant increase in per capita income". Similarly, Young (2005) finds that the AIDS epidemic as a health shock has a *positive* impact on income per capita for future generations of South Africans; his study suggests that the epidemic will slow population growth less than income growth.

The explanation given by both of the latter papers for this negative impact of health improvements is that increases in life expectancy lead to a net increase in population. Neither of the two papers, however, directly tests whether in the long run increases in life expectancy lead to a decrease in birth rates. More evidence and more rigorous research are therefore needed to learn about this key relationship. Two other strands of the literature have analyzed the impact of changes in mortality on fertility: theoretical work from the economics literature and empirical studies from the demography literature. The theory literature gives conflicting predictions: early papers such as Becker and Barro (1988) or more recent papers such as Hazan and Zoabi (2006) arrive at the conclusion that increases in longevity may lead to an increase in the number of children, for example

through an increase in the returns not only to the quality but also the quantity of children. In contrast, Kalemli-Ozcan (2003) finds that in the presence of uncertainty, a decline in child mortality should reduce the precautionary demand for children.

Turning to the empirical evidence from the demography literature, there is consensus that the loss of a child leads to a subsequent replacement of that child, but there is substantial disagreement about whether full replacement of deaths or only partial replacement of deaths takes place (for a survey article, see Schultz 1997). In other words, whereas there is consensus that higher mortality leads to an increase of gross fertility, the evidence is not clear whether child mortality in- or decreases net fertility. This literature is however constrained by two factors: first, different studies estimate the magnitude of replacement at different aggregation levels (although most studies are restricted to the individual level), which makes a comparison of these studies difficult. Second, most of these studies do not take into account the problem of endogeneity when estimating the impact of mortality on fertility.

In this paper, we argue that the correct level for measuring the impact of mortality on fertility is at the regional or country level, where the insurance effect for child replacement can be taken into account. Parents not only seem to react to the death of a child (the direct replacement effect) but fertility decisions also seem to be driven by expected future child mortality (the indirect or insurance replacement effect). We suggest an instrumental variables strategy based on global vaccination campaigns to address the

potential reverse causality running from fertility to child mortality that the existing empirical literature addresses only unsatisfactorily.

Using DHS data for a sample of 65 low-income countries, we find that the replacement effect at the regional level is considerably larger than the direct replacement effect as measured at the individual level typically investigated in the demography literature. This finding holds both for the OLS estimates and when using an IV strategy. We interpret this finding as evidence for parents taking mortality expectations into consideration when deciding on the desired number of births. We also find that this relationship when estimated at the regional level is equal to or larger than one-for-one for a subset of countries: a reduction in child mortality by one results in a reduction in fertility by more than one child, i.e. leading to a decrease not only in gross but also in net fertility. This finding holds important implications at the macroeconomic level. Health interventions have the potential for a large pay-off in terms of reduced population growth.

This paper is structured as follows: the next section discusses the existing theory and evidence regarding the impact of mortality expectations on fertility decisions, followed by a discussion of the empirical strategy in sections 3 and 4. In section 5, the data are presented. The results are discussed in section 6, followed by the conclusion in section 7.

## 2. Mortality expectations: theory and literature review

Total replacement of child mortality consists of the direct, ex-post replacement effect following the death of a child, and the precautionary, *a priori* replacement effect due to uncertainty about future child mortality (Schultz 1997). The literature to date has focused on the first of these two effects, while the second effect has been largely neglected, apart from a few studies such as Ben-Porath (1976) or LeGrand et al. (2003).

In order to test the impact of mortality expectations on fertility decisions, it is important to investigate how expectations about current or future child mortality are formed. Montgomery (1998) suggests that rather than going through complex calculations, people often prefer to use simple rules of thumb in order to update their beliefs about child mortality, for example by turning to their reference group for information. Information exchange between the group and an individual and interpretation of this information constitute two of the three main forms of social interactions in the classification of Bongaarts and Watkins (1996). Such abbreviated information gathering seems particularly relevant for country settings characterized by low literacy rates and incomplete information (for the sample at hand, about 75% of women have less than six years of education).

Grether (1980) finds that people overly rely on small sample observations compared to objective information. For the study at hand we argue that groups in relatively close physical proximity such as a region may influence the infant mortality expectations of

mothers. Geographical proximity has been cited by several studies as the basis for spillover effects, as mothers can turn to their reference group in order to observe the prevalent child mortality rate (Coale and Watkins, 1986; Bongaarts and Watkins, 1996; Durlauf and Walker, 2001; Kohler, 2001).[1]

We can formalize this idea by positing that the fertility decision of a mother is influenced by her expectation of group level mortality, as well as by group-level fertility:

$$y_{gi} = x_{gi}\delta + E_{gi}[x_{gi} \mid I_{gi}]\gamma + \alpha_g + \varepsilon_{gi}, \qquad (1)$$

where $y_{gi}$ denotes the fertility decision of an individual $i$ in group $g$. $x$ is the observed child mortality and $E_{gi}[x_{gi} \mid I_{gi}]$ the expected child mortality conditional on the individual's characteristics and her information-set.

Equation (1) cannot be estimated as we have no information on individual child mortality expectations and because each individual influences each individual's fertility decision simultaneously. Taking the group means of equation (1) and then solving for $E_g[y_{gi}]$ we get

[1] In a similar vein, they can also follow the fertility decision of a reference individual or group, thereby obeying to social norms. In the classification of Manski (1993), such a form of social interaction is called an endogenous interaction, where the outcome of interest of the individual is influenced by the group-level outcome.

$$E_g[y_{gi}] = E_g\left(E_{gi}[x_{gi} \mid I_{gi}]\right)(\gamma + \delta) + \alpha_g\,, \tag{2}$$

where we can apply the law of iterated expectations to overcome the problem of individual mortality expectations to obtain:

$$E_g[y_{gi}] = E_g[x_{gi}](\gamma + \delta) + \alpha_g\,, \tag{3}$$

This equation shows that in comparison with the marginal impact of child mortality on fertility on the individual level $\delta$, the coefficient on child mortality on the aggregate level is increased by $\gamma$.

To get at the direct replacement effect, we estimate the *within-group* variation:

$$y_{gi} = \tilde{x}_{gi}\delta + \varepsilon_{gi}\,, \tag{4}$$

where $y_{gi}$ denotes the number of children ever born to a women *i* in group *g*. $x_{gi}$ is the number of children born who died and $r_{gi}$ a vector of control variables. This specification provides consistent and efficient estimates of $\delta$. Next, the coefficient on $E_g[x_{gi}]$ can be estimated using *between-group* variations (3). Since we want to control for correlated group effects we either have to assume the existence of an instrument or – and we follow this approach - assume that group specific unobservables are time-invariant and use data on the same *N* groups over *T* periods *(t=1,…,T).* If the group specific effect $\alpha_g$ in

equation ( 3) can be split up into a time trend and a constant group-specific effect $\alpha_{gt} = \mu_t + \xi_g$ we can then estimate:

$$E_{tg}[y_{tgi}] = E_{tg}[x_{tgi}](\gamma + \delta) + \mu_t + \xi_{g,}, \quad (5)$$

where $E_{tg}[y_{tgi}]$ denotes the mean number of children born in group *g* at time *t*. $E_{tg}[x_{tgi}]$ is the expected child mortality within the reference group. A difficulty that the theory model above makes clear is that at the individual level, the mother's expectation about the currently prevailing group-level child mortality rate entering the equation is difficult to capture. Only once we aggregate the equation to the group-level can we confidently estimate the replacement effect. At the individual level, we can try to control for group-level effects by using group fixed effects, and to use a proxy for the individual-level expectation as described below. However, our preferred specification is the one at the regional/country level that we can estimate with confidence.

## 3. Estimating the replacement effect at the individual level

There are two main challenges for estimating replacement effects: first, simply regressing the number of born children on the number of dead children results in a spurious correlation between the two variables as there is a direct link between the number of born children and the number of deceased children. To address this problem, a large part of the literature employs a correction method propagated by Olsen (1980) and Olsen and Trussell (1983)[2]. This method cannot credibly correct for the potential endogeneity between fertility and mortality. For example, we typically observe an increasing fraction of children dying with the number of children. One explanation for this positive correlation is crowding: a higher number of children can signify less resources or time commitment per child, leading to increased mortality risk for children in larger families.

In order to circumvent this problem of reverse causality, we need to find a variable that has predictive power for child mortality but is not correlated with a woman's fertility decision, i.e. we need to find a valid instrument. As described in detail below, we suggest an instrument that is based on the intuition that global vaccination campaigns were reasonably exogenous to the characteristics of the countries in which they were implemented. The variation stems from the fact that child mortality prior to these interventions differed widely by country; therefore countries with higher child mortality benefitted more from these interventions than countries with relatively low child mortality rates. In contrast to an otherwise very similar instrument in Acemoglu and

[2] For examples of papers applying this technique see for example Maglad (1994), Bath (1998), Haines (1998), or Palloni and Rafalimanana (1999).

Johnson (2006) that can essentially only be used as a binary variable (i.e. before/after), the instrument proposed here provides time-series variation due to yearly changing averages of global vaccination rates

While this instrument is correlated with reductions in child mortality at the aggregate level (regional or country), it cannot be used for the estimation of the direct replacement effect at the individual level as the instrument is correlated with group-level child mortality. This correlation would then introduce the group-level expectations into the individual level regression, therefore we cannot identify the direct replacement effect using this instrument.

We employ a different variable as an instrument for child mortality at the individual level: we suggest to instrument child mortality at the individual level with the mortality rate of the mother's siblings. This variable represents private information of the mother that in most other datasets except the DHS is not available. The information could guide the mother's fertility decision if her siblings' death rate is correlated with family-specific gene quality, for example.

We construct this instrument by dividing the number of deceased siblings of the mother by the total number of her siblings. This information is available in a subset of DHS surveys that collect information on the sibling history of the mother (see the appendix table A.1 for details on the surveys providing this information). This instrument has, however, three drawbacks: First, this instrument is only available for a subset of the DHS

surveys. By excluding countries in which this information was not collected, we potentially introduce selection bias in the analysis. Sibling information is available for only 52.50% of the observations in the sample. By relying on this type of instrument, we therefore lose about half of our sample. When we look at Table A.2 in the Appendix, we see that on average, countries for which the surveys include the sibling information are more developed than those without this information. They also have fewer children and lower child mortality rates. Given these differences in socioeconomic characteristics, the exclusion of all surveys without this information might lead to a selected sample.

Second, the variable might have a direct impact on fertility through family size preference. We can, however, purge the variable from this direct influence on fertility by including the number of siblings of the mother in the regression. The results for this robustness check are presented in Appendix B. Lastly, it is likely that mothers use past child mortality rates for the formation of their expectations about current child mortality rates, leading to a potential overestimation of the direct replacement effect that we would like to estimate at the individual level.[3]

[3] Even if this is the case, when we compare individual level estimates with those at the regional level using this instrument, we can get an idea of the lower bound for the social multiplier (as the individual estimate is likely upward biased if it also captures group expectations rather than private information alone), the ratio of the regional versus individual level coefficient estimate that gives an indication of the magnitude of the group-level spillover effect.

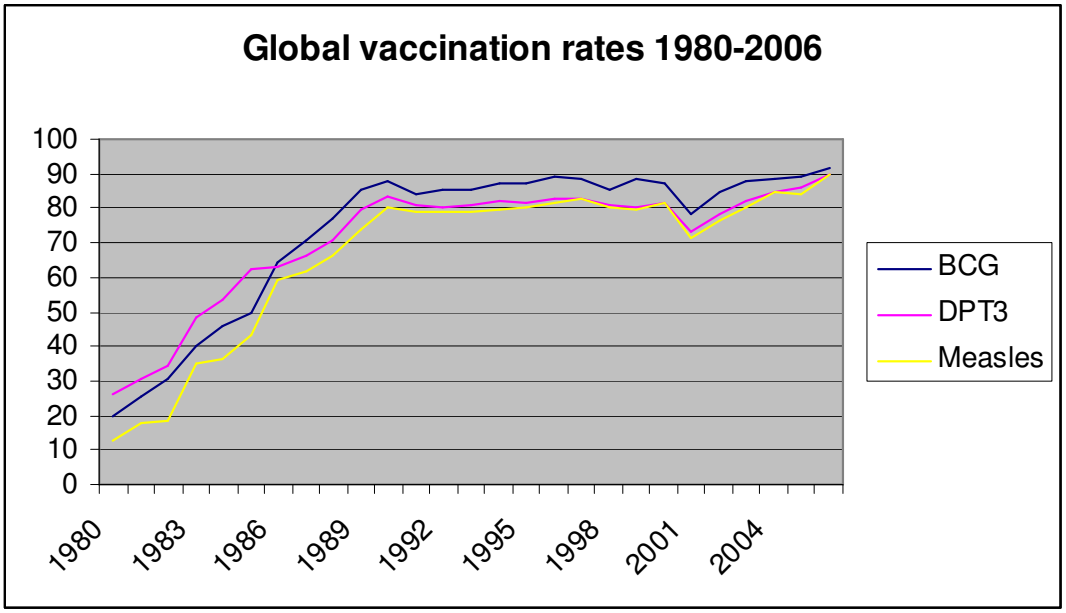
## 4. Estimating the replacement effect at the aggregate level

As discussed above, we suggest an instrument based on global vaccination campaigns for use at the group level. The intuition behind this instrument is that these campaigns were exogenously put in force by the international community and were aimed at reaching certain coverage levels in all countries. We deliberately abstain from using country-specific vaccination coverage rates that could be correlated with country characteristics if they are demand-driven or if a government is particularly effective at securing such interventions for its population, but use the global average of the vaccination rates for a given year instead. We interact these vaccination rates with initial child mortality assuming that countries with the highest initial child mortality would benefit most from the increase in vaccinations available. There is a large variation for this instrument as before the onset of the interventions, child mortality levels were vastly different in the countries under consideration (for example, in Uzbekistan 71 out of 1000 children died before their fifth birthday, compared to 354 in Nicaragua or even 500 in Mali). These campaigns should not have a direct influence on the fertility decisions of mothers if they were not combined with interventions directed at family planning.

We construct the instrument as follows: first, we collected data on child mortality from the Demographic Yearbooks from 1960, a time before major global vaccination campaigns. We then collected the global vaccination rates for three vaccines: measles, DPT3, and BCG. We selected these three diseases as they are responsible for a large fraction of child deaths in developing countries and because they were the subject of

international vaccination campaigns for which relatively good data is available. Starting in the late 1970s, the international community advocated and implemented large vaccination drives for developing countries, dramatically increasing their vaccination rates. Figure 1, shows the increase in vaccination rates between 1980 and 2005[4]. The vaccination patterns are relatively similar for the three vaccinations we use in our analysis. We therefore restrict our analysis to measles as one of the diseases claiming most children's lives of the three (UNICEF 2002.

Figure 1: Global vaccination coverage rates for all three vaccines used, 1980-2006




*Source:* WHO: http://www.who.int/immunization_monitoring/data/data_subject/en/index.html#a

Given potential non-linearities in the effect of the vaccination coverage (for example, due to the potential existence of thresholds), we also include the square of the coverage rate in the first stage regression. We make the assumption that the mothers benefit from the coverage rate when they are thirty years of age. One can interpret this as an average rate

[4] We extrapolated the vaccination rates back to 1974 using a simple linear function; simply setting the vaccination rate to 0 pre-1980 results in very similar estimates that are available from the authors.

of the coverage that the mother experienced over her fertile period (30 being a midpoint between a hypothetical start of fertility at age 15 and end at age 45).

Our approach is similar to Acemoglu and Johnson (2006) but differs in two major respects. First, these authors derive their source of exogeneity from the timing of the *invention* of the vaccine, while the exogeneity of the instrument we use stems from the actual *implementation* of the vaccine. In addition, the information we rely on gives us a time series proper, unlike the pre-/post- invention measure of A&J. Exploiting the temporal variation of vaccination coverage allows us to have a true exogenous measure of reduction in mortality.

Given the differing age structure between the regions, the regression is also identified at the regional level to avoid small sample sizes when performing the analysis at the country level. The main results presented in Table 2 therefore relate to the regional level, while we present the country level results in Table 3 for completeness. At the individual level, it is not possible to use the variable as an instrument for child mortality as individual child mortality is only weakly correlated with the global vaccination rate, and most importantly because the instrument captures an aggregate phenomenon that we would want to exclude when investigating the direct replacement effect.

## 5. Data setting[5]

The data underlying this analysis comes from the Demographic and Health Surveys (DHS). These surveys are nationally representative and cover a wide range of data, including the birth history of women 15-49 years of age. The dependent variable is number of children born to a woman. Besides child mortality of a women, we include the control variables woman's and husband's education, urban residence, an asset index approximating income, and dummies indicating the religion of the woman (Muslim, Catholic, Protestant or Others), as well as the year of the survey. We do not include behavioral variables, such as time of first marriage, as these variables are endogenous to desired fertility.

We construct an asset index to approximate household's wellbeing as the DHS surveys do not contain any information about the income or consumption of households. The asset index is estimated via principal component analysis. The assets underlying the index are electricity, possession of a radio, TV, and source of drinking water (piped water, well or open source of water). This limited number of assets was chosen to keep the sample size as large as possible.

The maximum number of countries publicly available from the DHS site is 65[6]. Each country has between 1 and 6 surveys between 1988 and 2005, leaving us with a total of 206 surveys at the country level. As not all variables were collected in each survey round

[5] For a more detailed description of the data, the reader is referred to Bloom et al. (2008a).
[6] For the country level estimations, we treat Indian states as countries due to their size. This procedure gives us 28 extra observations.

in each country we lose some surveys. The final number of surveys analyzed is 184, and 1,123 regions can be traced over time within those countries.

We further limit our sample to women who have completed their fertility, i.e. to women between the age of 45 and 49[7] and to women who have given birth to at least one child. Women without children are excluded from the analysis as they do not provide any information about the relationship between fertility and mortality. This leaves us with a data set of 118,627 women.

## 6. Empirical Results

In Table 1, we estimate the impact of child mortality on fertility at the individual level, first using the Olsen-Trussell technique that is standard in the literature, and then using our proposed IV strategy in columns 3 to 6, using fraction of siblings of the mother who died as an instrument for the number of dead children. The Olsen Trussell technique in essence uses the fraction of children died of the total number of children in place of the number of children who have died. This way, the mechanical relationship between the total number of children and the number of dead children is done away with to a certain extent.

[7] The estimates do not change significantly when we vary the cut-off age for completed fertility history.

We find that at the individual level, about 0.28 children get replaced when using the Olsen-Trussell technique, the standard in the replacement literature. The coefficient more than doubles in magnitude to .75 when using the fraction of deceased siblings of the mother as an instrument (column 2), but goes down to about .43 when controlling for the family size preference in column 2 of Table B1. As discussed above, we cannot be sure whether the higher coefficient reflects an estimate that is truly different from the one used for the Olsen-Trussell technique, or whether mortality expectations based on the experienced mortality of the former generation inflate the direct replacement estimate.

Table 2 presents the results at the regional level, first using the fraction of deceased siblings and then global vaccination rates as an instrument. We exclude the religion variable at this level as this information is missing for more than 25% of the sample[8]. In addition, this variable hardly varies over time at the regional level for the relatively short time period under consideration. As the three vaccinations presented graphically above in figure 1 covary greatly, we focus first on measles for the results in columns 2 and 3. In column 4, we also include the other two vaccination rates as instruments and arrive at largely the same results as in column 3, while the value of the Cragg-Donald test goes down significantly. For the last two specifications, we include a square and a cube term for the vaccination rate in order to account for potential non-linearities of the vaccination rate on child mortality as discussed above.

[8] While we view religion at the individual level as a legitimate and important determinants of fertility that are jointly significant at the individual but not the regional or country level , we also ran the regression without religion variables at the individual level for comparison. This specification increases the coefficient on child mortality by about 0.1 child.

There are a total of 165 surveys from 64 countries, leaving us with 927 regions. We find that the coefficient for the replacement effect is not statistically significant different from one. We therefore cannot reject the hypothesis of full replacement at the regional level. When looking at the first stage, we find the vaccination rates interacted with initial child mortality to have a negative impact on child mortality as expected.

In Table 3, we stratify the countries by their stage in the demographic transition, where we take countries with less than 5.3 children on average per mother in 1990 to be further into the transition, and countries with a higher average fertility rate as being early transition countries. For countries with lower average fertility, the coefficient on child deaths is about 1.9, a significant increase to Table 2 where we average over all countries. For this subset of countries, we can reject the hypothesis that the coefficient equals one, we therefore find evidence for more than full replacement.

As surveys including the sibling information differ substantially from surveys for which the information is not available, we also present in Appendix B the results when including the number of siblings of the mother thereby dropping close to half of our sample. The results remain similar in character, but the coefficient estimate is somewhat less precisely estimated. The slight change in coefficient values stems mainly from sample selection rather than the inclusion of the sibling variable: when restricting the sample to surveys with sibling history but excluding the sibling variable, the results for the coefficient of interest for this specification are virtually identical to the ones when including the sibling variable. Our preferred specification, however, is without the sibling

variable as it gives us a broader spectrum of countries for estimation. We present the alternative results in order to have the same set of countries for comparison with the estimates at the individual level.

For completeness, we also present the country-level regressions in Tables 4 and 5. There are only 64 countries in the sample with more than one survey over time, leaving us with a sample of only 165 country-time observations. When we include the sibling variable for this level of aggregation, the sample size is reduced to 56 country-year observations from 25 countries. The variation of the vaccination instrument at the country level is much smaller as indicated by low Cragg-Donald statistics in the first-stage regressions. Most likely due to small sample size in combination with relatively low variation at the country level, the coefficient estimate for the replacement effect is statistically insignificant when using the whole sample, but turns significant and similar in magnitude to the regional level estimates when stratifying by the stage of the demographic transition.

We suggest in this section that using an instrument we can improve on the estimates in the demographic literature based on the Olsen-Trussell technique. We employ an instrument at the individual level formed on the fraction of siblings of the mother who died as a fraction of total siblings to estimate the direct replacement effect. We argue, however, that this instrument potentially overestimates the direct replacement at the individual level. In the result tables we find that based on the estimation strategy discussed above mothers replace about 0.75 of their deceased children, dropping to .43 when including family size of the household the mother grew up in to control for fertility

preferences. At the regional level, where we instrument mortality with global vaccination rates interacted with initial child mortality rates, we take into account potential mortality expectations. We find a coefficient estimate whose confidence interval includes unity. When stratifying by the stage of the demographic transition, we find mothers in countries with relatively low fertility levels to react much stronger to child deaths (experienced or expected using the regional/country level) than mothers in countries further behind in their demographic transition. Our results furthermore suggest that estimation of the total replacement effect at the right level of aggregation and when accounting for the potential endogeneity of fertility and mortality, we cannot rule out the possibility that at the regional or country level reductions in child mortality lead to reduced fertility.

## 7. Conclusion

In this paper, we investigate the causal pathway running from child mortality (as a measure of health) to fertility. This channel is important as recent studies claim that improvements in health do not lead to subsequent economic growth. The health-fertility pathway in these studies is, however, not thoroughly analyzed. In the economics literature, there are few direct investigations of this relationship. In the demography literature, studies typically focus on the direct replacement effect only by estimating the relationship only at the individual level, thereby neglecting the indirect, insurance replacement effect. In addition, in this literature the potential endogeneity of fertility and child mortality is only unsatisfactorily dealt with.

In order to investigate this relationship, we use data from 165 surveys from 65 low-income countries, and investigate the extent of the replacement effect at different levels of aggregation. Throughout the paper, we stress that in order to capture the total replacement effect, the analysis needs to be conducted at a level of aggregation that can capture mortality expectations. We improve upon the methods used in the extensive demography literature by relying on the use of instruments for child mortality, i.e. variables that have a direct impact on mortality but not on fertility, instead of the currently used Olsen-Trussell technique that only partially addresses endogeneity issues.

We find that the estimated impact of mortality on fertility based on the instrumental variables strategy is general higher than OLS estimates but it is comforting to note that they do not deviate substantially either. At the regional level, we find much higher

coefficients than on the individual level and cannot reject the unitary coefficient on the child mortality variable, i.e. the hypothesis of full replacement. This result is particularly true for countries far into the fertility transition, for which we find evidence of more than one-for-one replacement of deceased children. Our findings therefore indicate that when evaluating the relationship at the right level of aggregation and when taking into account the potential endogeneity of fertility and child mortality, we cannot rule out the possibility that reductions in child mortality lead to reduced fertility, shedding doubt on the conclusions drawn from some recent studies.

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Table 1: Individual-level regressions

| | Olsen-Trussell (1) | Sibling_dead (2) | Far in transition (3) | Early in transition (4) | SSA=0 (5) | SSA=1 (6) |
|---|---|---|---|---|---|---|
| | total children ever born | total children ever born | total children ever born | total children ever born | total children ever born | total children ever born |
| # of dead children | 0.284*** | 0.746*** | 0.942** | 0.698*** | 1.496*** | 0.582*** |
| | (0.008) | (0.129) | (0.371) | (0.139) | (0.371) | (0.139) |
| Education in single years | -0.128*** | -0.108*** | -0.125*** | -0.085*** | -0.101*** | -0.086*** |
| | (0.003) | (0.008) | (0.019) | (0.009) | (0.019) | (0.010) |
| Urban | -0.311*** | -0.343*** | -0.207*** | -0.435*** | -0.242*** | -0.458*** |
| | (0.025) | (0.039) | (0.058) | (0.051) | (0.053) | (0.056) |
| Wealth index | -0.242*** | -0.100*** | -0.184** | -0.041 | -0.072 | -0.027 |
| | (0.015) | (0.035) | (0.076) | (0.044) | (0.080) | (0.045) |
| Religion: Muslim | 0.707*** | 0.146** | -0.058 | 0.250*** | -0.071 | 0.232*** |
| | (0.034) | (0.057) | (0.095) | (0.071) | (0.097) | (0.073) |
| Religion: Catholic | 0.759*** | 0.532*** | 0.745*** | 0.352*** | 0.668*** | 0.317*** |
| | (0.037) | (0.052) | (0.094) | (0.067) | (0.098) | (0.068) |
| Religion: Christian | 0.535*** | 0.421*** | 0.504*** | 0.326*** | 0.497*** | 0.306*** |
| | (0.035) | (0.054) | (0.090) | (0.066) | (0.097) | (0.066) |
| Current age - respondent | 0.078*** | 0.050*** | 0.059*** | 0.050*** | 0.020 | 0.064*** |
| | (0.006) | (0.012) | (0.019) | (0.014) | (0.021) | (0.015) |
| Observations | 71893 | 36326 | 10239 | 26087 | 12397 | 23929 |
| Number of year*regions | 668 | 444 | 102 | 342 | 110 | 334 |

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 2: Regional regressions using the Olsen-Trussell technique and the vaccination instruments

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Olsen-Trussell | Measles + $^2$ | Measles + $^2$ + $^3$ | All instruments + $^2$ + $^3$ |
| # of dead children | 0.575*** | 0.552** | 0.739*** | 0.873*** |
| | (0.083) | (0.279) | (0.258) | (0.248) |
| Urban | -0.655*** | -0.661*** | -0.609*** | -0.571** |
| | (0.218) | (0.231) | (0.225) | (0.223) |
| Education | -0.127*** | -0.128*** | -0.118*** | -0.111*** |
| | (0.024) | (0.028) | (0.027) | (0.026) |
| Wealth index | -0.218* | -0.224 | -0.172 | -0.134 |
| | (0.115) | (0.137) | (0.132) | (0.130) |
| Year_1995 | -0.238*** | -0.242*** | -0.216*** | -0.197*** |
| | (0.050) | (0.062) | (0.060) | (0.059) |
| Year_2000 | -0.373*** | -0.378*** | -0.338*** | -0.309*** |
| | (0.059) | (0.082) | (0.078) | (0.076) |
| Observations | 927 | 927 | 927 | 927 |
| Number of regions | 351 | 351 | 351 | 351 |

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Results stratified by stage in the demographic transition

| | Far in transition (1) | Early in transition (2) | SSA=0 (3) | SSA=1 (4) |
|---|---|---|---|---|
| # of dead children | 1.890*** | 0.224 | 1.720*** | -0.051 |
| | (0.352) | (0.348) | (0.280) | (0.424) |
| Urban | -0.916*** | -0.426 | -0.813*** | -0.555 |
| | (0.280) | (0.340) | (0.269) | (0.379) |
| Education | -0.105*** | -0.138*** | -0.125*** | -0.157*** |
| | (0.032) | (0.042) | (0.029) | (0.049) |
| Wealth index | 0.248 | -0.459** | 0.214 | -0.391* |
| | (0.166) | (0.194) | (0.158) | (0.212) |
| Year_1995 | -0.144* | -0.171* | -0.190*** | -0.213** |
| | (0.075) | (0.091) | (0.072) | (0.097) |
| Year_2000 | -0.232** | -0.173 | -0.270*** | -0.265** |
| | (0.111) | (0.107) | (0.099) | (0.116) |
| Observations | 473 | 454 | 478 | 449 |
| Number of regions | 184 | 167 | 189 | 162 |

Table 4: Country-level regressions using the Olsen-Trussell technique and the vaccination instruments

| | Olsen-Trussell | Measles + $^2$ | Measles + $^2$ + $^3$ | All instruments + $^2$ + $^3$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| # of dead children | 1.068*** | -0.194 | 0.229 | 0.466 |
| | (0.216) | (0.738) | (0.591) | (0.526) |
| Urban | -0.958** | -1.131* | -1.073* | -1.041** |
| | (0.483) | (0.617) | (0.557) | (0.529) |
| Education | -0.229*** | -0.333*** | -0.298*** | -0.279*** |
| | (0.058) | (0.092) | (0.079) | (0.074) |
| Wealth index | 0.296 | -0.122 | 0.018 | 0.097 |
| | (0.265) | (0.404) | (0.350) | (0.327) |
| Year_1995 | -0.070 | -0.183 | -0.145 | -0.124 |
| | (0.089) | (0.128) | (0.113) | (0.106) |
| Year_2000 | -0.129 | -0.313* | -0.251* | -0.217 |
| | (0.107) | (0.168) | (0.145) | (0.135) |
| Observations | 165 | 165 | 165 | 165 |
| Number of countries | 64 | 64 | 64 | 64 |

Note: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Results stratified by stage in the demographic transition

| | Far in transition | Early in transition | SSA=0 | SSA==1 |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| # of dead children | 1.741** | 0.080 | 1.646** | -0.139 |
| | (0.835) | (0.557) | (0.687) | (0.623) |
| Urban | -0.383 | -1.087 | -0.398 | -1.259 |
| | (0.533) | (0.846) | (0.568) | (0.926) |
| Education | -0.148 | -0.308*** | -0.203** | -0.316*** |
| | (0.091) | (0.097) | (0.080) | (0.117) |
| Wealth index | 0.352 | -0.303 | 0.287 | -0.070 |
| | (0.327) | (0.478) | (0.365) | (0.519) |
| Year_1995 | -0.145 | -0.001 | -0.065 | -0.083 |
| | (0.171) | (0.125) | (0.155) | (0.139) |
| Year_2000 | -0.312 | 0.105 | -0.170 | -0.037 |
| | (0.220) | (0.149) | (0.192) | (0.171) |
| Observations | 100 | 65 | 103 | 62 |
| Number of countries | 41 | 23 | 42 | 22 |

**Appendix**

Table A1: Surveys and years

| DHS Code | Country | Year | SSA | Country panel | Consistent regions | All variables | Sibling history |
|---|---|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| AMIR42FL | Armenia | 2000 | | | | | no |
| AMIR52FL | Armenia | 2005 | | | | | no |
| BDIR31FL | Bangladesh | 1993 | | | | | no |
| BDIR3AFL | Bangladesh | 1996 | | | | | no |
| BDIR41FL | Bangladesh | 1999 | | | | | no |
| BDIR4JFL | Bangladesh | 2004 | | | | | no |
| BJIR31FL | Benin | 1996 | SSA | | | | |
| BJIR41FL | Benin | 2001 | SSA | | | | no |
| BOIR01FL | Bolivia | 1989 | | | no | no | no |
| BOIR31FL | Bolivia | 1994 | | | no | | |
| BOIR3BFL | Bolivia | 1998 | | | | | no |
| BOIR41FL | Bolivia | 2003 | | | | | |
| BRIR01FL | Brazil | 1986 | | | | | no |
| BRIR31FL | Brazil | 1996 | | | small adjust. | | |
| BFIR21FL | Burkina Faso | 1992 | SSA | | | | no |
| BFIR31FL | Burkina Faso | 1998 | SSA | | | | |
| BFIR43FL | Burkina Faso | 2003 | SSA | | large adjust. | | |
| KHIR41FL | Cambodia | 2000 | | | small adjust. | | |
| KHIR50FL | Cambodia | 2005 | | | | no | |
| CMIR21FL | Cameroon | 1991 | SSA | | small adjust. | | no |
| CMIR31FL | Cameroon | 1998 | SSA | | | | |
| CMIR42FL | Cameroon | 2004 | SSA | | small adjust. | | |
| CFIR31FL | Central African Republic | 1994 | SSA | no | | | |
| TDIR31FL | Chad | 1996 | SSA | | large adjust. | | |
| TDIR40FL | Chad | 2004 | SSA | | | | |
| COIR01FL | Colombia | 1986 | | | small adjust. | | no |
| COIR21FL | Colombia | 1990 | | | | | no |
| COIR31FL | Colombia | 1995 | | | | | no |
| COIR41FL | Colombia | 2000 | | | | | no |
| COIR51FL | Colombia | 2005 | | | | | no |
| KMIR32FL | Comoros | 1996 | | no | | | no |
| CGIR50FL | Congo, Rep. | 2005 | SSA | no | | | |
| CIIR35FL | Cote d'Ivoire | 1994 | SSA | | | | |
| CIIR3AFL | Cote d'Ivoire | 1998 | SSA | | no | no | no |
| CIIR50FL | Cote d'Ivoire | 2005 | SSA | | small adjust. | no | |
| DRIR01FL | Dominican Republic | 1986 | | | no | | no |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| DRIR21FL | Dominican Republic | 1991 | | | small adjust. | | no |
| DRIR32FL | Dominican Republic | 1996 | | | | no | no |
| DRIR41FL | Dominican Republic | 1999 | | | | | no |
| DRIR4AFL | Dominican Republic | 2002 | | | no | | |
| ECIR01FL | Ecuador | 1987 | | no | | no | no |
| EGIR01FL | Egypt, Arab Rep. | 1988 | | | small adjust. | no | no |
| EGIR21FL | Egypt, Arab Rep. | 1992 | | | | | no |
| EGIR33FL | Egypt, Arab Rep. | 1995 | | | | | no |
| EGIR41FL | Egypt, Arab Rep. | 2000 | | | | | no |
| EGIR4AFL | Egypt, Arab Rep. | 2003 | | | | | no |
| EGIR51FL | Egypt, Arab Rep. | 2005 | | | | | no |
| ESIR00FL | El Salvador | 1985 | | no | | | no |
| ETIR41FL | Ethiopia | 2000 | SSA | | | | |
| ETIR50FL | Ethiopia | 2005 | SSA | | | | |
| GAIR41FL | Gabon | 2000 | SSA | no | | | |
| GHIR02FL | Ghana | 1988 | SSA | | small adjust. | | no |
| GHIR31FL | Ghana | 1993 | SSA | | small adjust. | | no |
| GHIR41FL | Ghana | 1998 | SSA | | small adjust. | | no |
| GHIR4AFL | Ghana | 2003 | SSA | | small adjust. | | no |
| GUIR01FL | Guatemala | 1987 | | | small adjust. | | no |
| GUIR34FL | Guatemala | 1995 | | | | | |
| GUIR41FL | Guatemala | 1998 | | | | | no |
| GNIR41FL | Guinea | 1999 | SSA | | | | |
| GNIR51FL | Guinea | 2005 | SSA | | large adjust. | | |
| GYIR50FL | Guyana | 2005 | | no | | no | no |
| HTIR31FL | Haiti | 1994 | | | no | | no |
| HTIR41FL | Haiti | 2000 | | | | | |
| HTIR50FL | Haiti | 2005 | | | small adjust. | | |
| HNIR51FL | Honduras | 2005 | | no | yes | no | no |
| IAIR42FL | India – Andhra Pradesh | 1998 | | | | | |
| IAIR50FL | India – Andhra Pradesh | 2005 | | | | | no |
| IAIR42FL | India – Assam | 1998 | | | | | |
| IAIR50FL | India – Assam | 2005 | | | | | no |
| IAIR42FL | India – Bihar | 1998 | | | | | |
| IAIR50FL | India – Bihar | 2005 | | | | | no |
| IAIR42FL | India – Goa | 1998 | | | | | |
| IAIR50FL | India – Goa | 2005 | | | | | no |
| IAIR42FL | India – Gujarat | 1998 | | | | | |
| IAIR50FL | India – Gujarat | 2005 | | | | | no |
| IAIR42FL | India – Haryana | 1998 | | | | | |
| IAIR50FL | India – Haryana | 2005 | | | | | no |
| IAIR42FL | India – Himachal Pradesh | 1998 | | | | | |

| | | | | | |
|---|---|---|---|---|---|
| IAIR50FL | India – Himachal Pradesh | 2005 | | | no |
| IAIR42FL | India – Jammu and Kashmir | 1998 | | | |
| IAIR50FL | India – Jammu and Kashmir | 2005 | | | no |
| IAIR42FL | India – Karnataka | 1998 | | | |
| IAIR50FL | India – Karnataka | 2005 | | | no |
| IAIR42FL | India – Kerala | 1998 | | | |
| IAIR50FL | India – Kerala | 2005 | | | no |
| IAIR42FL | India – Madhya Pradesh | 1998 | | | |
| IAIR50FL | India – Madhya Pradesh | 2005 | | | no |
| IAIR42FL | India – Maharashtra | 1998 | | | |
| IAIR50FL | India – Maharashtra | 2005 | | | no |
| IAIR42FL | India – Manipur | 1998 | | | |
| IAIR50FL | India – Manipur | 2005 | | | no |
| IAIR42FL | India – Meghalaya | 1998 | | | |
| IAIR50FL | India – Meghalaya | 2005 | | | no |
| IAIR42FL | India – Mizoram | 1998 | | | |
| IAIR50FL | India – Mizoram | 2005 | | | no |
| IAIR42FL | India – Nagaland | 1998 | | | |
| IAIR50FL | India – Nagaland | 2005 | | | no |
| IAIR42FL | India – Orissa | 1998 | | | |
| IAIR50FL | India – Orissa | 2005 | | | no |
| IAIR42FL | India – Punjab | 1998 | | | |
| IAIR50FL | India – Punjab | 2005 | | | no |
| IAIR42FL | India – Rajasthan | 1998 | | | |
| IAIR50FL | India – Rajasthan | 2005 | | | no |
| IAIR42FL | India – Sikkim | 1998 | | | |
| IAIR50FL | India – Sikkim | 2005 | | | no |
| IAIR42FL | India – Tamil Nadu | 1998 | | | |
| IAIR50FL | India – Tamil Nadu | 2005 | | | no |
| IAIR42FL | India – West Bengal | 1998 | | | |
| IAIR50FL | India – West Bengal | 2005 | | | no |
| IAIR42FL | India – Uttar Pradesh | 1998 | | | |
| IAIR50FL | India – Uttar Pradesh | 2005 | | | no |
| IAIR42FL | India - New Delhi | 1998 | | | |
| IAIR50FL | India – New Delhi | 2005 | | | no |
| IAIR42FL | India – Arunachal Pradesh | 1998 | | | |
| IAIR50FL | India – Arunachal Pradesh | 2005 | | | no |
| IAIR42FL | India – Tripura | 1998 | | | no |
| IAIR50FL | India – Tripura | 2005 | | | no |
| IDIR01FL | Indonesia | 1987 | no | no | no |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| IDIR21FL | Indonesia | 1991 | | | small adjust. | | no |
| IDIR31FL | Indonesia | 1994 | | | | | |
| IDIR3AFL | Indonesia | 1997 | | | | | |
| IDIR41FL | Indonesia | 2002 | | | small adjust. | | |
| KKIR31FL | Kazakhstan | 1995 | | | no | | no |
| KKIR41FL | Kazakhstan | 1999 | | | | | no |
| KEIR03FL | Kenya | 1989 | SSA | | | | no |
| KEIR33FL | Kenya | 1993 | SSA | | | | no |
| KEIR3AFL | Kenya | 1998 | SSA | | | | |
| KEIR41FL | Kenya | 2003 | SSA | | | | |
| KYIR31FL | Kyrgyz Republic | 1997 | | no | | | no |
| LSIR41FL | Lesotho | 2004 | SSA | no | | | |
| LBIR01FL | Liberia | 1986 | SSA | no | | no | no |
| MDIR21FL | Madagascar | 1992 | SSA | | | | |
| MDIR31FL | Madagascar | 1997 | SSA | | | | |
| MDIR41FL | Madagascar | 2003 | SSA | | small adjust | | |
| MWIR22FL | Malawi | 1992 | SSA | | | no | |
| MWIR41FL | Malawi | 2000 | SSA | | | | |
| MWIR4CFL | Malawi | 2004 | SSA | | | | |
| MLIR01FL | Mali | 1987 | SSA | | | | No |
| MLIR32FL | Mali | 1995 | SSA | | small adjust | | |
| MLIR41FL | Mali | 2001 | SSA | | small adjust | | |
| MXIR00FL | Mexico | 1987 | | no | | no | no |
| MAIR01FL | Morocco | 1987 | | | small adjust | no | no |
| MAIR21FL | Morocco | 1992 | | | | | |
| MAIR42FL | Morocco | 2003 | | | large adjust. | | |
| MZIR31FL | Mozambique | 1997 | SSA | | | | |
| MZIR41FL | Mozambique | 2003 | SSA | | | | |
| NMIR21FL | Namibia | 1992 | SSA | | no | | |
| NMIR41FL | Namibia | 2000 | SSA | | | | |
| NPIR31FL | Nepal | 1996 | | | | | |
| NPIR41FL | Nepal | 2001 | | | | | no |
| NPIR50FL | Nepal | 2006 | | | | | |
| NCIR31FL | Nicaragua | 1997 | | | | | no |
| NCIR41FL | Nicaragua | 2001 | | | | | no |
| NIIR22FL | Niger | 1992 | SSA | | small adjust. | | |
| NIIR31FL | Niger | 1998 | SSA | | | | no |
| NGIR21FL | Nigeria | 1990 | SSA | | | | no |
| NGIR41FL | Nigeria | 1999 | SSA | | small adjust. | | |
| NGIR4BFL | Nigeria | 2003 | SSA | | small adjust. | | no |
| PKIR21FL | Pakistan | 1990 | | no | | | no |
| PYIR21FL | Paraguay | 1990 | | no | | | no |
| PEIR01FL | Peru | 1986 | | | no | | no |
| PEIR21FL | Peru | 1992 | | | no | | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| PEIR31FL | Peru | 1996 | | | | | |
| PEIR41FL | Peru | 2000 | | | small adjust. | | |
| PEIR50FL | Peru | 2004 | | | | | |
| PHIR31FL | Philippines | 1993 | | | | no | |
| PHIR33FL | Philippines | 1998 | | | small adjust. | | |
| PHIR41FL | Philippines | 2003 | | | small adjust. | | |
| RWIR21FL | Rwanda | 1992 | SSA | | no | no | no |
| RWIR41FL | Rwanda | 2000 | SSA | | | | |
| RWIR52FL | Rwanda | 2005 | SSA | | small adjust. | | |
| SNIR02FL | Senegal | 1986 | SSA | | small adjust. | no | no |
| SNIR21FL | Senegal | 1992 | SSA | | | | |
| SNIR32FL | Senegal | 1997 | SSA | | | | no |
| SNIR4HFL | Senegal | 2005 | SSA | | large adjust. | | |
| ZAIR31FL | South Africa | 1998 | SSA | no | | | |
| LKIR02FL | Sri Lanka | 1987 | | no | | | no |
| SDIR02FL | Sudan | 1990 | SSA | | | | no |
| TZIR21FL | Tanzania | 1992 | SSA | | no | | no |
| TZIR3AFL | Tanzania | 1996 | SSA | | | | |
| TZIR41FL | Tanzania | 1999 | SSA | | | no | no |
| TZIR4HFL | Tanzania | 2003 | SSA | | small adjust. | | no |
| TZIR4QFL | Tanzania | 2004 | SSA | | small adjust. | | |
| THIR01FL | Thailand | 1987 | | no | | | no |
| TGIR01FL | Togo | 1988 | SSA | | | no | no |
| TGIR31FL | Togo | 1998 | SSA | | small adjust. | | |
| TTIR01FL | Trinidad and Tobago | 1987 | | no | | | no |
| TNIR02FL | Tunisia | 1988 | | no | | no | no |
| TRIR31FL | Turkey | 1993 | | | | no | no |
| TRIR41FL | Turkey | 1998 | | | | no | no |
| UGIR01FL | Uganda | 1988 | SSA | | small adjust. | | no |
| UGIR33FL | Uganda | 1995 | SSA | | | | |
| UGIR41FL | Uganda | 2000 | SSA | | | | |
| UGIR50FL | Uganda | 2006 | SSA | | small adjust. | | |
| UZIR31FL | Uzbekistan | 1996 | | no | | | no |
| VNIR31FL | Vietnam | 1997 | | | | | no |
| VNIR41FL | Vietnam | 2002 | | | | | no |
| YEIR21FL | Yemen, Rep. | 1991 | | no | | | no |
| ZMIR21FL | Zambia | 1992 | SSA | | | | no |
| ZMIR31FL | Zambia | 1996 | SSA | | | | |
| ZMIR42FL | Zambia | 2001 | SSA | | | | |
| ZWIR01FL | Zimbabwe | 1988 | SSA | | small adjust. | no | no |
| ZWIR31FL | Zimbabwe | 1994 | SSA | | | | |
| ZWIR41FL | Zimbabwe | 1999 | SSA | | | | |
| ZWIR50FL | Zimbabwe | 2005 | SSA | | | | |

Table A2: Socioeconomic Characteristics of DHS surveys with and without sibling

| | Sibling history | No sibling history | t-test |
|---|---|---|---|
| Children ever born | 6.115 | 5.480 | 0.000 |
| Children died | 1.169 | 0.904 | 0.000 |
| Education in years | 3.368 | 3.732 | 0.000 |
| Urban | 0.367 | 0.438 | 0.000 |
| Wealth index | -0.249 | 0.241 | 0.000 |
| Observations | 65352 | 72243 | |

Appendix B: Additional results

1) Including siblings

Table B1: Individual level with siblings

| | (1) Olsen-Trussell total children ever born | (2) Instrument: Sibling death total children ever born | (3) Far in transition total children ever born | (4) Early in transition total children ever born | (5) SSA=0 total children ever born | (6) SSA=1 total children ever born |
|---|---|---|---|---|---|---|
| # of dead children | 0.209*** | 0.433*** | 0.564 | 0.392*** | 1.188*** | 0.259* |
| | (0.011) | (0.139) | (0.393) | (0.152) | (0.378) | (0.154) |
| Education | -0.137*** | -0.125*** | -0.144*** | -0.104*** | -0.116*** | -0.107*** |
| | (0.004) | (0.008) | (0.020) | (0.010) | (0.020) | (0.010) |
| Urban | -0.396*** | -0.373*** | -0.208*** | -0.478*** | -0.246*** | -0.510*** |
| | (0.036) | (0.040) | (0.060) | (0.054) | (0.051) | (0.060) |
| Wealth index | -0.224*** | -0.174*** | -0.257*** | -0.122*** | -0.138* | -0.108** |
| | (0.022) | (0.038) | (0.080) | (0.047) | (0.081) | (0.049) |
| Religion: Muslim | 0.187*** | 0.155*** | -0.059 | 0.270*** | -0.055 | 0.244*** |
| | (0.057) | (0.059) | (0.098) | (0.074) | (0.095) | (0.077) |
| Religion: Catholic | 0.500*** | 0.509*** | 0.765*** | 0.315*** | 0.692*** | 0.272*** |
| | (0.052) | (0.054) | (0.097) | (0.070) | (0.096) | (0.072) |
| Religion: Christian | 0.350*** | 0.377*** | 0.475*** | 0.279*** | 0.484*** | 0.255*** |
| | (0.053) | (0.056) | (0.093) | (0.069) | (0.095) | (0.070) |
| Age | 0.087*** | 0.071*** | 0.072*** | 0.074*** | 0.033 | 0.090*** |
| | (0.009) | (0.012) | (0.020) | (0.015) | (0.021) | (0.016) |
| Sibling | 0.054*** | 0.059*** | 0.046*** | 0.063*** | 0.038*** | 0.068*** |
| | (0.004) | (0.005) | (0.008) | (0.006) | (0.008) | (0.007) |
| Observations | 41308 | 36326 | 10239 | 26087 | 12397 | 23929 |
| Number of year*regions | 464 | 444 | 102 | 342 | 110 | 334 |

Table B2: Regional level including siblings

| | (1) Olsen-Trussel | (2) Instrument: Sibling death | (3) Instrument: measles | (4) Far in transition | (5) Early in transition |
|---|---|---|---|---|---|
| # of dead children | 0.486*** | 0.198 | 0.670** | 2.237*** | 0.063 |
| | (0.112) | (0.375) | (0.310) | (0.509) | (0.439) |
| Urban | -0.885*** | -1.063*** | -0.771** | -0.629 | -1.299** |
| | (0.320) | (0.405) | (0.360) | (0.475) | (0.563) |
| Education | -0.207*** | -0.231*** | -0.192*** | -0.082 | -0.228** |
| | (0.040) | (0.052) | (0.046) | (0.052) | (0.091) |
| Wealth index | -0.206 | -0.276 | -0.161 | 0.491** | -0.455* |
| | (0.156) | (0.187) | (0.167) | (0.226) | (0.253) |
| Year_1995 | -0.170** | -0.197** | -0.152** | -0.082 | -0.136 |
| | (0.067) | (0.079) | (0.071) | (0.076) | (0.142) |
| Year_2000 | -0.285*** | -0.331*** | -0.254*** | -0.182 | -0.140 |
| | (0.075) | (0.098) | (0.087) | (0.137) | (0.163) |
| Sibling | 0.005 | 0.011 | 0.001 | -0.032* | 0.046 |
| | (0.020) | (0.023) | (0.021) | (0.019) | (0.095) |
| Observations | 513 | 513 | 513 | 221 | 292 |
| Number of regions | 223 | 223 | 223 | 82 | 141 |

B3: Country level

| | (1) Olsen-Trussell | (2) Instrument: Sibling death | (3) Instrument: measles | (4) Far in transition | (5) Early in transition |
|---|---|---|---|---|---|
| # of dead children | 0.726** | -0.055 | 0.465 | 2.034 | 0.280 |
| | (0.289) | (0.670) | (0.450) | (1.375) | (0.461) |
| Urban | -1.275 | -1.835 | -1.462 | -0.465 | -1.804 |
| | (0.964) | (1.243) | (1.039) | (1.709) | (1.243) |
| Education | -0.203* | -0.230* | -0.212* | 0.105 | -0.278** |
| | (0.112) | (0.138) | (0.118) | (0.269) | (0.136) |
| Wealth index | -0.110 | -0.422 | -0.215 | 0.352 | -0.219 |
| | (0.497) | (0.646) | (0.538) | (1.988) | (0.511) |
| Year_1995 | -0.150 | -0.169 | -0.156 | -0.120 | -0.144 |
| | (0.134) | (0.164) | (0.141) | (0.174) | (0.173) |
| Year_2000 | -0.117 | -0.224 | -0.153 | -0.369 | -0.073 |
| | (0.155) | (0.204) | (0.169) | (0.415) | (0.233) |
| Sibling | 0.014 | 0.015 | 0.014 | -0.004 | 0.115 |
| | (0.045) | (0.055) | (0.047) | (0.051) | (0.234) |
| Observations | 56 | 56 | 56 | 16 | 40 |
| Number of countries | 25 | 25 | 25 | 6 | 19 |