Social interactions and fertility in developing Countries

Citation

Bloom, David E., David Canning, Isabel Gunther, Sebastian Linnemayer. 2008. Social interactions and fertility in developing countries. PGDA Working Paper No. 34.

PROGRAM ON THE GLOBAL DEMOGRAPHY OF AGING
Working Paper Series

Social Interactions and Fertility in Developing Countries

David E. Bloom
David Canning
Isabel Günther
Sebastian Linnemayr

Harvard School of Public Health

June 2008

PGDA Working Paper No. 34
http://www.hsph.harvard.edu/pgda/working.htm

The Program on the Global Demography of Aging receives funding from the National Institute on Aging, Grant No. 1 P30 AG024409-01.

Abstract

There is strong evidence that, in addition to individual and household characteristics, social interactions are important in determining fertility rates. Social interactions can lead to a multiplier effect where an individual's ideas, and fertility choice, can affect the fertility decisions of others. We merge all available Demographic and Health Surveys to investigate the factors that influence both individual and average group fertility. We find that in the early phase of the fertility transition the impact of a woman's education and experience of child death on her group's average fertility are more than three times as large as their direct effect on her own fertility decision.

1. Introduction

During the last century all industrialized countries and most developing countries have experienced various phases of the demographic transition, moving from high to low levels of mortality and fertility. While several socioeconomic factors have been shown to affect individual fertility decisions, the pattern of fertility decline suggests that social interaction and diffusion processes are also at work. The movement to lower fertility tends to occur throughout a population, and not just among women of high socioeconomic status. This pattern occurred historically in Europe during its fertility transition and occurs today in developing countries. The need for considering diffusion as a central part of the fertility transition has been emphasized by many authors (for example: Coale and Watkins 1986; Bongaarts and Watkins 1996; Durlauf and Walker 2001; Kohler 2001; Munshi and Myaux 2006).

While there is a consensus that social interactions are important for understanding the fertility transition there is little evidence regarding the magnitude of these effects. One problem is that there are several mechanisms through which social interactions can operate. Bongaarts and Watkins (1996) indentify three mechanisms for social interaction. One is the transmission of ideas and knowledge, e.g. about contraception methods. A second is observing the actions of others to learn about appropriate behavior in complex situations where evaluation is difficult. The third is social influence where fertility norms are enforced through explicit or implicit group pressures and cultural norms.

In addition to the issue of multiple mechanisms, it appears that a full understanding of social interaction requires longitudinal data, with detailed information on each individual's social network, so we can see how ideas and behavior are transmitted between individuals. There are few such data sets at present. While a full map of social networks would be ideal, it is possible to make inferences about social interactions from existing datasets. Social interaction means that the characteristics and behavior of one's neighbors and friends affect one's own behavior. In the absence of detailed data on social networks we can use data on the average characteristics and behavior of people within a group as factors that affect the decisions of individuals within this group.

Estimation of social interactions in this framework, however, presents several difficulties. Each individual's behavior depends on the average behavior of others but the individual's choice also affects average behavior; Manski (1993) calls this the "reflection problem". In addition, unobserved group characteristics that affect fertility will cause a large bias in the estimates. If these unobserved group characteristics are omitted from the estimation, average fertility will predict individual fertility because everyone's fertility is correlated with the unobserved group characteristic, and not necessarily because of a social spillover in behavior.

The approach we use to address this issue is to begin by modeling individual fertility behavior, including social interactions, and then derive from this a model of aggregate fertility behavior. At the aggregate level social interactions lead to a social multiplier, where the impact of the exogenous variables on aggregate fertility may be much higher than their direct impact on the individual, because of spillovers to other people and the reinforcing feedback loop in fertility they create. Our statistical approach to estimating the size of the social spillovers is based on the differences in the size of the effect of a variable at the individual and aggregate levels, as in Glaeser and Scheinkman (2000) and Graham and Hahn (2005). Our approach is different from that used by Montgomery and Casterline (1993) who use a dynamic model of diffusion in which current fertility depends on past fertility.

Our data on fertility choice come from Demographic and Health Surveys (DHS); we use 206 surveys from 65 countries, with multiple surveys from each country in different years between 1988 and 2005. We focus on how each woman's education, her husband's education, the household's socioeconomic status, and child mortality experience affect her completed fertility. We consider only women aged 45-49 years. These women have usually completed their fertility, so we do not have to consider timing and tempo effects. In total we have 118,629 such women in our sample. We begin by estimating the direct effect of each woman's characteristics on her own fertility behavior. We then aggregate the data to give group data (creating regional (sub-national) and national averages) and investigate how the average characteristics of the households in a group affect the group's fertility behavior. The fact that we have multiple surveys at different times from each region and country allows us to control for group fixed effects that can capture unobserved cultural and institutional factors that could influence fertility.

In the absence of social interactions the coefficients using grouped data should be the same as the coefficients found at the individual level. We find that the effects of child mortality and female education on fertility are significantly higher in the grouped data than at the individual level, suggesting the presence of significant social spillovers. We further find that the effects of a woman's education and child mortality on her own decisions are smaller in countries with high fertility that are in the initial stages of the fertility transition, than in low-fertility countries that are in the later stages of the transition.

This is consistent with the view that in high-fertility countries desired fertility may exceed actual fertility, so that changes in socioeconomic factors that affect desired fertility have little impact on observed fertility. On the other hand, we find that the social multiplier (the ratio of the effect at the aggregate to the individual level) is somewhat larger in high-fertility countries that are in the early stages of the transition. This is consistent with the idea that it is in the early phase of the fertility transition that social learning and cultural norms are strongest, and it is at this stage that social spillovers can have their largest effects.

Our work also contributes to the existing literature on the role of expected child mortality on fertility decisions. Women do not only respond ex-post to an experienced child death, but might also ex-ante insure against the possible death of children by having more births than desired to insure a certain number of surviving offspring (e.g. Schultz 1997). If fertility is based on expected child mortality, people may use average mortality rates in their group as an indicator of expected mortality, thus creating a social spillover. This argument has been used to explain the consistent finding that the effect of infant mortality on fertility is larger in national data than at the individual level (Schultz 1997; Palloni and Rafalimanana 1999). One advantage of our approach is that rather than use individual-level and aggregate data, we construct our group averages directly from the individual-level data, allowing us to estimate exactly the same relationship at different levels of aggregation. For countries in the early stages of the demographic transition, we find that a child death averted reduces overall births by somewhat less than one, so that reductions in infant mortality lead to population growth. However, for countries in which the fertility transition is well under way, we estimate that each child death averted reduces total births in the group by more than one, making reductions in child mortality a source of reductions in family size and a source of slower population growth.

We assume that the social spillovers are geographic, either at the regional or national level. It is likely the spillovers occur within groups that share a common language, ethnicity, and religion. For example, the Princeton European Fertility Project (Coale and Watkins 1986) found that fertility decline in Europe diffused rapidly throughout the continent but preceded fastest within cultural and linguistic groups. Munshi and Myaux (2006) find that fertility interactions occur within but not across religious groups in rural Bangladesh. Our approach measures the average social effect in a geographical area; we leave to future work the issue of differential spillovers across distinct groups within a geographical region.

We find that social multipliers at the national level are larger than those found at the regional level within a country. The regional level analysis does not capture social interactions across regions, thus giving an underestimate of the total social multiplier. It is likely there are also social spillovers across countries. If we would like to look at higher levels of aggregation to capture these wider spillovers we face a problem of having very few groups and a small sample size; in the limit we have just one group, the whole globe. Our estimates of the social multiplier are therefore lower bound, and measure only social multiplier effects due to interactions within a country.

The paper is structured as follows. Section 2 explains the methodology, followed by Section 3, which discusses the data and empirical specifications. The results are presented in Section 4. Section 5 discusses robustness, and Section 6 concludes.

2. Methodology

We base our approach on the methodology proposed by Graham and Hahn (2005) to distinguish social interactions from group unobservables and exogenous from endogenous social interactions. We speak of exogenous interactions whenever the behavior of an individual depends on the characteristics of his or her reference group, e.g., individual fertility depending on the observed child mortality within a social reference group. We refer to endogenous interactions when the behavior of the group has an impact on individuals' behavior, i.e. individual fertility decisions depending on the fertility decisions of other individuals.

Assume that we have N (non-overlapping) groups $(g=1,\ldots,N)$, where in each group M at time t individuals $(i=1,\ldots,M)$ are sampled. Assuming independence across social groups, we can write down a linear-in-means functional form:

$$y_{gti} = \alpha x_{gti} + \beta x_{gt} + \gamma y_{gt} + f_{gt} + \varepsilon_{gti} \qquad (1)$$

where y_{gti} denotes the fertility decision of an individual *i* in group *g* at time *t*. The variable *x* is an exogenous factor that influences fertility. For the purposes of exposition we assume one exogenous variable but the model generalizes to several variables in a straightforward way. As well as an individual's own exogenous characteristics x_{gti} the fertility behavior of the individual may depend on the average of its group's characteristics x_{gt} and on the fertility behavior of the group y_{gt}. There is also a group effect at time *t*, f_{gt} to allow for unobserved group heterogeneity, and an individual error term, ε_{gti}.

There are a number of problems involved in estimating equation (1). The major problem is that the average fertility of the group is clearly endogenous to individual fertility. A second issue is that the effects of the group averages x_{gt} and y_{gt} are not identified. These group averages will be co-linear with the group's fixed effect; we cannot tell if a woman has high fertility because the rest of her group does, or if it is because of a hidden variable that affects the group and causes them all simultaneously to have high fertility.

While the group effects are not identified we can write the model as:

$$y_{gti} = \alpha x_{gti} + v_{gt} + \varepsilon_{gti} \qquad (2)$$

$$where \quad v_{gt} = \beta x_{gt} + \gamma y_{gt} + f_{gt} \qquad (3)$$

There is a group effect v_{gt} that contains the unobserved heterogeneity as well as the effects of the group's characteristics and fertility behavior. By controlling for time-specific group effects we can estimate the effect of individual variations in exogenous characteristics on fertility given by αx_{gti}.

In order to estimate the effect of the group characteristics we have to put some structure on the unobserved group effects. One approach would be to assume that the group effects are random and uncorrelated with the exogenous variables. However, this rules out unobserved factors that influence both our "exogenous" factors and fertility. For example, a cultural pattern of early marriage (that we do not control for) might both reduce female education levels and lead to increased fertility. We assume instead that the group effects can be decomposed into two components, a fixed effect that holds for the group over time and a time effect that is common across groups. That is, we can write $f_{gt} = v_g + \mu_t$. Hence we have:

$$y_{gti} = \alpha x_{gti} + \beta x_{gt} + \gamma y_{gt} + v_g + \mu_t + \varepsilon_{gti} \qquad (4)$$

Now averaging over our observations *i* within a group at time t we can derive

$$\overline{y}_{gt} = \alpha \overline{x}_{gt} + \beta x_{gt} + \gamma y_{gt} + v_g + \mu_t + \overline{\varepsilon}_{gt} \qquad (5)$$

Note we distinguish between the population averages x_{gt} and y_{gt} that affect behavior and the sample averages $\overline{x}_{gt}$ and $\overline{y}_{gt}$. Rearranging terms we have

$$\bar{y}_{gt} = (\alpha+\beta)\bar{x}_{gt} + \gamma\bar{y}_{gt} + \nu_g + \mu_t + [\beta(x_{gt} - \bar{x}_{gt}) + \gamma(y_{gt} - \bar{y}_{gt}) + \bar{\varepsilon}_{gt}] \tag{6}$$

Hence

$$\bar{y}_{gt} = \frac{\alpha+\beta}{1-\gamma}\bar{x}_{gt} + \nu'_g + \mu'_t + \varepsilon'_{gt} \tag{7}$$

where

$$\nu'_g = \frac{\nu_g}{1-\gamma}, \quad \mu'_g = \frac{\mu_g}{1-\gamma}, \quad \varepsilon'_{gt} = \frac{\beta(x_{gt} - \bar{x}_{gt}) + \gamma(y_{gt} - \bar{y}_{gt}) + \varepsilon_{gt}}{1-\gamma} \tag{8}$$

We can estimate equation (7) by regressing a group's average fertility on the group's average characteristics, controlling for group fixed effects and time dummies. There is still the problem of measurement error in equation (7); we use the sample average of the group characteristics rather than the population average (the measurement error in group fertility can be regarded simply as additional noise). This measurement error will tend to bias our estimated coefficients towards zero. The error term ε'_{gt} includes this measurement error. In our estimation we assume that this measurement error is negligible and can be ignored.[1]

---

[1] Graham and Hahn (2005) suggest using an instrument for the group average characteristics when estimating equation (7) to correct for the measurement error. We did this by using half our sample clusters to compute the group average for regions and countries and the other half to instrument (this is a valid instrument since the clusters are chosen randomly implying that the measurement errors of the two subsamples are uncorrelated). This had, however, very little effect on our results, implying that in practice measurement error in group averages is not a significant problem.

The social multiplier is the ratio of the effect $\frac{\alpha+\beta}{1-\gamma}$ of a variable using grouped data to the individual effect α. If there are no social spillovers this ratio will be one, while in the presence of social spillovers it may be much larger than one. Our social multiplier is closely linked to the dynamic analysis of social spillovers in fertility behavior in Montgomery and Casterline (1993). They assume that the spillover is from lagged fertility (five years before) to current fertility rather than contemporaneous. In this dynamic formulation (if we replace current group fertility with lagged fertility in equation (1)) the effect of a change in an exogenous variable Δx after k periods on fertility is $(\alpha+\beta)(1+\gamma+\gamma^2+...+\gamma^k)\Delta x$. As k becomes large this converges to $\frac{\alpha+\beta}{1-\gamma}$, the long-run effect of a change in the exogenous variable. Our approach is essential to estimate the steady-state or long-run effect of the exogenous variables, on the assumption that the dynamics of the diffusion process are fast.

The social spillovers we are examining may have a short range or a long range, and different spillovers may have different ranges. The spillovers may be geographically limited or may move over wide areas within distinct groups. In this paper we focus on evidence for spillovers within geographical areas. Given the nature of the Demographic and Health Surveys there are three geographical levels of analysis that are possible: the cluster level, the regional level, and the national level.

We do not use cluster-level averages. The difficulty with clusters is that while we have several surveys from each country, the clusters selected change in each survey. It is therefore not possible to estimate a cluster fixed effect. In addition, the number of sampled women in the age group 45-49 that we use for analysis tends to be very small within each cluster (an average of less than three women), making the issue of measurement error in calculating group averages troublesome.

In addition to the issue of social interactions we have a further challenge that is specific to the estimation of the effect of child mortality. 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 dead children.

We apply the standard Trussell-Olsen technique (Olsen 1980; Trussell and Olsen 1983) to address this problem (see e.g. Maglad 1994; Bhat 1998; Haines 1998; Palloni and Rafalimanana 1999 for applications of this technique). Olsen and Trussell (1983) show that an unbiased replacement rate can be obtained by using an instrumental variable approach and a further adjustment of this estimator, depending on whether the mortality rate in the sample is constant, random, or correlated with fertility. For the instrumental variable, the number of dead children is instrumented with the proportion of children who died relative to the total number of children ever born to a woman. After applying the diagnostic tools proposed by Trussell and Olsen (1983) to our data set, we follow the adjustment factor D of the Trussell-Olsen technique.

3. Data

The data underlying this analysis come 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. We include as control variables the number of children of the women who have died, the woman's years of education, her partner's years of education, urban or rural residence, an asset index to proxy for income or socioeconomic status, and a time dummy. In later checks of robustness we also include deaths of the woman's siblings in childhood and dummies indicating the religion of the woman (Muslim, Catholic, Protestant, or Others). These variables are not used in the main results since their inclusion reduces the size of the sample significantly. We do not include behavioral variables, such as age at first marriage, as these variables may be endogenous to desired fertility. Summary statistics and a correlation matrix for the dependent and explanatory variables are presented in Tables 1 and 2.

DHS surveys do not contain any direct information about the income or consumption of households. To overcome this lack of data we construct an asset index to approximate a household's permanent income level, estimated via principal component analysis. Filmer and Pritchett (2001) suggest that this type of asset index is a good proxy for a household's permanent income. The assets underlying the index we use are: electricity supply to the household, possession of a radio, possession of a television, and three types of water access (dummy variables for piped water, use of a well or borehole, and use of rainwater or surface water). Insofar as the asset set surveyed in each country differs from one DHS to another, we focus on a limited set of assets to keep the sample size as large as possible. The asset index is the principal component of the vector of assets (which explains most of the variation in assets across households), which we interpret as an indicator of a household's permanent income. The weights used for each asset in the index are shown in Table 3.

The number of countries publicly available from the DHS site is 65. Each country has between 1 and 6 surveys between 1988 and 2005, with a total of 206 surveys at the country level. As not all variables were collected in each survey round in each country, we lose some surveys. The final number of surveys analyzed is 184. Some countries and regions do not allow for a panel fixed-effects specification (equation (7)), either because there is only one survey available, or because the definition of the regions within a specific country across different survey waves changed in a manner that did not allow us to reconcile regions over time.

We further limit our sample to women who have completed their fertility, that is, to women between the age of 45 and 49[2], and to women who have given birth to at least one child. Women without children are excluded from the analysis as they do not have a ratio of children who have died to use as an instrument for the number of dead children (the Trussell-Olsen technique). This removes only 4% of the women in our sample and leaves a data set of 118,627 women. We average the variables across individual women in each survey to get regional and national averages for the survey year. We have 131 country-level observations on 58 countries and 1123

---

[2] The estimates do not change significantly when we vary the cut-off age for completed fertility. In addition, in a regression of fertility that included age dummies, fertility rises with age, but levels off at age 45, with the coefficients on the age dummies above 45 not being statistically different.

region-level observations on 579 regions. In our regional and national level regression we also include time dummies, one for 1995-2000 and one for 2001-2005, with observations from 1994 and before being the reference group. A more detailed discussion of the datasets used and how they were merged is given in the appendix.

4. Results

Table 4 shows the estimation results for the whole sample. Column 1 gives results for the estimation based on individuals using equation (2), while column 2 gives results for equation (7) using data grouped at the regional level. Column 3 gives results using data grouped at the national level.

Our merging of surveys raises the issue of weighting. Each survey has weights that can be used to reflect the different sampling probabilities of each woman in the population. In addition, each DHS survey tends to be about the same size, giving individuals in small countries higher probability of being sampled. In theory we could construct weights that would make our sample representative of the whole population of developing countries that have a DHS. However, there are strong theoretical arguments for not weighting in regression analysis. If the model is correctly specified, the unweighted regression is consistent and is the most efficient estimator available, while if the model is not correctly specified, due to parameter heterogeneity across groups, weighting will not resolve the problem (Deaton 1997).

In column 1 we include dummies for each survey cluster (48,956 clusters). On average we have less than three women aged 45-49 years in each cluster. Controlling for cluster fixed effects in this way removes the need for controlling for clustered sample in the standard errors of the estimates, which is usually done by allowing for cluster random effects.

As we move from column 1 to column 2 the number of observations goes down dramatically. We have 118,627 individuals in the regression in column 1 but these are aggregated in to 1,123 observations at the regional level in column 2. When we further aggregate to the national level, in column 3, we have only 184 observations. The decline in the number of observations as we move to columns 2 and 3 is reflected in the higher estimated standard errors on the coefficients.

The impact of a child death on fertility at the individual level, i.e., the direct replacement effect, is 0.33. This is much lower than the impact of child mortality on fertility at the regional or national level, which – as argued above – combines the direct replacement and exogenous (group child mortality) and endogenous (group fertility) social interaction effects. At the regional level one additional death leads to 0.54 more births and on the national level the marginal impact of a child death is 1.05 and is not statistically different from unity (the standard error is 0.24).[3] A coefficient of one implies that a dead child is fully replaced and that saving a child's life has no effect on the number of surviving children.

---

[3] For the estimation on the national level we consider Indian states as countries. Dropping Indian states does not much change the magnitude of the estimated coefficient but leads to insignificant results as the sample becomes small.

Each year of schooling a woman undergoes appears to reduce fertility by about 0.11 children. However, this effect appears larger in the grouped data, rising to a reduction of about 0.26 children per year of education at the national level. At the household level, the partner's education level also affects fertility, but the effect is much smaller than the effect of the woman's education. The effect of male education does not appear to be significant in data grouped at the regional or national level. The asset index does not appear to be statistically significant in any of our regressions. This may be due to the small number of variables used in constructing this index.

At the individual level we have cluster fixed effects. These cluster fixed effects mean we cannot estimate the effects of urbanization, or time. The cluster is either an urban or rural area, and each cluster in a survey is sampled at the same time, making it impossible to identify these effects when cluster effects are used. At the group level we do see a negative effect of urbanization on fertility, and fertility is lower in later surveys than in earlier ones. We treat this time effect as exogenous, but it could be evidence of worldwide spillovers in fertility behavior.

Table 5 provides the results separately for countries that are early in the fertility transition or perhaps have not even started the transition (a total fertility rate above 5.3 in 1990) and those that are later in the transition and have already undergone substantial fertility decline (a total fertility rate below 5.3 in 1990). This split puts half the countries in our sample in each group (the group with lower fertility, which is later in the transition, turns out to have more observations due to having more surveys per country).

Table 5 shows that the impact of falling mortality appears to be much lower in high-fertility countries than in countries that are well into the transition and already have low fertility rates. The lower replacement rate in high-fertility countries could be due to parents that wish to avoid the possibility of having too few surviving children at the end of their reproductive age (Ben-Porath 1976; LeGrand et al. 2003). In an environment where desired fertility (and the desired number of surviving children) is very high, the best strategy may simply to be to maximize the number of children born, independently of the actual mortality outcomes. When desired number of surviving children falls below the maximum fertility level attainable, a strategy of reducing fertility and replacing children who die becomes more feasible.

The effect of education at the level of the individual appears to be similar in both groups of countries. The decline of fertility over time appears greatest in the countries that are already well into their fertility transitions. Once the transition is underway it appears to proceed of its own accord. Of course some of this decline in fertility over time may reflect social spillovers and diffusion that we are not capturing in our model.

In Table 6 we report the coefficients on female education and child deaths again and also calculate the ratio of the group coefficient to the coefficient found at the individual level. In the absence of social spillovers this ratio should be 1. Calculating standard errors for ratios of regression coefficients is difficult due to the fact that the ratio becomes large when the denominator is close zero and the small sample properties can be very different from large sample asymptotic results. Li and Maddala (1999) recommend using bootstrap methods to calculate standard errors in the case of ratios. We calculate standard errors and 95% confidence intervals for the ratios using bootstrap methods where 1000 bootstrap samples are taken with randomization at the country and regional level respectively (to maintain the panel structure of the data). Note that the 95% confidence interval is not symmetric – symmetry of the confidence interval for a ratio only occurs in very large samples. The stars (*) for the estimated social multipliers reported in Table 6 are based on probability levels for a test of the null hypothesis that the multiplier is unity.

For child deaths we find multipliers that exceed unity; we have values of about 1.7 at the regional level and 3.2 at the national level. This suggests that women's fertility decisions depend not only on their own experience of child mortality, but also on average group child mortality or that there are spillovers from other women's fertility decisions to their own. Note that while the ratio exceeding one is evidence of social spillovers we cannot identify the precise nature of the social spillover with our approach, i.e. distinguish between exogenous and endogenous social interactions.

The results for the size of the multiplier of child deaths are somewhat higher in countries that are in the early phase of the fertility transition. This might either indicate a higher influence of social norms in the early phase of the demographic transition; or, given the problem that replacement may not be possible when a child dies due to infertility in later life, a higher insurance effect in high mortality environments. The main difference between these countries in terms of the response to child deaths is, however, the response at the individual level, which is much greater in countries that are well into the transition; and the social multiplier acts on a larger initial effect in the latter case.

Overall we find a similar large social multiplier for education. At the regional level the effect of education is about 1.5 times as large as that found at the individual level; this multiplier grows to around 2.4 at the national level. The social multiplier for education seems to work, however, primarily in countries that are in the very early stages of the fertility transition. Both early- and late-transition countries have similar effects of education on fertility at the individual level. But the social multiplier only seems to be a factor for education in countries that still have very high fertility rates; we estimate the social multiplier at the national level in these countries to be around 3.5. For countries that are later in the fertility transition and already have fertility below 5.3, we cannot reject the hypothesis that the social multiplier for education is one and women's education affects only their own fertility. It may be that information and idea interactions are most important early in the fertility transition; once any knowledge gaps have been filled, there may be little additional spillover from education.

We find no significant social multiplier from partner's education or assets – in neither case can we reject that the multiplier is one and there are no spillovers. Little can be made of these results, however, since the size of the estimated effect at the group level is small relative to the standard error of these estimates; it is difficult to measure the ratio accurately.

5. Robustness

Section 4 contains our main results. We can, however, check the robustness of these results by investigating alternative specifications. It has been argued that social perceptions of child mortality often lag considerably behind actual mortality (Cleland 2001; Montgomery 1998). Montgomery (1998) emphasizes that people seem to have difficulty in forming expectations about an improvement in child survival, as it involves noticing the absence of an event, rather than the event (mortality) itself. In addition, even if people notice that children in their social surrounding are surviving, this might be attributed to luck rather than a change in underlying survival probabilities (Oppenheim 1997). The consequence is that a considerable time gap may occur between changes in group child mortality and changes in fertility. The lag could even be generational in length as women assume the mortality environment they experienced in their youth still holds.

To try to capture this lag effect we include data on the child mortality rate among a woman's siblings when she was young. For 67 out of the 184 DHS surveys we used in our first specification (Table 4) we have additional information about the number of siblings ever born to the interviewed women as well as the number and age of siblings who have died. We use this information to create a variable of sibling child death, which is the number of siblings (of each woman) who died before the age of 15, and include it in our regression. As before, we instrument the number of sibling deaths with sibling death rates to control for the effect of fertility on the number of child deaths. The results for this specification are presented in Table 7. We lose many observations in this approach since sibling death information is available only in a limited number of DHS.

Sibling mortality has an additional impact on the fertility decisions at the individual level over and above the effect of the current mortality environment. This implies that individuals do not only consider deaths among their own children but also the deaths of their own siblings when deciding on their fertility. However, the size of this lagged mortality effect is very small – a sibling death in childhood has about one-tenth the effect of an own child death on a woman's fertility. At the aggregate level average sibling child deaths in the previous generation seem to have little impact on group fertility, while deaths among the current generation of women's children have social multiplier effects similar to those we found before. Our results hence do not support the hypothesis of a long lag between general declines in child mortality and fertility decisions. These results should, however, be treated with caution; it may be that there are measurement issues with using sibling child mortality. Recall may be poor; particularly for siblings who died before the woman responding was born, leading to underestimates of siblings' child mortality.

A second possible robustness issue is the presence of cultural factors that might affect fertility decisions. To some extent we address this issue. All of our regressions have fixed effects, with cluster, regional, or national dummy variables, depending on the level of aggregation being employed. This means that cultural factors that either are uniform within clusters, or fixed in regions and countries over time, are already controlled for in our regressions. In table 8 we add dummy variables for religion ("other religion" is the baseline). We find evidence that women who are Protestant or Catholic, and particularly those that are Muslim, have a higher completed fertility, when we look at fertility at the individual level. These religious variables are not significant at the regional or national level. In aggregate data there is too little variation in the number of women of the different religions over time to identify the effect of religion at the group level. But these results show that even controlling for religion, the social multiplier is apparent both for child deaths and for a women's education.

6. Conclusion

Our paper builds on three strands of the existing literature. The first is the idea that social interactions are important for fertility. The second is that group infant mortality matters for fertility, because women use this information to form their expectations about their own future child mortality experience. The third stems from the different determinants of fertility in individual-level data and in aggregate data. Our model combines all of these issues in a single framework. By aggregating data from multiple DHS we are able to estimate the same equation at different levels of aggregation. Because the aggregate data are simply the average of the individual-level data and the specifications of the relationships at the individual and aggregate levels are identical, we can identify the difference between the results at the individual and aggregate levels as social spillovers. Our results particularly indicate that social interactions are important when considering the effect of child deaths and female education on fertility decisions. Further research is needed to explore the mechanisms through which these interactions operate.

Appendix: Creation of the Data Set

We merged all unrestricted and recoded data sets that were publicly available on the Demographic and Health Survey site (http://www.measuredhs.com) by October 2007. An overview of the countries and years available together with the survey code is given in Table A2. The maximum number of countries publicly available is 65. Given their size, we consider Indian states as nation states, thus adding additional 26 states. Each country has between 1 and 6 surveys between 1988 and 2005, leaving us with a total of 206 surveys (see Table A2). For each country we used the individual recode (IR) files that contain all data collected for women between the age of 15 and 49.

The DHS surveys differ slightly in the variables included. We tried to keep a balance between including as many relevant variables as possible and excluding as few surveys as possible. The variables we finally used are specified in Table A1. In addition to the provided variables by the DHS we constructed the variables child mortality rate and sibling mortality rate, which we used as instruments for number of dead children and number of dead siblings, as well as the variables country, year, and SSA, which are equal across all observations within one survey (see Table A2 column1-3). Variables 8 – 15 in Table A2 were combined into an asset index estimated via principal component analysis (Table 3). We further constructed year-cluster (for the individual level), year-region (for the regional level), and year-country (for the country level) specific groups, i.e., identifier (row 23-25, Table A1).

In Table A2 column 7 all surveys that contain all the variables listed in Table A1 – except variable mm2_* and mm7_* – and could therefore be included in the specifications without sibling mortality (Table 4 and 5), are indicated. Table A2 column 8 highlights the surveys that did not include the variable sibling history and could therefore not be analyzed in the specification with sibling histories (Table 7).

Table A1: Variables

	DHS Code	Description
1	v201	# Children born
2	v206 , v207	# Dead children, Child mortality rate
3	mm2_*	# Siblings born
4	mm7_*	Age at sibling's death, # dead siblings, sibling mortality rate
5	v133	Years of female education
6	v701	Partner's education
7	v025, v102	Urban residence
8	v119	Electricity
9	v120	Radio
11	v121	TV
14	v113	Piped water
15	v113	Surface water
16	v130	Religion
17	v012	Age
18	v001	Cluster
19	v101	Region
20	Country	Country
21	Year	Year
22	SSA	SSA
23	year, country, v001	Year & Cluster specific group
24	year, country, v101	Year & Region specific group
25	year, country	Year & Country specific group

Note that in the DHS surveys, regions (variable v101) are often not recoded consistently across years for each country. Whenever this was the case we did everything to make regions comparable over time to derive regional identifiers. Sometimes this meant simply recoding the regions in one year, sometimes this meant grouping regions in one year, and sometimes this meant using geographic maps to reconcile changes in administrative regions over time. In some cases none of theses options was feasible.

Table A2 column (6) specifies which countries had consistent regional identifiers (blank), which countries have adjusted regions (small and large adjustment), and which countries did not allow for a regional fixed effect panel because of inconsistent regional identifier over time (no). Some countries allow for neither a regional fixed effect panel nor a country fixed effect panel because only one DHS survey was conducted. Those countries are marked in Table A2 column (5).

Last, we limit our sample to women above the age of 44 who had completed their birth history at the date of survey. In addition, we excluded all women who did not give birth to any children since they do not reveal any information about replacement effects. This leaves us with a data set of 118,627 observations.

Table A2: Data Set

DHS Code (1)	Country (2)	Year (3)	SSA (4)	Country panel (5)	Consistent regions (6)	All variables (7)	Sibling history (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	1998					

	Kashmir				
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				

Notes*:* SSA: countries in sub-Saharan Africa. Consistent Regions: small adjust.: countries for which we had to make small adjustments to obtain consistent regional identifiers over time (recoding and regrouping). large adjust.: countries for which we had to make large adjustments to obtain consistent regional identifiers over time (geographic mapping). no: it was not possible to reconcile consistent regions over time. All variables: countries for which we have all variables except sibling histories. Sibling history: Surveys for which sibling histories are available.

References

Ben-Porath, Y. 1976. “Fertility Response to Child Mortality: Micro Data from Israel.” *Journal of Political Economy*, 84(4): 163-78.

Bhat, P.N.M. 1998. “Micro and Macro Effects of Child Mortality on Fertility: The Case of India,” in: M.R. Montgomery and B. Cohen (eds.), *From Death to Birth: Mortality Decline and Reproductive Change*. Washington, D.C.: National Academy Press.

Bongaarts, J. and S.C. Watkins. 1996. “Social Interactions and Contemporary Fertility Transitions.” *Population and Development Review*, 22(4): 639-82.

Cleland, J. 2001. “The Effect of Improved Survival on Fertility: A Reassessment.” *Population and Development Review*, 27: 60-92.

Coale, A.J. and S.C. Watkins. 1986. *The Decline of Fertility in Europe*. Princeton: Princeton University Press.

Deaton, A. 1997. *The Analysis of Household Surveys*. Baltimore: Johns Hopkins University Press.

Durlauf, S. and J. Walker. 2001. "Social Interactions and Fertility Transitions," in: J. B Casterline (ed.), *Diffusion Processes and Fertility Transition: Selected Perspectives*. National Academies Press.

Filmer, D. and L. Pritchett. 2001. "Estimating Wealth Effects without Expenditure Data--Or Tears: An Application to Educational Enrollments in States of India." *Demography*, 38(1): 115-32.

Glaeser, E. and J. Scheinkman. 2000. "Non-Market Interactions." NBER Working Paper Series, Working Paper 8053.

Graham, B. and J. Hahn. 2005. "Identification and estimation of the linear in means model of social interaction." *Economics Letters*, 88: 1-6.

Haines, M. 1998. "The relationship between infant and child mortality and fertility. Some historical and contemporary evidence for the United States." in: M.R. Montgomery and B. Cohen (eds.), *From Death to Birth: Mortality Decline and Reproductive Change*. Washington, D.C.: National Academy Press.

Kohler, H.P. 2001. *Fertility and social interaction: an economic perspective*. Oxford: Oxford University Press.

LeGrand, T., Koppenhaver, T., Mondain, N. and S. Randall. 2003. “Reassessing the Insurance effect. A qualitative Analysis of Fertility Behavior in Senegal and Zimbabwe.” *Population and Development Review*, 29(3): 375-403.

Li, H. and G. S. Maddala. 1999. “Bootstrap Variance Estimation of Nonlinear Functions Of Parameters: An Application To Long-Run Elasticities Of Energy Demand.” *The Review of Economics and Statistics*, 81(4): 728-33.

Maglad, N.E. 1994. “Fertility in Rural Sudan: The Effect of Landholding and Child Mortality.”” *Economic Development and Cultural Change* 42(4): 761-72.

Manski, C.F. 1993. “Identification of Endogenous Social Effects: The Reflection Problem.” *Review of Economic Studies*, 60: 531-42.

Montgomery, M.R. (1998). “Learning and lags in mortality perceptions,” in: M.R. Montgomery and B. Cohen (eds.), *From Death to Birth: Mortality Decline and Reproductive Change*. Washington, D.C.: National Academy Press.

Montgomery, M.R. and Casterline J.B. 1993. “The Diffusion of Fertility Control in Taiwan: Evidence from Pooled Cross- Section Time-Series Models.” *Population Studies*, 47: 457-79.

Munshi, K. and J. Myaux. 2006. “Social norms and the fertility transition.” *Journal of Development Economics* 80: 1-38.

Olsen, R. 1980. “Estimating the Effects of Child Mortality on the Number of Births.” *Demography* 17: 429-44.

Oppenheim-Mason, K. 1997. “’Explaining Fertility Transitions.” *Demography* 34(4): 443-54.

Palloni A., and H. Rafalimanana. 1999. “The Effect of Infant Mortality on Fertility Revisited: New Evidence from Latin America.” *Demography* 36(1): 41-58.

Schultz, T.P. 1997. “Demand for Children in Low Income countries.” in: Rosenzweig, M.R. und O. Stark (ed.) : *Handbook of Population and Family Economics*. Elsevier Science.

Trusell, J. and R. Olsen. 1983. “Evaluation of the Olsen Technique for Estimating the Fertility Response to Child Mortality.” *Demography* 20(3): 391-405.

Table 1: Descriptive statistics

	Mean	Std. Dev.	Obs.
Children ever born	5.782	[2.968]	137,583
Deaths of children	1.030	[1.465]	137,583
Child mortality	0.150	[0.200]	137,583
Deaths of siblings	0.864	[1.489]	59,228
Sibling mortality	0.150	[0.225]	59,228
Female education	3.559	[4.403]	137,516
Partner's education	6.194	[5.310]	131,416
Urban	0.405	[0.491]	137,583
Asset index	0.000	[1.000]	124,190
Muslim	0.352	[0.478]	101,265
Catholic	0.171	[0.376]	101,265
Protestant	0.184	[0.387]	101,265

Notes: Education in years.

Table 2: Correlation matrix

	Children	Deaths of children	Child mortality	Deaths of	Sibling mortality	Female education	Partner’s education	Urban	Asset index	Muslim	Catholic	Protestant
Children ever born	1											
Deaths of children	0.567	1										
Child mortality	0.219	0.812	1									
Deaths of siblings	0.099	0.133	0.115	1								
Sibling mortality	0.095	0.140	0.139	0.827	1							
Female education	-0.307	-0.309	-0.280	-0.119	-0.141	1						
Partner’s education	-0.263	-0.292	-0.259	-0.099	-0.119	0.679	1					
Urban	-0.220	-0.193	-0.156	-0.044	-0.049	0.382	0.362	1				
Asset index	-0.272	-0.278	-0.242	-0.075	-0.083	0.492	0.463	0.583	1			
Muslim	-0.011	0.062	0.051	-0.038	-0.016	-0.096	-0.139	0.032	0.007	1		
Catholic	-0.026	-0.078	-0.064	0.004	0.006	0.169	0.154	0.127	0.141	-0.444	1	
Protestant	0.053	-0.005	-0.021	0.030	-0.006	0.048	0.054	0.016	-0.079	-0.417	-0.032	1

Table 3: Asset index

	Factor 1		
	Scoring Coefficients	Eigenvalue	Variance explained
Electricity	0.327	2.510	0.502
Radio	0.186		
TV	0.323		
Piped water	0.306		
Surface water	-0.243		

Table 4: Determinants of fertility (whole sample)

	Individual	**Regional**	**National**
	Cluster fixed effects	Region fixed effects	Country fixed effects
Child death (IV)	0.326*** (0.008)	0.545*** (0.089)	1.049*** (0.240)
Female education	-0.110*** (0.003)	-0.161*** (0.028)	-0.264*** (0.066)
Partner's education	-0.016*** (0.002)	0.020 (0.026)	0.040 (0.043)
Urban	--- [a)]	-0.858*** (0.240)	-0.994* (0.530)
Asset index	-0.016 (0.017)	-0.166 (0.120)	0.219 (0.290)
Year 1996-year 2000 [c)]	--- [a)]	-0.266*** (0.052)	-0.108 (0.099)
Year 2001-year 2005	--- [a)]	-0.396*** (0.061)	-0.124 (0.120)
R^2 (adj.)	0.33	0.96	0.98
Observations	118,627	1,123	184
Groups	48,956		

Source: DHS Surveys I, II, III, IV.

Notes: *** denotes significance at 1% level. ** denotes significance at 5% level. * denotes significance at 10% level. Standard errors are reported in parentheses. Dependent variable is the number of children born. The sample only consists of women who have completed their fertility, which in our sample we take as women 45 years of age and older. We further controlled for year fixed effects (not shown here). [a)] "Urban" and "year" are a cluster-variable; therefore they drop out when we use cluster fixed effects. [c)] "Year 1988-year 1995" is the reference group.

Table 5: Determinants of fertility in early transition countries and late transition countries

	Early in the transition TFR>5.3 in 1990			**Late in the transition TFR<=5.3 in 1990**		
	Individual	Regional	National	Individual	Regional	National
	Cluster fixed effects	Region fixed effects	Country fixed effects	Cluster fixed effects	Region fixed effects	Country fixed effects
Child death (IV)	0.158***	0.350***	0.590**	0.452***	0.856***	1.525***
	(0.014)	(0.110)	(0.280)	(0.010)	(0.140)	(0.340)
Female education	-0.083***	-0.193***	-0.287**	-0.110***	-0.131***	-0.154**
	(0.009)	(0.043)	(0.110)	(0.003)	(0.033)	(0.073)
Partner's education	-0.012**	0.071*	0.0504	-0.016***	0.053*	-0.034
	(0.006)	(0.039)	(0.110)	(0.003)	(0.031)	(0.044)
Urban	--- a)	-0.821**	-1.415	--- a)	-0.879***	-0.362
		(0.370)	(0.970)		(0.280)	(0.530)
Asset index	0.059	-0.399**	-0.113	-0.027	0.009	0.367
	(0.041)	(0.180)	(0.410)	(0.018)	(0.150)	(0.340)
Year 1996-year 2000 c)	--- a)	-0.188**	-0.012	--- a)	-0.276***	-0.172
		(0.085)	(0.150)		(0.059)	(0.120)
Year 2001-year 2005	--- a)	-0.175*	0.139	--- a)	-0.503***	-0.377**
		(0.100)	(0.180)		(0.074)	(0.150)
R^2 (adj.)		0.90	0.94		0.97	0.97
Observations	38,255	505	70	80,372	618	114
Groups	16,952			32,004		

Source: DHS Surveys I, II, III, IV.
Notes: *** denotes significance at 1% level. ** denotes significance at 5% level. * denotes significance at 10% level. Standard errors are reported in parentheses. Dependent variable is number of children born. The sample only consists of women who have completed their fertility, which in our sample we take as women 45 years of age and older. We further controlled for year fixed effects (not shown here). a) "Urban" and "year" are a cluster-variable; therefore they drop out when we use cluster fixed effects. c) "Year 1988-year 1995" is the reference group.

Table 6: The Social Multiplier in Fertility Behavior

	Coefficient individual level reg.	Coefficient regional level reg.	Coefficient country level reg.	Ratio regional to individual reg.	Ratio country to individual reg.
All Countries					
Mortality	0.326***	0.545***	1.049***	1.673**	3.224**
				(0.311)	(0.668)
	(0.008)	(0.089)	(0.240)	[1.038; 2.269]	[1.359; 3.929]
Education	-0.110***	-0.161***	-0.264***	1.469**	2.403**
				(0.249)	(0.466)
	(0.003)	(0.028)	(0.066)	[1.352; 2.322]	[1.636; 3.506]
Early-Transition Countries					
Mortality	0.158***	0.350***	0.590**	2.211*	3.725*
				(0.863)	(1.644)
	(0.014)	(0.110)	(0.280)	[0.679; 4.151]	[0.576; 7.068]
Education	-0.083***	-0.193***	-0.287**	2.335**	3.469**
				(0.641)	(1.304)
	(0.009)	(0.043)	(0.110)	[1.952; 4.533]	[1.310; 6.517]
Late-Transition Countries					
Mortality	0.452***	0.856***	1.525***	1.894**	3.373**
				(0.283)	(0.812)
	(0.010)	(0.140)	(0.340)	[1.124; 2.258]	[2.630; 4.930]
Education	-0.110***	-0.131***	-0.154**	1.187	1.393
				(0.305)	(0.553)
	(0.003)	(0.033)	(0.073)	[0.609; 1.820]	[0.587; 2.573]

Notes: *** denotes significance at 1% level. ** denotes significance at 5% level. * denotes significance at 10% level. Significance levels with regard to coefficients means significantly different from zero. Significance levels with regard to ratios means significantly greater than 1. Standard errors are reported in parentheses. […] denotes 95% confidence interval. Confidence intervals are percentile confidence intervals. We bootstrapped the standard errors and confidence intervals, applying a panel bootstrap using 1000 replications.

Table 7: Including sibling mortality as a determinant of fertility

	Individual	**Regional**	**Country**
	Cluster fixed effects	Region fixed effects	Country fixed effects
Child death (IV)	0.236*** (0.013)	0.561*** (0.130)	0.854** (0.400)
Sibling death (IV)	0.027** (0.012)	0.166 (0.101)	0.018 (0.320)
Female education	-0.105*** (0.006)	-0.259*** (0.045)	-0.371* (0.180)
Partner's education	-0.016*** (0.005)	0.091** (0.046)	-0.078 (0.130)
Urban	--- a)	-1.169*** (0.340)	-0.168 (1.240)
Asset index	0.106*** (0.031)	-0.103 (0.170)	0.205 (0.570)
Year 1996-year 2000 c)	--- a)	-0.260*** (0.070)	-0.042 (0.150)
Year 2001-year 2005	--- a)	-0.381*** (0.082)	0.174 (0.210)
R^2 (adj.)	0.27	0.96	0.98
Observations	52,628	662	67
Groups	23,892		

Source: DHS Surveys I, II, III, IV.

Notes: *** denotes significance at 1% level. ** denotes significance at 5% level. * denotes significance at 10% level. Standard errors are reported in parentheses. Dependent variable is number of children born. The sample only consists of women who have completed their fertility, which in our sample we take as women 45 years of age and older. We further controlled for year fixed effects (not shown here). a) "Urban" and "year" are a cluster-variable; therefore they drop out when we use cluster fixed effects. c) "Year 1988-year 1995" is the reference group.

Table 8: Including religion as a determinant of fertility

	Individual	**Regional**	**Country**
	Cluster fixed effects	Region fixed effects	Country fixed effects
Child death (IV)	0.312*** (0.009)	0.435*** (0.096)	0.930*** (0.220)
Female education	-0.102*** (0.004)	-0.162*** (0.033)	-0.255*** (0.073)
Partner's education	-0.013*** (0.003)	0.041 (0.029)	-0.091** (0.044)
Urban	--- a)	-0.932*** (0.280)	-1.070** (0.510)
Asset index	-0.029 (0.018)	-0.158 (0.140)	-0.150 (0.260)
Muslim b)	0.659*** (0.048)	0.303 (0.430)	0.145 (1.160)
Catholic	0.330*** (0.053)	0.521 (0.330)	0.359 (1.150)
Protestant	0.249*** (0.047)	0.256 (0.230)	0.185 (0.570)
year 1996-year 2000 c)	--- a)	-0.256*** (0.058)	-0.074 (0.110)
year 2001-year 2005	--- a)	-0.377*** (0.069)	-0.089 (0.120)
R^2 (adj.)	0.33	0.96	0.98
Observations	90,229	854	153
Groups	34,577		

Source: DHS Surveys I, II, III, IV.

Notes: *** denotes significance at 1% level. ** denotes significance at 5% level. * denotes significance at 10% level. Standard errors are reported in parentheses. Dependent variable is number of children born. The sample only consists of women who have completed their fertility, which in our sample we take as women 45 years of age and older. We further controlled for year fixed effects (not shown here). a) "Urban" and "year" are a cluster-variable; therefore they drop out when we use cluster fixed effects. b) "Other religion" is the reference group. c) "Year 1988-year 1995" is the reference group.