Infant Death Clustering in India:

Genuine Scarring vs Unobserved Heterogeneity

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ABSTRACT

Data from a range of different environments indicate that the incidence of death is not randomly distributed across children or households but, rather, that there is death clustering within households. A hypothesis of considerable interest for both theory and policy is that there is a causal process whereby the death of a child influences the risk of death of the succeeding child in the family. This causal effect which, drawing language from the literature on unemployment, we term scarring or genuine state dependence tends to be confounded with both observable and unobservable inter-family heterogeneity. In this paper, we investigate the extent of genuine scarring in three Indian states, controlling for these confounding factors. The paper offers a number of methodological innovations upon previous research in the area and, thereby, offers what we expect are more robust estimates of the scarring effect.

1. INTRODUCTION

Data from a range of different environments indicate that the incidence of childhood death is not randomly distributed across children or households but, rather, that there is a positive association of sibling deaths. This appears to persist even after controlling for relevant socio-economic, behavioural and biological variables[1]. It has, in the last decade, been recognised that this invalidates the assumption of independence of observations for siblings that characterised earlier statistical models of child mortality.

A hypothesis that is of considerable interest for both theory and policy is that there is a causal process whereby the death of a child influences the risk of death of the succeeding child. That is, a family that experiences a child death is "scarred" in the sense that, by a causal process, the subsequent child in that family is predisposed to a higher death risk.[2] This type of scarring is popularly known as 'genuine state dependence' in the literature on unemployment (Heckman, 1981b). The clustering observed in the data or in simple regressions of death risk of the index child on survival status of the preceding sibling will tend to over-estimate state dependence to the extent that there is observed and unobserved heterogeneity between families that cannot be held constant. The main objective of this paper is to revisit the issue of death clustering using a unified statistical framework in order to identify the extent of genuine state dependence or scarring, after controlling for the confounding effects of observed and unobserved heterogeneity.

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[1] See, for example, Zenger (1993), Miller *et al* (1992), Das Gupta (1990), Bean *et al* (1988), Hobcraft *et al* (1985).

[2] Defining a state as a realisation of a stochastic process, this is equivalently described as the death risk facing the index child being dependent upon the "state" revealed [/experienced] for the preceding child.

A particular [causal] dynamic that has interested demographers operates by the death of a child modifying both birth spacing and birth spacing effects on the mortality risk of the subsequent child.[3] The death of a child tends to shorten the time to the next birth because the mother stops breastfeeding and, thereby, is able to conceive sooner than otherwise (see, for example, Zenger (1993), Cantrelle et al (1978), Chen et al (1974)). As it can take up to 24 months for the mother to recuperate physiologically from a birth, a short preceding birth interval elevates mortality risk[4]. If the family-level clustering in deaths that is observed in the data reflects genuine state dependence of this kind, then there are clear implications for policy such as that improving access to contraception will reduce death clustering and overall mortality rates. If, on the other hand, multiple child deaths in a family reflect a genetic vulnerability that all children in the family share then, while such families may be suitable targets for policy intervention, there is no particular reason to expect contraception to have a big impact. Thus an appropriate choice of policy interventions relies upon identifying the extent of scarring after controlling for confounding factors.

In the last decade, demographers have shown an active interest in understanding death clustering[5]. However, the common practise in the literature of discarding information on children born before a certain date raises the problem that the start of the sample period

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[3] The use of the term "dynamic" may deserve explanation. A dynamic model is typically one in which X_t is modelled as a function of lags of X_t. Thus the commonly used first-order Markov model is $X_t=\alpha+\beta X_{t-1}+u_t$, where the regressor is termed the lagged dependent variable. In this paper, while time is implicitly involved, X_t is the mortality risk of the *index* child and the lagged regressor is the survival status of the *preceding* child.

[4] This is the case of pure state dependence arising by the impact of a previous death on birth spacing to the next birth. A further twist on the story is that the risk-raising effect of a short birth interval tends to be smaller for children whose elder sibling has died (e.g., Zenger (1993), Pebley et al (1991), Nault et al (1990), Bean et al (1988)). This can be explained in terms of a surviving elder sibling creating physiological demands on the mother in terms of breastfeeding (e.g., NRC, 1989), increasing competition for resources such as food and parental care (e.g. Zenger, 1993), or transmitting infectious diseases to the index child (Aaby et al, 1984).

does not coincide with the start of the stochastic process under study (see Section 4.2). As a result, previous estimates of state dependence are potentially biased [upwards]. A contribution of this study is that it avoids this problem by using the complete birth history of each mother and specifying a separate reduced form model for first-born children. A test for exogeneity of the first observation is provided. Other specification issues raised in this paper relate to generalisation of the distribution of unobservables by allowing for mass points at the extrema of the distribution, avoiding time-inconsistency, and investigating sensitivity to recall bias or measurement error in reporting of the age of death (see Sections 3 and 4). Sensitivity of the estimated scarring effects to the choice of parametric model is also investigated. Results are presented to show the percentage of observed death clustering that can be explained by genuine state dependence (i.e. by the survival status of the preceding sibling).

The evidence on death clustering is almost entirely from developing countries. If this were entirely a reflection of inter-family differences in observable characteristics and unobservables such as genetic composition [or maternal health] then we would expect the degree of death clustering to be fairly independent of the level of socio-economic development. However, if there were genuine scarring then we would expect it to decrease with socio-economic development and demographic change[6]. Previous estimates of scarring effects show some geographic variation but it is difficult to say whether this represents a robust description of the geographical variation in scarring effects since these

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[5] See Zenger (1993), Guo (1993), Curtis, *et al* (1993). Economists have exhibited little awareness of the phenomenon. Bhargava (2002) is a recent exception, on which further comment is in Section 4.

[6] Socio-economic development is typically associated with a greater effective supply of and demand for contraception. Death of a preceding sibling would not necessarily lead to a short birth interval to the next child if contraception were available and if socio-economic variables were such that it was acceptable to use contraception. [Parity or fertility are also associated with death risk but as these are choice variables jointly determined with mortality [risk], it is not straightforward to make causal statements such as that a decline in fertility will tend to reduce state dependence.]

studies use different model specifications, making them strictly non-comparable[7]. By estimating the same models for three Indian states that are at very different levels of social, economic, political and demographic development this paper provides comparisons that enable a tentative association of the degree of state dependence with the level of development[8].

An early contribution to the now active literature on death clustering that has been much cited is a paper based on a small survey of households in the Indian state of Punjab[9]. This is the first paper to attempt a more rigorous statistical analysis of death clustering in India[10].

The next Section describes the data used and the incidence of death and family-level clustering found in India. A formal econometric model is set out in Section 3, where genuine state dependence or scarring is defined and distinguished from unobserved heterogeneity. Issues that arise in estimation of the model given the nature of the available data are discussed in Section 4, which also describes the relation of this paper to previous research. Section 5 describes the empirical model, defines the variables used in the study and presents descriptive statistics for India and for the three states selected for this study. The results are set out in Section 6 and Section 7 concludes with a discussion of what the study has shown and provides some suggestions for further research.

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[7] The one study that we are aware of that provides comparable region-specific estimates of scarring is that of Curtis, Diamond and McDonald (1993) who, using data for different regions of Brazil in 1986, find, consistent with our hypothesis, that state dependence is only significant in the poor North-eastern region of Brazil. They interact the previous child's survival status with region, which is similar to what is done in this paper, the difference being that we allow all model parameters and not just sibling survival status to be region-specific.

[8] The Indian states approximate European countries in size [and diversity] and each set of results is also interesting in itself.

[9] Das Gupta (1990). Also see Das Gupta (1997).

[10] Although see Bhargava (2002), who analyses data for the Indian state of UP, one of the three states that we investigate.

2. THE DATA & DEATH CLUSTERING IN INDIA

The Data

The National Family Health Survey (NFHS II) was conducted in 26 Indian states in 1998-99, covering more than 99 percent of India's population. The NFHS has a systematic, multistage, stratified sample design that was uniform over the states. It interviewed about 90,000 ever-married women in the age group 15-49. For each woman, the data contain a complete fertility history, including records of child deaths. As indicated earlier, we perform our analysis for each of three Indian states, Uttar Pradesh (UP), West Bengal (WB) and Kerala (KE). UP is the largest Indian state with social and demographic indicators that put it below the Indian average. Kerala is an exceptional state that leads India in almost every index of human development. West Bengal lies between the two in social-demographic development while exhibiting better economic indicators (level of per capita income, poverty incidence) than the other two states. A profile of the three states relative to India is presented in Appendix: Table 1.

Table 1 reports neonatal, infant and under-5 death rates. Of every 1000 births in India, 82 die before the age of 12 months. There is remarkable inter-state variation. The corresponding numbers are 116 in UP, 76 in WB and 36 in KE.

Death Clustering

How unequally is childhood mortality distributed across families? This Section investigates the extent of death clustering within families in the Indian data, using some alternative indices. Consider Table 3. Of families that have experienced at least one infant death, 27% experience *at least one further infant death*. Given that X% of families experience one and

only one infant death, this is suggestive of clustering[11]. Clustering is most evident in UP, least evident in KE and at about the national average in WB[12].

In the formal analysis conducted in this study and also in some previous studies, a first-order Markov model is specified in which, conditional on the survival status of the preceding child, the survival status of earlier children does not influence the survival status of the index child. For this reason, Table 4 shows the sample probabilities of infant death conditional on the survival status of the preceding child. In UP, the probability of infant death is higher by 0.16 (i.e. it is 0.25 rather than 0.09) if the preceding sibling died as an infant. An alternative expression of the relative risk is that an infant in UP is 2.8 times as likely to die if the preceding sibling died rather than survived. The Table shows that the difference in the probability of death conditional on death or survival of the previous child is similar in WB and UP and smaller (at 0.12) in KE. The ratio of these conditional probabilities is, however, largest in KE and smallest in UP. While KE has lower levels of mortality than the other states, the death of a previous child is, in this state, associated with a five-fold increase in death risk for the index child. (dropped because we are not going to work with cohorts in this paper). (dropped these graphs) Overall, the Indian data exhibit a remarkable degree of death clustering. Without further analysis, however, it is impossible to say whether this reflects genuine state dependence. In Section 3 we set out an econometric model which defines genuine state dependence as distinct from the confounding effects of unobserved inter-family heterogeneity.

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[11] To see this, suppose there was no family-level clustering or that the death risks of infants from the same family were independent. Then, if the probability of one and only one death in a family is 0.0X then the probability of two deaths in a family is 0.0X^2. Similarly, the probability of 3 deaths in a family is 0.0X^3. Table 3 shows that the 27% families that experience multiple deaths account for 48% of all infant deaths or that there are just less than 2 deaths per family in this group. Then the fact that 27% exceeds 0.0X^2 is evidence of death clustering.

[12] This is consistent with relatively low fertility levels in Kerala and with its relatively high level of contraceptive use.

3. THE ECONOMETRIC MODEL

The econometric model for child j ($j=2,\ldots,n_i$) in family i ($i=1,2,\ldots, N$) is specified as

$$y_{ij}^{*} = \boldsymbol{x_{ij}}'\boldsymbol{\beta} + \gamma y_{ij-1} + \alpha_i + u_{ij}, \qquad (1)$$

where y_{ij}^{*} denotes the unobservable propensity for child j in family i to experience an early death, $\boldsymbol{x}$ is a vector of strictly exogenous observable child and family specific characteristics that influence y^{*}, $\boldsymbol{\beta}$ is the vector of coefficients associated with $\boldsymbol{x}$. It is assumed that there are n_i children in family i. A child is observed to die when his/her propensity for death crosses a threshold (zero in this case), that is, if $y_{ij}^{*} > 0$. For reasons stated in the previous section, it is assumed that this unobservable propensity is a function of the *observed* survival status of the previous child in the family; that is, it is the actual *experience* of a death of the previous child, rather than the *propensity* to die, that affects the survival status of the index child. The inclusion of the survival status of the previous child on the right hand side of (1) allows one to test for the presence of state dependence.[13] A family specific term α_i is included to account for the possibility that there may be unobserved and, possibly, unobservable family characteristics which influence the index child's propensity to die.

Before proceeding with the estimation of the model, some assumptions regarding the survival status of the first child y_{i1} are required. A reduced form equation for the first child is specified as follows,

$$y_{i1}^{*} = \boldsymbol{\lambda}' \boldsymbol{z_i} + \eta_i \qquad i=1,\ldots,N \text{ and } j=1 \qquad (2)$$

where $\boldsymbol{z_j}$ is a vector of exogenous covariates that are assumed to influence y_{i1}^{*}, $var(\eta_i)=\sigma^2_{\eta}$ and $corr(\alpha_i, \eta_i)=\rho$. In principle, the vector of covariates in x and z need not be

the same. These covariates are also allowed to have different effects in equations (1) and (2)To account for the possibility of non-zero ρ, we adopt a linear specification, in terms of orthogonal error components, :

$$\eta_{i} = \theta\, \alpha_i + u_{i1} \qquad (3)$$

By construction, α_i and u_{i1} in (3) are orthogonal to one another, $\theta = \rho\, \sigma_\eta/\sigma_\alpha$ and $var(u_{i1}) = \sigma^2_\eta(1-\rho^2)$. Hence, it follows that,

$$y_{i1}^{*} = \boldsymbol{\lambda}' \boldsymbol{z_i} + \theta\, \alpha_i + u_{i1} \qquad i=1,....,N \text{ and } j=1 \qquad (4)$$

$$y_{ij}^{*} = \boldsymbol{x_{ij}}'\boldsymbol{\beta} + \gamma y_{ij-1} + \alpha_i + u_{ij}, \qquad i=1,....,N \text{ and } j=2,..,n_i \qquad (1)$$

Equation (4) together with (1) specifies a complete model for the infant survival process.

Assumptions regarding the distribution of α_i and for y_{ij}^{*} conditional on α_i, $\boldsymbol{x_{ij}}$ and y_{ij-1} are now required. First it is assumed that α_i are independent and identically distributed with density h, and y_{ij}^{*} conditional on α_i, $\boldsymbol{x_{ij}}$ and y_{ij-1} is independently distributed with a distribution function F, both to be made precise shortly.

Marginalising the likelihood with respect to α_i gives the likelihood function for family *i*

$$L_i = \int_{-\infty}^{\infty} \Big(\prod_{j=2}^{n_i} F[(\boldsymbol{x_{ij}}'\boldsymbol{\beta} + \gamma\, y_{ij-1} + \sigma_\alpha\, \tilde{\alpha})(2y_{ij} - 1)\Big]$$

$$F[(\boldsymbol{z}_i'\boldsymbol{\lambda} + \theta\ \sigma_\alpha\ \tilde{\alpha})\ (2y_{i1} - 1)]\ \Big)\ h(\tilde{\alpha})\, d\tilde{\alpha} \qquad (5)$$

where , $\tilde{\alpha} = \alpha/\sigma_\alpha$.

[13] For a survey of some of these models, see Hsiao (1986) and Maddala (1987).

Distributional assumption for the conditional distribution of y^*

The most popular assumption regarding the distribution of y_{ij}^* conditional on α_i, $\boldsymbol{x_{ij}}$ and y_{ij-1}, F, is the Logistic function, which is symmetric with respect to the mean. In order to check for the sensitivity of the estimates to the distributional assumption for F, the models are estimated under three different assumptions for F: logistic, standard normal and extreme-value. Unlike the logistic and the standard normal, the extreme value distribution is not symmetric. If tail behaviour is important in determining infant death probabilities, then results from the standard normal model might differ from that of the logistic

Assessing the Size of State Dependence Effects

A convenient way to interpret the estimated state dependence effect, γ, is required. One such method is to look at the change in the predicted probabilities conditional on the survival status of the previous child (e.g. Chamberlain, 1984). This involves the fairly standard calculation of marginal effects that is common to qualitative dependent variable models, modified to account for the distribution of unobserved heterogeneity in the population. For each (non-first born) child within a family, predicted probabilities of death are calculated conditional first on the death of the previous child and, secondly, on the survival of the previous child. The *difference* between these two probabilities is averaged over the sample to obtain an estimate of the contribution of state dependence. We also report the *ratio* of the two average conditional probabilities.

4. ISSUES OF MODEL SPECIFICATION AND TESTING

This Section describes potential problems that arise in an empirical specification of the model, indicating the nature of the resulting bias in some previous studies and how this paper attempts to avoid such biases. Section 4.1 argues that the practice of left-truncation of the data common to most previous studies results in potential over-estimation of the

extent of state dependence. In Section.4.2, it is argued that conditioning on the preceding birth interval will tend to lead to an under-estimation of state dependence. Discussion of an appropriate distributional assumption for unobserved heterogeneity (h) is in Section 4.3. Measurement error in age of death is argued, in Section 4.4, to create a possible upward bias in the state dependence coefficient. In Section 4.5, it is argued that it is inappropriate to use time-varying covariates measured at the time of the survey as explanatory variables when the infant deaths that are being analysed may have occurred decades before the survey. Section 4.6 sets out some testable restrictions on the model.

4.1. The Initial Conditions Problem in a Dynamic Model

It is customary in the literature to discard information on children born before an arbitrarily selected date, such as ten or fifteen years before the date of the survey (e.g., Zenger (1993), Guo (1993), Curtis, *et al*, (1993), Madise and Diamond (1995), Bhargava (2002)). [Other studies also explicitly discard first-born children when including the previous sibling's survival status as a regressor]. When the sample is selected in this manner, the start of the sample does not coincide with the start of the stochastic process under study. This produces an 'initial conditions' problem in dynamic models. On account of the presence of family unobservable characteristics, α, in equation (1), the survival status of the previous child, y_{ij-1} is endogenous. Thus discarding observations at the beginning of the sample results in an endogenously truncated sample. In principle, consistent estimates can be obtained from an endogenously truncated sample if an appropriate identifying restriction can be found, that is, a variable that influences the first sample observation but does not appear in the equations for higher birth order children. In practise, it is very difficult to find a valid identifying restriction. This study takes the alternative route of using all of the retrospective information available so that the first observation *does* refer to the first-born child for each

mother. Most previous studies neglect to recognise this problem[14]. This is very important in analyses of death clustering, as it will tend to bias [upwards] estimates of persistence, that is, to over-estimate the extent of genuine state dependence. Discarding initial observations also has the problem that it creates an unnecessary loss of information. For example, in the all-India sample, 29.7% of children are first-born, of whom 12.2% are the only-child.

A test of the null hypothesis that $\theta=0$ in (3) is a test for exogeneity of the first sample observation. Clearly, if $\theta=0$ in (3) then unobservables in the equation for the first observation are uncorrelated with unobservables in the [dynamic] equations for subsequent observations. In this case, a separate specification of the equation for the initial sample observation is unnecessary.

4.2. Specification of State Dependence or Scarring Effects

Previous studies of death clustering differ in their specification of "state dependence effects", variously using one or more of the number of surviving siblings, the survival status of the previous sibling and the preceding birth interval[15]. Recall the causal process hypothesised in the demographic literature to drive state dependence (see Section 1). The survival status of the previous sibling has a direct bearing on the birth interval to the index child in a way that the number of surviving siblings does not. [The number of surviving siblings is a compound indicator of fertility and mortality in the family]. If the purpose is to

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[14] This includes Muhuri and Preston (1991), Muhuri (1996), Pal and Makepeace (2001), Zenger (1993), Guo (1993), Curtis, *et al*, (1993), Madise and Diamond (1995).The only study we are aware of that recognises the endogeneity problem coming via the correlation of the survival status of previous children and family unobservables, is that of Bhargava (2002). He uses a sample of data restricted to ten (check) years before the date of the survey (NFHS-I, 1991/92). The variable used for the identification of the parameters of interest is the number of live births before that of the index child. The validity of this variable as a suitable instrument is questionable since fertility may be considered to be a choice variable [/endogenous].

[15] For example, Bhargava (2002) uses number of surviving siblings, the number of children born before the mother adopted family planning and the preceding birth interval; Curtis et al (1993) use survival status of previous sibling in interaction with the birth interval. Zenger (1993) presents alternative specifications

identify genuine state dependence then the number of surviving siblings is an inappropriate substitute for survival status of the previous sibling.

A number of demographic studies describe the index child's mortality risk as a function of *both* previous child's survival status and the preceding birth interval. To the extent that the previous child's survival status impacts on the index child's death risk by altering the length of the birth interval, holding constant the birth interval in the model will tend to weaken the coefficient on previous child's survival status. As a result, the degree of state dependence will tend to be under-estimated. A further problem with this specification is that the birth interval is an endogenous variable and one for which valid instruments may be difficult to find[16]. There are also measurement problems with birth intervals as they may be shorter on account of premature birth or longer on account of miscarriage [e.g. Madise and Diamond (1993)]. If these events are sufficiently common in the data, the coefficient on birth interval will reflect a compound of these mechanisms.

In this paper, the state dependence effect is captured entirely by the coefficient on previous sibling's survival status. Birth interval is not used as one of the regressors.

4.3. Distributional Assumption for Unobserved Heterogeneity

Following the literature, it is initially assumed that α_i, unobserved family-level heterogeneity, is independently and identically distributed as a normal variate. One possible weakness of this assumption is that it does not allow enough flexibility to model the fact that some families never experience any child deaths and that in some families all children die.(insert % of hhs in India at each end point).

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using the birth interval in every case and either survival status of previous child or family-level random effects in the alternative cases. [add more]

[16] Endogeneity means that the birth interval is potentially correlated with the error term in the model describing mortality risk for the index child. Although uptake of contraception is a choice variable (endogenous), the availability of contraception is a potential instrument for birth interval. This does not

Referring back to equations (1) and (4), a very large positive (negative) value for the unobservable α will give a very large (small) value for y^* and hence a very large (small) probability of observing death of the index child. This can be accommodated by allowing for empirically determined masses at the two extremes, that is, at plus and minus infinity of the Normal mixing distribution[17]. This gives the following likelihood for family *i*,

$$L_i^* = \frac{\psi_0}{1+\psi_0+\psi_1}\left[\prod_{j=1}^{n_i}(1-y_{ij})\right] + \frac{\psi_1}{1+\psi_0+\psi_1}\left[\prod_{j=1}^{n_i} y_{ij}\right] + \frac{L_i}{1+\psi_0+\psi_1} \qquad (6)$$

where, L_i is given by equation (5) and ψ_0 and ψ_1 are the unknown end-point parameters. Hence, the estimated proportion of families who will have a very large or a very small unobserved family component are given by p_0 and p_1 respectively, where,

$$p_0 = \frac{\psi_0}{1+\psi_0+\psi_1} \qquad \textbf{and} \qquad p_1 = \frac{\psi_1}{1+\psi_0+\psi_1}. \qquad (7)$$

In practice, the data may not contain enough variation in order to allow us to identify ψ_1 (this is, indeed, what was found in this study).

4.4. Measurement Error

A reason that many previous studies left-truncate the sample is to minimise recall error in the recorded date of child death, which is assumed to be larger the further away the mother is from the event. It may seem implausible, *a priori*, that mothers ever forget the date of death of a child but the data do exhibit some age-heaping. In particular, the Indian data that are used in this study show heaping at six-month intervals. Since the model has infant death on both sides of the equation, with the index child's risk a function of the preceding

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appear to have been considered in the previous literature. As we do not use the birth interval in our model in this paper, this exploration is left to future work.

[17] See Barry *et al* (1989), Narendranathan and Elias (1982) for an example in the case of unemployment experiences.

child's survival status, positively correlated measurement error in these variables will tend to create an upward bias on the state dependence coefficient.

The dependent variable and the survival status of the preceding child in the model estimated in this paper are both coded as binary variables that are unity if the child dies before the age of 12 months and zero otherwise. To investigate sensitivity of the estimates to age-heaping at 12 months the models were re-estimated with these variables defined to include deaths occurring at 12 months. The results were very similar (and so are not shown but available on request). This is unsurprising since the problem of recall error may be expected to be less severe when the dependent variable is binary (as in a probit or logistic model) than when it records time till death (as in hazard models that are sometimes estimated instead).

4.5. Time Inconsistency

Survey data used to study childhood mortality typically contain complete retrospective histories of births and child deaths experienced by ever-married women aged 15-49. The data we use for India are similar. A woman aged 49 in 1999 may have experienced a birth and an infant death as long ago as 1969. As a result, data on the current assets of her household or the facilities available in her village are unlikely to be informative in an analysis of childhood deaths[18]. This is the time-inconsistency problem. Several previous analyses use time-inconsistent information for variables such as toilet facility, electricity or access to piped water and they do not seem to acknowledge the problem (e.g. Madise and

[18] There is plenty of evidence in the literature that both income mobility and geographical mobility in developing countries is considerable. The recent availability of household and individual-level panel data for developing countries has made it possible to study income distribution dynamics. This research indicates considerable "churning" in the distribution with the identity of households classified as poor changing quite rapidly through time (see Baulch and Hoddinott, 2000). There is also a non-negligible degree of geographical migration (see Williamson, 1998). Together, these facts make implausible the assumption that current household assets or current community infrastructure are a good proxy for the

Diamond (1995), DeVanzo et al (1983), Gubhaju et al (1991), Bhargava (2002)). A few recent papers model community-level random effects (e.g. Bolstad and Manda (2000), Sastry (2001)) which run into the same problem when unobservables (ranging from community infrastructure to social norms) are subject to rapid change[19].

The left-truncation of the data referred to in Section 4.1 mitigates the time inconsistency problem by severing the retrospective information before it gets into the distant past. Sometimes this selection is forced upon the researcher by the nature of the survey[20]. However, as already discussed, this truncation of the data can bias key coefficients in the analysis if the model is dynamic and appropriate identifying restrictions are unavailable. In this paper, since information on the entire history of births for every woman in the sample is used, the problem of time inconsistency is avoided by including in the model only those conditioning variables that can be reasonably assumed to be exogenous and that are time-invariant or at least relatively sluggish (see Section 5).

4.6 Some Testable Restrictions on the Model

1. Exogeneity of the initial sample observation: $\theta=0$ in (4) is a test of the hypothesis that the initial sample observation can be treated as exogenous. Under the assumption that the initial sample observation is exogenous, the model reduces to a simple random effects model.

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socio-economic status of the household at the time that the children in question were exposed to the risk of infant death.

[19] Mother-specific random effects included in this and previous studies are much more likely to be stable. We expect mother-specific unobservables in a model of child mortality to include genetic factors, attitudes or inherent maternal ability, all of which can plausibly be assumed stable over time.

[20] Thus, in the Demographic and Health Surveys of which the Indian survey used in this study is an example, information on certain variables (like vaccinations, breastfeeding) is collected only for births occurring in the three or five years preceding the survey.

2. Process observed from the beginning: This is equivalent to a test of θ=1 in (4). This model can be estimated simply by creating a time dummy (*dum*) which equals one when *j=1* and zero otherwise. Equations (4) and (1) together now become

$$y_{ij}^{*} = \left[\boldsymbol{x}_{ij} * (1 - dum) \right] \, '\boldsymbol{\beta} + \gamma (1 - dum) \, y_{ij-1} + (\boldsymbol{z}_{\mathrm{i}} * dum) \, '\boldsymbol{\lambda} + \alpha_{\mathrm{i}} + u_{ij} \qquad (8)$$

Equation (8) can then be estimated using all the data with standard software packages, which allow estimation of random effects models.

3. No unobservable family characteristics:

Let $corr(\alpha_i + u_{ij}, \alpha_i + u_{ik}) = \dfrac{\sigma_{\alpha}^2}{\sigma_{\alpha}^2 + \sigma_{\mathrm{u}}^2} = r$ say, for all $j \neq k \neq 1$. (9)

The correlation coefficient *r* gives the proportion of total error variance that is attributed to the unobservable component. Given the binary nature of the dependent variable, model identification requires a normalisation. The most common normalisation is that $\sigma^2_{\mathrm{u}}=1$. This is the standard normalisation that is used in probit models where F is assumed to be the standard normal density. But, when F is assumed to be either a logistic or an extreme value distribution, as is the case in this study, then $\sigma^2_{\mathrm{u}} = \pi^2/3$ and $\pi^2/6$.

A test of H_0: $\sigma_{\alpha}^2=0$ (which is a test that there are no unobservable family characteristics in the sample and therefore that the model collapses to a simple binary dependent variable model) is equivalent to a test of H_0: r=0 in equation (9). This can be tested as a likelihood ratio test but the test statistic will not be a standard chi-sq test since the parameter restriction is on the boundary of the parameter space. The standard likelihood ratio test statistic has a probability mass of 0.5 at zero and 0.5 $\chi^2(1)$ for positive values. Thus a one-sided 5% significance level test requires the use of the 10% critical value (Lawless (1987)).

5. AN EMPIRCAL MODEL

This Section describes the variables in the empirical model. Variable definitions and with their means and standard deviations is provided in Appendix Table 2.The dependent variable is defined as unity if the child is observed to die before the age of 12 months and zero otherwise (infant death). The regressor of interest is the infant survival status of the preceding sibling[21].(removed because all this is said in Sec 4.1). .,

Like most previous studies, mother's education and age at first birth are included. Since child mortality risk is known to be U-shaped in mother's age at first birth, this is specified as a quadratic. A relatively flexible specification of mother's education is used:a set of dummy variables for level of education attained. This is preferred to years of education as it allows for non-linearities and because knowledge of whether it is say, secondary, rather than primary education that makes a big difference is of direct interest to policy makers. Unlike most other studies, a similar set of indicators for educational level of father is also included. This is likely to be an important additional control for socio-economic status to the extent that fathers are the main earners. Exclusion of this variable will tend to raise the proportion of the residual variance attributable to unobserved inter-family heterogeneity. Other family-level observable variables included in the model are religion and caste. Child-specific regressors in the equation are child birth-order, gender and an indicator for whether the child is one of a multiple birth (twin, triplet, etc).

In contrast to the common procedure of throwing away observations with missing values, dummy variables were created to indicate missing values and these werencluded in the model estimation.

6. EMPIRICAL RESULTS

[21] [ref Southampton paper: we shd make sure that previous child had 12 months of exposure before index child was born- so I shd check how many cases we have of preceding birth intervals<12 months.]

Estimates of the full model are presented in Table 6. The main result is that we find evidence of genuine state dependence in each of the three Indian states after controlling for a number of exogenous child and family-specific characteristics and for all unobserved differences between families. Genuine state dependence explains 52% of the clustering (or persistence) observed in the data in UP, the corresponding proportions being 30% for WB and 43% for KE (Table 7). These proportions are not vastly different from one another. While it is difficult to make general statements based on a comparison of three statistics, it is interesting that scarring is smaller in WB than in KE.

To assess the importance of controlling for inter-family heterogeneity and also for exogenous observable variables, we estimate models that do not include these terms. The proportion of observed clustering that such a model would (spuriously) attribute to state dependence rises to 87%, 82% and 69% in UP, WB and KE respectively. The large part of this difference is on account of unobserved heterogeneity (see Table 7). We conclude that genuine state dependence tends to be substantially over-estimated in the absence of controls for unobserved heterogeneity.

Some of the coefficients on the interaction terms between the exogenous covariates and an indicator variable for first-born children are significant. This confirms the importance of our strategy (see Section 4.1). The estimate of θ is not significantly different from unity, which implies that unobserved heterogeneity enters the equations for the first child and for subsequent children in a similar way (see Section 3). Estimates of p0 are insignificant in each column, indicating that the mass point correction for very large probabilities of death was not necessary, the assumption of normality offering a fair approximation to the data.

We find that Muslim children face a significantly lower death risk than others in UP, while religion has no impact in other states. Children from the lower castes do not

appear to be more vulnerable after conditioning on the other covariates. The education of both mothers and fathers has a significant effect in reducing death risk in each state, there typically being some additional benefit to each level of education. The quadratic in age of the mother at birth of the index child is significant in each state. However, it is striking that it is clearly less significant in Kerala, where average age at birth is higher and where there appears to be less variation in this variable than in the other states. Birth order effects (order 2 and upwards) are only significant in UP: this is consistent with it being the most backward state and one with relatively high fertility and low availability and use of contraception.

7. CONCLUSIONS

This paper investigates the phenomenon of death clustering in India. This is a phenomenon of considerable theoretical interest, understanding which contributes to understanding the inter-relations of family behaviour, fertility and mortality. It is also clearly of interest to policy since finding a concentration of child deaths within certain households raises the question of identifying and targeting high-risk families. The main aim of the paper is to identify the degree of pure state dependence and it offers some statistical innovations in disentangling this from unobserved heterogeneity. In doing this, it borrows insights from the literature on the economics of unemployment which, it can be argued, has (statistical) properties similar to mortality. The main result is that there is a significant degree of state dependence in each of the three states. We show the extent to which this would be over-estimated if we did not hold constant a range of observables and family-specific unobservables.

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Table 1 - Mortality Rates

Neonatal	5.21%	7.39%	4.82%	2.54%
Infant	8.22%	11.64%	7.59%	3.56%
Under 5	11.28%	15.92%	9.82%	4.47%

Notes:

(i) Authors' calculations from NFHS-II. These are the percentage of all children born to mothers aged 15-49 in 1998-99 that are reported to have died before a certain age.

(ii) Neonatal death is in the first 28 days of life, infant is in the first 12 months and under-5 is in the first 60 months. The post-neonatal mortality rate may be computed as the difference between infant and neonatal mortality rates.

Table 3
Death Clustering in India

	INDIA	UP	WB	KE
Infant Deaths				
Children (m2/m1)	48.22%	57.76%	46.22%	27.36%
Mothers (n2/n1)	27.23%	34.40%	25.04%	13.48%
Under5 Deaths				
Children (m2/m1)	55.73%	66.39%	50.19%	28.57%
Mothers (n2/n1)	32.74%	41.58%	27.78%	14.41%

Notes: To illustrate the interpretation of these figures, consider the upper left cell. This tells us 27% of families (mothers) experienced multiple child deaths and that 48% of infant deaths in India came from such families.
n1: number of mothers with 1 or more child deaths
n2: number of mothers with 2 or more child deaths
m1: number of child deaths amongst mothers with 1 or more child deaths
m2: number of child deaths amongst mothers with 2 or more child deaths

TABLE 4
Previous Child's Survival Status: Clustering and State Dependence in Infant Death

	Uttar Pradesh	West Bengal	Kerala
All Women			
Incidence of infant death (%)	11.10	7.31	3.32
Conditional Probabilities			
Prob(y_{ij}=1|y_{ij-1}=1)	0.250	0.210	0.145
Prob(y_{ij}=1|y_{ij-1}=0)	0.090	0.058	0.028
Raw data persistence due to y_{ij-1} (difference measure)[(ii)]	0.160	0.152	0.117
Raw data persistence due to y_{ij-1} (ratio measure)[(iii)]	2.78	3.62	5.18
Total number of women with more than one child ever born (% of all women)	22640 (75.6)	7021 (66.1)	3610 (60.7)
Age 15-30			
Incidence of infant death (%)	9.45	5.11	1.19
Conditional Probabilities			
Prob(y_{ij}=1|y_{ij-1}=1)	0.205	0.165	0.059
Prob(y_{ij}=1|y_{ij-1}=0)	0.078	0.039	0.011
Raw data persistence due to y_{ij-1} (difference measure)[(ii)]	0.127	0.126	0.048
Raw data persistence due to y_{ij-1} (ratio measure)[(iii)]	2.63	4.23	5.60
Total number of women with more than one child ever born (% of all women)	5649 (78.7)	1740 (53.2)	590 (82.5)
Age 31-40			
Incidence of infant death (%)	9.92	7.36	3.18
Conditional Probabilities			
Prob(y_{ij}=1|y_{ij-1}=1)	0.211	0.215	0.079
Prob(y_{ij}=1|y_{ij-1}=0)	0.084	0.058	0.030
Raw data persistence due to y_{ij-1} (difference measure)[(ii)]	0.127	0.157	0.049
Raw data persistence due to y_{ij-1} (ratio measure)[(iii)]	2.51	3.71	2.68
Total number of women with more than one child ever born (% of all women)	10034 (79.6)	2920 (69.1)	1446 (61.0)
Age 41-49			
Incidence of infant death (%)	14.13	8.85	4.26
Conditional Probabilities			
Prob(y_{ij}=1|y_{ij-1}=1)	0.320	0.234	0.209
Prob(y_{ij}=1|y_{ij-1}=0)	0.108	0.078	0.033
Raw data persistence due to y_{ij-1} (difference measure)[(ii)]	0.212	0.156	0.176
Raw data persistence due to y_{ij-1} (ratio measure)[(iii)]	2.96	3.00	6.33
Total number of women with more than one child ever born (% of all women)	6957 (82.8)	2361 (75.3)	1574 (69.2)

Notes: (i) y_{ij}=1 if child j in family i has died before the age of 12 months.
(ii) Raw data persistence due to y_{ij-1} is calculated as the difference in the two conditional probabilities – see text for further details.
(iii) Raw data persistence due to y_{ij-1} is calculated as the ratio of the two conditional probabilities – see text for further details.

Table 5
Descriptive Statistics

	INDIA		**UP**		**WB**		**KE**	
	mean	**s.d.**	**mean**	**s.d.**	**mean**	**s.d..**	**mean**	**s.d.**
Infant mortality	0.08	0.27	0.12	0.32	0.08	0.26	0.04	0.19
Infant mortality (sibling)	0.07	0.25	0.10	0.30	0.07	0.25	0.03	0.16
Female	0.48	0.50	0.47	0.50	0.49	0.50	0.48	0.50
Multiple birth	0.01	0.11	0.01	0.12	0.02	0.12	0.02	0.12
Birth order 1	0.30	0.46	0.24	0.43	0.34	0.47	0.39	0.49
Birth order 2	0.25	0.43	0.21	0.41	0.26	0.44	0.32	0.47
Birth order 3	0.18	0.39	0.17	0.38	0.17	0.37	0.16	0.37
Birth order 4	0.12	0.32	0.13	0.34	0.10	0.30	0.07	0.25
Birth order 5	0.07	0.26	0.09	0.29	0.06	0.23	0.03	0.17
Birth order >5	0.08	0.27	0.13	0.34	0.07	0.25	0.03	0.16
Hindu	0.76	0.43	0.82	0.38	0.73	0.45	0.47	0.50
Muslim	0.14	0.34	0.17	0.37	0.25	0.43	0.38	0.48
Other Religion	0.10	0.30	0.01	0.09	0.02	0.15	0.15	0.36
Scheduled caste	0.18	0.39	0.20	0.40	0.23	0.42	0.09	0.28
Scheduled tribe	0.13	0.34	0.02	0.14	0.06	0.23	0.01	0.10
Caste data missing	0.01	0.09	0.05	0.22	0.00	0.07	0.00	0.00
Ma educ missing	0.00	0.02	0.00	0.02	0.00	0.04	0.00	0.00
Ma no education	0.60	0.49	0.75	0.43	0.50	0.50	0.11	0.32
Ma incomp primary ed	0.10	0.30	0.05	0.21	0.20	0.40	0.20	0.40
Ma complete prim ed	0.07	0.26	0.08	0.27	0.05	0.22	0.09	0.28
Ma incomp sec ed	0.13	0.33	0.06	0.24	0.16	0.36	0.32	0.47
Ma secondary, higher	0.10	0.30	0.06	0.24	0.09	0.28	0.28	0.45
Pa educ missing	0.00	0.05	0.00	0.06	0.01	0.09	0.00	0.06
Pa no education	0.32	0.47	0.33	0.47	0.30	0.46	0.07	0.26
Pa incomp priPary ed	0.12	0.32	0.07	0.25	0.22	0.41	0.20	0.40
Pa complete prim ed	0.09	0.28	0.11	0.31	0.06	0.23	0.11	0.32
Pa incomp sec ed	0.22	0.41	0.19	0.40	0.22	0.41	0.33	0.47
Pa secondary ed	0.12	0.32	0.12	0.33	0.07	0.26	0.17	0.37
Pa higher ed	0.14	0.34	0.17	0.37	0.12	0.32	0.10	0.30
Age ma at 1st birth	18.56	3.27	18.01	2.99	18.08	3.24	20.25	3.54
Age ma at 1st birth, sq	355.17	132.54	333.28	116.92	337.47	129.43	422.54	154.66

Authors' calculations based on NFHS-2.

TABLE 6
Infant Mortality

	Uttar Pradesh		West Bengal		Kerala	
	Coef.	**z**	**Coef.**	**z**	**Coef.**	**z**
1(prev. child died)	**0.396**	**10.54**	**0.296**	**3.27**	**0.518**	**2.54**
1(girl)	-0.020	-0.98	-0.107	-2.6	-0.168	-2.35
muslim	-0.211	-5.63	-0.090	-1.21	-0.060	-0.51
other religion	-0.160	-1.01	0.034	0.16	0.142	0.99
scheduled caste	0.001	0.02	0.036	0.48	0.121	0.76
other caste	0.062	0.71	-0.060	-0.45	0.113	0.29
caste missing	-0.001	-0.01				
ma:ed<primary	-0.106	-1.64	-0.032	-0.43	-0.304	-2.14
ma:ed=primary	-0.127	-2.34	-0.393	-2.55	-0.149	-0.85
ma:ed<secondary	-0.121	-1.93	-0.474	-3.97	-0.322	-2.13
ma:ed=secondary+	-0.364	-4.57	-0.288	-1.47	-0.551	-2.72
pa:ed<primary	0.076	1.48	-0.055	-0.72	0.064	0.4
pa:ed=primary	-0.101	-2.25	0.044	0.36	-0.005	-0.03
pa:ed<secondary	-0.111	-2.92	-0.018	-0.21	-0.112	-0.64
pa:ed=secondary	-0.111	-2.41	-0.116	-0.81	-0.203	-0.89
pa:ed>secondary	-0.099	-2.12	-0.237	-1.43	-0.071	-0.27
ma age@birth	-0.117	-6.84	-0.197	-5.61	-0.132	-1.82
sq[ma age@birth]	0.002	4.98	0.003	4.81	0.003	1.81
multiple birth	1.075	14.36	1.263	9.3	0.983	4.69
birthorder2	-0.484	-4.75	0.033	0.12	-0.263	-0.52
birthorder3	-0.429	-4.26	0.195	0.71	-0.501	-1
birthorder4	-0.326	-3.27	0.230	0.84	-0.315	-0.63
birthorder5	-0.267	-2.68	0.256	0.93	-0.300	-0.58
birthorder6	-0.110	-1.17	0.195	0.73	0.091	0.19
Interactions with birthorder 1:						
muslim	0.102	1.51	0.039	0.35	0.084	0.49
other religion	0.053	0.2	-0.334	-1.02	-0.229	-1.01
scheduled caste	0.002	0.03	-0.079	-0.71	0.030	0.13
other caste	-0.002	-0.01	0.287	1.53	-4.341	-0.05
caste missing	0.129	1.29				
ma:ed<primary	0.133	1.25	-0.073	-0.63	0.211	0.89
ma:ed=primary	-0.100	-1.06	0.306	1.47	0.022	0.07
ma:ed<secondary	-0.014	-0.14	0.289	1.83	0.223	0.92
ma:ed=secondary+	-0.010	-0.08	-0.143	-0.55	0.126	0.39
pa:ed<primary	-0.241	-2.46	-0.110	-0.92	-0.064	-0.25
pa:ed=primary	0.166	2.11	-0.076	-0.4	-0.183	-0.61
pa:ed<secondary	0.012	0.18	-0.084	-0.66	-0.244	-0.88
pa:ed=secondary	0.035	0.44	0.016	0.08	-0.456	-1.24
pa:ed>secondary	-0.013	-0.16	0.092	0.41	-0.613	-1.43
ma age@birth	-0.046	-3.26	0.008	0.25	-0.027	-0.44
sq[ma age@birth]	0.001	2.72	0.000	0.3	0.001	0.62
multiple birth	0.271	1	-0.605	-1.71	0.478	0.85
constant	0.915	3.88	0.964	2.16	0.357	0.33
rho	**0.106**	**6.47**	**0.192**	**4.58**	**0.052**	**0.46**
theta	0.814	4.19	0.780	3.28	1.956	0.56
p0	-12.692	-0.1	-11.456	-0.19	-3.396	-0.23

Notes: Dependent variable: Probability of death in first 12 months of life. Prev=previous, ma=mother, pa=father, ed=education, sq=square. See Section 3 for definitions of rho, theta and p0 and for discussion of the ML estimator.

TABLE 7
Estimated State Dependence (Persistence): Difference[ii] [Ratio[iii]]

	Uttar Pradesh	West Bengal	Kerala
Raw data			
Persistence: difference measure [ratio measure]	0.160 [2.8]	0.152 [3.6]	0.117 [5.2]
Model with unobserved heterogeneity but no covariates[iv] - model has one mass point at - ∞			
Estimated Persistence: difference measure [ratio measure]	0.093 [2.2]	0.055 [2.4]	0.049 [3.4]
θ - see text (t-ratio for θ=0) [t-ratio for θ=1]	0.835 (4.76) [0.939]	0.771 (3.32) [0.987]	1.351 (1.34) [0.347]
Proportion of variance attributed to unobservables (standard error)	0.114 (0.02)	0.181 (0.038)	0.115 (0.069)
Percentage of **observed** persistence explained by the survival status of the previous child	58	36	42
Likelihood ratio test for $\sigma^2_\alpha = 0$ [p-value - see text for details)	84.77 [0.000]	37.38 [0.000]	6.01 [0.007]
Model with unobserved heterogeneity but no covariates[iv] - model has two mass points at + - ∞			
Estimated Persistence: difference measure [ratio measure]	0.093 [2.2]		
θ - see text (t-ratio for θ=0) [t-ratio for θ=1]	0.836 (4.60) [0.901]		
Proportion of variance attributed to unobservables (standard error)	0.111 (0.02)		
Percentage of **observed** persistence explained by the survival status of the previous child	58		
Likelihood ratio test for $\sigma^2_\alpha = 0$ [p-value - see text for details)	87.50 [0.000]		
Model with unobserved heterogeneity but WITH covariates - model has ONE mass points at - ∞			
Estimated Persistence: difference measure [ratio measure]	0.083 [2.0]	0.045 [2.0]	0.050 [3.0]
θ - see text (t-ratio for θ=0) [t-ratio for θ=1]	0.836 (4.25) [0.834]	0.881 (3.15) [0.429]	1.66 (0.68) [0.27]
Proportion of variance attributed to unobservables (standard error)	0.105 (0.02)	0.171 (0.04)	0.060 (0.08)
Percentage of **observed** persistence explained by the survival status of the previous child	52	30	43
Likelihood ratio test for $\sigma^2_\alpha = 0$ [p-value - see text for details)	65.40 [0.000]	30.70 [0.00]	1.61 [0.102]
Model WITHOUT unobserved heterogeneity			
Estimated Persistence: difference measure [ratio measure]	0.140 [2.5]	0.125 [3.1]	0.081 [3.8]
Percentage of **observed** persistence explained by the survival status of the previous child	87	82	69
Sample Size	29937	10627	5950

Notes: (i) y_{ij}=1 if child j in family i has died before the age of 12 months.
(ii) Raw data persistence due to yij-1 is calculated as the difference in the two conditional probabilities – see text for further details.
(iii) Raw data persistence due to yij-1 is calculated as the ratio of the two conditional probabilities – see text for further details.
(iv) The model has a common intercept, and dummy indicators for the first-born and the survival status of the previous child.

Appendix: Table 1 -
Background Information: India and Three States

	INDIA	UP	WB	KE
Rank in per capita income	n.a.	12	6	8
Growth rate	3.2	2.2	3.2	3
Poverty incidence	36.5	40.2	26	29.2
Drinking Water: pipe/hand pump	77.9	85.6	89.3	19.9
Toilet facility	36.0	26.7	45.1	85.2
Electricity	60.1	36.6	36.7	71.8
Female literacy rate, 6+	51.4	42.7	57.4	85.1
Mother's literacy rate, 15-49	39.6	24.5	50.0	88.5
Total fertility rate	2.85	3.99	2.29	1.96
Contraceptive use	48.2	28.1	66.6	63.7
Breastfed only, 0-3 months	55.2	56.9	48.8	68.5
Breastfed & solid/mush food, 6-9m	33.5	17.3	46.3	72.9
Women with low BMI	35.8	35.8	43.7	18.7
Population share	100	17.1	7.91	3.2
Population in millions	1002.1	171.5	79.3	32.4
Sample size	248785	29937	10627	5950

Data Sources and Notes:

Except for income-rank, population size and sample size, all figures are percentages.*From World Bank (2000):* Poverty incidence in 1994 (based on Datt (1997), (1999)); Rank of states in per capita income in 1996-97; Growth rate of economy (per cent) during the period 1991-2 to 1996-7. (Rank and growth rate calculations use the 1980/81-based GDP series). *All other data are from the NFHS-2 Fact Sheets in the NFHS-2 final report (2000),* with the exception of mother's literacy and population share which are the authors' calculations from the NFHS-2 data. The water, toilet and electricity data refer to the percent of households in the sample that have these facilities. Total fertility rate (TFR) is based on data for 1996-98. This is the number of children a woman would bear during her reproductive years if she were to experience the age-specific fertility rates prevailing at the time of the survey. Contraceptive use is the percent of currently married women aged 15-49 using any contraceptive method. "Breastfed only" is the percent of children aged 0-3 months who were exclusively breastfed. "Breastfed & solid/mushy food" refers to the percent of children aged 6-9 months. "Women with low BMI" is the percent of women with body mass index less than 18.5kg/m^2. Low BMI is an indicator of poor health. Population is as recorded by the Registrar-General's Office of the 2000 Census on 1 July 2000.