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Fatal Fluctuations?
Cyclicality in Infant Mortality in India

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ABSTRACT

Fatal Fluctuations? Cyclicality in Infant Mortality in India*

This paper investigates the impact of macroeconomic shocks on infant mortality in India and investigates likely mechanisms. A recent OECD-dominated literature shows that mortality at most ages is pro-cyclical but similar analyses for poorer countries are scarce, and both income risk and mortality risk are greater in poor countries. This paper uses individual data on infant mortality for about 150000 children born in 1970-1997, merged by birth-cohort with a state panel containing information on aggregate income. Identification rests upon comparing the effects of annual deviations in income from trend on the mortality risks of children born at different times to the same mother, conditional upon a number of state-time varying covariates including rainshocks. I cannot reject the null that income shocks have no effect on mortality in urban households, but I find that rural infant mortality is counter-cyclical, the elasticity being about -0.46. This is despite the possibility that relatively high risk women avert birth or suffer fetal loss in recessions. It seems related to the fact that women's participation in the (informal) labour market increases in recessions, presumably, to compensate a decline in their husband's wages. Consistent with this but, in contrast to results for richer countries, antenatal and postnatal health-care decline in recessions. These effects are reinforced by pro-cyclicality in state health and development expenditure. Another interesting finding that is informative about the underlying mechanisms is that the effect of aggregate income on rural mortality is driven by non-agricultural income.

JEL Classification: I12, J10, O49

Keywords: infant mortality, income volatility, business cycles, India, health care, maternal labour supply, public expenditure

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Fatal Fluctuations? -
Cyclicality in Infant Mortality in India

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1. Introduction

1.1. Motivation and Summary

Infant mortality is a commonly used indicator of welfare in poor countries, where 30% of all deaths occur in childhood as against 1% in richer countries (Cutler et al. 2006). Mortality is sensitive to economic fluctuations and especially so in childhood (e.g. van der Berg et al. 2006). Studying the effects of income fluctuations is empirically pertinent. Income growth in poor countries is more volatile than in richer countries, being subject not only to episodic crises but also to cyclical fluctuations that tend to be larger and more abrupt (e.g. Pritchett 2000, Koren and Tenreyro 2007). There is limited evidence on the welfare costs of income volatility in developing countries (Pallage and Robe 2003). These welfare costs are likely to be high for two reasons. First, macroeconomic shocks in poor countries often affect the level of social expenditure (e.g. Frankenberg, Thomas and Beegle 1999, Lustig 1999), so that there is limited insurance from the state. Second, a weaker financial infrastructure makes it difficult to borrow to smooth fluctuations in private incomes (e.g. Koren and Tenreyo 2007). As a result, households resort to other, often more costly sources of insurance (e.g. Morduch 1995). Of particular interest here are adjustments induced by macroeconomic shocks to, for example, fertility or labour supply, which may impact upon the survival of children.

This paper investigates the effects of aggregate income fluctuations on infant mortality in India, contributing to a recent debate on whether mortality is pro-cyclical or counter-cyclical. It uses individual data on infant mortality for about 152000 children born in 1970-1997 to some 50000 mothers, which are merged by birth cohort with a state panel containing information on aggregate income. Identification rests upon comparing the effects of deviations in income from trend on the mortality risks of children born at different times to the same mother. The micro-panel is exploited to control for selectivity in the composition of births and the state panel is exploited to control for the potentially confounding effects of omitted trends, such as in medical technical progress. To allow for state-time varying shocks that may be correlated with both income and mortality, I condition upon other state-level covariates including rainfall shocks and on the child's birth-month (see Section 4). The paper also offers interesting new evidence on a number of potential mechanisms. Using household-level data, it shows how maternal health-seeking and labour supply react to swings in the economy. It also shows how the composition of births varies across the business cycle. Using state-level panel data, it further investigates the extent to which income effects work through changing public expenditure and inflation rather than simply the incomes of poorer households, amongst whom mortality risk is concentrated.

The question of mechanisms is of particular interest in view of a recent OECD-dominated literature that supports the provocative result that mortality at most ages, including infancy, is pro-cyclical.[1,2] A behavioural explanation offered to support this result is that, in recessions, when the opportunity cost of time is low, people (mothers in the case of infant mortality) substitute their time away from the labour market and towards health-preserving activities, and this substitution effect overwhelms the income effect, so that the net effect on health is positive. This outcome may be reinforced by selection into the sample of births. In particular, if adverse shocks induce women to defer fertility, and this effect is stronger amongst women with inherently higher risks of infant mortality, then the composition of births in a recession will be selectively low-risk (see Dehejia and Lleras-Muney 2004). There is little evidence of the scope of these mechanisms in a low-income setting where, as discussed, neither markets nor states provide much insurance against shocks (see section 1.2).

This paper offers some evidence on both the behavioural and the selection effects. A few earlier studies have investigated maternal health seeking in response to macroeconomic shocks, but with mixed results. Using American and Colombian data respectively, Dehejia and Lleras-Muney (2004) and Miller and Urdinola (2007) find that more healthcare is sought in recessions. In contrast, Paxson and Schady (2005) find that, in Peru, less healthcare was sought in the wake of a macroeconomic

[1] See Ruhm 2000, Dehejia and Lleras-Muney 2004, Gerdtham and Ruhm 2004, although see Adda et al. 2007, Halliday 2007 for some contention

[2] The literature on OECD countries uses the term "recessions" to refer to deviations of the unemployment rate from trend (e.g. Ruhm 2000). This paper refers to recessions as deviations of aggregate income from trend. Given the informal nature of labour markets in poor countries and survival constraints on unemployment amongst the poor, aggregate income fluctuations are not as clearly mirrored in unemployment as in richer countries.

collapse. No previous study in this literature has investigated cyclicality in maternal labour supply. Instead, it is assumed that women work less in recessions. This paper argues that poor families may seek to insure themselves by *increasing* women's labour supply in recessions, in which case they will have *less* time to spend on antenatal and postnatal care, and infant mortality is then more likely to be counter-cyclical. Not many studies have taken account of fertility selection in modeling infant mortality. The closest relative of the current study is Dehejia and Lleras-Muney (2004), who raise the issue of fertility selection in analysing infant mortality, and estimate a mother fixed effects model for their related analysis of the effects of the business cycle on prenatal care and on birth-weight. Although they do not directly use a fixed effects approach, Paxson and Schady (2005) and Miller and Urdinola (2007) discuss sensitivity of their results to possible selection. Dehejia and Lleras-Muney (2004) find that, in the US, high risk women are more likely to avert birth in a recession while, using Peruvian data, Paxson and Schady (2005) find the opposite.

The birth selection mechanism suggested in the literature so far refers to conscious deferral of fertility, and will apply in India only to the (limited) extent that Indian women use birth-spacing methods (see section 3). Here I suggest an alternative mechanism that is especially pertinent in an environment where maternal health is poor, namely that high-risk women are relatively likely to suffer spontaneous abortion and stillbirth in the wake of adverse income shocks. This will generate endogeneity in the composition of live births, with low-risk births over-represented in recessions. This mechanism has been ignored in the literature even though available data are typically on live births, producing the same problem at birth as at later ages of having to condition upon survival until the start of the exposure period.

The main findings are as follows. For urban households, the effect of income shocks on infant mortality is poorly determined so that, while we cannot reject the null of no effect, the evidence is in fact ambiguous. In rural households, income shocks have a significant negative effect on infant mortality that is robust to a number of specification checks. The estimates imply that a negative income shock of median size will raise mortality by 0.2 percentage points, which is two-thirds of the annual linear rate of decline in rural mortality in India over the period.

Exploring the extent to which the results for rural India might be driven by (endogenous) heterogeneity in the inflow of births, I find some evidence that higher-risk women are more likely than others to defer birth in a recession. In this way, they act to avert infant mortality. Controlling for mother level unobservables does not significantly alter the income effect on average, suggesting that there is a causal effect of income on mortality. However, controlling for unobservables does matter once the sample is split. In particular, differences in the income effect by caste and religion appear to be driven by selection.

To investigate routes through which a causal effect may operate, I looked at variation across the cycle in health-seeking behaviours that are known to be important determinants of infant mortality in poor countries. I find that delivery outside the home, antenatal care, child vaccinations, and the chances of treatment for a child's diarrhea, cough or fever improve in economic upturns. These findings are consistent with changes in both the supply and the demand for health services. To investigate this further, I investigated cyclicality in maternal labour supply, which will influence demand, and cyclicality in state social expenditure, which will influence supply. I now summarise these results in turn.

The analysis indicates that rural women are, on average, *more* likely to work in downturns, especially in the informal sector.[3] There is an interesting contrast here with results for richer countries. What appears to be common between, say the results for American mothers reported in Dehejia and Lleras-Muney (2004) and these results for rural India, is that the demand curve for health slopes downwards, with the relative price of health varying across the business cycle as the opportunity cost of maternal time changes. The difference is that, for American mothers, the opportunity cost of maternal time is assumed to be lower in recessions while, for rural Indian women, we find that it is (on average) higher in recessions. These results suggest that maternal labour supply is used as an insurance mechanism (as in Kochar 1995), but that this imposes a cost in terms of the health of children (as in Artadi 2005).

In contrast with the conventional wisdom that fiscal policy is counter-cyclical, being used to smooth the effects of fluctuations (e.g. Lane 2003), I find that deterioration in state income in India is associated with unfavorable changes in public expenditure and inflation; see Woo 2005, who argues this is typical of developing countries. I show that decreases in state health and development expenditure and increases in inflation are associated with increased infant mortality, thereby establishing a mechanism at the state level. However, conditioning upon these and

[3] Increased labour supply is consistent with a slackening of labour demand in recessions if the increased participation is in the informal sector or in self-employment.

other potential mediator variables does not eliminate the impact of income on mortality. Another interesting result is that the effect of aggregate income changes on rural mortality is driven by non-agricultural income. To the extent that public expenditure is a mechanism linking income and mortality, this is consistent with the fact that agricultural income in India is not taxed. However, since growth in the non-agricultural sector can well translate into growth in the incomes of rural households, it is also consistent with mechanisms that work through household incomes. What this result does suggest is that rural mortality is not very sensitive to foodgrain production.

The paper is organized as follows. The rest of this section summarises the relevant literature and delineates the contributions of this paper. Section 2 describes the data and section 3 sets out the empirical strategy. Descriptive statistics are presented in section 4. Results and robustness checks are presented in section 5. Section 6 analyses mechanisms at the household and state levels, and section 7 concludes.

1.2. Related Literature

There is a vast literature on the relationship between income and health but recent surveys argue that it is inconclusive (see Cutler et al. 2006, Deaton 2006a, Fuchs 2004). For example, Deaton (2006a) argues that, although income growth appears to have contributed to poverty reduction, its effects on health are more uncertain. The ambiguity in the literature is partly genuine, reflecting the fact that the determinants of health are not as straightforward as the determinants of poverty and that they might be quite context-specific. However it also reflects varying degrees of success in dealing with specification issues. First, there are identification problems involving potential feedback from health to income through productivity effects, the presence of unobservables that influence both health and income, and endogenous selection into birth or up to a certain age (e.g. Smith 1999, Adda et al. 2006). Second, results vary according to the level of aggregation (as highlighted, for example, by Deaton and Paxson 2004). Third, fluctuations may have different effects from secular income growth, for instance because substitution effects are neutralized in the long run (Ruhm 2000) but the effects of secular growth are statistically difficult to disentangle from other trends, such as in scientific progress or information dissemination and, in the long run, these other processes are causally related to income change (see Cutler et al. 2006). The empirical strategy used in this paper avoids the common identification problems noted in the literature. It focuses on the effects of aggregate rather than individual income shocks. These will include the effects not only of changes in average incomes, but also of changes in, for example, the distribution of income, inflation, and the supply of public services. It identifies the income effect from annual fluctuations and, in this way, purges the effects of trended unobservables.

Research on the effects of macroeconomic shocks on infant mortality in poor countries is fairly limited. A tranche of the literature has studied the effects on infant mortality of episodic macroeconomic crises. Some find an adverse effect and others find no effect; these results are summarized in Paxson and Schady (2005). For example, looking at the macroeconomic collapse of 1988 in Peru, Paxson and Schady find that a fall in GDP of about 30% was associated with a rise in infant mortality of about 2.5 percentage points. They show that public health expenditure fell by about 58%, its share falling from 4.3% to 3%, and that mothers were less likely to use healthcare in pregnancy and childbirth during the crisis. In contrast, Miller and Urdinola (2007) find that improvements in income driven by exogenous changes in coffee prices raise infant mortality and lower health investments in Colombia. They exploit three supply shocks that each altered prices by as much as 50% of the long run mean. Some other researchers have similarly found positive associations between GDP growth and infant or adult mortality in Chile, Brazil and Argentina (Ortega and Reher 1997, Rios and Carvalho 1997, Abdala et al. 2000). These contrasting results establish the pertinence of the current analysis.

A distinction of the current analysis from the studies of crises is that it considers the average effect of much smaller annual fluctuations in income (about 3% p.a.). Although crises offer quasi-experimental conditions for analysis, it is unclear that we can generalize from them. In particular, it is unclear that similar effects would arise in response to smaller shocks, or economic cycles.[4] In general, smaller negative shocks will tend to generate smaller adverse effects but, at the same time, they tend to stimulate less of a reaction amongst governments and donors, so that their net impact may be more or less adverse.[5]

[4] Adda et al. 2006 (p.20) also observe that the effects of big changes in income may be different from the effects of smaller changes.

[5] Interventions following crises are of a more specific one-off nature, easier to finance, and more likely to be consistent with political incentives, given heightened media activity around

A recent literature on OECD countries- cited in section 1.1- has focused on the health effects of economic cycles, defined as annual fluctuations in the unemployment rate (see Ruhm 2005 for a review). But there are few similar studies of the health effects of cyclicality in developing countries. In their landmark paper, Pritchett and Summers (1996) show, using quinquennial data[6] for 58 developing countries in 1960-85, that growth in GDP is associated with proportional declines in mortality. But, using annual panel data for the Indian states in 1980-99, Deolalikar (2005, chapter 2) finds that there is no significant relationship between income and infant mortality once a linear trend is included in the model. Using microdata for 1994-98, the same study finds a positive association of infant mortality with state income. Palloni and Hill (1997) show that short-term mortality responses to recessions in the second half of the 20th century in nine Latin American countries were erratic, and statistically insignificant. So, overall, the available evidence is inconclusive, and evidence on underlying mechanisms is scarce.

2. The Data

India provides an appropriate setting for this analysis. More children die in India than anywhere else in the world, the under-5 death toll in 2000 being estimated at 2.4 million p.a., which is a quarter of all child deaths (Black et al. 2003). This is a reflection of both the size and poverty of India, which has one in six of the world's people, and one in three of the world's poor. It is a democracy with a federal political structure and health is a state subject.

The micro-data are constructed from the second round of the National Family Health Survey of India (NFHS-2)[7]. This recorded complete fertility histories for ever-married women aged 15-49 in 1998-99, including the time and incidence of child deaths. Individual mortality data are thus available for cohorts of children (implicitly) followed over time from birth. Children in the sample are born in 1961-1999. These micro-data are merged by state and year of birth with a panel of data on real net state

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a crisis, and the relative ease with which the impact of an intervention can be observed (see Sen 1981, Besley and Burgess 2002).

[6] The quinquennial data may smooth out some genuine fluctuations in mortality

[7] For details on sampling strategy and context, see IIPS and ORC Macro (2000).

domestic product per capita (henceforth *income*) and other relevant statistics for the major Indian states.[8]

To allow every child full exposure to infant mortality risk, children born in the twelve months before the survey date are dropped. I also drop mothers that have ever had a multiple birth since death risk for multiple births is known to greatly exceed that for singletons and, when we are comparing siblings, it is cleaner to compare singletons. Since births in the 1960s are relatively scarce, the sample analysed is restricted to births occurring 1970 onwards. This truncation of the data also limits recall and selectivity issues discussed in section 5.2. Another issue with retrospective data is that we need to account for any inter-state migration. The survey records where the mother lives at the time of interview rather than at the time of each birth, but we need to know which state each birth was in if we want to match state income at birth to the risk of dying before the age of one. Exploiting a question in the survey that asks the mother how long she has lived in her current place of residence (village, city or town), I have limited the sample to births that occurred in her current location. Applying this rather strict criterion, I retained 86% of births. The reported results use the restricted sample, but since migration may be correlated with health, I also confirm that failing to control for migration does not make a significant difference to the results. This is possibly because inter-state migration in India is small (e.g. Topalova 2005). The sample analysed contains 117088 rural children of 36068 mothers (average of 3.3 per mother) and 35783 urban children of 13414 mothers (average of 2.7 per mother) born during 1970-97 in one of the 15 major states.

Previous cross-country studies have been disadvantaged by the fact that international statistics on infant mortality in developing countries are unreliable, with different UN sources providing different estimates, and there are issues of comparability of both mortality and GDP data across countries; see Pritchett and Summers (1996) and Ross (2006) for a discussion of the international data. Also, the fact that these data are only available at five-yearly intervals limits their usefulness in studying the effects of income volatility. The NFHS is one of a family of about 200

[8] These data were assembled by Ozler, Datt and Ravallion (1996) and then extended by Besley and Burgess (2002, 2004), who were kind enough to supply me with their database. Detailed definitions of the state-level variables used in these analyses can be found at http://sticerd.lse.ac.uk/eopp/research/indian.asp and in the Appendices of the cited papers. The state health expenditure and rainfall data that I use were collected by Juan Pedro Schmid, to whom I am grateful for their use. The definitions of all variables used in the analysis are in Appendix Table 1.

Demographic and Health Surveys (DHS) conducted in some 75 developing and transition economies (see www.measuredhs.com). This paper highlights the enormous and little-exploited potential of these data in generating annual (in fact monthly) mortality statistics over long periods of time that are comparable across countries.[9]

3. Empirical Strategy

As explained in sections 1 and 2, the data are a micro-panel data of about 50000 mothers with, on average, about three children per mother and this is nested in a state-level panel consisting of 15 states and 29 years. The estimated equation is

$$(1)\ M_{imst}=\alpha_m + \eta_t + \alpha_s.t + \beta\ lnY_{st} + \theta' X_{st} + \lambda' Z_{imst} + \eta_s' R_{st}^f + \gamma_s' R_{st}^d + \varepsilon_{imst}$$

M is a dummy that indicates whether index child i of mother m born in year t in state s died by the age of 12 months and lnY is the logarithm of real per capita state net domestic product (henceforth *income*). β is the parameter of interest and it measures the change in infant mortality associated with a 100% change in income. To avoid clutter, I do not show dynamics or interaction (and quadratic) terms, though these are investigated, and discussed in the Results section. α_m and η_t are mother and birth year (or cohort) fixed effects and $\alpha_s.t$ are state-specific trends; Figures 1 and 2 suggest that it is restrictive to impose common trends across states. Since, by construction (see section 2), mothers do not migrate between states, the mother fixed effect incorporates a state fixed effect. Z is a vector of child-specific controls and X is a vector of state-level controls. R^f and R^d are vectors of positive and negative state-specific rainfall shocks the coefficients of which are allowed to vary by state; superscripts f and d denote "flood" and "drought" respectively.

Equation (1) is estimated using the linear probability model since fixed effects probit estimates are inconsistent in short panels and the relevant panel in this case is the micro-panel, where T is the number of children per mother. Standard errors are robust to arbitrary forms of heteroskedasticity and clustered at the state-level to allow for serial correlation within states (e.g. Bertrand et al. 2004). Since exposure to the risk of infant mortality starts at birth, we avoid the problem faced by studies that look

[9] Two recent studies have exploited these data: see Paxson and Schady (2005), discussed in section 1, and Kudamatsu (2006), who studies the effect of democratization on infant mortality in sub-Saharan Africa.

at age-specific mortality of older groups of selection on survival up until the age in consideration. However, as the data record only live births, we are forced to condition upon survival until birth. This is not innocuous if the composition of live births varies with the cycle.[10] In a departure from previous work, this paper controls for endogenous selection into the sample of births by using mother fixed effects**.** The fixed effects capture unobserved heterogeneity in fertility responses to the business cycle as well as in the risk of recession-induced fetal loss. The fixed effects estimates are compared with estimates that instead condition only upon observable characteristics of mothers. This indicates the extent to which the income coefficient is biased by neglect of selection on unobservables.

The state fixed effects that are implicit in the mother fixed effects control for initial conditions and for persistent elements of history, climate, culture (e.g. status of women) and political institutions (e.g. public service delivery, corruption) that may simultaneously affect mortality and income. They also control for state-specific time-invariant sources of measurement error. The year fixed effects control for aggregate time-variation associated with, for example, secular improvements in health technology, or episodic shocks like famines, floods and epidemics. State-specific trends are included to allow for state-specific evolution of health technology, and trended omitted variables at the state level.[11] In this way, the state panel is exploited to disentangle the effects of income from the effects of trended unobservables (e.g. Deaton and Paxson 2004, Cutler et al. 2006, Ruhm 2000). Identification of a causal effect of income on mortality relies upon there being independent macroeconomic fluctuations within the states around a linear trend. The relatively long time dimension of the data makes it more likely that this is the case. In the income panel, the standard deviation of the within-state variation is almost identical to that of the between-state variation.

Much of the literature has focused on identification problems relating to reverse causality, since the health of adults may be expected to influence their income through productivity (see Adda et al. 2007, Halliday 2007, Smith 1999). Here, this

[10] This problem of endogenous composition of inflow is analytically similar to that involved in modeling the effects of the business cycle on individual probabilities of exit from unemployment allowing for the possibility that the cycle also affects the composition of the inflow into unemployment (see van der Berg and van der Klaauw 2001).

[11] An advantage of the panel being sub-national is that it is less likely than with cross-country panels that there are state-specific health technology shocks.

problem is limited by looking at child rather than adult health and by analysing individual risk as a function of aggregate income. Conditioning upon fixed effects and state-specific covariates (X_{st}) further mitigates potential endogeneity problems. But there remains the possibility that state-specific shocks induce a non-causal relationship between mortality and income. The most likely such shocks in developing countries are weather shocks. For example, rainfall variation will directly affect income through agricultural production and may also directly affect health through altering the disease environment. The analysis therefore controls for rainshocks using a very flexible formulation (see below). Since almost two-thirds of infant deaths occur in the first month of life (neonatally), I also control for month of birth in order to capture other seasonal variation, such as in temperature. Previous studies in this domain have tended to ignore the role of climate.

To further limit any contemporaneous feedback, I investigate replacing current income with its lags. There are also substantive reasons to generalize the model by introducing lags. For example, there may be state dependence in mortality within families (Arulampalam and Bhalotra 2007, Bhalotra and van Soest 2007): controlling for mother-level unobserved heterogeneity, these studies identify a causal effect of a preceding sibling's death on the risk of death of the index child. This implies that a negative income shock that killed a sibling, say, three years ago, can raise the mortality risk of the index child. Alternatively, if income changes impact mortality through public expenditure, it may take longer than a year for these changes to reach the ground. The long panel used here allows me to explore dynamics of the income-health relationship which, so far, have been little investigated (although see Adda et al. 2006, Halliday 2007).

Rainfall shocks are measured as the absolute deviation of rainfall in each state-year from the 30-year state mean. I define a positive shock (R_{st}^{f}) as equal to this deviation when the deviation is positive, and equal to zero otherwise. A negative shock (R_{st}^{d}) is symmetrically defined. To allow their effects to be different in different states, each of these variables is interacted with the fifteen state dummies, so that the regressions include thirty shock variables. The richness of this specification is justified by the results. When rainshocks are restricted to have common effects across states, they are insignificant but once state-specific coefficients are allowed, they are jointly significant at 1%. The results also show that it is restrictive to force positive and negative deviations to have the same effect.[12]

The child-specific variables, Z_{imst}, are dummies for gender, birth-order, birth-month and age of mother at the birth of the index child. These characteristics have been shown to be significant predictors of mortality risk in a number of previous studies, and also on these data (e.g. Bhalotra and van Soest 2007). The vector of state-level variables (X_{st}) includes population, the ratio of the rural to the total population, income inequality, poverty, the ratio of agricultural to non-agricultural income in the state, inflation in consumer prices, state health, education and development expenditure and newspaper circulation per capita (described in Appendix Table 1).

The empirical specifications used to estimate the effects of macroeconomic shocks on household behaviour (health seeking and maternal labour supply), and the models estimated to explore mediation by state level variables are described in section 6.

4. Descriptive Statistics

Figure 1 plots state-specific trends in infant mortality rates derived from the micro-data using sample weights. Averaging over 1970-1998, the infant mortality rate in rural India was 9.4% and it decreased at 3.15% p.a. In urban India, the corresponding figures are 5.9% and 1.77% p.a. The median of the yearly change in infant mortality at the state level was -0.33% in rural and -0.23% in urban areas. However, only in 52% (rural) and 51% (urban) state-year observations is the change negative.[13] There were vast differences in level and rates of change across the states. Although these have narrowed over time, Kerala achieved the fastest reduction even though its initial mortality was lowest, and its income growth was about average.[14]

[12] A natural alternative to using absolute deviations is to use the z-score of rainfall which normalises deviations with respect to the standard deviation in the state. The specification used here allows a big deviation in rainfall to impact infant mortality as much in a state that often experiences rainfall fluctuations as it would in a state with a more stable weather pattern. This seems to me a better specification, but I have confirmed that using z-scores does not alter the main results of this analysis.

[13] The median change when positive (negative) is 2.2% (-2.6%) in rural areas and 2.6% and (-2.8%) in urban areas.

[14] Of course a given reduction will appear as a larger percentage reduction when the initial level of mortality is lower. But the case of Kerala is striking even in absolute terms.

State trends in income are in Figure 2. The average annual growth rate of state income over the period 1970-97 is 2.98% p.a., and most states exhibit an acceleration after about the early-1980s, at which time an economic liberalization program was being phased in. State incomes are volatile. A regression of the log of state income on a linear trend in a pooled regression has an R-square of just 0.41, and the standard deviation of the residual is 0.30. This is typical of developing countries, the R-square for most industrialized countries being much higher (Pritchett 2000). The annual change in log income is positive in only 65% of state-year cells. In these cases, the median size of the change is 6.2%, and the change at the 25th and 75th percentiles is 3.2% and 10.6% respectively. When negative, the median change is -4.4%, the 25th and 75th percentiles being -9.1% and -1.7% respectively.[15]

Figures 3-5 explore the relationship between infant mortality and income.[16] Figure 3 plots the scatter of state-level data and displays a linear fit by state. The slope is negative in most cases. Aggregating these data to the country level with population weights, I find that, while the trends in the two variables are clearly inversely related (Figure 4a), the relationship of the deviations from trend is weaker (Figure 4b).[17,18] So far, I have used the level of mortality rather than its log. In Figure 4c, I display the relationship of the trend-deviations in mortality and income for the case where mortality is logged. The proportional deviations are larger and the relationship with income looks stronger than in Figure 4b.

This is pertinent to the discussion in Deaton (2006a). He shows, with a cross-country panel, that the negative relationship between income growth and mortality reduction is weaker for absolute as compared with proportional changes in mortality. He observes that the relationship often estimated as significantly negative, as, for example, in Pritchett and Summers (1996), involves proportional changes in

[15] State NDP (income) data is subject to measurement error. In the econometric analysis, time-invariant measurement error specific to the state and arising, for example, on account of different accounting conventions, is captured by the state fixed effects.

[16] Graphs that distinguish rural and urban mortality are available on request. The all-India mortality graphs look very similar to the rural graphs since the rural sample is 73% of all births.

[17] Of course, no correlation does not imply no causation any more than correlation implies causation (Deaton 2006b).

[18] If I plot the detrended variables by state, I find that the relationship is negative in only 7 of the 15 states (these figures are available on request). I also computed the rank correlation coefficient for the ranking of states by income growth and mortality reduction. This is weak and examination of the data show many deviations from the expected inverse ranking..

mortality.[19] The dependent variable in the model I estimate is the individual risk of infant mortality, which aggregated to the state level (in a linear probability model) produces the level (not log) of infant mortality. This suggests I am less likely to find a significant relationship between income and mortality but that, if I do, it is more likely to be causal.

I also investigated the relationship by decade. Over time, as mortality levels fall, the slope is attenuated and the curve shifts down, consistent with a secular decline in mortality arising, for example, from medical technical progress (figures available on request).[20] Although this is not exploited in the analysis, Figure 5 shows the between-state variation. The fitted line is negatively sloped but the data are quite dispersed around it.

In section 1, we argued that women may consciously defer birth in recessions. This requires that they have some control over the timing of their births. Only 33% of women interviewed in rural India in 1992/3 reported current use of a modern contraceptive method, 63% reporting no method and the rest a traditional method. Amongst urban women, 44.6% used modern methods and 48.6% used no method. Average use in the period 1970-98 will have been lower.[21]

5. Results

The model was estimated for all-India, and then, separately, for rural and urban households (Tables 1, 2). There is a significant negative effect of income shocks on mortality in rural households but, conditional upon year dummies, no significant effect in the urban sample. The urban coefficient is not small and it is insignificantly different from the rural coefficient, but it is poorly determined. The rural sample is bigger (it contains 73% of births) and, in rural areas, family incomes are lower and more volatile, and both markets and public services are relatively scarce and unevenly distributed. The discussion that follows pertains to the rural sample.

[19] He argues that if income growth has no influence on absolute changes then it follows that the relationship is really between income growth and the level of mortality and, in this case, it is hard to argue for a causal effect of income on mortality.

[20] A role for technical progress is also suggested by convergence of mortality levels across the states over time (Figure 1).

[21] These are author's tabulations using NFHS1 conducted in 1992/3 since these seem more relevant to the entire period than figures from NFHS2 which refer to the last year of the sample, 1998/9.

Here, I focus on the income effect; covariate effects for the baseline model (Table 2, column 1) are reported in Appendix Table 2.

5.1. The Baseline Model

Refer Table 1. The unconditional marginal effect of aggregate income on infant mortality risk is a significant –0.042 (column 1).[22] Controlling for state fixed effects, this rises to -0.084 (column 2) and, controlling for mother fixed effects, it rises further to -0.15 (column 3). So, between-state and between-mother heterogeneity tend to obscure the underlying relationship. We are now comparing children of the same mother who, in their first year of life, were exposed to different economic conditions. This eliminates the potential concern that the income effect is simply a compositional effect driven by higher-risk women selecting or being selected into birth in a recession. Indeed, as discussed in section 1, the estimates in columns 1-3 are consistent with our hunch that births in recessions are selectively low-risk, whether because of intentional deferral of fertility or because of selective miscarriage or stillbirth.

Mother fixed effects will, of course, control comprehensively for relevant unobservables, including genetic traits, tastes, maternal ability, household environment, and also for time-invariant observed traits including the education of the mother and her spouse (if permanent), her caste, height, religion and permanent income. This is relevant because some previous studies have found that controlling for education diminishes any protective effect of income on health (e.g., Deaton and Paxson 2001, Fuchs 2004). Indeed, Deaton and Paxson state that "one of the issues that remains to be settled is whether health differences are caused by income, or by factors correlated with income, such as education". The results in columns 1-3 show that there is an income effect conditional upon education. Note that education is not time-invariant because it varies with cohort.[23]

Once year dummies are included, the income effect falls to -0.06 (column 4) and, if I further condition upon state-specific trends, it falls to -0.034 (column 5). This is consistent with the mortality-reducing influence of secular trends in medical

[22] It is notable that if I do not cluster standard errors at the state-level, they are larger by about 30%.

[23] Below, I report results of replacing mother fixed effects with observed maternal characteristics.There are large and significant effects of maternal (and paternal) education on infant mortality, but conditioning upon education does not diminish the income effect.

technical progress, the importance of which has been emphasized in recent studies (e.g. Cutler et al. 2006). The mother and year fixed effects and the state-specific trends are all jointly significant at the 1% level.

Controlling for child characteristics (Z: gender, birth-order, birth-month and maternal age at birth) makes no difference to the income effect (column 6). Since about two-thirds of infant deaths occur in the first month of life (neonatal), controlling for birth-month is a way of controlling for seasonality in income as well as in the disease environment. I find that November-borns are significantly less likely to die and the (eleven) birth-month dummies are jointly highly significant. As discussed in section 2, conditioning upon mother's age at birth is important to controlling for age-determined selection into the sample of mothers in these data. But, while maternal age is significant, including it does not alter the income coefficient. Since age-selectivity increases as we go further back in time, it is also reassuring that the marginal effect of income is insignificantly different if the sample is restricted to start in 1980 rather than in 1970 (Table 2, column 2). A likely reason for this robustness is that Indian mothers give birth relatively young. So the truncation in our data of mothers who are relatively old at the time of birth, induced by an upper limit to the age of the mother at the time of the interview, is not so important (also see the Appendix).

It is striking that controlling for rainfall shocks makes no significant difference to the income effect which remains at -0.035 (column 7). Both positive and negative rainfall shocks impact mortality, each set of state-specific coefficients being jointly significant at the 1% level. The signs of these effects vary by state but, mostly, excess rainfall reduces mortality and dry conditions increase it.

A possible concern is that, at a given level of aggregate income, an increase in fertility lowers per capita income and, given the positive association of fertility and mortality (e.g. Bhalotra and van Soest 2007), this shows up as a negative impact of income on mortality. Although fertility trends will be captured by the time effects in the model, to control for any state-specific fertility shocks, I included the logarithm of the state population in the model. This is insignificant, and the income elasticity rises to -0.044 (Table 2, column 1). I also explored the alternatives of conditioning upon the state population under the age of 15 as this will proxy fertility better, and upon the number of births per mother recorded in the data, but in neither case was there a significant change in the income effect. Another possibility is that infant mortality raises the share of the working age population and, in this way, raises productivity and income. Failing to control for this, we will be less likely to see a negative effect of income on mortality. Although this mechanism seems less important in analysis of fluctuations than trends, I investigated it by including the share of the working age population in the model. The income coefficient was unaffected. The model in column 1, Table 2 therefore remains the baseline model.

The marginal effect in the baseline model is -0.044. Since the average infant mortality rate in the rural sample is 0.094, the elasticity at the mean is -0.46. So average annual income growth, which is about 3% p.a. in the sample period, reduces infant mortality by 1.3% of the sample mean. But this is an average over positive and negative shocks. The median size of an income shock, when it is negative, is 4.4%. The estimates imply that an income shock of this size will raise mortality by 0.2 percentage points. This is two-thirds of the annual linear rate of decline in rural mortality in India over the period. The median positive income shock in these data, at 5.8%, is larger, so the simulated beneficial effects are accordingly larger.

Within the rural sample, income effects are significantly larger amongst children of uneducated mothers, children of mothers who had their first birth before the age of eighteen, and girls (these results are in Bhalotra 2007a). The rest of this section investigates robustness of the (average) income coefficient to specification checks and additional controls.

5.2. Robustness Checks

The Specification of Income

To allow big income shocks to have a different marginal effect than smaller ones, I included the square of log income in the model. This was insignificant, indicating that the semi-log form approximates the curvature in the mortality-income relationship sufficiently well. I also investigated an alternative generalization in which I included, together with log income, its first difference, allowing different coefficients according to whether this difference is positive or negative. These variables were insignificant.[24]

In a further extension of the baseline model, I exploited information on the birth-month of the child to create a child-specific average of state income over the

[24] So changes in mortality risk depend upon deviations of log income from trend but not upon deviations in the growth rate of income.

relevant exposure period, where the weights are the fraction of the child's life spent in each year.[25] This adjustment made little difference to the average effect. I also conducted a placebo check. The mortality risk in age 0-1 should not be affected by future income. I ran the specification in the last column of Table 1 with income in period (t+1) replacing contemporary income. The coefficient on lead income is 0.025 and its p-value is 0.15.

The baseline model (column 1, Table 2) was extended to include lags of income. Results are in Appendix Table 3. Although the specification has purged most likely sources of endogeneity bias on the income coefficient, dropping current income from the model provides a further check against the possibility that unobserved mortality shocks correlated with income are driving the main result. The results show that the long run elasticity- driven by the third lag of income- remains negative, and it is larger than in the baseline model.

Income shocks that pre-date conception of the index child may affect their mortality risk through depleting the stock of maternal health[26] or the stock of material wealth.[27] I find that the first lag of income is insignificant. There is thus no evidence here that macroeconomic conditions during pregnancy affect survival *after* birth. They may impact upon survival *to* birth, but we cannot estimate this effect since the data record only live births. To investigate whether significance of the third lag of income was a reflection of state dependence (which it might be given that the median birth interval in these data is about 27 months), I re-estimated the model excluding first-borns and then included as a regressor an indicator of the infant survival status of the immediately preceding sibling. The pattern of income effects is unchanged and, in particular, the third lag of income and its long run elasticity both remain negative and significant (these results are available on request).

[25] For instance, for a child born in June 1980, the chances that she dies within a year of birth (infancy) will depend upon income in June-December 1980 and January-May 1981.

[26] Most infant deaths occur in the first week of life, and most of these in the first day. This suggests an important role for maternal health in explaining infant death.

[27] For example, if a negative shock some years ago stimulated distress sales of assets by poor households, this may be expressed in mortality in the current year if this is the year in which a birth occurs.

Outliers

The data show, across India, a dip in mortality in 1973 (Figure 4a). And Kerala has unusually low mortality throughout the period (Figure 1).[28] The time dummy for 1973 and the state effect for Kerala will capture these effects to the extent that they are additive. I nevertheless allow for parameter differences in these cases by excluding them (columns 2 & 3). Rather than simply drop the year 1973, I drop all years in the 1970s so as to check, at the same time, for any selectivity associated with the retrospective nature of the mortality data (section 2). The income effect is insignificantly different in each case.[29]

Selection on Unobservables

Column 4 shows the results of replacing mother fixed effects with state fixed effects and observable characteristics of the mother (her education, height, ethnicity, religion and the education of her partner). The average income effect is not significantly biased by failure to control for mother-level unobservables (compare with column 1). However, the income effect specific to certain population groups is sensitive to this omission (see Bhalotra 2007a). In particular, there are differences in the income effect by caste and religion that disappear once mother fixed effects are introduced. This is consistent with differential selection into fertility across the cycle that depends upon group-specific unobservables. Dehejia and Lleras-Muney (2004) report, similarly, that selection effects are more important amongst black than amongst white women in the United States.

Estimator

In the absence of mother fixed effects, probit estimates are consistent and these were compared with estimates of the linear probability model for the specification in column 4. The results were insignificantly different, and are not displayed. Column 5 shows that the income effect obtained is again very similar if I aggregate the mortality data to the state level using sample weights and estimate the model by within-groups, weighting by the square root of the number of births in each state-year. If, instead of within-groups, I estimate the panel data model in long (fifth) differences, the income coefficient is, again, hardly altered (column 6).

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[28] Indeed, the case of Kerala is widely recognized in the public health and economics literature as illustrating that good health is possible without high income. See, for example, Halstead et al. (1985).

[29] I have confirmed that the income effect is not significantly different if, instead, I start the sample in 1974 or 1975 rather than in 1980.

State-Level Covariates

I included in the model a number of state-level variables that are likely to be correlated with both mortality and income (column 7). These are the sectoral composition of income growth, urbanization, poverty, inequality, price shocks, state health, education and development expenditure, and newspaper circulation per capita. The coefficient on total income is now not directly comparable to that in the other columns because it is conditional upon the composition of income. It is nevertheless clear that conditioning upon all of these state variables does not eliminate the income effect- in contrast, for example, to the results of Anand and Ravallion (1993) who show, using a single cross-section of data on 22 countries, that the correlation between GDP and life expectancy becomes insignificant once the effects of public health expenditure and poverty are partialled out. Some further discussion of the state-level variables is in section 6.2.

Retrospective Data

A potential concern with the sort of retrospective data that I use to generate infant mortality by birth cohort is recall bias: the concern that mothers are more likely to forget the incidence or dates of events the further back in time they are. Although the DHS surveys have numerous checks built in to ensure the quality of birth history data (see ORC Macro 2006, p.14), I investigated this problem by estimating the model starting in 1980 rather than 1970 (column 2, Table 2). Another recall-related issue is rounding off of age. Since the data reveal age-heaping at six-month intervals, I define infant mortality as inclusive of the twelfth month, and find that this does not alter the results. A third potential problem is that, as we go further back in time, the births captured in the sample are fewer, and disproportionately of relatively young (high risk) mothers.[30] This problem is addressed by conditioning upon maternal age at birth (already discussed in section 5.1).

The survey does not, of course, record births of mothers who died before the date of the interview. If it is the frail or poor mothers who die early then we will have a selectively low-risk sample of children, especially for older cohorts of mothers. In principle, this could go the other way if, for example, women who die young are those least likely to benefit from growth for some other reason, like low education, but in

[30] For example, a woman who gave birth at age 15 in 1965 will be 49 in 1999, and her birth will be recorded. However, births to women any older than 15 years in 1965 will not be recorded since then women will be older than 49 years in 1999.

fact I find that income effects in the sample analysed are larger in households with less-educated women (Bhalotra 2007a). Another issue is that the survey records only live births, and the survivors are likely to be selectively healthy on average.[31] In this case, if the relationship between income and mortality is convex, we will under-estimate the effect of income.[32] As a check on the representativeness of the retrospective survey data used here, I compared the trend in the all-India infant mortality rate in these data (NFHS2) with trends derived from an earlier round of the same survey (NFHS1) and with administrative data (SRS): see the Appendix, where these comparisons are displayed and discussed. Overall, trends from the three sources are fairly similar, especially after 1973, and I have investigated robustness to starting the sample post-1973 (column 2, Table 2). Also, as long as any bias is time-invariant, it will be removed by the fact that the analysis is effectively in differences.

6. Mechanisms

We have found that recessions increase infant mortality. In this section we consider why. In section 6.1, I first look for evidence that behaviours that we expect will influence mortality risk – namely prenatal and postnatal care- are influenced by cyclical variation in income. I find that they are, with less care-seeking in recessions. I then consider whether this might be explained by the fact that mothers are busier at these times, possibly increasing their work engagement to compensate for a decline in their husband's wages. I complement this analysis of behavioural mechanisms with another look at selection. In particular, I investigate whether cyclical changes in the composition of births by mother's SES contribute to or detract from the mortality-increasing effects of negative income shocks. In section 6.2, I consider mechanisms that operate at the state level. First, I distinguish the agricultural and non-agricultural components of state income. Then I investigate alternative channels, for example, the extent to which fluctuations in state income affect mortality by altering the level of public expenditure, inflation, poverty, and other likely mediators.

[31] UN statistics also typically define infant and under-5 mortality rates with reference to live births; e.g UNICEF 2007.
[32] This relationship is convex in cross-country data but it is unclear that it is so in time series data (see Bishai et. al. 2007).

6.1. Micro-Mechanisms

Health-Seeking Behaviors

Infant mortality in poor countries is caused primarily by an interaction between low birth weight and infectious disease. It is widely accepted that the relevant interventions involve improvements in maternal health, antenatal care, skilled attendance at delivery, immunization, and treatment of diarrhea, malaria and respiratory infections (which are the proximate cause of most deaths); see Black et al. (2003), Jones et al. (2003), for example. I therefore analyse a number of these indicators. Definitions, means and standard deviations of the dependent variables are in Appendix Table 1.

Information on health-seeking is available in the NFHS data (described in section 2), but only for children born in the three years preceding the date of the survey. To gain more time-variation in these data, I pooled them with similar data from the previous round of the survey, conducted six years before (in 1992/3), which recorded information on health-seeking for children born four years before the survey. The resulting data have range 1988-1998, in which the years 1994 and 1995 are empty. The analysis is conducted separately for rural and urban households, with sample sizes of 50195 and 15819 children respectively. I use the linear probability estimator and show standard errors clustered at the mother and the state levels.[33]

The equations include mother characteristics - her age, indicators for the level of her education and that of her spouse, her caste and religion- and controls for child characteristics, including gender and whether the child is the first-born.[34] The equations also include state dummies, state-specific trends and time dummies (which encompass a survey-year dummy). So, as before, we are identifying the effects of deviations in state income from trend.

Results are in Tables 3A for rural and 3B for urban households. They are consistent with the finding of counter-cyclical infant mortality reported in section 5. Overall, the evidence is that Indian mothers are significantly less likely to seek

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[33] I do not use mother fixed effects because only 19.5% of births in the sample are from mothers who have at least two children. The sample of siblings is not only small but also endogenously selected as it will include only women with very short birth intervals (i.e. two children in a 3 or 4 year span).

[34] Health-care-seeking is often different for the first child, and there is considerable interest in aspects of son-preference in India.

antenatal care, deliver outside the home, get children immunized, and get treatment for their child's diarrhea or respiratory infections in downturns (cols. 1-11). [35]

The number of antenatal visits sought and the probability that a child is fully immunized attract very similar income coefficients in the rural and urban samples, and are significant in both. Place of delivery and child treatment effects are stronger in the rural sample. There is little evidence that the results for treatment arise on account of endogenous selection into the sample of children that contracted an infection. Columns 12-14 in Table 3A show that contracting fever is independent of the cycle, and the chances of getting diarrhea or a cough are lower in upturns. Once I cluster by state then, consistent with within-state autocorrelation in the equation errors, the chances of getting any of these infections is invariant to the cycle. These results contrast with those in the two previous investigations of cyclicality in health-seeking (Dehejia and Lleras-Muney 2004, Miller and Urdinola 2007), but are consistent with the results in Paxson and Schady (2005), who study a deeper crisis in a poorer environment. [36]

One explanation of our finding that more healthcare is obtained in upturns is that the supply of health services is better at these times. There is some support for this in that when I run a within-groups panel data regression of state health and development expenditure on state income, I find that these expenditures are significantly pro-cyclical with elasticities 0.30 and 0.45 respectively (Table 5, columns 1, 3).

However, there are reinforcing demand-side effects. The data on antenatal care and place of delivery permit some distinction between demand and supply effects. For example, the income coefficient for antenatal care sought by the mother (demand) is three times as big as that for antenatal visits received from a health worker (which are more likely to be related to supply); see Table 3, columns 6-7. For deliveries outside the home, we have information on whether they are in government or privately run

[35] For correspondence with the results for infant death presented earlier, the equations for child vaccinations are run on a sample that excludes children born less than a year before the survey date. Results for full immunization are presented for children aged one (column 9a) as well as for children aged 1-4 years at the survey date (column 9b), the latter being relevant because some vaccinations (e.g. measles) are supposed to be given only after the child is a year old.

[36] Note that Dehejia and Lleras-Muney find little evidence of behavioural changes amongst Black women. It is the white women in their sample who appear to seek more health care in downturns.

clinics. The results show that two-thirds of the decline in home deliveries in an upturn is balanced by an increase in deliveries in private clinics, the remaining one-third being accounted for by government clinics (Table 3, columns 3-5).[37] So it does seem that the cyclical variation in health-seeking is not merely on account of cyclical changes in the availability of public health services. And this suggests that the mechanisms driving the identified income effect on mortality are, in part, related to family resources of money and (maternal) time.

We cannot directly explore the effects of macroeconomic shocks on private expenditures on nutrition and healthcare, as these are not available at the household level. However, I have state-level time series on per capita household consumption derived as aggregates from a different household survey (the National Sample Survey; see Ozler et al. 1996). Using these data, I ran a within-groups regression of rural household consumption on state income. The elasticity is 0.21 and significant (Table 5, column 7). These results show that aggregate income fluctuations affect household consumption, consistent with previous evidence that households are not fully insured. The role of mother's disposable time is analysed in the next section.

Maternal Labour Supply

Information on maternal labour supply is available only at the time of interview, rather than retrospectively. It therefore cannot be matched to birth histories. As in the analysis of health-seeking behaviour, I pool data from the two surveys, so that I have information on women's work in four years, 1992, 1993, 1998 and 1999. All women in the sample are mothers and the age range is 15-49. The individual data are merged with the state-level panel, so that I can study the effects on women's work of deviations of state income in these four years from trend. I use the linear probability model and condition upon a quadratic in current age, indicators for the woman's level of education, the level of education of her partner, her caste and her religion, state dummies, time dummies and state-specific trends.

Results for the rural and urban samples are in Tables 4A and 4B. Even with the fairly limited variation available, there are some significant effects of aggregate income shocks on women's work in the rural sample. Rural Indian mothers are more

[37] Once the standard errors are clustered by state, the results are more sharply differentiated: antenatal visits received and deliveries in government clinics becomes insignificant, but the significance of antenatal care sought and of private deliveries persists.

likely to work in recessions, when they face lower own-wages and deteriorating incomes of their partners. So it appears that their labour supply curve is forward falling (negative at low wages).[38] A 5% increase in state income is associated with a 2% decrease in work participation (column 1). Although I do not have individual information on the wage rates or earnings of men and women, I find that the average rural agricultural wage is significantly lower in downturns, the elasticity being 0.37 (see Table 5, column 8).

Although the previous studies I refer to do not directly estimate cyclical variation in maternal labour supply, they argue from conventional wisdom that the opportunity cost of mother's time is higher in upturns. In rural India, it appears that the opposite is the case. This contrast may be explained by the poverty of these households, and the "cultural" fact that, with the exception of those with secondary and higher education, Indian women tend not to work outside the home unless they need to. The temporal effect that I report here is consistent with cross-sectional evidence on women's labour supply in India, which is U-shaped, with participation rates being highest amongst the very poor and the highly educated, and quite low in-between (Das and Desai 2003; also see Goldin 1995, Mammen and Paxson 2000). The results here may also be interpreted as a dominance of the added worker effect over the discouraged worker effect. It has been shown in other contexts that liquidity-constrained households may raise labour supply in order to smooth consumption in the face of an income shock; see Kochar (1995), Frankenberg, Smith and Thomas (2003) and Halliday (2007), for example.

The results are driven by informal work, that is, unpaid and agricultural work (columns 2-5). Although these effects are much larger and more significant in rural areas, they go in the same direction in urban areas. Paid and nonagricultural work in rural areas behave conventionally, increasing in good times, but the opposing effects of unpaid and agricultural work dominate. These results are consistent with the interpretation offered above since unpaid and agricultural work are more likely to be subsistence work. The Table reports the proportion of women in the sample in each work-category. In rural areas, a greater proportion of women are in unpaid and agricultural work than in other types of work and this contributes to their dominating the results. In urban areas, overall participation in work is about half that in rural areas

[38] For discussion of the shape of the labour supply curve under subsistence constraints, see Barzel and McDonald 1973, Bhalotra 2007b.

and the share of women in subsistence work is tiny. Cyclical variation has small and largely insignificant effects on women's participation in the urban sector.

Recall that, in section 5, we noted that the evidence for urban households is ambiguous: the marginal effect of income on mortality is not significantly different from that in the rural sample, but it is insignificant. We have now seen that antenatal care and immunization (but not delivery outside the home and not treatment for child disease) in urban households increase in upturns, just as in rural households but that, while there is a tendency for mothers' subsistence work to decline in upturns, this is much weaker than in the rural sample, and the baseline proportion of women in such work is very small.

To tie things in, for the rural sample, I introduced maternal labour force participation as a regressor in the models of health-seeking behaviour discussed above. In this case, maternal work is matched to antenatal or postnatal care of the most recent birth in the three or four years prior to the survey. I find consistently negative effects of mother's participation on place of delivery, antenatal care and treatment of child diseases and I also find that children are more likely to contract disease when the mother works. The exception is immunization, which is either insensitive to or benefits from maternal work engagement. These results are not sensitive to whether or not state income is held constant (results available on request).

Overall, the results in this section contribute to the evidence that, faced with limited consumption smoothing opportunities, poor families attempt to achieve income smoothing, but not without costs. The literature has highlighted costs in terms of sub-optimal production and profits (see Morduch 1995) rather in terms of health, although see Artadi (2005). These results also underline the importance of maternal time, in addition to maternal education in maintaining child health.

Composition of Births by Mother's SES

We have argued that it is important to control for mother-level unobservables since these may be correlated with the timing of fertility and the risk of mortality over the business cycle. In section 5.1, we saw that when we use a mother-fixed effects specification then the mortality-increasing effect of downturns is larger. This is consistent with the view that women with inherently higher risk are more likely than others to avert birth in recessions. This section shows how the composition of births varies over the cycle with observable maternal characteristics. I estimated the following model on a state-level panel created by aggregating the individual data to the state level using sample weights:

$$(2)\ C_{st} = a_s + a_t + \mu_{st} + b\ lnY_{st} + e_{st}$$

where C is the percentage of births in the sample with a given characteristic, Y is state aggregate income and the other terms denote state and year dummies and state-specific trends. This regression is run for each of several definitions of C. These are the educational level of the mother, of her partner, her age at birth, her caste and her religion.

Results are in Appendix Tables 4A and 4B for the rural and urban samples respectively. I find some evidence that women who are high-risk in observable terms (low SES) defer birth in a recession. In rural areas, illiterate women, women with illiterate husbands and women from scheduled tribes (ST, a disadvantaged ethnic group) are significantly less likely to give birth in a recession. This pattern of results is preserved if I add rainshocks (with state-specific coefficients, see section 4) to the model, although now the effects of women's education are weaker and of partner's education are stronger. In the urban sample, I again find that illiterate women are more likely to avert birth in recessions. The Tables suppress the results for caste and religion because they are insignificant in the rural sample. However, in the urban sample, the fraction of Hindu women giving birth in recessions is larger (income coefficient of -0.07), suggesting that the minority religions are more likely to avert birth in recessions. The results are consistent with the idea that fertility timing in credit-constrained households is more sensitive to the cycle, and in line with the finding in Dehejia and Lleras-Muney (2004) that the proportion of US births contributed by black women is smaller in recessions. The Tables show that the results are robust to conditioning upon rainshocks but sensitive to the exclusion of state-trends but as these are jointly significant at the 1% level, the preferred model is the one that includes them. The results are not sensitive to weighting by the square root of the state population.

6.2. Macro-Mechanisms

Sectoral Income

Column 7 of Table 2 reported estimates of a model that allows the sectoral composition of income to influence mortality. I find that, conditional upon total state income, rural mortality is decreasing in the share of non-agricultural income. In an alternative specification that directly includes agricultural and non-agricultural income, the marginal effect of agricultural income is effectively zero (-0.006, with t-statistic 0.83) and of non-agricultural income is -0.058 (t=4.13).[39] Consistent with this, the next section shows that the state-level variables that have a direct effect of mortality are sensitive to non-agricultural rather than agricultural income.

The result that it is non-agricultural income that drives rural mortality is pertinent to models of the growth process that emphasize spillovers between the sectors (e.g. Lewis 1954) and to recent debates regarding the role of agricultural productivity and nutrition in achieving historical declines in mortality (Fogel 1994, 2004, Deaton 2006b). It suggests that fluctuations in food production are not terribly important, although this does not undermine the role of nutrition since poor or remotely located households may not have sufficient nutrition even when production is adequate.[40] The result is also relevant since, in the period analysed, nonagricultural income was growing much faster than agricultural income.

Since rural households are predominantly agricultural, we may have expected that agricultural growth would have the greater potential to reduce rural mortality. However, a good share of non-agricultural growth in India has been in small-scale rural enterprises (e.g. Burgess and Pande 2003, pp.17-18). And even agricultural incomes may be raised by non-agricultural growth drawing surplus labour off the land (indeed, evidence of this is in the fact that the average male agricultural wage is higher when non-agricultural income is above trend- see Table 5 column 8; next section). Non-agricultural income changes may also affect rural incomes through

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[39] Since the volatility of agricultural income is closely related to rainfall variation, I re-estimated these models removing controls for rainshocks. In the first specification, this renders the share of non-agriculture in total state income insignificant. In the second specification, it narrows the gap, but the effect of non-agricultural income, at -0.040 (t=2.15), is still significantly larger than that of agricultural income, at -0.011 (t=1.65).

[40] Sen (1998) argues that the incidence of malnutrition fell in England during the second World War even as total food supplies fell, and this is because the public distribution of food that was initiated during the war ensured that the available supply was more evenly distributed. Measham and Chatterjee (1999) show that India has very high rates of malnutrition amongst children, higher than predicted by its per capita income.

changes in remittances. In these ways, growth in aggregate non-agricultural income is consistent with growth in rural household incomes. An alternative and possibly complementary mechanism is that non-agricultural income growth is more effective in stimulating increases in state social spending, for example, because non-agricultural incomes are more taxable. The taxation of agricultural income rests with state governments but no state had attempted it in the period analysed (Ahluwalia 2002, p. 71). I find support for this hypothesis in that non-agricultural (and not agricultural) growth stimulates changes in health, education and development spending (Table 5 column 8; next section).

Consistent with our findings, poverty in India has been shown to have been more responsive to non-agricultural growth (Besley et al. 2005)[41]. There are also some consistent findings in the literature on mortality. van der Berg et al. (2006a) show that non-agricultural growth was relatively important in reducing mortality in nineteenth century Holland. Banerjee et al. (2007) show that a shock to agricultural production had no effect on mortality in nineteenth century France.

Channels

To investigate which of a set of candidate variables, X, mediates the identified effect of income (Y) on mortality (M), a pair of auxiliary regressions were run on the state-level panel as follows:

$$(3)\ X_{st} = \mu_s + \mu_t + \mu_{st} + l \, logY_{st} + u_{st}$$

$$(4)\ M_{ifst} = h_s + h_t + h_{st} + j \, X_{st} + q' \, lnZ_{ifst} + e_{ifst}$$

The notation is the same as in equation 1. Equation 3 tells us whether a potential mediator, X_{st}, is driven by income and equation 4 tells us whether it, in turn, drives rural infant mortality.

Estimates of l in equation 3 are in Table 5. Increases in income are associated with increases in state health, education and other development expenditure, rural but not urban household consumption and rural wages. Income improvements are also associated with decreases in price levels and inflation in both sectors, and with decreases in rural but not urban poverty. There is a weak tendency for rural inequality

[41] Although see Datt and Ravallion (1996, 1998) for a dissenting view.

to rise in upturns, but there is no effect on urban inequality, and no impact on urbanization measured as the inverse of the share of the rural to the total population in the state.

I re-estimated (3) with agricultural and nonagricultural income included separately (row B in Table 5). This shows that increases in non-agricultural income are positively associated with health and development expenditure and rural wages and negatively associated with inflation in prices facing both agricultural and industrial workers. Increases in agricultural income raise average rural consumption and lower rural inequality and inflation in prices facing agricultural workers.

Our finding that it is non-agricultural income that drives rural mortality suggests that average consumption, inequality and inflation in the rural sector are relatively unimportant, other things equal. Indeed, estimates of j in equation 4 see Appendix Table 5) show that the only mediator variables that have a direct effect on mortality are lagged state health expenditure[42], inflation in prices facing industrial workers[43], rural wages and rural inequality. With the exception of rural inequality, which appears to be insensitive to the cycle, the results in Table 5 show that these variables change in a favourable direction with growth in non-agricultural income.

The direct effects of mediator variables on mortality are hard to identify in equation 4 because they tend to be highly correlated with the aggregate and state-specific trends. Income, in contrast, varies sufficiently around these trends within a state. This might explain why, in contrast with the main result in Anand and Ravallion (1993), including these mediator variables directly into equation (1) does not eliminate- indeed, does not change at all- the income effect (column 7, Table 2). Overall, the results in this paper suggest that there are some as yet unidentified routes through which fluctuations in aggregate income impact mortality risk. One possibility is that changes in state income affect the composition of state expenditure or the effectiveness with which it is translated into broad-based public services. This would

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[42] See Bhalotra (2007c) for a more detailed analysis of the effects of health expenditure. There is no contemporaneous effect but a significant long run effect driven by the third lag.

[43] I know of no previous evidence of inflation effects on mortality, although there is evidence that inflation differentially hurts the poor (Datt and Ravallion 2002, Easterly and Fischer 2001). Although inflation in prices facing industrial workers raises mortality (marginal effect 0.15, t=1.82), inflation in prices facing agricultural workers has a negative but insignificant impact, possibly because farmers are both producers and consumers. These effects emerge from the specification in Table 2 column 7, but are not significantly altered if I drop the ratio of non-agricultural to total income and the ratio of the rural to the urban population in the state.

be consistent with the argument that the poor benefit (or lose) disproportionately from incremental changes in public expenditure when there is early political capture.[44] There is some evidence, fom Africa, that reductions in government resources for health care result in less effective health care delivery (Ogbu and Gallagher 1992).

7. Conclusions

Shocks to state-level income in India appear to cause substantial variation in infant mortality in rural Indian households, even after we adjust for possible selection associated with heterogeneity in fertility timing or in survival until birth. A recession involving a one standard deviation change in log income (0.36) is estimated to raise mortality risk by 1.6%, other things equal. Taking the UN estimate of live births in India in 1990 of 26.3 million, this implies an additional 0.42 million infant deaths. The effects of income shocks on lifetime health will tend to be even greater since, where children survive income shocks in childhood, early exposure to poor living conditions has lasting adverse effects on their health (e.g. Banerjee et al. 2007, Case, Fertig and Paxson 2003, van der Berg et al. 2006a).

The paper shows that recessions are associated with an increase in rural maternal labour supply and that, consistent with this, antenatal and postnatal health-seeking are lower in recessions, in contrast to recent results for richer countries. Although, on average, Indian mothers appear to time fertility to lower death risk, it seems that they are constrained in the extent to which they can time their labour supply to maximize infant survival chances.

Non-agricultural growth is more effective in reducing rural mortality than similar changes in agricultural income. This suggests that neither the overall supply of food in the state nor rural poverty (both of which are driven by agricultural income growth) is key to mortality reduction in India in the period analysed. Investigating the effects of sectoral income on potential mediator variables, I find that non-agricultural (and not agricultural) income drives reductions in inflation and increases in state health and development expenditure that, in turn, create reductions in mortality.

This paper has brought together two empirical features of poor countries that distinguish them from richer countries: their astounding scale of childhood mortality

[44] See Lanjouw and Ravallion (1998), who provide supporting evidence for India for the case of primary education and anti-poverty programmes.

and their greater income volatility. It demonstrates that, even if income fluctuations are temporary, they cause irreversible damage in terms of infant mortality. The results suggest a need to put in place mechanisms that shield the most vulnerable from suffering irreversible consequences like child death from transient shocks, like temporary increases in unemployment or in food prices.

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Tables

Table 1: The Baseline Model :

Impact of State Aggregate Income on Infant Mortality Risk

Panel A: Rural	(1) **No controls**	(2) **State fixed effects**	(3) **Mother fixed effects**	(4) **Year dummies**	(5) **State-trends**	(6) **Child charac**	(7) **Rainshocks**
Income	-0.042	-0.084	-0.154	-0.060	-0.034	-0.035	-0.035
	[2.03]	[7.44]	[10.89]	[3.01]	[2.95]	[3.03]	[2.98]
Income elasticity	-0.443	-0.886	-1.624	-0.633	-0.359	-0.369	-0.369
Panel B: Urban							
Income	-0.045	-0.054	-0.132	-0.051	-0.040	-0.037	-0.036
	[3.57]	[5.58]	[7.55]	[1.60]	[1.04]	[0.99]	[0.96]
Income elasticity	-0.751	-0.902	-2.204	-0.851	-0.668	-0.618	-0.601

Notes: These are estimates of a linear probability model with robust standard errors clustered at the state level and t-statistics are in parentheses. The dependent variable is an indicator for infant mortality and income is the logarithm of real per capita net domestic product at the state level. The number of children and mothers is 117088 and 36068 in the rural sample and 35783 and 13414 in the urban sample. Elasticities are calculated at the mean of infant mortality which is 0.09481 in the rural and 0.0599 in the urban sample. Mother fixed effects imply state fixed effects since, by construction, mothers do not move between states. Child characteristics include gender, birth-order, birth-month and age of the mother at the birth of the child. Rainshocks are allowed to have different effects by state and according to whether they are positive or negative.

Table 2: Robustness Checks and Extensions

Panel A: Rural Households	(1) State popul	(2) Start in 1980	(3) Drop Kerala	(4) Drop mother FE	(5) Panel data: WG	(6) Panel data: long diff.	(7) Add state-level vars.
Income	-0.044	-0.047	-0.043	-0.038	-0.034	-0.045	-0.067
	[3.41]	[2.60]	[3.23]	[2.82]	[3.18]	[2.29]	[3.41]
Share of nonagri in income							-0.043
							[3.03]
Mean dep var	0.0948	0.0875	0.0964	0.0948	0.0948	-0.003	0.0948
elasticity	-0.464	-0.537	-0.446	-0.401	-0.359		
N(children)	117088	97000	114294	115585	420	345	116743
N(mothers)	36068	34366	34837	n.a.	n.a.	n.a.	35955
Panel B: Urban Households							
Income	-0.028	-0.064	-0.025	-0.033	-0.014	-0.024	-0.034
	[0.66]	[1.02]	[0.58]	[1.01]	[0.45]	[0.71]	[0.91]
Share of nonagri in income							-0.056
							[1.03]
Mean dep var	0.0599	0.0548	0.0609	0.0599	0.0599	-0.002	0.0599
elasticity	-0.467	-1.167	-0.411	-0.551	-0.234		
N(children)	35783	29083	34849	35584	420	345	35719
N(mothers)	13414	12537	12946	n.a.	n.a.	n.a.	13394

Notes: The baseline model is that in column 7 of Table 1, so every column includes mother fixed effects, year dummies, state-specific trends, child characteristics and rainfall shocks and these are LPM coefficients with robust standard errors clustered at the state level. Absolute t-statistics are reported in parentheses. In column 4, mother fixed effects are replaced by mother characteristics and state fixed effects. Columns 5 and 6 use panel data created from the individual data. Column 5 reports within-group estimates using the full sample of 15 states and 28 years, and column 6 reports estimates using long (5-year) differences, which results in a shortening of the sample. The within-groups income coefficient on the shorter sample is -0.024, which is just under two s.d. smaller than the reported long-difference estimate. Column 7 extends the model in column 1 by adding the following state-level variables: log share of rural in total population, log share of nonagricultural in total income, inflation in consumer prices facing industrial and agricultural workers respectively, the logarithm of the rural and urban poverty headcount rates, the logarithm of the rural and urban gini coefficients, log per capita state expenditure on each of health and education and other development spending and a quadratic in the log of newspaper circulation per capita.

Table 3A: Income Effects on (Disease and) Health-Seeking Behavior in Rural Households

	Place of Delivery			Antenatal Care			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Cluster(mother)	*Home*	*Gov.*	*Private*	*Complete Care*	*In First Trimester*	*Visits Sought*	*Visits Received*
Income	-0.124	0.051	0.084	0.041	0.058	1.907	0.066
	[5.34]	[2.46]	[4.34]	[1.62]	[2.26]	[13.02]	[2.44]
B. Cluster (state)							
Income	-0.123	0.051	0.083	0.04	0.058	1.903	0.065
	[2.61]	[1.21]	[1.82]	[0.53]	[0.76]	[3.53]	[0.49]
Observations	49515	49515	49515	49060	49592	43451	49357

	Child Vaccinations			Child Treatment		Child Disease Incidence		
	(8)	(9a)	(9b)	(10)	(11)	(12)	(13)	(14)
A..Cluster(mother)	*Number*	*Full*	*Full*	*Diarrhea*	*Respiratory*	*Diarrhea*	*Fever*	*Cough*
Income	2.742	0.101	0.163	0.141	0.111	-0.035	-0.025	-0.091
	[8.40]	[2.95]	[5.04]	[1.51]	[1.86]	[1.54]	[0.88]	[3.12]
B. Cluster (state)								
Income	2.742	0.101	0.162	0.145	0.116	-0.034	-0.026	-0.091
	[2.54]	[1.41]	[2.07]	[1.34]	[1.35]	[0.36]	[0.52]	[1.38]
Observations	13754	13754	35028	6162	13295	45345	45348	45351

Notes: These estimates are on data for rural children born in 1988-1998. The dependent variables are either binary or counts, and these are OLS estimates. Income is log real p.c. state net domestic product. Standard errors are clustered at the mother level in Panel A and at the state level in Panel B. Columns 8 and 9(a) restrict the sample to children aged 1 and column 9(b) is for children aged 1-4. The estimated model includes state and year dummies, state-specific trends, an indicator for the child's gender, whether the child is the first-born, the current age of the mother, indicators for the level of maternal and paternal education, mother's caste and religion. Sample construction is described in section 6.1.

Table 3B: Income Effects on (Disease and) Health-Seeking Behavior in Urban Households

	Place of Delivery			Antenatal Care			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Cluster(mother)	*Home*	*Gov.*	*Private*	*Complete Care*	*In First Trimester*	*Visits Sought*	*Visits Received*
Income	-0.060	0.084	-0.012	0.140	0.112	1.956	0.099
	[1.25]	[1.63]	[0.24]	[2.75]	[2.19]	[6.05]	[2.63]
B. Cluster (state)							
Income	-0.057	0.086	-0.017	0.137	0.110	1.932	0.100
	[1.10]	[0.80]	[0.18]	[0.84]	[1.79]	[2.33]	[0.87]
Observations	15663	15663	15663	15539	15690	15162	15647

	Child Vaccinations			Child Treatment		Child Disease Incidence		
	(8)	(9a)	(9b)	(10)	(11)	(12)	(13)	(14)
A..Cluster(mother)	*Number*	*Full*	*Full*	*Diarrhea*	*Respiratory*	*Diarrhea*	*Fever*	*Cough*
Income	1.402	1.049	0.216	0.030	0.018	-0.096	0.620	0.458
	[0.73]	[2.33]	[3.39]	[0.78]	[0.37]	[1.86]	[3.81]	[4.55]
B. Cluster (state)								
Income	1.402	1.049	0.214	0.031	0.020	-0.093	0.577	0.452
	[1.20]	[2.94]	[1.69]	[0.34]	[0.21]	[0.77]	[2.58]	[3.01]
Observations	340	348	11184	14809	14813	14808	1865	4198

Notes: See notes to Table 3A.

Table 4A: Cyclicality in Maternal Work Participation in Rural Households

	(1)	(2)	(3)	(4)	(5)
A. Cluster(mother)	**Any work**	**Paid**	**Unpaid**	**Non-agri**	**Agriculture**
Income	-0.123	0.256	-0.345	0.172	-0.290
	[1.49]	[3.59]	[4.94]	[3.40]	[3.77]
B. Cluster(state)					
Income	-0.123	0.256	-0.345	0.172	-0.290
	[0.52]	[2.18]	[1.83]	[1.78]	[0.94]
Mean (dependent variable)	0.291	0.173	0.114	0.095	0.196
Observations	40092	40051	40032	40030	40030

Table 4B: Cyclicality in Maternal Work Participation in Urban Households

	(1)	(2)	(3)	(4)	(5)
A. Cluster(mother)	**Any work**	**Paid**	**Unpaid**	**Non-agri**	**Agriculture**
Income	0.007	-0.036	-0.062	0.081	-0.075
	[0.06]	[0.33]	[1.45]	[0.68]	[1.43]
B. Cluster(state)					
Income	0.007	-0.036	-0.062	0.081	-0.075
	[0.03]	[0.22]	[0.80]	[0.74]	[0.51]
Mean (dependent variable)	0.151	0.103	0.019	0.124	0.026
Observations	12799	12789	12789	12777	12777

Notes: These estimates are for women aged 15-49 for whom labour force participation is recorded at the time of interview. Women are interviewed in 1992, 1993, 1998 and 1999. The dependent variables are binary and these are LPM estimates. A question on type of employment is used to construct the paid/unpaid classification and a question on occupation is used to construct the non-agriculture/ agriculture classification. Participation in paid and unpaid work add up approximately to total work (in column 1), as do participation in nonagri/agri work. This adding up is not exact because the unclassified people in the employment and occupation questions are not identical and because the employment question has a third response, which is self-employment (less than 5% of women). Unpaid and agricultural work are likely to be mostly subsistence work. Income is log real p.c. state net domestic product. The models include state dummies, time dummies and state-specific trends, a quadratic in current age and indicators for the woman's level of education, the level of education of her partner, her caste and her religion.

Table 5: Effect of State Aggregate Income on Potential Mediators

	(1) **Health expend.**	(2) **Education Expend.**	(3) **Devel. Expend.**	(4) **Average consump.**	(5) **Headcount ratio**	(6) **Share rural pop**
(A) Total						
Income	0.326**	0.306*	0.425*	0.116*	-0.149	0.001
	[3.17]	[2.77]	[2.97]	[2.29]	[2.04]	[0.11]
(B) Sectoral						
Ag income	0.050	0.059	0.039	0.071**	-0.087	0.002
	[1.36]	[0.99]	[0.61]	[4.32]	[1.99]	[0.54]
Nonag income	0.236	0.188	0.432*	0.038	-0.047	-0.007
	[1.99]	[1.59]	[2.72]	[0.51]	[0.42]	[0.68]
<u>RURAL</u>	(7) **Av rural consump.**	(8) **Rural wage**	(9) **Rural headcount**	(10) **Rural inequality**	(11) **CPI agri workers**	(12) **Inflation agri workers**
(A) Total						
Income	0.175*	0.362**	-0.181	0.146	-0.270**	-0.237**
	[2.89]	[3.40]	[1.94]	[1.62]	[6.11]	[7.65]
(B) Sectoral						
Ag income	0.104**	0.077	-0.104	0.041	-0.124**	-0.103**
	[3.77]	[0.87]	[2.02]	[1.24]	[5.18]	[4.65]
Nonag income	0.046	0.221	-0.035	0.123	-0.008	-0.046
	[0.52]	[1.54]	[0.27]	[0.97]	[0.32]	[1.68]
<u>URBAN</u>	(13) **Av urban consump.**	(14) **Urban headcount**	(15) **Urban inequality**	(16) **CPI ind workers**	(17) **Inflation ind wkers**	
(A) Total						
Income	-0.050	0.045	0.011	-0.078*	-0.049**	
	[0.54]	[0.55]	[0.13]	[2.94]	[3.05]	
(B) Sectoral						
Ag income	0.012	-0.031	0.023	-0.020	-0.003	
	[0.47]	[0.85]	[0.45]	[1.55]	[0.34]	
Nonag income	-0.052	0.050	0.029	-0.076	-0.047**	
	[0.42]	[0.49]	[0.23]	[1.84]	[4.18]	

Notes: Estimated on panel data for 15 states in 1970-98, NT=420. Every equation includes state and year dummies and state-specific trends. All dependent variables are in logarithms. Robust t-statistics in parentheses. Row A shows results for total state income and row B shows results when this is entered by sector. Regressions are weighted by the square root of the state population. This tends to reduce the standard errors but in no case does this alter the significance of a coefficient at conventional significance levels. The results are robust to including rainshocks and demographics. Inequality is measured as the Gini coefficient. CPI is consumer price index. Inflation is the first difference of log price (CPI). Agri is agricultural and Ind is industrial. Average consumption is the state mean of household consumption from a nationwide household survey. Effects of these variables on mortality are displayed in Appendix Table 5.

Figures

Figure 1: State-Specific Trends in the Infant Mortality Rate

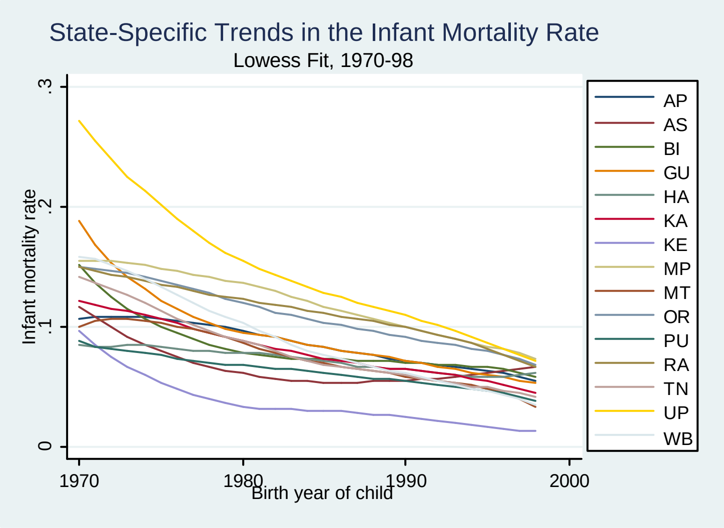


Figure 2: State-Specific Trends in Log State Income

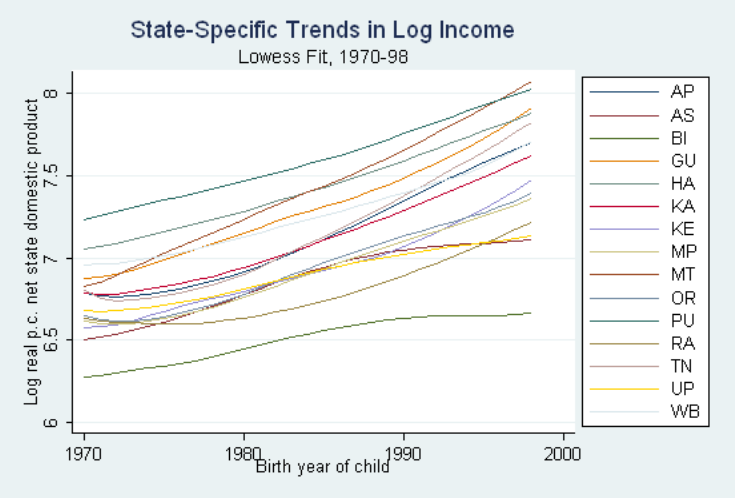


Figure 3: Infant Mortality Against Log Income by State

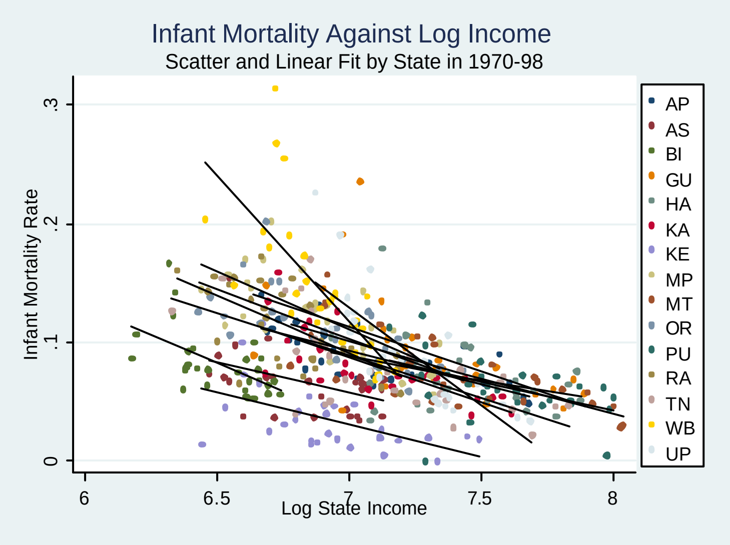


Figure 4A: Infant Mortality and Log State Income: Population-Weighted Averages for All-India

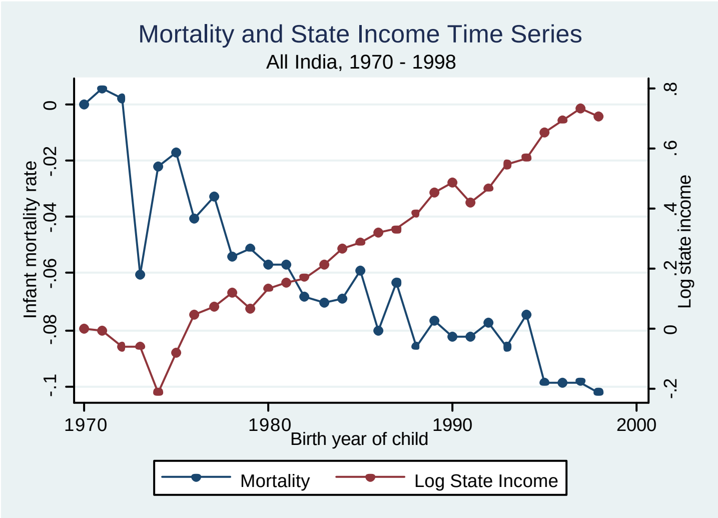


Figure 4b: Infant Mortality and Log State Income: Detrended Series for All-India

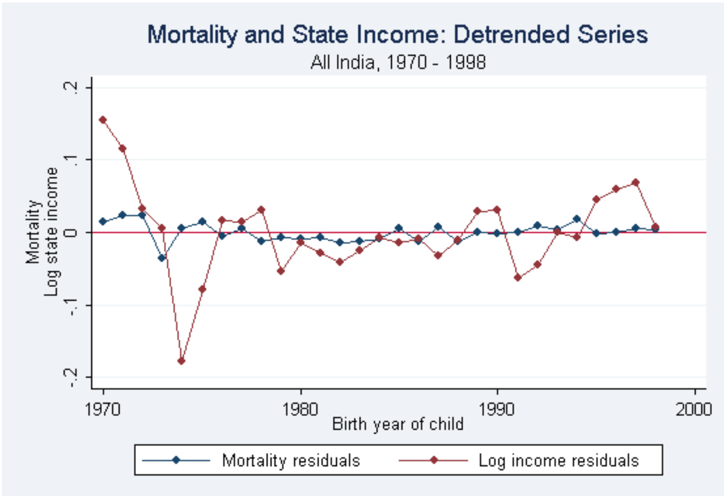


Figure 4c: *Log* Infant Mortality and Log State Income: Detrended Series for All-India

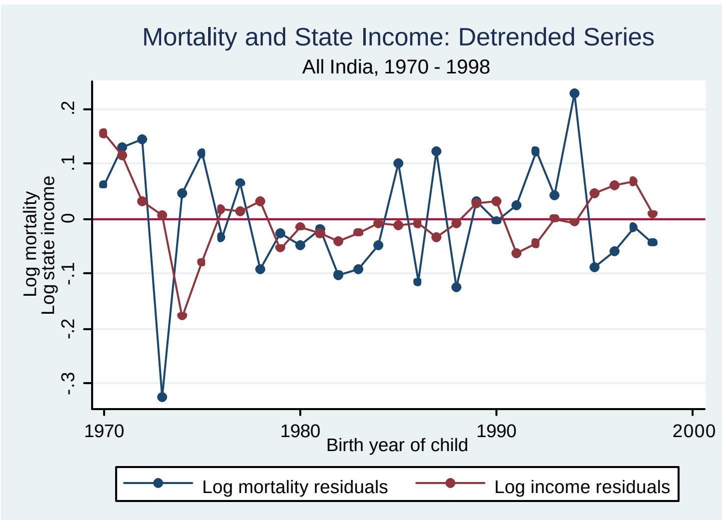
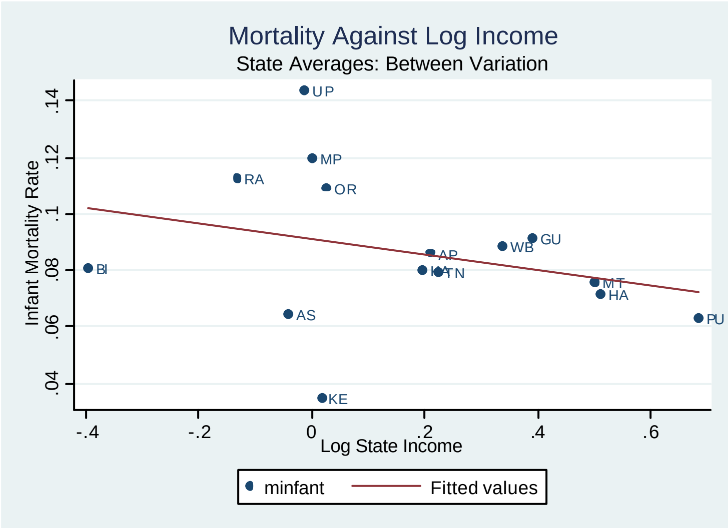


Figure 5: Infant Mortality Against Log Income: The Between-State Variation



Appendix : Selection of Births on Maternal Age at Birth in the DHS

The births recorded in the retrospective data in NFHS-2 occur in 1961-99. I have dropped births in the 1960s since the data for this decade are scarce, and most skewed. For 1970-1998, I studied the distribution of maternal age at birth by decade. For births in the sample in the 1970s, the median maternal age is 20 and the maximum is 30 years. In the 1980s, the median and maximum are 22 and 40, and in the 1990s, the corresponding statistics are 23 and 48. Because Indian women give birth at relatively young ages (see Appendix Figure 1), the 1970s data are fairly representative.[45] One way of capturing this is to note that, in 1980-98, although the maximum age at birth is 48, 90% of all births occurred by the age of 30 (the maximum age in our sample for the 1970s).

Further assessment of the extent to which the changing composition of maternal age at birth over time in these data alters trends in mortality is sought by comparing these trends (from NFHS-2) with trends derived from an earlier round of the same survey (NFHS-1), conducted six years earlier (see Figure A2). This comparison suggests that the NFHS-2 data do over-estimate mortality but only until about 1973. Since the more recent survey has the higher estimates, it seems that selection on survival until the interview is dominated by selection on maternal age at birth, which the analysis controls for. Figure A3 plots trends from the NFHS-2 on the same axes as trends derived from administrative data (the Sample Registration System or SRS; see http://www.censusindia.net/).[46] Although the survey estimates are lower and more variable, they do not seem to deviate more from the administrative estimates earlier in the period than later. Overall, it appears that the NFHS data provide a fairly reliable description of trends in mortality over time, at least from the early 1970s onwards. I nevertheless investigate robustness of the results to starting the sample at different dates- 1974, 1975, 1980.

As the analysis in this paper relies upon variations around trend, it is unlikely to be affected by any bias in trends. The direct effect of maternal age at birth on individual mortality risk is captured in the model since this is available for every child.

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[45] The tendency for women to give birth young was stronger in the 1970s than later. A simple regression of maternal age at birth on a linear trend for the period 1970-98 shows a significant growth of 0.15 years per annum. This implies a growth in maternal age at birth of 4.44 years over the 29 year period. Since births to older mothers are truncated for earlier years in the NFHS data, this is an over-estimate but it is likely to be close to the true growth rate given that some 90% of births occur by age 30 even when the sample is not truncated (see text).

[46] The SRS is an ongoing large-scale demographic survey that provides yearly estimates of fertility and mortality indicators at the national level and for major states. The SRS sample frame covers 6 million people, living in about 1·1 million households in 35 states or union territories of India.

Figure A1: Mortality and fertility by maternal age at birth

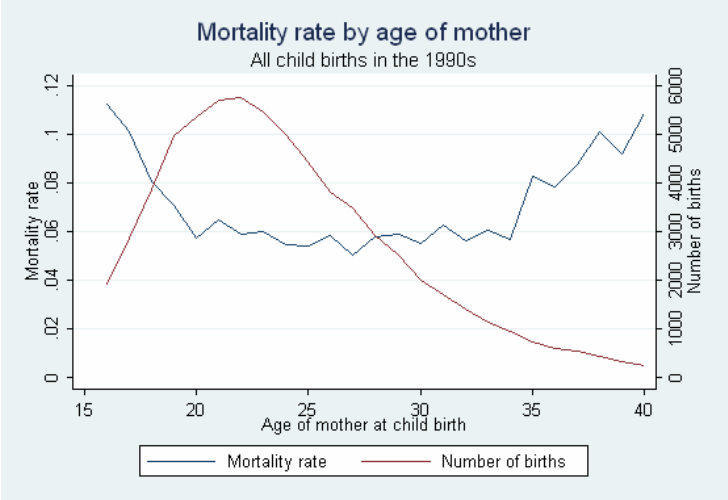
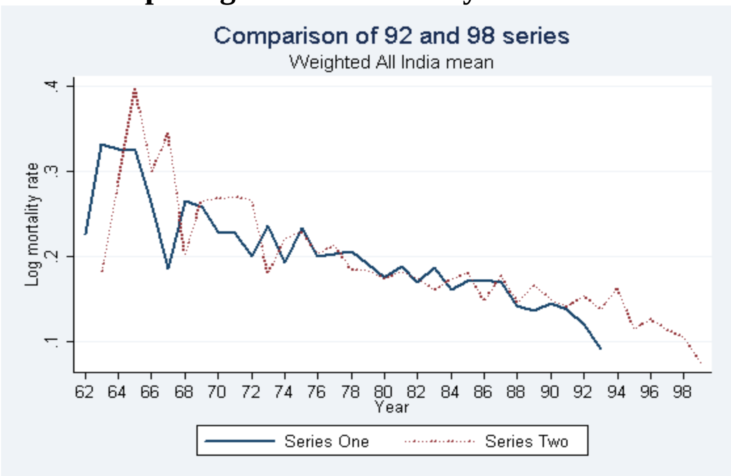
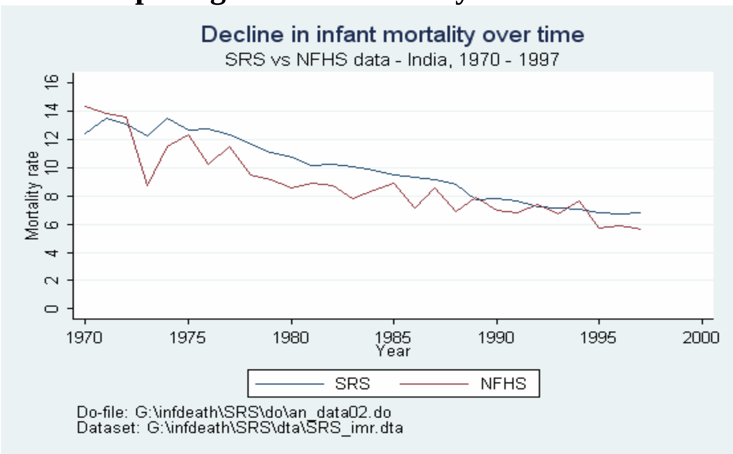


Figure A2: Comparing Trends in Survey Data from Two Rounds



Series 1 is constructed from NFHS1 conducted in 1992/3 and series 2 from NFHS2 conducted in 1998/9.

Figure A3: Comparing Trends in Survey and Administrative Data



Appendix Tables

Appendix Table A1: Summary Statistics

Variable	Mean	Std. Dev	Min	Max
Main Variables				
infant mortality (1 if child died at or before 12 months of age)	0.095	0.293	0.000	1.000
state income (log real per capita net domestic product of the state)	0.123	0.345	-0.731	1.131
Individual-Level Covariates:				
Child gender (omitted: male)				
Boy	0.521	0.500	0	1
Girl	0.479	0.500	0	1
Child birth month (omitted: January)				
January	0.068	0.251	0	1
February	0.065	0.246	0	1
March	0.082	0.275	0	1
April	0.079	0.270	0	1
May	0.079	0.269	0	1
June	0.086	0.280	0	1
July	0.087	0.282	0	1
August	0.106	0.308	0	1
September	0.090	0.287	0	1
October	0.094	0.292	0	1
November	0.088	0.283	0	1
December	0.076	0.264	0	1
Child birth order (omitted: first child)				
first child	0.228	0.420	0	1
second child	0.252	0.434	0	1
third child	0.198	0.398	0	1
fourth child	0.134	0.341	0	1
fifth or later child	0.188	0.391	0	1
Mother's education (omitted: no education)				
No education	0.737	0.440	0	1
Incomplete primary	0.087	0.282	0	1
Complete primary	0.060	0.237	0	1
Incomplete secondary	0.082	0.274	0	1
Secondary or higher	0.035	0.183	0	1
Father's education (omitted: no education)				
No education	0.401	0.490	0	1
Incomplete primary	0.123	0.329	0	1
Complete primary	0.100	0.300	0	1
Incomplete secondary	0.201	0.400	0	1
Complete secondary	0.092	0.289	0	1
Higher	0.083	0.276	0	1
Household ethnic group (omitted: high caste)				
High caste (or no caste)	0.332	0.471	0	1
Scheduled caste	0.204	0.403	0	1

Scheduled tribe	0.118	0.323	0	1
Other backward caste	0.337	0.473	0	1
Household religion (omitted: Hindu)				
Hindu	0.845	0.362	0	1
Muslim	0.112	0.316	0	1
Christian	0.013	0.114	0	1
other religion	0.030	0.171	0	1
Maternal age at birth of index child (omitted: age 19-24)				
Age 9-15	0.037	0.188	0	1
Age 16-18	0.160	0.366	0	1
Age 19-24	0.469	0.499	0	1
Age 25-30	0.247	0.431	0	1
Age 31-49	0.088	0.283	0	1
Rainshocks (All-India means)				
Regressions include these interacted with state dummies				
positive deviations of rain from within-state trend *x 100*	0.074	0.122	0.000	0.726
negative deviations of rain from within-state trend *x 100*	-0.073	0.116	-0.865	0.000
State-Level Covariates:				
Sectoral output and prices				
log ratio of non-agri to total net domestic product p.c.	-0.566	0.196	-1.338	-0.177
log agricultural income of the state, p.c., real	6.116	0.341	5.214	7.172
log non-agricultural income of the state, p.c., real	6.493	0.482	5.272	7.574
log ratio of rural to urban population	-0.247	0.096	-0.525	-0.085
log consumer price index for agri workers	0.770	0.658	-0.555	2.022
log consumer price index for industrial workers	0.804	0.683	-0.454	2.247
inflation in agricultural consumer prices (dlogP)	0.077	0.076	-0.261	0.511
inflation in industrial worker consumer prices (dlogP)	0.082	0.044	-0.152	0.296
Consumption, poverty, inequality				
log rural poverty headcount rate	3.801	0.300	2.403	4.396
log rural Gini coefficient	3.298	0.183	2.663	3.835
log urban poverty headcount rate	3.592	0.374	1.880	4.139
log urban Gini coefficient	3.467	0.148	1.565	3.870
log mean private consumption of rural households	4.106	0.177	3.722	4.645
log mean private consumption of urban households	4.427	0.204	3.027	5.042
State social expenditures, log real per capita				
health expenditure	2.799	0.476	1.191	3.731
education expenditure	3.667	0.465	2.361	4.677
development expenditure other than health, education (includes expenditure on agriculture, rural development, irrigation, public works and community development programs)	4.185	0.564	2.523	5.647
Other state-level covariates				
total newspaper circulation per capita	-3.041	0.625	-4.858	-1.145
log state population	17.501	0.621	16.089	18.906
Health-Seeking Behaviours, Rural (N=50195)				
Available for recent births, see section 6.1.				
Place of delivery				
Home	0.805	0.396	0	1

Government facility	0.108	0.310	0	1
Private facility	0.083	0.276	0	1
Antenatal care				
Complete care (defined, in India, as at least 3 antenatal care visits, at least 1 tetanus shot & iron folic tablets)	0.278	0.448	0	1
Visit made in first trimester	0.222	0.415	0	1
Number of visits sought	1.869	2.624	0	28
Number of visits received from a health worker	0.224	0.417	0	1
Child vaccinations (exclude children <13 months at interview)				
Number of vaccinations had (sample age 1)	4.283	3.349	0	8
1 if full set (3 DPT, 3 Polio and 1 measles shot) (sample age 1)	0.281	0.450	0	1
1 if full set (sample age 1-4)	0.293	0.455	0	1
Treatment for child diseases in two weeks before survey				
Whether child was taken to a medical facility				
Diarrhea	0.590	0.492	0	1
Respiratory (includes cough & fever)	0.596	0.491	0	1
Incidence of child disease				
Diarrhea	0.136	0.343	0	1
Fever	0.241	0.428	0	1
Cough	0.250	0.433	0	1
Health-Seeking Behaviours, Urban (N=15819)				
Place of delivery				
Home	0.406	0.491	0	1
Government facility	0.301	0.459	0	1
Private facility	0.290	0.454	0	1
Antenatal care				
Complete care (defined, in India, as at least 3 antenatal care visits, at least 1 tetanus shot & iron folic tablets)	0.556	0.497	0	1
Visit made in first trimester	0.463	0.499	0	1
Number of visits sought	4.110	3.345	0	30
Number of visits received from a health worker	0.108	0.310	0	1
Child vaccinations (exclude children <13 months at interview)				
Number of vaccinations had (sample age 1)	6.199	2.681	0	8
1 if full set (3 DPT, 3 Polio and 1 measles shot) (sample age 1)	0.494	0.500	0	1
1 if full set (sample age 1-4)	0.507	0.500	0	1
Treatment for child diseases in two weeks before survey				
Whether child was taken to a medical facility				
Diarrhea	0.713	0.452	0	1
Respiratory (includes cough & fever)	0.729	0.444	0	1
Incidence of child disease				
Diarrhea	0.126	0.332	0	1
Fever	0.222	0.416	0	1
Cough	0.246	0.430	0	1
Maternal Work Participation Rates, Rural (N=40578)				
Available for four years, see section 6.2				
Paid/unpaid refers to current employment and nonagri/agri to current occupation. These are alternative classifications.				
Any work (paid+unpaid), (non-agri+agri)	0.335	0.472	0	1

Paid work	0.164	0.370	0	1
Unpaid work	0.145	0.352	0	1
Non-agricultural work	0.086	0.280	0	1
Agricultural work	0.249	0.433	0	1
Maternal Work Participation Rates, Urban (N=12900)				
Any work (paid+unpaid), (non-agri+agri)	0.151	0.358	0	1
Paid work	0.103	0.304	0	1
Unpaid work	0.019	0.135	0	1
Non-agricultural work	0.124	0.329	0	1
Agricultural work	0.026	0.160	0	1

Notes: N=117088 in the sample of rural households restricted to children born in the mother's current place of residence, the sample analysed in section 5. Number of observations for analysis in section 6 reported in Tables 3,.4. The deflator of state income and social expenditures is the consumer price index for agricultural workers. Mean consumption, the poverty rates, and inequality are computed from household consumption data in the National Sample Survey. All individual-level variables are from NFHS-2 and all state-level variables are from the state-level panel compiled by Timothy Besley and Robin Burgess (see Besley and Burgess 2002, 2004). Exceptions are health expenditure and rainfall, which were collected from publications of the Centre for Monitoring of the Indian Economy by Juan Pedro Schmid. The state expenditure data are from the Handbook of Statistics on State Government Finances published by the Reserve Bank of India.

Appendix Table 2: Full Results for the Baseline Model

	RURAL		**URBAN**	
	FE	no FE	FE	no FE
income	-0.044**	-0.038*	-0.028	-0.033
	[3.41]	[2.82]	[0.66]	[1.01]
1 if female	0.002	0.002	-0.009**	-0.004*
	[0.75]	[0.79]	[3.59]	[2.45]
Child birth month (omitted: January)				
February	0.003	-0.007	-0.003	0.006
	[0.57]	[1.59]	[0.30]	[0.94]
March	-0.005	-0.009	-0.011**	-0.005
	[1.14]	[1.90]	[3.12]	[0.83]
April	-0.001	-0.002	-0.001	0.007
	[0.17]	[0.46]	[0.19]	[1.24]
May	-0.002	-0.006	0.004	0.006
	[0.37]	[1.07]	[0.48]	[1.22]
June	0.001	0.000	-0.005	0.005
	[0.27]	[0.10]	[1.06]	[1.13]
July	0.000	0.001	0.000	0.009
	[0.07]	[0.14]	[0.01]	[1.46]
August	0.007	0.005	-0.004	0.005
	[1.13]	[1.07]	[0.37]	[0.68]
September	0.000	-0.001	-0.007	-0.001
	[0.01]	[0.27]	[0.94]	[0.19]
October	-0.005	-0.004	-0.005	0.001
	[0.92]	[1.04]	[0.84]	[0.18]
November	-0.013*	-0.009	-0.010	-0.001
	[2.19]	[1.64]	[1.25]	[0.16]
December	-0.004	-0.004	-0.001	0.002
	[0.65]	[0.71]	[0.19]	[0.29]
Child birth order (omitted: first child)				
second child	0.003	0.003	-0.002	-0.004
	[1.29]	[1.15]	[0.87]	[1.25]
third child	0.000	0.000	-0.014**	-0.006
	[0.10]	[0.06]	[3.87]	[1.72]
fourth child	-0.003	0.010**	-0.027**	-0.007
	[0.65]	[4.56]	[6.45]	[1.66]
fifth or later child	-0.005	0.017**	-0.017**	0.007**
	[1.66]	[8.44]	[3.42]	[4.60]
Maternal age at birth (omitted: age 19-24)				
Age 9-15	0.051**	0.053**	0.035*	0.048**
	[7.00]	[7.44]	[2.24]	[7.20]
Age 16-18	0.017**	0.024**	0.008	0.022**
	[5.15]	[7.21]	[1.36]	[5.67]
Age 25-30	-0.006	-0.013**	0.014	0.000
	[1.86]	[7.77]	[1.96]	[0.08]
Age 31-49	-0.003	-0.010**	0.027	0.005
	[0.43]	[5.62]	[2.07]	[0.71]

log population	-0.209	-0.205	0.448	0.127
	[1.07]	[1.16]	[1.17]	[0.62]
Mother's education (omitted: no education)				
Incomplete primary		-0.009*		-0.011
		[2.30]		[1.29]
Complete primary		-0.010*		-0.012
		[2.89]		[2.03]
Incomplete secondary		-0.013**		-0.018
		[3.74]		[2.12]
Secondary or higher		-0.023**		-0.029*
		[4.80]		[2.50]
Father's education (omitted: no education)				
Incomplete primary		-0.003		0.004
		[0.81]		[0.42]
Complete primary		-0.004		-0.011
		[0.96]		[1.21]
Incomplete secondary		-0.013**		-0.006
		[5.42]		[1.16]
Complete secondary		-0.022**		-0.010
		[8.08]		[1.09]
Higher		-0.019**		-0.012
		[4.56]		[1.34]
Household ethnic group (omitted: high caste)				
Scheduled caste		0.010*		0.002
		[2.60]		[0.45]
Scheduled tribe		0.002		-0.004
		[0.48]		[0.26]
Other backward caste		0.007		0.004
		[1.60]		[1.23]
Household religion (omitted: Hindu)				
Muslim		-0.006*		-0.020**
		[2.50]		[5.16]
Christian		-0.002		-0.014**
		[0.19]		[3.25]
other religion		-0.016**		-0.011*
		[3.19]		[2.17]
Mother's height				
log height		1.027**		0.513
		[5.51]		[2.04]
square log height		-0.121**		-0.063*
		[5.40]		[2.21]
Observations	117088	115585	35783	35584
Number of group(caseid)	36068		13414	
R-squared	0.01	0.02	0.02	0.02

Notes: Columns 1 and 3 report the mother fixed effects (FE) specification summarized in column 1 of Table 2 and columns 2 and 4 report the specification in column 4 of Table 2 in which mother fixed effects are replaced with state dummies and mother-level covariates. * significant at 5%; ** significant at 1%.

Appendix Table 3: Introducing Lags of Income

	(1)	(2)	(3)	(4)
Panel A: Rural				
income	-0.044*		-0.041*	
	[3.00]		[2.48]	
L.income	0.009	-0.004	0.005	-0.007
	[0.42]	[0.21]	[0.20]	[0.34]
L2.income	-0.007	-0.001	-0.001	0.004
	[0.42]	[0.08]	[0.07]	[0.26]
L3.income	-0.048*	-0.051*	-0.064*	-0.067*
	[3.55]	[3.55]	[4.05]	[4.08]
L4.income			0.056*	0.059*
			[4.13]	[4.33]
Long run income elasticity	-0.96*	-0.60*	-0.48	-0.13
Observations	117053	117053	116973	116973
Number of group(caseid)	36066	36066	36059	36059
Panel B: Urban				
income	-0.021		-0.017	
	[0.49]		[0.37]	
L.income	-0.017	-0.023	-0.018	-0.023
	[0.89]	[1.50]	[0.87]	[1.42]
L2.income	0.048*	0.052*	0.055*	0.057*
	[2.47]	[2.55]	[3.26]	[3.19]
L3.income	0.020	0.020	0.007	0.006
	[1.26]	[1.26]	[0.44]	[0.41]
L4.income			0.049	0.050
			[1.41]	[1.54]
Long run income elasticity	0.49	0.77	1.21	1.44*
Observations	35773	35773	35758	35758
Number of group(caseid)	13413	13413	13411	13411

Notes: Lags of income are introduced into the baseline model reported in column 1 of Table 2. L is the lag operator. The long run elasticity is calculated as the sum of the marginal effects across all income terms divided by the mean infant mortality rate in the sector. An asterisk denotes significance at the 5% level. Columns 1 and 2 include three lags and columns 3 and 4 include four lags. In columns 2 and 4, the current income term is dropped.

Appendix Table 4A
Cyclical Variation in the Composition of Births by Mother's SES: Rural Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Caste			**Mother's Education**			**Father's Education**			**Maternal age at Birth**		
	High caste	**Scheduled caste**	**Scheduled tribe**	**None**	**Primary or less**	**Higher**	**None**	**Primary or less**	**Higher**	**9-18**	**19-30**	**31-49**
(1) Baseline												
Income	-0.003	-0.009	0.042	0.048	-0.044	-0.004	0.048	-0.073	0.025	-0.013	0.022	-0.009
	[0.09]	[0.51]	[2.05]	[1.58]	[1.83]	[0.22]	[1.86]	[1.82]	[0.88]	[0.48]	[0.87]	[0.66]
(2) Add rainshocks												
Income	0.002	-0.012	0.043	0.042	-0.038	-0.003	0.052	-0.088	0.035	-0.007	0.019	-0.012
	[0.06]	[0.59]	[2.07]	[1.30]	[1.58]	[0.16]	[2.21]	[2.20]	[1.25]	[0.24]	[0.65]	[0.77]
(3) Drop state-specific trends												
Income	-0.022	-0.020	0.021	-0.084	0.001	0.083	-0.007	-0.072	0.079	-0.021	0.109	-0.088
	[0.94]	[0.85]	[1.00]	[1.37]	[0.02]	[2.07]	[0.18]	[2.36]	[1.72]	[0.75]	[3.65]	[4.38]

Notes: Panel data for 15 states in 1970-97, NT=420. Within groups estimates that include year and state dummies. The dependent variable is the percentage of births in the socio-economic group indicated in the column-head. Income is log real per capita net state domestic product. The baseline model in row (1) includes state-specific trends. The changes described in rows (2) and (3) are each with respect to the baseline model. Robust t-statistics in parentheses.

Appendix Table 4B
Cyclical Variation in the Composition of Births by Mother's SES: Urban Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
		Caste			**Mother's Education**			**Father's Education**			**Maternal age at Birth**	
	High caste	**Scheduled caste**	**Scheduled tribe**	**None**	**Primary or less**	**Higher**	**None**	**Primary or less**	**Higher**	**9-18**	**19-30**	**31-49**
(1) Baseline												
Income	0.059	-0.040	-0.026	0.097	-0.065	-0.031	0.005	-0.005	-0.000	0.006	-0.007	0.001
	[0.81]	[0.66]	[0.61]	[2.24]	[1.45]	[0.42]	[0.18]	[0.10]	[0.00]	[0.10]	[0.11]	[0.04]
(2) Add rainshocks												
Income	0.045	-0.038	-0.020	0.102	-0.067	-0.035	0.022	-0.003	-0.019	0.017	-0.014	-0.003
	[0.55]	[0.56]	[0.46]	[2.36]	[1.52]	[0.49]	[0.67]	[0.06]	[0.33]	[0.24]	[0.19]	[0.15]
(3) Drop state-specific trends												
Income	0.089	-0.032	-0.004	0.033	-0.032	-0.001	0.023	-0.054	0.032	0.080	0.004	-0.084
	[1.27]	[0.84]	[0.15]	[0.63]	[1.17]	[0.01]	[0.52]	[1.64]	[0.72]	[1.83]	[0.09]	[3.47]

Notes: See notes to Table 4A.

Appendix Table 5
Effects of Potential Mediators on Mortality Rates

AGGREGATE STATE LEVEL	
health expenditure, L3	-0.036
	[2.96]
education expenditure	-0.003
	[0.22]
development expenditure	-0.006
	[0.76]
average household consumption	0.015
	[0.93]
poverty headcount ratio	-0.011
	[0.97]
share of rural in total population	-0.129
	[0.85]
RURAL	
average rural household consumption	0.011
	[0.66]
rural poverty headcount	-0.008
	[0.89]
rural inequality (Gini)	0.017
	[1.61]
CPI agricultural workers	0.037
	[0.88]
inflation agricultural workers	0.018
	[0.52]
real agricultural wage	-0.020
	[2.40]
URBAN	
average urban household consumption	0.010
	[0.91]
urban poverty headcount	-0.001
	[0.26]
urban inequality (Gini)	0.006
	[0.93]
CPI industrial workers	0.076
	[0.89]
inflation industrial workers	0.134
	[1.80]

Notes: Estimated on panel data for 15 states in 1970-98, NT=420. The dependent variable is the state-level rural infant mortality rate. Every equation includes state and year dummies, state-specific trends, positive and negative rainfall shocks with state-specific coefficients and a set of dummies for the demographic categories used in the micro-level equations reported in Table 2. The independent variables displayed in column 1 are all in logarithms, and included one at a time, so each row is a separate regression. Robust t-statistics in parentheses. Regressions are weighted by the square root of the state population. CPI is consumer price index. Inflation is the first difference of log price (CPI). Average consumption is the state mean of household consumption from a nationwide household survey.