REDISTRIBUTION FOR HUMAN DEVELOPMENT: WELFARE GAINS AT THE COST OF WELFARE LOSS?

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INTRODUCTION

Income redistribution for human development is in vogue in mainstream development policy circles. Public-private partnerships to deliver social services to *targeted* beneficiaries are believed to be efficient and equitable and have therefore stirred up much excitement—even among neoliberal institutions (Ravallion, 2006; Besley and Burgess, 2006; World Bank, 2004). However, skeptics worry that *targeting* is at best a quasi-equitable arrangement because it bypasses those who are not living in absolute poverty but are poor nonetheless.

The debate has generated more questions than answers, and, among them, I seek to shed some empirical light on the following: Does targeting work to improve the quality of life of the poor? And do the potential welfare gains from income redistribution to the targeted poor spill over to those not targeted but still in need?

Bangladesh provides an interesting opportunity to answer these questions because of its expansive redistributive programs and recent progress in reducing infant and child mortality—a key indicator of human development—in an inauspicious structural and institutional context. Not many countries sharing as many structural disadvantages as Bangladesh has as many targeted social transfer as Bangladesh. In fact, despite legacies of authoritarian rule, inequality, and Islamic and patriarchal values and practices, the Bangladeshi state, with the GDP per capita ranging between $255 and $325 during the 1990s, has apparently played an important redistributive role with its fourteen cash and in-kind social transfers programs, and partnered with nongovernmental organizations to transfer food, health, and education services to poor

households, i.e., those that own half an acre of land or less, are female-headed, and/or with primary/secondary school-age children/girls.[1]

However, the impact of the state's redistributive role has yet to be empirically documented and the question of the relative distribution of benefits between the poor and the very (absolute) poor is unknown. Using microevidence from Bangladesh, I demonstrate that the redistribution of income augments human welfare, as expected, but that targeted redistribution might limit overall gains. In particular, I find that the very poor and the rich are relatively more successful than their *middle-income* counterparts in suppressing deaths among children. Contrary to conventional understanding, therefore, the evidence I adduce suggests that the income-child mortality dynamic is nonlinear and that welfare gains in Bangladesh, and potentially in any developing country where targeted redistribution programs are in place, are ambivalent.

HUMAN DEVELOPMENT IN POOR COUNTRIES: COMPETING ACCOUNTS

The redistribution of income and wealth to the poor through social service provision and economic growth are frequently portrayed as necessary conditions for human development in poor countries that are wracked by inequality.

" Wealthier is Healthier"

Proponents of market-based thesis hold that income growth in households and the state reduces child mortality, and thus raising per capital incomes ought to be a key strategy towards child

[1] Bangladesh runs fourteen cash and in-kind social transfers programs. Among them, Food For Education (FFE), Vulnerable Group Feeding (VGF), Vulnerable Group Development (VGD), and Female Secondary School Assistance Program (FSSAP) have recognition beyond the host country. Bangladesh has one of the pioneer primary schooling-subsidy programs—its Food-for-Education program was one of the many school-enrollment subsidy programs now found in both developing and developed countries (Ravallion, 2006); it runs one of the largest systems of targeted food transfer programs in the world (Murgai and Zaidi, 2005); and it runs a secondary school subsidy programs designed especially for women. Bangladesh spends about 10 percent of its public expenditure budget to serve nearly 4-5 million households a year with its targeted social transfer programs (The World Bank, 2006).

mortality reduction in low-income developing countries (McKeown and Record, 1962; McKeown, 1983; Pritchett and Summers, 1996; Easterly, 1999; Filmer and Pritchett, 1999; Filmer, Hammer and Pritchett, 2000, 2002; Dollar and Kraay, 2004). Child mortality decline, typically viewed as a natural byproduct of economic growth, decreases with income under the assumption that public and private spending on health inputs increases with income. In particular, with a rising real national income per capita the state increases its expenditure on health care and its correlates, and households are better able to suppress premature deaths by increasing individual purchases of health inputs, broadly defined to include food, health care, medical services, and basic education.

The influence of economic conditions on mortality has the oldest legacy among alternative accounts—dating back to the late 18th century or older. Thomas Malthus (1798) in *An Essay on the Principal of Population* proposed a relationship between food supply and mortality, as mediated by population growth. Malthus argued that the population grows exponentially until stopped by famine, plague, war, or other forms to "natural" checks; food supply and the demand for it among people returns to a reasonable balance as a result. Thomas McKeown, in the post-Malthus era, reconsiders the relationship, and argues that mortality responds to improved nutrition more than it does to other correlates of economic growth, such as advancement in medical technologies. McKeown (1976) in this famous book, *The Modern Rise of Population* (1976), compares epidemiological records from England and Wales since the mid-nineteenth century, and essentially endorses the Malthusian argument after nearly two centuries.

After nearly 20 years, Pritchett and Summers (1996) revitalized the market-based, economic deterministic explanation of childhood deaths by expanding the reference base to developing nations. The authors claimed that "wealthier is (indeed) healthier," and invoked

causal arguments that rising national income—proxied by GDP per capita—increases private and public health inputs spending and suppresses child mortality as a result. Pritchett and Summers (1996) predict that as many as 33,000 infants and 53,000 children in developing countries could be saved each year if countries were to raise income by 1 percent (p. 844). In more recent years, scholars have further shown that higher economic growth moves countries towards greater income equality and less poverty, and thereby oust the structural predicaments against child mortality (Filmer and Pritchett, 1999; David and Kraay, 2001).

Redistribution for Human Development

Alternatively the redistribution account of child mortality argues that the public provision of social programs—primary health care, sanitation, clean drinking water, and especially female education—is responsible for improved health and literacy outcomes in poor countries (Preston, 1980; Caldwell, 1986; Hill and Pebley, 1989; Anand and Ravallion, 1993; Dreze and Sen, 1991). Child mortality decline, typically viewed as orthogonal to economic growth, is possible with public provisioning of basic social services that is found to often benefit the poor in particular yet are available to ALL. The redistribution accounts are more consistent with rapid child mortality declines experienced low-income countries—Costa Rica, Sri Lanka, and Kerala (India) than the economic growth accounts.

The redistribution account thus challenges the "Wealthier is Healthier" account of mortality reduction, and questions its premise based on (a) the historical and contemporary empirical evidences, and (b) over-assumption about market effectives in health. Scholars argue that McKeown does not present any direct evidence on nutrition of individuals to test this thesis (McKeown 1976, p: 130; Easterlin, 1999), and the age-old association between growth-induced

nutritional improvement and mortality reduction remains open. Stolnitz (1965) highlights a seemingly unrelated trend between economic development and mortality in Asia, Latin America and Africa, but it is Samuel Preston whose analysis poses the biggest challenge to the economic deterministic explanation of mortality. In what is commonly known as the "Preston Curve", the author shows that at a fixed income, its association with mortality shows temporal variation, and at a fixed period in time, the association changes spatially. Considering the presence of a potential third factor behind precipitation across time and space, Preston argues that economic development and mortality are far from being causally related.[2] Among the more contemporary scholars, Anand and Ravallion (1993) show that the association between economic growth and child mortality depends on public policies to reduce poverty and/or improve health and health correlates. And, perhaps the most comprehensive critique is Caldwell's (1986) correlation analysis between GDP per capita and child mortality rates, and his comparative historical analysis of Sri Lanka, Kerala (of India) and Costa Rica to show that economic development is not a necessary condition for mortality breakthrough.

Targeted Redistribution for Human Development

In recent years, targeted social transfers have stirred up excitement—even among neoliberal institutions as an alternative to transfers to all or universal transfers to best reduce intergenerational poverty and income inequality. And, in the process, scholars and development practitioners engage in the following long debated question: should only the poor be qualified to

[2] In particular, Preston (1975) shows that (a) individuals lived longer in 1960s than 1930s at comparable levels of economic development, and (b) at a given year, economic development has a closer association with mortality in the countries with per capita GDP $400 or less than those with per capita GDP of $600 and above. Preston believes that that mortality increasingly dissociates with economic growth (p. 231), and that only 16% of the increase in life expectancy between 1938 to 1963 can be attributable to increase in GDP growth (p. 238).

receive transfer benefits, or should every citizen be assured as a social right of "government-protected minimum standards of income, nutrition, health and safety, education, and housing" (Wilensky, 1965: xii). While non-economists in general support universal transfers on the ground of greater equality, economists as well as development practitioners and policy makers support targeted transfers on the grounds that universal social transfers negatively affects labor supply and savings—and thereby is detrimental to economic growth, and that "a comprehensive approach to poverty reduction…calls for a program of well-targeted transfers and safety nets" (World Bank, 1990: 3). However, universalism is often critiqued even among its supporters as they believe equality via universalism is less likely to take hold as long as universalism ensures earnings-related—as opposed to flat-rate—benefits (Castles and Mitchell, 1992). And, it critiques argue that universalism is best suited to maintain—instead of reduce—income inequality and poverty. The critiques argue that the nonpoor's participation is not only a waste of resources but also counterproductive in that "the more non-poor benefit, the less redistributive (or, hence, egalitarian) the impact of the welfare state will be" (Goodin and Le Grand, 1987: 215).

On practical (empirical) grounds, targeted social transfers are believed to be as equitable as universal provisioning, more efficient than either universal or market transfers (Ravallion, 2006; World Bank, 2004). Transfers for all has far-reaching human developmental gains; yet scholars doubt gains in actuality as the big administration is often plagued by maintenance costs, local capture, and leakage, absenteeism among other metrics of inefficiency. Unannounced visits to primary schools and health clinics in Bangladesh, Ecuador, India, Indonesia, Peru and Uganda show that 19 percent of teachers and 35 percent of health workers were absent. Moreover, one-quarter of government primary school teachers in India were absent from school, but only about

one-half of the teachers were actually teaching when enumerators arrived at the schools (Chaudhury et al., 2006; Banerjee and Duflo, 2006). Anecdotal evidences from Bangladesh suggest local captures of redistribution programs. Hartman and Boyce (1983) talk about how rich local farmers captured a publicly provided local irrigation facility intended for poor farmers. Un Nabi (1999) talk about the local power structure and how local elites often consulted when a development project is undertaken in community. This says that transfers can often bypass the poor considering imbalances in economic, social, and political power between service providers and recipients, and thereby the lack of ability among the poor hold public officials accountable.

Efficiency might still be possible if those bureaucrats at work, or those refraining from leaks generate enough human development gains to offset absenteeism or leakage among fellow bureaucrats. But, speculations such as these remain just so, and comparability across cases do not necessarily offer robust understanding.

Nonetheless, targeted transfers have gained traction on the grounds of higher efficiency, and lower leakage than the alternatives (Ravallion et al., 2006; van de Walle, 1998; Besley and Kanvur, 1993). Even the World Bank sidetracks from only economic growth to social transfers in addition to economic growth as route to poverty reduction (The World Bank, 1990, 1997; 2000, 2004; Lyn Squire, 1993; Birdsall and Londono, 1997; Besley and Kanbur, 1993; Mosely et al., 2002; van de Walle 1998). Scholars argue that the poor are not homogeneous—some are more poor and vulnerable to personal and natural calamities than others. Cost-effectiveness in targeted transfers is possible by channeling limited state resources to those in the greatest need. Targeting does not necessarily solve leakage, absenteeism or other institutional failure, and decentralized, community based service delivery are proposed as institutional checks. As such, partnerships between public and private (not-for-profit) organizations are believed able to oust

leakage, absenteeism, and poor quality of service among public officials serving targeted beneficiaries (Shleifer, 1998; Besley and Ghatak, 2001; Besley and Ghatak, 2007).

Targeted transfers have generated much debate about prospect for human resources development. Targeted transfers are more than only supply-side interventions (like transfers for all); the most common forms of transfers create parental demand for children's human resources development (for example, Food For Education, Conditional Cash Transfers). Transfers thereby combine components of human capital into a single transfer mechanism (education with nutrition in Food For Education, education with income in Female Secondary School Assistance Program, and education, nutrition, and health in Conditional Cash Transfers). Their interventions at particular points in life-course have far-reaching human development consequences. Such as, FSSAP in Bangladesh makes cash stipend available for secondary school-age girls and thus prevent girls from dropping out of school and perhaps getting married. Finally, demographic and social externalities are sizeable in (conditional) targeted transfers. FFE not only increases schooling but also suppress child labor; FSSAP increase girls' age of marriage and child-bearing as well as schooling

Nonetheless, "Targeting is almost never costless" (Van de Walle, 1998: 232). Wilensky (1965) argued that the debate about effectiveness of social transfers goes beyond whether or not targeted social transfers are more efficient and/or equitable than its universal counterpart. Wilensky argued targeted social transfers could have a more fundamental and often political implication in that transfers can serve public officials' political aims. Citing France's Family Allowance Program to increase fertility, he talked about how the program had produced a pronatalist clientele organized to lobby for benefits (p. 114). Second, some argue that gender-based targeted social programs tend to disadvantage women in that, among others, spouse and

widow benefits may justify unpaid domestic work, transfers can reduce the capacity to form or maintain personal autonomy by insulating women from the pressure of the labor market (Harrington Meyer, 1996; O'Connor, 1993; Orloff, 1993). Moreover, transfers to the poor are often viewed as a structural adjustment of social policies. They are criticized as a broader continuation of economic reforms in developing countries in the 1980s and 1990s, and that social transfers to the poor are "compatible with the logic of the market" as the state interventions are restricted to the neediest section of the population to avoid "distortions in relative prices" (Lomeli, 2008).

On practical grounds, targeted transfers share similar critiques as its universal counterparts, and some more with regards to "targeting". Perhaps the biggest drawback of targeted transfers is its high reliance on "successful" targeting of the poor, and especially when broad-based survey responses—as opposed to context-specific local knowledge—are utilized. As such, Ravallion (2006) cautions about using conventional poverty covariates to target the poor in actuality. He fails to explain more than half the variance in consumption or income across households using even the most comprehensive, high quality survey. When income proxies err in assessing the actual income, the basis of targeting is questioned.

Second, targeted transfers share similar criticisms as universal transfers. Skeptics worry that transfers discourage labor supply and savings, which negatively affects economic growth. Institutional inefficiency does not wither away with targeted transfers. Moreover, evaluations of targeted transfers are rarely embedded in the broader developmental contexts. Do targeted transfers work by themselves? Do they depend on universal targets to take effect? For example, in addition to universal primary education, Bangladesh initiates FFE program. And, educational

payoffs from FFE in isolation, especially if FFE model is exported to alternative developmental context might be contested.

Finally, even when targeting reaches those in the greatest need, universal welfare might be an issue due to not only alleged quality-quantity tradeoff, but also possible welfare contraction among those not living in absolute poverty but are poor nonetheless. As such, transfers are criticized to achieve less in quality in pursuit of achieving more in numbers among the targets. Ahmed and Arends-Kuenning (2006) find that FFE in Bangladesh has increased, as intended, enrollment, especially among primary-school age girls. However, performance, measured with test scores, has not corroborated the rate of enrollment especially among FFE non-beneficiaries.

RESEARCH METHODS

Data, Variables, and Measurements

I employ surveys conducted by the Bangladesh Institute of Development Studies and the World Bank (BIDS-World Bank) in 1991/92 and 1998/99. In addition to providing detailed information on employment, income, and expenditures, the surveys include a module on marriage and pregnancy history for all women between 12 and 50 years; land ownership; food and non-food expenditures; participation in agricultural or nonagricultural employment; as well as data on participation in rural financial services and the amount of credit borrowed. Information on family planning program participation, religion, and education in governmental and non-governmental educational institutions is also available. And the survey documents a diverse set of village-specific attributes --i.e., prices of staple food items, wage rates for male, female, and child labor, availability of state-led employment programs (food-for-work, road construction), government

and non-governmental food programs, NGOs, formal financial institutions, markets/*haat* in the village, and health and education facilities-- that allow me to understand the interrelationships of community-level and household characteristics. Of the village-specific attributes, food and health program availability is particularly important for child health, and while surveys include questions to gauge their availability, the survey questions differ in specificity between the first and the last round. For example, the 1991/92 surveys asks: "Is there any government run food programs in the village?" The 1991/92 survey therefore does not differentiate among the available government food programs by early 1990s (VGF, VGD, FFW), and the respondents were likely to answer to this question positively if VGD, VGF, or FFW is available in the village—however small its scale and scope of operation might be during the time of survey. And, the 1998/99 survey asks: "Is there any Food-for-Education project in any of the village schools?" The 1998/99 survey thereby asks about government's specific food program which started in 1993 after the 1991/92 survey, and the respondents were likely to answer to this question positively if ONLY FFE is available in the village. Nonetheless, these all are government food programs, I can crudely compare health status among children living in villages with and without them in a single survey year (1991/92 or 1998/999), and over survey-years (1991/92—1998/99).

The surveys include 1,798 and 2,599 households—1,638 of which were surveyed in both periods—from 87 villages of 29 *thanas* across Bangladesh. Evidences from same households over time allow getting a better understanding of "causal" variations in child survival prospects due to variations in explanatory variables than cross-sectional evidences do.[3] To note, parental and village attributes typically change only slowly, if at all, in general, and prior literature on child health or its indicators, more broadly, reports findings using the survey data from a single

[3] A thana is an administrative unit, which is smaller than a district but bigger than a village; a thana consists of a number of villages. Bangladesh's administrative units are divided into: Divisions (N= 6), Districts (N= 64), Thanas (N=507), Unions (N=4484), Villages (N=59,990), and Households (N=25,362,321).

year (Strauss, 1990; Thomas et al., 1996; Barrera, 1990; Frankenberg, 1995; Maitra, 2004, Rosenzweig and Schultz, 1982; Haughton and Haughton, 1997; Jalan and Ravallion, 2003; Suwal, 2001; among others; See Rosenzweig and Wolpin, 1988; Edmonds, 2004; Fedorov and Sahn, 2005; Cebu Study Team, 1992; Wagstaff and Nguyen, 2002 for exceptions). However, based on the review of targeted social transfer programs, its availability in villages is likely to change in scale and scope, and effects of parental attributes on child mortality can alter with and without targeted social transfers. Panel data thus is a better option under this assumption.

The households sampled in both periods are overwhelmingly poor. More than half of the households are functionally landless in both waves with an average income per capita per week of 81/127.5 *taka* (equivalent to $1.5/$2) and a landholding of 0.68/0.66 acres in 1991-92/1998-99.[4,5]

Variables

I estimate household determinants of child mortality using households' economic status, social status, religion, and demographics, and control for child's sex. I measure income alternatively with expenditure per capita and land ownership.

Child mortality: I use mortality among children born during the ten years preceding the surveys using the respondents' answers to the following questions. In both rounds, the surveys ask a woman respondent to: (a) list her pregnancy order, (b) identify if pregnancy resulted into a child-birth or otherwise, (c) identify the gender of the child if born alive, (b) state date of birth of the child, (d) state if the child is still alive? Given that the data on mortality presents that as a rare event if the survey is not the Demographic and Health Survey and therefore does not have large

[4] The 1998-99 measure for income is not adjusted for inflation.
[5] Households spending 117.28 taka and 158.62 taka or less per person per week are considered poor in 1991-92 and 1998-99 respectively.

enough sample size. To deal with this, I select ten years preceding the surveys to allow the mortality measure to contain enough variation for meaningful inferential analyses while allowing as much as possible the current data on mortality's structural covariates to represent those data from the actual time of mortality.

Income (in Taka): The BIDS-World Bank survey asks about expenditures on specific food and non-food item in the last week and year, respectively. I calculate the food expenditure by adding all expenses on food items in the last week, four times a week. And, I calculate the non-food expenditure by adding all expenses on nonfood items in the last week, divided over twelve months. I calculate the food and non-food expenditure as normalized to be a monthly estimate of total expenditures. I divide the sum by the total household members, and include the logarithm of hence per capita expenditure. The survey uses a comprehensive (i.e. standardized for all) matrix of usual food and nonfood items (and allows respondents to specify "others" expenditures). This should suppress recall bias.

Land (decimals): I measure land-ownership by adding irrigated and non-irrigated land households own. I include the logarithm of land in the analysis.

Maternal education (years): Maternal education is based on years of completed formal education.

Education among the oldest male member in the household (years): The oldest male in the household should be, but not necessarily, child's father. This is thus a proxy for paternal education, and should be a close correlate. The survey does not ask, and thus readily provide, information about children's father since the child health module in the survey asks questions only about mothers and children. I could secure parental education by matching mothers' information from the child health module to her information in the background module, but this

effort seems a hard job with limited gain considering the role of paternal education on child health or mortality. And, considering that decisions in rural Bangladesh are household as opposed to individual productions, I don't expect paternal education to resume effects different from this proxy measure.

Demographics: I include (a) the logarithm of household size as a quantitative attribute of demographic profile of households, and (b) ratios of male and female members in certain age-brackets to the total household size as a qualitative attribute of demographic profile. While household size is straightforward in concept and measurement, the ratios are motivated by (a) shortcoming in the traditional calculation of dependency ratio in the context of Bangladesh, and (b) economic role across age and gender groups has special significance in the development context of Bangladesh. First, the measurement of dependency ratio (DR) as commonly referred to as: $DR = \frac{Population \quad \leq 14 \ years \ + Population \quad \geq 65}{Populaiton \quad 15 - 64 \quad years} \times 100$ does not really capture dependency and therefore defies the purpose since children in rural Bangladesh village is economically active from the age of six Cain (1977). Jacoby and Skoufias (1997) find similar trend in South Asia at large where child labor is found to help smooth the income of rural Indian families. Scholars commonly disaggregate male and female household members into numerous age groups (less than 5 years, between 5 and 9, 10 and 16, 17 and 40, and above 40) and find the ratios of each group to the total household size (Ravallion and Wodon, 2000; Wodon, 2000). Alternatively, I calculate dependency ratio with $DR = \frac{Population \quad \leq 6 \ years \ + Population \quad \geq 65}{Populaiton \quad 15 - 64 \quad years} \times 100$ and expect this to capture the nature of dependence in the context of rural Bangladesh.

Estimation Strategy

The estimation strategy is no more complex than is needed to demonstrate (a) the income-welfare dynamic in the context of expansive redistributive interventions in rural Bangladesh since the mid 1980s, and (b) though suggestively, why this relationship deviates from the conventional wisdom, if indeed it does. In particular, I estimate a linear relationship between household income and child mortality outcomes and expect a ***negative*** income coefficient in that prospects for child mortality tend to diminish monotonically with an increase in income (Equation 1). If I find that the relationship is not negative, I include income-squared in Equation 2 and examine the possibility that prospects for child mortality could portray a curvilinear association with income. I estimate Equation 3 if the observed association between income and child mortality is indeed nonlinear, and use it to bring evidence to bear on the possibility that very poor households are targeted for nutrition, education, and/or health interventions, and, as a result, (a) the children in the targeted households and better off households have comparable survival prospects and (b) children in relatively poor but plausibly non-targeted households have higher mortality than those in either the very poor (targeted) or rich households.

[1] $Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 Z_{it} + \varepsilon_{it}$

[2] $Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 X_{it}^2 + \beta_3 Z_{it} + \varepsilon_{it}$

[3] $Y_{it} = \beta_0 + \beta_1 X_{it} + \beta_2 X_{it}^2 + \beta_3 Z_{it} + \beta_4 J_{it} + \beta_5 X_{it} \times J_{it} + \varepsilon_{it}$

where Y_{it} is the mortality outcome of the child *i* at time *t*, and X_{it} is household income in which the child *i* is born, and X_{it}^2 is the polynomial construction of the income variable, *Z* is a vector of control variables based on their robustness as distal determinants of child mortality—parental education, households demographic characteristics, and Islamic belief, *J* is redistribution

programs—food and health programs—in the village *j* at the time *t is* the time of the interview, namely, 1992 and 1999.

To present robust parameter estimates, I (a) run cross-sectional and panels models using data from each survey year and over time; (b) subject my analysis to alternative—Weibull and PROBIT—estimation strategies (and present the Weibull estimates given their appropriateness in dealing with mortality data and overall consistency with PROBIT estimates); and (c) correct for the possibility of heteroskedasticity in standard errors and therefore conduct meaningful significance tests. The Weibull model estimates the probability of mortality among children born during the ten years preceding the surveys conditional on the covariates in equations 1, 2, and 3. For all children, the surveys include the age at death or age at the survey date if the child is still alive and are therefore right censored. The Weibull functional form makes sense theoretically as well as empirically since hazard for death is a decreasing function of age among children, and the model estimates independent variables using a proportional hazards specification.

The standard errors are robust, and the significance tests are therefore based on heteroskedasticity-consistent estimates of the variance-covariance matrix. In particular, variance-covariance matrix is corrected for (a) heteroskedastic and (b) clustered (within 104 villages) residuals.[6] Although children are the units of analysis, the fact that (sampled) children live in households, and households are available in villages, and failure to adjust for this hierarchical structure in data could plague analysis. Random sampling of clusters implies that clusters are "independently and identically distributed" (*iid*) (inter-cluster correlations = 0), yet random

[6] Both are common in the survey data, and clustered residuals are more common in rural Bangladesh. Heteroskedasticity occurs from deviation of each household from (aggregate/all) household mean due to that particular household's unique conditions, known to households, unknown to researchers. Clustered residuals occur when households living in a same cluster behavior similarly among household living within that cluster and differently from households living outside clusters due to cluster-specific attributes, unknown to researchers through survey instruments.

sampling of households does not eliminate the fact that households within clusters somehow correlate (due to unobserved cluster effects). Jackknife variances are clustered over villages, and I use those to tackle some sources of *iid* violation.[7]

Nevertheless, the following methodological constraints could confound findings. Firstly, errors almost always plague measures of household income using survey data from developing countries like Bangladesh, and a consequent systematic difference between actual and observed household income poses a credible threat to the analysis. I use total household expenditure per capita per week and land ownership as alternative measures to estimate the income effect.[8] While both are commonly used as indicators or predictors of *permanent* household income, land ownership captures household wealth, which is a key predictor of income and is often the basis of targeting for redistribution programs.[9] I have logarithmically transformed both variables to correct for possible skewness and to attain normalcy in the distribution of income, and identify effects on child mortality as income increases by a percent regardless of initial income. More fundamental problem perhaps is the fact that income measures are from the survey years, and mortality incidences are from prior years. With this, I assume household income stays statistically unchanged from the years when children died till the years when the surveys were conducted. I therefore make an assumption, which may or may hold in empirical reality. Secondly, the fact that data on redistribution programs is available by village whereas that on households' program participation is not prevents the optimal test of the nonlinear association between income and child mortality. The optimal estimation requires an interaction effect

[7] Studies identify several routes to inter-household correlation within villages: (a) household exposure to identical amenities in villages, (b) households' selective migration to villages with certain amenities, and/or (c) households seeking community approval adapt behavior and utilization of village amenities.
[8] In addition, I conduct a descriptive analysis of the distribution of the key income correlates across income groups and check for external validity in income measures. I find that the average years of education among mothers, fertility rates, etc. are of expected magnitude across income quintiles.
[9]Especially where the credit market is absent or inaccessible

between household income and program participation when income correlates are controlled and household and village (perhaps selective) participation in redistribution programs are accounted for. Since program participation is not measured at the household level, I instead conduct an interaction analysis between household income measures and the binary outcome for village participation in health and food (redistribution) programs.

REGRESSION RESULTS

The Income Effect

Tables 2-5 report Weibull estimated effects of household determinants on the mortality outcome for the children, born during 10 years preceding the surveys using cross-sectional evidences from 1991-92 (Columns 1 and 2) and 1998-99 (3 and 4), and panel evidences (Columns 5 and 6). [10]

Household income has a significant and ***positive*** effect on the probability of child mortality when parental education and proportions of dependent children are controlled (Column 1 in Table 2). A 10 percent increase in household income, for example, speeds up the median time to mortality by a 0.3 percent.[11] Let's note, the 1991-92 Weibull income estimate maintains a positive sign, and this estimate is significant. This suggests household income has a ***positive*** effect on child mortality after I control for variation in household's education attainment, demographic attribute, and religious affiliation. And, income's positive effect on mortality is significant at α=0.01 (Column 1 of Table 2). The magnitude of the effect is also sizeable. A 10 percent increase in household income, for example, speeds up the median time to child mortality

[10] The analysis includes children aged 10 years or less at times of surveys. By doing so, I respond to the constraint I face from not having enough variability in the outcome variable as I use the survey that provides rich information on the explanatory variables of my interest but is not primarily intended to assess child mortality as thoroughly as DHS, for instance. I therefore stretch the population from its actual space—children aged 5 or less (infants and children under 5). However, I follow the fix, commonly practiced in empirical research on child mortality in low-income developing countries facing similar constraints (Lavya et al., 1996).

[11] We know, $S(x)=\exp[-h(x)]$, where $S(x)$ is the time-to-event(mortality) function and $h(x)$ is the hazard rate. The STATA output indicates that the hazard rate for income is 1.12, and based on that I calculate this coefficient.

by 2.4 percent. Household income coefficient retains it positive sign when I re-estimate the baseline specification in Equation 1 with Probit modeling technique (Column 2 of Table 2). The 1998/99 Weibull income estimate also maintains a positive sign, and this estimate is significant. This suggests household income has a ***positive*** effect on child mortality. Income effect is robust to alternative Probit estimation. Columns 5 and 6 of Table 2 present Weibull and Probit parameter estimates using household survey data from both 1991/92 and 1998/99. Parameter estimates based on these panel evidences are consistent to those from each round of the cross-sectional evidences from 1991/92 and 1998/99. In substantive terms, children in poor households have *higher* survival likelihood than those in rich households.

Among controls, maternal education, dependency ratio, and Islamic belief have significant impact on the child probability of death in rural Bangladesh in the 1990s. With regards to impact based on the 1991/92 survey data of each of these household characteristics, maternal education confirms the prior finding in that maternal education has a negative and significant effect on child mortality (Column 1 of Table 2). One more year in maternal education reduces median time to a child's death by a 0.4 percent. [12] Education of adult male members in the household has no significant impact on child mortality, however. Household size has a significant and negative impact on child mortality. As such, a 10 percent increase in household size reduces the median time to child death by 2 percent. In substantive terms, between the two same-sex children whose parents have identical levels of income and education, one would have the lower likelihood to death than the other if it lives in a household with more members. Finally, household's Islamic belief has a negative but insignificant impact on the child probability of death. This suggests Islamic belief neither accelerates nor delays death. Among the child-specific

[12] We know, $S(x)=\exp[-h(x)]$, where $S(x)$ is the time-to-event(mortality) function and $h(x)$ is the hazard rate. The STATA output indicates that the hazard rate for education is 0.952, and based on that I calculate this coefficient.

attributes, I include only child sex due to its availability in the survey data. And, I find child sex coefficient to take on a positive but statistically insignificant coefficient. This suggests preferential treatment, if there is any, towards male children in the household is not high enough, and female as well male children face comparable likelihood to live or die.

What does explain household income's positive effect on mortality? Respondents' bias in recalling income information could play a role. Therefore, I re-estimate the baseline specification with household's land ownership as an alternative and perhaps more robust measure of income in Table 3. As such, Ravallion and Sen (1994) find strong collinearity between landownership and poverty in that landless households have a high poverty in rural areas in Bangladesh (Ravallion and Sen, 1994).

Table 3 presents land ownership's estimated effects, in addition to effects of other explanatory variables, on child probability of mortality using the cross-sectional data from the 1991-92 survey (Columns 1 and 2) and the 1998-99 survey (3 and 4), and the household survey data in both rounds of 1991/92 and 1998/99 (Columns 5 and 6). Land coefficients are positive when they are significant (Columns 1, 2, 5, and 6). And, land effect on child probability to death is robust to alternative Weibull and Probit estimation techniques.

While I find counterintuitive patterns of association between several explanatory variables and child mortality, nothing seems to be as surprising as income's positive effect is on child mortality. Household income's positive coefficients are nearly impossible to explain in substantive terms in that it is difficult to imagine why children from rich households should die at higher rates than those from poor households. Additionally, even an insignificant income effect is no less significant as a finding especially in the context of rural Bangladesh where

poverty is widespread and any increase in income should retain a significant and ***negative*** effect on child mortality (Anand and Ravallion, 1993; Preston, 1975; Deaton, 2002).

The Income-Mortality Dynamic

Although significant, the coefficient on income is nearly impossible to explain in substantive terms in that it is hard to imagine why children in rich households should die at higher rates than those in poor households. To further investigate this association, therefore, I include income and its square along with controls in Column 2, and explore a possible nonlinear association. Using the 1991/92 survey data, I find maternal education, household size and dependency ratio are inversely associated with child mortality. This finding is consistent with my previous findings in Tables 2-3, and a causal speculation based on this association suggests while maternal education reduces child probability of mortality as do household's size and dependency ratio. And, when income enters the model in the first-order monotonic and second-order polynomial functional forms (as the level and the squared terms), income coefficient remains positive but its squared-term takes on a negative coefficient value (Columns 1-2 in Table 4). While the Weibull and Probit estimates of income and its square are individually insignificant, a chi-square test of joint significance of income and its square has a probability of 0.026 (Column 1 of Table 4), and 0.021 (Column 2 of Table 4). This suggests (a) as a set, income and income-square are associated with child mortality, and (b) household income maintains a concave, nonlinear relation with child mortality. In other words, a child from a poor household has a higher likelihood than a child from a "middle-income" household and as high likelihood as a child from a rich household to avoid premature death.

Columns 3 and 4 of Table 4 present Weibull and Probit parameter estimates of Equation 2 using the 1998/99 survey data. When income enters the model in the first-order monotonic and second-order polynomial functional forms, income coefficient remains positive but its squared-term takes on a negative coefficient value. But these coefficients are neither independently nor jointly significant (Columns 3 and 4 of Table 4). A chi-square test of joint significance of income and its square has a probability of 0.136 (Column 3), and 0.159 (Column 4). In other words, a child from a poor, middle-class, or rich household has the similar likelihood to live or die. Columns 5 and 6 of Table 4 present Weibull and Probit parameter estimates of Equation 2 using the survey data from 1991/92 and 1998/99. I find chances are significantly low that children would die prematurely when their mothers are educated, they live in households with large number of dependent members, and are Muslims. And, when income enters the model in the first-order monotonic and second-order polynomial functional forms, income coefficient remains positive but its squared-term takes on a negative coefficient value. These coefficients are independently and jointly significant in the Weibull estimation (Column 5), but not in the Probit Estimation (Column 6). A chi-square test of joint significance of income and its square has a probability of 0.000 (Column 5). This finding confirms those from the 1991/92 survey data that children in very poor and rich households have a lower probability to die prematurely than those born in middle-income households.

I re-estimate Equation 2 and present in Table 5 Weibull and Probit estimates of household land ownership as an alternative measure of household income. Results are consistent.

The Nonlinear Income-Mortality Relationship: Explanations

Can the relationship between income and child mortality be nonlinear due to targeted transfers? The direction between the income-child mortality relationship can range from a negative (income$\uparrow \rightarrow$ child mortality$\downarrow$) to nonexistent based on the scope or scale of transfers for all. However, this relationship is non-linear only when the rich and the poor are similarly likely to prevent child mortality, and more likely than those not-so-poor-or-rich. While the rich can buy health inputs with private income, the poor can get access to health inputs via targeted transfers and thereby safeguard children's deaths from nutritional deficiency, ignorance, and other difficulties. This assumes that the middle-income households with neither enough private income nor access to transfers generally fail to suppress child deaths as well as the targeted poor or the rich.

Do the poor households in Bangladesh have access to the social transfers, which are beneficial to health directly, or beneficial to health indirectly via social transfers' effect on health correlates? The targeting principals that social transfer programs follow would suggest so.

In Bangladesh, targeting is land-, gender-, and/or age-based. Land is the most robust poverty covariate in Bangladesh and South Asian at large, and is used to select the poorest among the poor in all social transfer programs in Bangladesh (Ravallion and Sen, 1994). In particular, households that own up to half an acre of land are considered functionally landless and poor, and household becomes a target to receive social transfers if it owns half an acre of land or less. Alternative explanations of income-mortality non-linearity include poor households' access to a microfinance program and the state's universal education, and maternal and child health services. The state spends 2.2 percent of GDP or 15 percent of the public budget on education, and 40 percent of education spending is devoted to primary education (Glinskaya,

2005). And, the maternal and child health components of the state's health policy appear beneficial to the poor in that 20 and 23 percent of public expenditures on, respectively, maternal health (pre-natal, post-natal, and family planning services) and child health (Oral Rehydration Therapy, immunization) has been spent on the poorest single quintile of the income distribution in 2000 (Glinskaya, 2005). However, considering universality in access among the poor and non-poor, health and education services cannot explain children's differential propensity to die across different income-groups. And, if any, the poor should do worse considering that the poor usually receives worse public services (Keefer and Khemani, 2004) Finally, poor households' participation in the microfinance programs as an alternative explanation is contested on the ground that microfinance programs are often criticized for bypassing the poorest of the poor (Rahman and Razzak, 2004).

Results from interaction analyses are revealing, and, despite methodological constraints, suggest that targeted social transfers can explain the non-linear association between income and child mortality (Tables 6 and 7). Coefficients for the interactions between households' income and residence in village having a food-program are positive and significant at α=0.01 using the 1998/99 cross-sectional survey (Columns 3 and 4 in Table 6), and panel evidence (Columns 5 and 6 in Table 6). A positive coefficient suggests that, for all villages, the income-mortality association is not the same, and this association is "more" positive in village with a food program than that in village without a food program. In other words, income effect on child mortality depends on availability in village of the food program. Children born to very poor households have a higher probability of avoiding untimely deaths than those born to relatively less poor households ***when*** very poor households live in a village where the food-program is available. Food transfers seem to have been effective in offsetting the economic constraints very poor

households face in child mortality. Let's note that interaction coefficients in the 1998/99 cross-sectional models are statistically significant, whereas those in 1991/92 are not. This might speak to a possible food-program's lagged effect on reducing child mortality since social transfer programs are often placed in the relatively poorer villages.

Positive interaction effects might seem rather counterintuitive. However, they make sense under the following assumptions/empirical grounds. First, poorer households receive food whereas relatively less yet absolutely poor households are left out from the food-programs. Second, local capture, or leakage of food-subsidies to non-targeted households seems not to take place as often as ethnographic evidence from the early 1980s' rural Bangladesh or more recent quantitative accounts suggest (Ravallion and Wodon, 2000; Ahmed and Del Ninno, 2002; Hartmann and Boyce, 1983). And, third, less poor households in food- program villages are perhaps still poor in absolute terms (Ravallion and Wodon, 2000; Ravallion, 2006). I find support of prior assessment that food programs are generally available in impoverished villages using the 1998/99 evidence, and I find partial support using the 1991/92 evidence. As such, in food-program villages, households' average per capita expenditure is Taka 113.5 per week, which is significantly lower than the average per capita expenditure of Taka 130.2 per week among households living in non-food program villages (t =5.49; p-value<0.001). The 1991/92 evidence shows that households' average per capita expenditure is significantly higher in villages with food programs (t = 3.79; p=0.01). However, standard deviations for per capita income are higher among households living in program compared to non-food program villages (49.20 vs. 46.29 in non food-program villages). And, the maximum per capita income is higher among households in "No" compared to "Yes" food-program villages (353.9 vs. 710.7 in non food-program villages). Taken together, the 1991/92 descriptive trends suggest that although

households' maximum per capita income is lower in inter-group comparison, but "high" enough in comparison to neighbors' average per capita income to have inflated the average, and, via that, its dispersion from the average. High average income among households living in food-program villages thereby under-represents a low average income among households in food-program villages, and provides a "revealed" endorsement that food programs tend to be available in relatively poor villages.

Income attributes and exposure to targeted social programs jointly determine child mortality differentials, and explain a significant portion of variation in child mortality in the 1990s. An inverted-U relationship still holds across estimations in that income maintains a positive sign in its level and a negative sign in its quadratic form (Columns 1-6), and is significant even when interactions enter as a control in panel specifications (Columns 1, 5 and 6). Also, a chi-sq test of joint significance shows that probability values are low for this block of variables—income, income-squared, food and health programs, and their interactions.

Notably, mortality is considerably lower among children whose parents reside in the village with targeted food- or heath- programs. In particular, children born to the households living in food-program villages have a lower likelihood of mortality than those born to households living in non food-program villages. This effect is significant using cross-sectional evidence in 1998/99 and panel evidence. Considering that food programs are available in impoverished villages, programs' negative coefficients are notable. This suggests, these programs seem to have a rather immediate as opposed to the similar lag—and positive—effect prior studies identified between village-level availability of social programs (placement based on endogenous/low human development conditions) and human development (Rosenzweig and Wolpin, 1986). And, food program's large and more significant effect in late than early 1990s is

attributable to the shift from all to land-, and demography-based targeting criteria in food program (Columns 1-2 vs. Columns 5-6).

Interactions between households' per capita income and residence in villages having health programs maintain a positive direction of association. However, this interaction secures less statistical support than interactions between income and village availability of food program. Village health programs are not targeted towards particular income- or demographic-groups. Village availability of health programs maintains a consistent negative effect in direction of association across specifications using alternative cross-sectional and panel estimations (Columns 1-6 in Table 6). However, health program effects are never statistically insignificant. A similar pattern is evident when income interacts with village availability of health program. In particular, these interactions do not secure enough statistical power to suggest that income effects are mediated as much by the availability of health programs as they are by the availability of food-programs (Columns 1—6 in Table 6). A crude comparison of child survival prospects between villages with and without food and health programs is warranted, and this suggests that targeted food program seems to have a greater and more immediate impact on child survival than health program in rural Bangladesh (Table 6).

I re-estimate in Table 7 interactions between households' land ownership as an alternative income measure and residence in food- and health-program villages. Direction of associations remains consistent regardless of the fact that I have used alternative income measures. The coefficients for the interactions between households' income and residence in village having food-program are positively significant in the 1991/92 cross-sectional model (Columns 1 and 2 in Table 7), and are positively significant at α=0.10 in the panel model (Columns 5 and 6 in Table 7). A positive coefficient suggests that the land-mortality association

depends on the food program availability in villages, and is "more" positive in village with food program than in village without food program. In other words, children born to households with less land have a higher probability of avoiding untimely deaths than those born in households with relatively more land ***when*** households live in village with food-program. This hints to the fact food subsidy is targeted to the poor households, and food transfers seem to have been effective—though seemingly intermittently—in attenuating economic constraints the land-poor households face in suppressing child mortality.

The assumption that food programs are available in impoverished villages finds more direct support in the 1998/99 survey evidence than it does in 1991/92 survey evidence. Difference is considerable in the average land-ownership between households living in villages with food program and households living in villages without food program. In 1998/99 average land-size among households living in village with and without food-programs is 60.7 and 67.2 decimals, respectively.[13] However, this difference is not statistically significant at the conventional level (t =1.57; p-value=0.12). The 1991/92 survey evidence shows that households' average land ownership is higher in food-program than non food-program villages. Also, the average deviation from households' mean landownership is higher in food-program villages. In particular, households' average land-size is 101.6(63.10) decimals with a standard deviation of 445.2(156.4) decimals in food-program (non food-program) villages in 1991/92. Over-dispersion (i.e. the standard deviation exceeds the mean) in land distribution among households in food-program village is clear. This suggests that the parametric measure of the average land-size (a) does not represent the bulk of land-poor households in food program-villages, and (b) is swayed by a few households owning a large amount of land. In sum, land-ownership is not normally distributed.

[13] 100 decimals=1 acre

Village availability of food and health program effect remains largely indeterminate with regard to saving lives among children born to households living in those villages. Table 7 shows that village availability of food program is negatively associated with child mortality in 1991/92 (Columns 1 and 2), but this association is not significant in the 1998/99 cross-sectional model (Columns 3 and 4) and panel model (Columns 5 and 6). Village availability of food program seems to have a relatively larger impact than village availability of health program. This is suggested by statistical significance of program coefficients. As such, having a health program in village is negatively associated with the child probability of mortality but this association is never significant (Columns 1—6).

Overall, land-ownership maintains a nonlinear association in that an inverted-U relationship holds across specifications (Columns 1-6 in Table 7). Prospects for child mortality are lower among land-poor and land-rich households and relatively higher among landed middle-range households. This association is statistically significant—though intermittently. However, a chi-sq test of joint significance shows that probability values (*p*-values) are small when I consider joint significance of land and land-squared terms. And, a chi-sq test of joint significance shows that probability values are also small for a block of variables—income, income-squared, food and health programs, and their interactions (Joint Significance Test 1 in Table 7). In substantive terms, this suggests that households' land-ownership and residence in food- and health-program village jointly determine how effectively they succeed in saving children's lives (Joint Significance Test 2 in Table 7).

CONCLUSION

Public-private partnerships to deliver social services to *targeted* beneficiaries are believed to be efficient, equitable, and therefore imperative for economic and human development in low-income developing countries. I use microevidences from Bangladesh in the 1990s to shed empirical light to this policy conclusion and examine, in particular, how the new forms of income redistribution have actually fared in augmenting or compromising welfare not only among targeted households but also those not targeted. Findings are revealing and have a number of implications for policy. First, I find a nonlinear association between income and child mortality in that children in the very poor and rich households have higher likelihoods of survival than those born in poor but perhaps non-targeted households. Results suggest that by targeting transfers of nutrition and health services at very poor households, state redistributive programs might have offset poverty-induced threats to human development among beneficiary families but left the non-beneficiaries—many of whom are still poor—to fend (or in many cases not fend) for themselves. Although the welfare gains among the targeted poor are encouraging, as targeted redistribution appears effective and beneficial, the fact that the non-targeted poor are less successful in suppressing premature death, perhaps because they are cut off from redistribution, is troubling, and calls for a reevaluation of targeting criteria or perhaps the very practice of targeting.

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Table 1: Summary Statistics

Variables	Year	N	Mean	Standard Deviation	Minimum	Maximum
Child Mortality	1991/92	3478	0.146	0.353	0	1
	1998/99	4245	0.095	0.294	0	1
Child sex (Male=1)	1991/92	3478	0.512	0.499	0	1
	1998/99	4245	0.503	0.500	0	1
Income per capita (Taka)	1991/92	3478	81.129	46.735	20.640	710.677
	1998/99	4245	127.463	93.00	22.807	1574.808
Land ownership (in decimal)	1991/92	3478	67.566	211.433	1	4575
	1998/99	4245	66.207	125.043	0.500	2108
Maternal Education	1991/92	3478	1.1263	2.280	0	14
	1998/99	4245	1.706	2.836	0	14
Education among adult male	1991/92	3478	2.330	3.234	0	16
	1998/99	4245	2.379	3.424	0	16
Dependency ratio	1991/92	3478	38.076	31.053	0	300
	1998/99	4245	32.946	29.827	0	200
Household size	1991/92	3478	6.244	2.464	2	19
	1998/99	4245	6.300	2.541	2	20
Religion (Islam=1)	1991/92	3478	0.890	0.312	0	1
	1998/99	4245	0.892	0.310	0	1

Table 2: Parameter Estimates of Child Mortality Determinants

	1991-92		1998-99		Panel	
	(1) Weibull	(2) Probit	(3) Weibull	(4) Probit	(5) Weibull	(6) Probit
(Log of) Income per capita	0.344*** (0.106)	0.202*** (0.063)	0.210** (0.100)	0.106* (0.062)	0.381 (0.066)	0.019 (0.040)
Maternal education	-0.048* (0.025)	-0.026* 0.014	-0.076*** (0.024)	-0.044*** (0.013)	-0.065*** (0.018)	-0.038*** (0.009)
Education among adult male	0.001 (0.016)	-0.001 (0.009)	-0.063*** (0.019)	-0.033*** (0.010)	-0.028** (0.012)	-0.008 (0.007)
(Log of) Household size	-0.153*** (0.028)	-0.075*** (0.011)	-0.110*** (0.029)	-0.052*** (0.012)	-0.133 (0.020)	-0.064*** (0.008)
Dependency ratio	-0.002* (0.001)	-0.002*** (0.000)	-0.004*** (0.001)	-0.003*** (0.001)	-0.002** (0.001)	-0.002*** (0.000)
Child sex (Male=1)	0.079 (0.089)	0.049 (0.052)	-0.169* (0.095)	-0.104* (0.054)	-0.029 (0.066)	-0.014 (0.037)
Religion (Islam=1)	-0.064 (0.137)	-0.031 (0.082)	-0.579*** (0.131)	-0.303*** (0.078)	-0.271*** (0.095)	-0.168*** (0.056)
Constant	-4.437*** (0.528)	-1.337*** (0.294)	-5.109*** (.574)	-0.931*** (0.321)	-4.920*** (0.359)	-0.560** (0.201)
Obs	3478	3478	4245	4245	7723	7723
Log pseudo likelihood ratio	-2567.515	-1417.817	-1856.767	-1292.4031	-4523.054	-2752.523

***p<0.01; **p<0.05; *p<0.10

Note: Heteroskedasticity adjusted robust standard errors are in parentheses.

Table 3: Parameter Estimates of Child Mortality Determinants

	1991-92		1998-99		Panel	
	(1) Weibull	(2) Probit	(3) Weibull	(4) Probit	(5) Weibull	(6) Probit
(Log of) Land	0.073*** (0.021)	0.047*** (0.013)	-0.009 (0.022)	-0.002 (0.013)	0.037** (0.015)	0.027** (0.009)
Maternal education	-0.044* (0.024)	-0.026* (0.014)	-0.070*** (0.024)	-0.040*** (0.013)	-0.052*** (0.017)	-0.042 (0.009)
Education among adult male	0.001 (0.016)	-0.001 (0.009)	-0.053*** (0.020)	-0.029*** (0.010)	-.022* (0.012)	-0.009 (0.007)
(Log of) Household size	-1.224*** (0.140)	-0.674*** (0.076)	-0.929*** (0.165)	-.464*** (.080)	-1.097*** (0.110)	-0.566*** (0.054)
Dependency ratio	-0.002*** (0.001)	-0.002*** (0.001)	-0.005*** (0.001)	-0.003*** (0.001)	-0.002*** (0.001)	-0.002*** (.001)
Child sex (Male=1)	0.080 (0.089)	0.050 (0.053)	-0.181* (0.095)	-0.107** (0.054)	-0.035 (0.066)	-0.018 (0.037)
Religion (Islam=1)	-0.062 (0.135)	-0.021 (0.082)	-0.574*** (0.131)	-0.303*** (0.079)	-0.274*** (0.095)	-0.157*** (0.056)
Constant	-1.931*** (0.283)	0.140 (0.156)	-3.170*** (0.394)	0.062 (0.162)	-2.157*** (0.218)	0.059 (0.111)
Obs	3478	3478	4245	4245	7723	7723
Log pseudo likelihood ratio	-2553.241	-1404.089	-1850.353	-1286.564	-4513.128	-2728.526

***p<0.01; **p<0.05; *p<0.10

Note: Heteroskedasticity adjusted robust standard errors are in parentheses.

Table 4: Parameter Estimates of the Income-Child Mortality Relationship

	1991/92		1998/99		Panel	
	(1) Weibull	(2) Probit	(3) Weibull	(4) Probit	(5) Weibull	(6) Probit
(Log of) Income per capita	1.148 (1.097)	0.593 (0.754)	1.691 (1.259)	1.030 (0.714)	2.366 *** (0.780)	0.631 (0.436)
(Log of) Income per capita^2	-0.094 (0.119)	-0.047 (0.084)	-0.153 (0.127)	-0.096 (0.072)	-0.214** (0.081)	-0.067 (0.046)
Maternal education	-0.046* (0.025)	-0.025* (0.014)	-0.078*** (0.025)	-0.045*** (0.013)	-0.064*** (0.018)	-0.038*** (0.009)
Education among adult male	0.002 (0.016)	0.001 (0.009)	-0.058*** (0.020)	-0.031*** (0.010)	-0.025** (0.012)	-0.006 (0.007)
(Log of) Household size	-1.092*** (0.140)	-0.586*** (0.073)	-0.901*** (0.164)	-0.451*** (0.079)	-0.998*** (0.107)	-0.529*** (0.053)
Dependency ratio	-0.002* (0.001)	-0.002*** (0.001)	-0.005*** (0.001)	-0.003*** (0.000)	-0.002*** (0.001)	-0.002*** (0.000)
Child gender (Male=1)	0.073 (0.089)	0.047 (0.053)	-0.180* (0.095)	-0.106** (0.054)	-0.037 (0.066)	-0.017 (0.037)
Religion (Islam=1)	-0.069 (0.136)	-0.034 (0.082)	-0.560*** (0.131)	-0.293*** (0.079)	-0.263*** (0.095)	-0.165*** (.056)
Constant	-5.184** (2.567)	-1.582 (1.690)	-7.768*** (3.174)	-2.674 (1.769)	-8.567*** (1.895)	-1.409 (1.024)
Obs	3478	3478	4245	4245	7723	7723
Log pseudo likelihood ratio	-2554.789	-2511.942	-1848.536	-1821.021	-4499.717	-2731.873
χ^2 test of joint significance	7.25	7.71	3.98	3.67	26.50	2.10
$p > \chi^2$	0.026	0.021	0.136	0.159	0.000	0.350
Inflection point: per capita total expenditure per month (in Taka[l])	180.93	184.19	170.95	167.98	170.98	159.95

***p<0.01; **p<0.05; *p<0.10

Note: Heteroskedasticity adjusted robust standard errors are in parentheses.

Table 5: Weibull Parameter Estimates of the Income-Child Mortality Relationship, Alternative Measurement of Income

	1991/92		1998/99		Panel	
	(1) Weibull	(2) Probit	(3) Weibull	(4) Probit	(5) Weibull	(6) Probit
(Log of) Land	0.121* (0.071)	0.073* (0.041)	0.048 (0.091)	0.038 (0.050)	0.084 (0.056)	0.050 (0.031)
(Log of) Land^2	-0.009 (0.014)	-0.005 (0.008)	-0.012 (0.019)	-0.008 (0.010)	-0.009 (0.011)	-0.004 (0.006)
Maternal education	-0.044* (0.024)	-0.026* (0.014)	-0.069*** (0.024)	-0.040*** (0.013)	-0.051*** (0.017)	-0.041*** (0.009)
Education among adult male	0.009 (0.016)	-0.001 (0.010)	-0.053*** (0.020)	-0.029*** (0.010)	-0.021* (0.012)	-0.009*** (0.007)
(Log of) Household size	-1.211*** (0.142)	-0.665*** (0.077)	-.918*** (0.168)	-0.456*** (0.081)	-1.086*** (0.111)	-0.560*** (0.055)
Dependency ratio	-0.002* (0.001)	-0.002*** (0.000)	-0.005*** (0.001)	-0.003*** (0.001)	-0.002*** (0.001)	-0.002*** (0.000)
Child gender (Male=1)	0.081 (0.089)	0.051 (0.053)	-0.179* (0.095)	-0.107** (0.054)	-0.034 (0.066)	-0.018 (0.037)
Religion (Islam=1)	-0.068 (0.135)	-0.025 (0.083)	-0.575*** (0.131)	-0.302*** (0.079)	-0.278*** (0.096)	-0.158*** (0.056)
Constant	-1.960 (0.286)	0.121 (0.159)	-3.198*** (0.398)	0.042 (0.164)	-2.183 (0.220)	0.045 (0.113)
Obs	3478	3478	4245	4245	7723	7723
Log pseudo likelihood ratio	-2552.982	-1403.871	-1850.152	-1286.205	-4512.755	-2728.254
χ^2 test of joint significance	12.37	13.56	0.58	0.74	6.43	9.41
$p > \chi^2$	0.002	0.001	0.749	0.689	0.040	0.009
Inflection point: total land (in acre[!])	0.82	0.86	0.30	0.37	0.67	0.79

***p<0.01; **p<0.05; *p<0.10

Note: Heteroskedasticity adjusted robust standard errors are in parentheses.

[!] 100 decimals =1 Acre

Table 6: Suggestive Causal Mechanisms-1

	1991/92		1998/99		Panel	
	(1) Weibull	(2) Probit	(3) Weibull	(4) Probit	(5) Weibull	(6) Probit
(Log of) Income per capita	1.13 (1.11)	0.52 (0.75)	1.43 (1.27)	0.88 (0.73)	2.54*** (0.80)	0.94** (0.45)
(Log of) Income per capita^2	-0.09 (0.12)	-0.04 (0.08)	-0.15 (0.13)	-0.09 (0.07)	-0.24*** (0.08)	-0.10** (0.04)
Maternal education	-0.04* (0.02)	-0.02* (.01)	-0.07*** (0.02)	-0.04*** (0.01)	-0.06*** (0.01)	-0.03*** (0.01)
Education among adult male in HH	0.01 (0.01)	0.01 (0.01)	-0.05*** (0.02)	-0.03*** (0.01)	-0.02** (0.01)	-0.01 (0.01)
(Log of) Household size	-1.07*** (0.14)	-0.57*** (0.07)	-0.89*** (0.16)	-0.44*** (0.07)	-0.99*** (0.10)	-0.51*** (0.05)
Dependency ratio	-0.01* (0.01)	-0.01*** (0.00)	-0.01*** (0.01)	-0.01*** (0.01)	-0.01 (0.01)	-0.01*** (0.01)
Child's gender (Male=1)	0.07 (0.08)	0.04 (0.05)	-0.18** (0.09)	-0.10** (0.05)	-0.03 (0.06)	-0.01 (0.03)
Religion (Islam=1)	-0.07 (0.13)	-0.03 (0.08)	-0.57**** (0.13)	-0.30*** (0.07)	-0.27** (0.09)	-0.16*** (0.05)
Government food program in village (Yes=1)	-0.31 (1.51)	-0.17 (0.86)	-2.31** (1.03)	-10.12** (0.58)	-1.29* (0.74)	-1.04** (0.43)
Health center in village (Yes=1)	-0.99 (1.40)	-1.02 (1.26)	-1.49 (1.30)	-0.96 (0.76)	-0.84 (0.90)	-0.71 (0.55)
(Log of) Income per capita (×) Government food program in the village (Yes=1)	-0.09 (1.40)	0.01 (0.19)	0.51** (0.22)	0.24** (0.12)	0.29* (0.16)	0.20** (0.09)
(Log of) Income per capita (×) Health center in the village (Yes=1)	0.22 (0.33)	0.23 (0.30)	0.29 (0.27)	0.19 (0.16)	0.18 (0.19)	0.13 (0.12)
Constant	-5.15	-1.42	-6.51	-1.99	-8.75***	-2.01

	(2.63)	(1.70)	(3.17)	(1.80)	(1.94)	(1.06)
Obs	3478	3478	4245	4245	7723	7723
Log Likelihood	-2552.80	-1404.99	-1844.23	-1281.25	-4497.49	-2721.73
Joint significance test 1: χ^2 ($p > \chi^2$)	5.82(0.05)	5.95(0.05)	1.72(0.42)	2.02(0.36)	15.56(0.00)	4.91(0.08)
Joint significance test 2: χ^2 ($p > \chi^2$)	12.17(0.06)	11.13(0.08)	13.69(0.03)	10.27(0.11)	33.48(0.00)	21.35(0.00)

***p<0.01; **p<0.05; *p<0.10

Note: Robust standard errors are in parentheses.

Table 7: Suggestive Causal Mechanisms-2

	1991/92		1998/99		Panel	
	(1) Weibull	(2) Probit	(3) Weibull	(4) Probit	(5) Weibull	(6) Probit
(Log of) Land	0.10 (0.07)	0.06 (0.04)	0.01 (0.09)	0.01 (0.05)	0.07 (0.05)	0.04 (0.03)
(Log of) Land^2	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Maternal education	-0.04 (0.02)	-0.02* (0.01)	-0.07*** (0.02)	-0.04*** (0.01)	-0.05*** (0.01)	-0.04*** (0.01)
Education among adult male in HH	0.00 (0.01)	-0.01 (0.01)	-0.05*** (0.01)	-0.02*** (0.01)	-0.02* (0.01)	-0.01 (0.01)
(Log of) Household size	-1.18*** (0.14)	-0.65*** (0.07)	-0.91*** (0.16)	-0.45*** (0.08)	-1.09*** (0.11)	-0.55*** (0.01)
Dependency ratio	-0.01* (0.00)	-0.01*** (0.01)	-0.01*** (0.00)	-0.01*** (0.01)	-0.01*** (0.00)	-0.01*** (0.01)
Child's gender (Male=1)	0.07 (0.09)	0.04 (0.05)	-0.18** (0.09)	-0.10** (0.05)	-0.03 (0.06)	-0.01 (0.03)
Religion (Islam=1)	-0.09 (0.13)	-0.04 (0.08)	-0.57*** (0.13)	-0.30*** (0.07)	-0.28*** (0.09)	-0.16*** (0.05)
Government food program in village (Yes=1)	-0.62*** (0.22)	-0.35*** (0.12)	-0.05 (0.12)	-0.04 (0.07)	0.04 (0.09)	-0.14*** (0.05)
Health center in village (Yes=1)	-0.25 (0.27)	-0.19 (0.16)	-0.12 (0.15)	-0.05 (0.09)	0.00 (0.13)	-0.09 (0.07)
(Log of) Income per capita (×) Government food program in the village (Yes=1)	0.17*** (0.06)	0.11*** (0.03)	0.09** (0.04)	0.04 (0.02)	0.05 (0.03)	0.02 (0.01)
(Log of) Income per capita (×) Health center in the village (Yes=1)	0.07 (0.09)	0.04 (0.05)	-0.01 (0.05)	0.01 (0.03)	-0.02 (0.04)	-0.01 (0.02)
Constant	-1.90 (0.28)	0.17 (0.16)	-3.15*** (0.39)	0.07 (0.16)	-2.19 (0.22)	0.09 (0.11)

Obs	3478	3478	4245	4245	7723	7723
Log Likelihood	-2548.13	-1398.58	-1847.27	-1284.61	-4509.93	-2721.49
Joint significance test 1: χ^2 ($p > \chi^2$)	5.22(0.07)	5.89(0.05)	2.48(0.28)	2.08(0.35)	2.93(0.23)	4.47(.11)
Joint significance test 2: χ^2 ($p > \chi^2$)	21.52(0.01)	22.93(0.00)	6.72(0.34)	3.87(0.69)	12.90(0.04)	22.47(0.00)

***p<0.01; **p<0.05; *p<0.10

Note: Robust standard errors are in parentheses.