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Working-Age Adult Mortality, Orphan Status, and Child Schooling in Rural Zambia

by

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EXECUTIVE SUMMARY

During the last decade, the Zambian government has dramatically increased expenditures on primary and secondary schooling, and enrollment rates have risen dramatically. At the same time, Zambia has faced the challenge of rising HIV prevalence and the possibility that recent gains in long-term human capital development could be eroded if households which suffer the death of a working-age (WA) adult pull their children out of school due to family labor shortages or financial constraints. This paper uses panel survey data from rural Zambia to measure the impact of WA adult mortality and morbidity on primary school attendance and school advancement, and separately tests the extent to which orphan status affects these schooling outcomes. There are five principal findings from our analysis.

First, we find that a homogenous conceptualization of WA adult mortality and morbidity shocks are not by themselves a reliable indicator of poor child schooling outcomes. For example, while we find that the effect of WA adult mortality and morbidity does not have a significant negative effect on primary school attendance using the full sample of children, we do find significant negative effects in some cases when we consider the gender of the child, the pre-death wealth level of the household, and/or the gender and household position of the deceased or ill adult.

Second, we find that effects of chronic adult illness on child school attendance depend upon the household position and gender of the ill adult. For example, when we disaggregate morbidity shocks by household position of the ill adult, we find that the presence of a chronically ill head or spouse reduces attendance by 4.1%. We also find that the presence of a chronically ill male adult reduces attendance of girls by 8.5%.

Third, we find that households in rural Zambia are more likely to respond to adult mortality or morbidity shocks by reducing the attendance of girls relative to boys. For example, when we stratify the sample by gender of the child, we find a recent WA adult death (0-4 years ago) reduces girls' probability of attending school by 7.9%, while the presence of a chronically ill male adult in the household reduces girls' school attendance by 8.5%. The fact that negative impacts of WA mortality on girls' schooling are larger in magnitude than those for boys, and are significant for girls from both poor and less-poor households (while insignificant for boys), suggests that there is a clear gender bias in rural Zambia in how households respond to the death or chronic illness of a working-age adult.

Fourth, we find that the effects of adult mortality and morbidity on girls' attendance are of larger magnitude and more likely to be significant for girls from poorer households. For example, while a recent WA adult death reduces girls' probability of attending school by 7.9%, this effect is stronger among girls from poorer households where a recent WA death reduces attendance by -10 points or roughly 12.8%. Likewise, while we find that the presence of a chronically ill male adult reduces girls' attendance by 8.5%, this effect is stronger among poorer households, where it reduces girls' schooling by 12.5%. The fact that we find significant or larger impacts among children from poor households suggests that the opportunity costs of children in such households become high during the illness or following the death of a WA adult. It is likely that the financial constraints and increased labor demands faced by poorer households who suffer a WA adult death leads them to reallocate the time of children from school to family labor following the death of a WA adult.

Fifth, although we find evidence that mortality and morbidity shocks reduce attendance for some children – namely, for girls, and especially girls from poor households – this negative effect on attendance does not appear to result in delayed grade progression, as we do not find evidence that these shocks result in significant losses for either school advancement or highest grade completed. Nevertheless, the magnitude of the reductions in attendance for girls due to mortality and morbidity shocks are large enough to warrant the concern of policymakers, though measuring the potential effects of these shocks on a child's actual learning would require a much different and more in-depth methodological approach.

Sixth, we find that there are no significant negative effects of orphan status (parental, maternal or double-parent orphans) on either child school attendance or school advancement, regardless of whether we use the full sample or samples stratified by household wealth or gender of the child. Because orphans in our sample are just as likely to be found in relatively *ex ante* poor or wealthy households, this appears to rule out the possibility that insignificant effects of orphan status on schooling outcomes is due to the orphans' migration from their original household. Rather, this suggests instead that orphan status is simply not a good indicator of potential schooling disadvantage in the context of rural Zambia.

There are several policy implications from these results. First, because the extent to which childrens' schooling outcomes are affected by adult mortality or morbidity is specific to the gender of the child, the household's wealth level, characteristics of the deceased or ill adult, and the timing of the adult death, it is inappropriate to categorize all children in Zambia who are directly or indirectly affected by HIV/AIDS-related morbidity and mortality as being especially vulnerable and in need of targeted school subsidies. Second, it follows that social protection and education policymakers concerned with primary school under-enrollment in Zambia need to tailor mitigation measures to the specific needs and situation of children in rural Zambia. The evidence in this paper suggests that girls from households with a currently ill head/spouse or male adult, as well as girls from households with a recent WA adult death (i.e., within in the past 0-4 years) – especially those from poorer households – are most likely to face losses in school attendance and advancement. Mitigation measures appropriate for rural Zambia may therefore include conditional cash transfers targeted to girls from poorer households which have incurred these mortality/morbidity shocks. Such assistance might not only ensure that these girls attend school but could also enable poorer households to hire additional labor rather than pulling other children from school to meet family labor demands.

Third, although Zambia abolished primary school fees over a decade ago, there may still be barriers to enrollment such as continued household demand for child labor, additional educational expenses for transport, school uniforms and books, and declining school quality if enrollment outpaces new school construction and teacher hiring. These additional barriers to enrollment may explain why we have found evidence of negative effects of adult mortality and morbidity on girls' schooling, even in a time period after the government had abolished primary school fees. In addition, targeted schooling subsidies alone may not reduce schooling deficits of some orphans, in the event that their poor schooling progress is due to the emotional and psychological trauma of losing one or both parents or a lack of interest by their adult guardians in their schooling.

Fourth, Zambia should continue to provide universal free primary schooling, as this policy has been found in a number of countries to improve the enrollment and schooling progress of those children most likely to suffer from poor schooling – namely children from poorer households, both orphan and non-orphan alike. For example, evidence from Malawi and Uganda suggest that improvements in enrollments among the poor through universal abolition of primary school fees can substantially raise the enrollment of orphans, even to the point of eradicating orphan schooling deficits (Ainsworth and Filmer 2006). Finally, it should be noted that because of the well-established positive correlation between educational attainment and safer sexual behavior (World Bank 1999), Education for All is itself an important policy that can help reduce the spread of HIV/AIDS and thus the potential for negative shocks to child schooling (Ainsworth and Filmer 2006).

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ACRONYMS

AF	Ainsworth and Filmer
BFS	Bureau of Food Security
CRE	correlated random effects
CSAs	Census Supervisory Areas
CSO	Central Statistical Office’s
DFID	UK Department for International Development
EGAT	(USAID) Economic Growth, Agriculture, and Trade
FE	Fixed Effects
FSRP/DSA	Food Security Research Project/Division of Agricultural Statistics
IFPRI	International Food Policy Research Institute
IOB	The Policy and Operations Evaluation Department, the Ministry of Foreign Affairs, The Netherlands
IPW	Inverse Probability Weighting
MACO	Ministry of Agriculture and Cooperatives
MINAG	Ministry of Agriculture (*Ministério de Agricultura*)
MSU	Michigan State University
NER	net enrollment ratio
OLS	Ordinary Least Squares
PHS	Post-Harvest Survey
PPS	Probability Proportional to Size
SEA	Standard Enumeration Area
SSA	Sub-Saharan Africa
SS	Supplementary Surveys
TIA	Trabalho do Inquérito Agrícola
USAID	United States Agency for International Development
WA	Working Age

1. INTRODUCTION

Many of the African countries hardest hit by the AIDS epidemic of the past two decades also suffer from low levels of human capital development. There is growing concern that the AIDS epidemic may reduce long-term human capital development through reductions in child schooling, thus severely limiting the long-term ability of orphans and their extended families to escape poverty. This prompted calls a decade ago for governments and donors to subsidize the schooling of orphans (USAID 2000; World Bank 2002). A multi-country study by Case, Paxson, and Ableidinger (2004) lends empirical support to this argument, reporting evidence of orphan schooling deficits and arguing that targeting of subsidies to orphans is justified because such deficits exist even after controlling for household wealth. By contrast, a larger multi-country study found so much diversity in their results that they concluded that the extent to which orphans are under-enrolled in SSA relative to other children – if at all in some cases – is country-specific and cannot be assumed (Ainsworth and Filmer 2006). Ainsworth and Filmer's (2006) results also question whether the schooling progress of orphans is on average worse than that of children from the poor households - therefore requiring a targeted intervention linked to their special needs - or whether the impact of becoming an orphan is to further increase the already large group of poor children currently under-enrolled in many SSA countries. In the latter case, one might argue for policies that will raise the levels of schooling of the under-enrolled poor, reaching the most vulnerable children, whether orphan or non-orphan. This paper contributes to this on-going empirical debate by using a large panel dataset from rural Zambia to measure the impact of WA adult mortality, morbidity and orphan status on child primary schooling.

One of the main challenges of measuring the effects of adult mortality on child schooling is that mortality from AIDS is not a random event. Most studies in the earlier years of the epidemic in SSA have found higher HIV incidence rates among individuals with higher income, higher education, and more mobility (Ainsworth and Semali 1998; Gregson, Waddell, and Chandiwana 2001).[1] If those with higher socioeconomic status also tend to invest more in child schooling, then orphans and children in households with an adult death may actually have higher school enrollment than children in other households. Failure to control for initial household characteristics may therefore generate biased estimates of the impact of adult mortality on schooling outcomes. We address this challenge by using panel data which enables us to control for pre-death household characteristics.

While there are various studies which have used panel data from SSA to measure the impacts of adult mortality on child education in SSA (Deininger, Garcia, and Subbarao 2003; Evans and Miguel 2007; Beegle, de Weerdt, and Dercon 2006; Ainsworth, Beegle, and Koda.2005; Yamano and Jayne 2005; Case and Ardington 2006; Yamano, Shimamura, and Sserunkuuma 2006; Yamauchi, Buthelezi, and Velia 2006; Ueyama 2007; Mather 2011a), these studies represent findings from six different countries. Yet, a recent review of this literature – as well as the orphan-schooling literature based on cross-sectional Demographic and Health Survey data – demonstrates that the impacts of adult mortality or orphan status on child schooling vary considerably across countries and by household wealth level (Mather 2011b). For example, the studies by Ainsworth, Beegle, and Koda (2005) and Yamano and Jayne (2005) use household data from Kagera, Tanzania, and rural Kenya, respectively, both areas with relatively high population density. Households in these areas may not face the same labor

[1] This pattern may be changing in some countries, as research using more recent data from west and east Africa finds that although adults with more education are still more likely to be HIV-positive, associations between wealth and HIV status vary considerably across countries (Fortson 2008).

constraints following an adult death as rural Zambia, which has a considerably lower population density. In addition, while several of the panel studies have tested for whether the effects of adult mortality on child schooling vary by pre-death household wealth levels, only one of them has tested whether the effects vary by gender or household position of the deceased or ill adult (Mather 2011a). The relatively large household sample in the panel survey dataset used in this paper enables us to estimate such effects both by household wealth level and by characteristics of the deceased or ill adult.

This paper is organized as follows. We first discuss the data sources used in this paper. Second, we develop a conceptual framework with which we investigate potential pathways by which adult mortality may affect child schooling. Third, we estimate reinterview models to assess the degree to which sample attrition may be a problem, and run regression-based attrition tests of our schooling models. Fourth, we estimate various regression models to measure the effect of adult mortality, morbidity and orphan status on child schooling outcomes. We then repeat the analyses after stratifying the sampled households by initial household wealth, by gender of the child, and by gender and household position of the deceased or ill adult.

2. DATA

2.1. Panel Household Surveys

This study uses data from a three-wave, nationally-representative longitudinal study of rural smallholder households in Zambia. The survey waves include the government's Central Statistical Office's (CSO) Post-Harvest Survey (PHS) of 1999/2000, and the linked 2001, 2004, and 2008 Supplementary Surveys (SS) that were designed and conducted jointly by the CSO and Michigan State University. A 3-wave panel data set is available for the three agricultural production seasons, 1999/2000 (SS01), 2002/2003 (SS04), and 2007/08 (SS08). For SS01, 6,922 of the 7,699 PHS9900 households were successfully reinterviewed (a re-interview rate of 89.9%). SS04 was conducted in May 2004. Of the households interviewed in SS01, 5,358 were re-interviewed in SS04 (a re-interview rate of 77.4%). Of the households interviewed in both SS01 and SS04, 4,286 of them (80.0%) were re-interviewed in SS08.

The PHS is a nationally representative survey of 7,699 rural households from 70 districts, which used a stratified three-stage sampling design. In the first stage, Census Supervisory Areas (CSA) were selected within each district based on the sampling frame of the 1990 Census and using probability-proportional-to-size sampling. Second, Standard Enumeration Areas (SEA) were sampled from each selected CSA (for a total of 394 SEAs). In the third stage, 20 households were randomly selected from a listing of households within each sample SEA. Ten of the 20 households selected were small-scale (0.1 to 5 hectares) and ten were medium-scale (5 to 20 hectares), though in many cases, there were not ten medium-scale households in the SEA, so additional small-scale households were selected at random as replacements. Further details on the CSO surveys are examined in Chapoto and Jayne (2008) and Mason (2011), while details on sampling weights are discussed in Megill (2005).

Some researchers have sought to increase the probability of having sufficient sample numbers of households suffering adult mortality by targeting areas known to have high HIV prevalence (Petty et al. 2005), or by over-sampling households likely to have experienced an adult illness or death (Beegle, de Weerdt, and Dercon 2006). The sampling for the CSO and SS surveys is designed to meet the primary purpose of measuring rural agricultural production and incomes, thus there were no modifications to the sampling for the purposes of morbidity- and mortality-related research.

2.2. Data on Working-age Adult Mortality and Morbidity from the Household Surveys

The supplemental survey instruments covered a range of aspects: agricultural and livestock production, land use, and income sources and services. The survey instruments also included several demographic sections, to capture the characteristics of each current member of the household, and to document new arrivals, departures, deaths, and prolonged illness of household members.

When enumerators revisited the 2001 sampled households in 2004 and 2008, they asked about each individual in the demographic roster of the 2001 survey, including their whereabouts. For individuals which had left the household, further information was sought from respondents concerning why that individual left. For example, for members reported as deceased, respondents were asked to identify the cause of death; approximately 62% of deaths of adults age 15-59 were caused by disease. Enumerators also asked about new members who may have joined the households since the previous survey, thus this survey captures deaths of individuals who may have returned home for terminal care. The surveys also recorded whether any individual in the household had suffered chronic illness in the previous 3 months and been unable to fulfill household duties.

Our analysis focuses on the deaths and chronic illness of WA adults, defined as ages 15-59 for both men and women, as these correspond to the age ranges hardest hit by HIV/AIDS. Following earlier studies of the impacts of disease-related adult mortality on household assets and demographics (Donovan et al. 2003; Yamano and Jayne 2004; Mather et al. 2004; Chapoto and Jayne 2008), we use demographic information from the Trabalho do Inquérito Agrícola (TIA) panel on the disease-related death of a PA household member as a rough proxy for an HIV/AIDS-related death.

In this research, we recognize that not all the WA illness-related deaths are due to HIV/AIDS, yet it is generally accepted that the epidemic has played a large role in the rapid increase in WA mortality rates in countries with increasing HIV prevalence (Ngom and Clark 2003). Opportunistic diseases such as tuberculosis and malaria are present in Zambia and are more likely to occur or to be more severe when adults have a compromised immune system. Such diseases confound any simple diagnosis as to cause of illness or death and are also responsible for numerous deaths even in the absence of HIV/AIDS. However, chronic illness and/or death of WA adults, whether HIV-related or not, is clearly an increasingly important development problem. This paper therefore aims to quantify the effects of illness-related WA death on child schooling in the interest of informing the design of policies and programs intended to mitigate the adverse effects of adult mortality.

2.3. Data on Primary School-Age Children from the Household Surveys

Although panel household data was collected for three survey waves, data on the age and schooling of children under 12 is only available in the latter two waves (for 2004 and 2008). In both 2004 and 2008, respondents were asked about each household member's schooling, including the grade completed at the time of the survey, whether each child was regularly attending school, and the orphan status of children age 7-11. We therefore have longitudinal data on children's attendance, years of schooling, age and gender, and household characteristics for 2004 and 2008 (as well as household characteristics in 2000 for panel households). Because orphan status is not collected for children over age 11 in either 2004 or 2008, we do not have panel information on orphan status. Thus, in order to analyze the effect of orphan status on schooling outcomes, we use two pooled cross-sections of children age 7-11 (for both 2004 and 2008) who are members of panel households.

The entrance age for primary school in Zambia is seven, and primary education consists of seven grades/years. However, given that many children do not begin primary school until age 8 to 10, most children will not complete primary school and begin secondary education at the earliest entrance age of 14. Given that our interest is primary school education, we therefore analyze the schooling of all children age 7 to 12 in 2004 (who are therefore age 11 to 16 in 2008), and we drop individual cases if the child has begun secondary education (i.e., n=199 children in this later age range were in secondary school in 2008, while n=3,585 were still in primary school).

2.4. Secondary Data

This study also uses secondary data on HIV prevalence rates drawn from the report *Zambia: HIV/AIDS Epidemiological Projections, 1985-2010* (CSO 2005). The HIV/AIDS estimates in this report are based on sentinel surveillance site (ante-natal clinic) data and the projections are computed using the cohort component method. The report lists estimated HIV prevalence rates and numbers of AIDS-related deaths for each district for the years 1985, 1990, 1995, and 2000-2010. We use this data to help estimate the probability of reinterview, which we use to help alleviate potential panel attrition bias, as well as to control for potential community-level effects of increased adult mortality.

3. PRIMARY EDUCATION AND WORKING-AGE ADULT MORTALITY

3.1. Primary Education in Zambia

Beginning in the late 1990s, the Government of Zambia abolished primary school fees and undertook plans to dramatically increase the access and quality of basic education. These reform efforts were supported by donor aid which moved away from project support and toward direct budget support for the ministry of education. For example, in 2000, there were approximately 5,300 basic schools in Zambia; by 2006, this number had increased to more than 8,000 (IOB 2008). Over the same period, the total number of classrooms increased from 25,000 to 35,000, and the total number of teachers increased by 35% (*ibid.*). The net enrollment ratio (NER) for primary school in rural Zambia increased from 40.3% in 1996, to 60.9 in 2000, to 76.4% in 2007 (CSO 1997; CSO 2003; CSO 2009).[2] There is no apparent gender gap in attendance in rural Zambia, as attendance rates for boys and girls were nearly the same in 2007 (76.7% for boys; 76.1% for girls) (CSO 2009).

3.2. Conceptual Framework

The factors which affect parents' decisions to send their children to school include the financial costs of schooling, opportunity costs of children's time in other activities, and the expected returns from schooling. The potential effects of WA mortality or morbidity on child schooling depends on how such events affects these factors (World Bank 1999). First, medical expenses during the pre-death illness period may make it difficult for parents to afford school fees, and such fees could be prohibitive in the post-death period if the deceased was a key cash-earner for the household. While there were no primary school fees during the time of this study, there may be additional educational-related expenses for transport, school uniforms and books.

Second, the opportunity costs of children's time, which increase with age, may also increase based on demands for care-giving (during the pre-death period) and family labor (during both the pre- and post-death periods). We expect that households with higher initial asset holdings would be less likely to pull children from school because such households may have sufficient income to hire additional labor to meet their labor demand or to attract new adults to the household (Ainsworth, Ghosh, and Semali 1995, Mather and Donovan 2007). Third, expected returns from schooling may decline if life expectancy and/or non-farm employment opportunities are eroded in the event of widespread HIV incidence in the community. In addition, the value of returns from education of a child may be lower for the guardians of two-parent orphans, who may well be less interested in investing in the long-term welfare of children who are not their own.

While we lack suitable instruments with which to empirically distinguish the impacts of one of these potential factors from the others, this conceptual framework suggests various ways by which adult mortality might affect child schooling. We consider three hypotheses in particular. First, if girls are more likely to become caregivers when an adult in their household becomes ill, then we would expect to find larger effects of adult morbidity on girls' schooling outcomes. Second, we would expect effects of adult mortality to be larger for children from households with lower initial asset holdings given that the opportunity costs of

[2] The NER for primary school is the percentage of the primary-school-age (6-12 years) population that is attending primary school.

children of such households may be quite high, and that such households are less likely to be able to hire labor or attract new members. Third, a few studies have found that mortality impacts on household income or assets are larger when the deceased was a household head or spouse relative to a non-head or spouse (Yamano and Jayne 2004; Mather and Donovan 2007). Thus, we expect to find larger negative effects on schooling following the deaths of a head or spouse relative to the death of other household members.

4. ESTIMATION STRATEGIES AND VARIABLES

4.1. Panel Attrition and Sampling

For most of the subsequent panel econometric work in this paper, we only use individuals from households which were initially interviewed in 2000/2001, and then re-interviewed in 2004 and 2008. Given that over time, some households move away from a village and others dissolve as part of a typical household life-cycle (or perhaps due to adult mortality), panel household surveys typically have to contend with at least some sample attrition over time. If households which are not re-interviewed are a non-random sub-sample of the population, then using the re-interviewed households to estimate the means or partial effects of variables during one of the later panel time periods may result in biased estimates.

As discussed in the Data section, of the 6,922 households interviewed in SS01, 5,358 were re-interviewed in SS04 (a re-interview rate of 77.4% for 2004). Of the households interviewed in both SS01 and SS04, 4,286 of them (80.0% for 2008) were re-interviewed in SS08. While there are a small number of households from SS01 which were not re-interviewed in SS04 but were re-interviewed in SS08, we do not use these households in the ensuing analyses as we treat attrition as an absorbing state (once a household drops out of the sample, it does not re-enter) as per Wooldridge (2002).

To test for household-level attrition bias in our individual-level regressions, we follow the approach described in Wooldridge (2002, p. 585) and define a selection indicator variable, $attrite_{i,t+1}$, which is equal to one if the child belongs to household which was not re-interviewed in the next wave of the panel survey, and equal to zero if the individual belongs to a household which was re-interviewed successfully. The binary variable $attrite_{i,t+1}$ is then included as an additional explanatory variable in each of our schooling regression models. If the coefficient on $attrite_{i,t+1}$ is statistically different from zero, this indicates the presence of attrition bias. Given that there are two waves of panel data on child schooling available, only the first wave (2004) is used in this test. A limitation of our attrition test is that because we only observed child schooling information in the 2004 and 2008 surveys, our attrition test only addresses attrition between these two later survey waves. Attrition bias test results are reported in Table 4.1.

Given that our panel household survey data is based upon a complex, stratified sampling design, we apply Stata's options for complex sampling weights to estimate standard errors used in the econometric analysis in this paper (StataCorp 2009). When this option is not available in Stata for a given regression model, we use the population weights. For regression models which appear to be significantly affected by attrition bias, we use sampling weights which are adjusted for panel attrition bias using the Inverse Probability Weighting (IPW) method (Wooldridge 2002), as discussed in the next section.

Information was not collected by the supplemental surveys concerning children who may have left households since the 2001 survey, either with their entire households or by themselves. We therefore lose some children due to attrition. While we test for attrition at the household level, we are not able to test for potential bias which may arise in the event that there is non-random child attrition (beyond that which we can control for at the household level).

4.2. Reinterview Model

Wooldridge (2002) proposes Inverse Probability Weights as a method to address this possible source of selection bias. IPW methods have been applied in HIV/AIDS impact analysis by Yamano and Jayne (2005), Chapoto and Jayne (2008), and Mather and Donovan (2007), and this study follows the same approach. A key assumption is that the observable characteristics of the household adequately explain re-interview status, and that unobservables are not strong predictors of re-interview.

Alderman et al. (2001) notes that while selective attrition on unobservables potentially remains a problem even after the analyses account for selection on observables, the possibilities for detecting selective attrition on unobservables using datasets from developing countries is very limited, given that such tests require comparisons with similar datasets which contain the same key variables yet no (or little) attrition. In addition, they argue that "using as much information as possible about selection on observables in the panel helps to reduce the amount of residual, unexplained variation in the data due to attrition. Controlling for selection on observables thus will likely reduce any biases due to selection on unobservables (*ibid.* 2001)." Following Alderman et al. (2001), we, therefore, rely upon observable characteristics to help explain attrition.

In our study, we use initial household and village characteristics, lagged HIV prevalence rates from the nearest sentinel site, and binary variables for provinces and different enumerator teams to predict reinterview. In short, we write our 2004 reinterview model as:

$$\Pr(R_{ht} = 1) = f(HIV_{t-j}, X_{bt}, T_{bt}, P). \quad (1)$$

where R_{ht} equals one if a household b is re-interviewed at time t, conditional on being interviewed in the previous survey, and zero otherwise; HIV_{t-j} is the average lagged district-level HIV prevalence rate from 1995 and 1999 for the 2004 survey and from 1999 to 2003 for the 2008 survey; X_{bt} is a set of household characteristics observed in the 2001 Supplemental survey; T_{bt} is a set of enumeration team dummies; and P is a set of eight provincial dummies. Note that all of the variables are observable even for households that were not reinterviewed in 2004. We match the lagged HIV prevalence information from 1995 and 1999 (1999-2003) with the observations of the 2004 (2008) survey because of the 6-10 year average lag time between HIV seroconversion and AIDS-related death. If these regressors are a good predictor of re-interview, then we will be able to use the inverse of the predicted probability as a weight in the outcome estimations to control for panel attrition bias. Using these characteristics observed for all households in the original sample, we estimate equation (1) with probit to determine the probability of being re-interviewed, φ_{2004}. The observations include 6,916 sample households in the 2001 supplemental survey.

We next estimate a probit of re-interview in 2008 using the 4,152 sample households that were interviewed in 2001 and 2004 and the same specification as used in the 2004 reinterview probit, though with household characteristics observed in 2004 (not 2001). From the 2008 reinterview probit, we obtain the predicted probability φ_{2008}. For observations in both the 2004 and 2008 survey, the inverse probability weight is the product of $1/\varphi_{2004}$ and

$1/\varphi_{2008}$.[3] We then multiply the IPW by the household-specific population weights and apply them to the models which are found to exhibit significant panel attrition bias.

4.3. School Attendance Model

4.3.1. Measuring the Effects of Mortality and Morbidity Shocks on Child School Attendance

The economic theory and estimation models on schooling have been discussed in numerous articles (Strauss and Thomas 1995; Glewwe 2002). Our first educational outcome of interest is primary school attendance (A_{it}), which is measured as a binary variable which equals one if the child is enrolled in school at the time of the survey, and zero otherwise. We restrict our analysis to children who either have not yet started school or who are still in primary school, because the household decision with respect to secondary school attendance may involve different financial and opportunity-cost constraints, given that primary school fees were abolished in Zambia the late 1990s (but not fees for secondary schools), and that the nearest secondary school may be further than the nearest primary school. In practice, this means that we use a pooled sample of children who were age 7-12 in 2004, age 11-16 in 2008, and still in primary school.

As discussed in Section 3.2, the impact of WA adult mortality on a child's schooling may start prior to the death due to the demand for caregivers for the sick member(s) and to medical costs. In addition, adult mortality may affect child schooling for a long time after the death because of reduced financial resources and labor. To measure the total impact of adult mortality, we disaggregate instances of adult mortality into those which occurred 4-8 years prior to time t, and those which occurred within 0-4 years of time t. This Past Period WA death binary variable PPD_{bt} equals one for each child which experienced a WA adult death in their household in the 4-8 years prior to year t, while the Recent WA death binary variable RD_{bt} equals one for each child which experienced a WA adult death in their household from 0-4 years prior to time t. Finally, to capture the potentially negative effects of adult mortality which occur during the illness period, we include the binary variable I_{bt} which equals one for any child which lives in a household with an adult who was reported to be chronically ill for the last 3 months, and zero otherwise. Thus, a base model for our analysis of child school attendance is:

$$\Pr(A_{it} = 1) = f(PPD_{it}, RD_{it}, I_{it}, HIV_{t-j}, C_{it}, X_{i2000}, V_{k2000}, DR_{kt}, DIST_{im}, YEAR_t) \qquad (2)$$

where C_{it} represent child-specific variables for the child's age, age-squared and gender; X_{i2000} represent household characteristics observed in 2000, V_{k2000} are village-level characteristics (some only observed in 2000), DR_{kt} is a vector of district-level variables; $DIST_{im}$ is a vector of binary variables for m=1 to 69 of the 70 districts in our sample; and $YEAR_t$ is a binary variable which equals one for 2008 and zero for 2004.

Household characteristics (observed in 2000) include: total household landholding, total farm asset value (which includes farm equipment and livestock), the age of the household head, the education level of the household head, the education level of the spouse of the household head, the maximum education level of adults in the household who are age 18 or older, and a

[3] Although technically the 2004 survey observations survived attrition once while those in 2008 survived twice, because we are doing panel analysis, we use a common inverse probability weight which is the probability that the household was reinterviewed in both 2004 and 2008, as per Wooldridge (2002).

binary variable which =1 if the head is polygamous. Village-level characteristics include: distance to the nearest vehicular transport, distance to the nearest feeder road (in 2000), and distance to the nearest boma (administrative town, in 2000). These village-level characteristics are included primarily because the household survey did not record a measure of the distance or travel time from the village to the nearest primary school. We assume that if most schools are built on or near a feeder road (or nearest administrative town), then distance to the nearest vehicular transport, feeder road, or administrative town may well proxy for distance to the nearest primary school. We also include these distance variables because they may serve as proxies for market access, under the assumption that market access may affect the household's demand for schooling (given higher relative returns to education in the production and marketing of higher-value crops as well as non-farm wage or own business activities, relative to the returns to education in semi-subsistence farming). District-level characteristics include HIV_{t-j}, the lagged HIV prevalence rate at the nearest surveillance site, and DR_{kt} which includes the log of cumulative rainfall during the main season in time t, and the number of 20-day periods with less than 40 mm of rainfall during the main season in time t.

Several points should be clarified about this equation. First, we use household characteristics as observed in 2000 instead of contemporary household characteristics in year t, because contemporary values of X_i for the years 2004 and 2008 may well be affected by mortality shocks. For example, when a male household head dies, it is possible that his household transitions from being relatively wealthy to relatively poor. To measure the full impact of his death on his children's schooling, we need to compare the schooling outcomes of his children with children who resided in relatively wealthy households, not in poorer households. Therefore, we hold the household characteristics at the levels observed prior to the mortality shocks. Technically, the past period mortality shocks for the year 2004 occurred in 1996-2000 and thus may affect household characteristics from 2000. Thus, if mortality shocks tend to reduce household wealth and asset levels, this means that we may underestimate the negative effect of past-period shocks.

Second, we include the lagged HIV prevalence rate at the nearest surveillance site expecting that it may pick up broader community effects of the AIDS epidemic on child school attendance (separate from the direct effect via afflicted households) (Yamano and Jayne 2005). However, it should be noted that the lagged HIV prevalence rate could be correlated with various district or provincial-level characteristics which are unobserved in our model. For example, we know from previous studies that HIV prevalence rates tend to be high in areas with major trunk roads where there is a steady influx of outsiders. Thus, we need to be cautious in interpreting the partial effect of this variable on schooling outcomes.

We first estimate equation (2) with pooled Probit. However, if unobservable characteristics such as household social status, mobility, and parents' preferences for schooling are correlated with the mortality/morbidity binary variables, this can lead to biased estimation of the effects of such shocks on child schooling. While a Fixed Effects (FE) estimator is usually the most practical way to control for unobserved household-level heterogeneity which can be assumed to be time-constant, the FE Probit estimator has been shown to be inconsistent (Wooldridge 2002). Although conditional Logit model can control for household or child fixed effects, this model can only include observations with within-group variation in the dependent variable – that is, it will drop observations from any child in our panel who either attended school in both 2004 and 2008 or did not attend in either year. Unfortunately, this would reduce our sample of children age 7-12 in 2004 (age 12 to 16 in 2008) by approximately one-half; from n=7,521 cases to n=3,851.

An alternative is to use pooled correlated random effects (CRE) Probit (Mundlak 1978; Chamberlain 1984), which explicitly accounts for unobserved heterogeneity and its correlation with observables, while yielding a fixed-effects-like interpretation. In contrast to traditional random effects, the CRE estimator allows for correlation between unobserved heterogeneity (*ci*) and the vector of explanatory variables across all time periods (X_{it}) by assuming that the correlation takes the form of: $c_i = \tau + Xi\text{-}bar\xi + a_i$, where *Xi-bar* is the time-average of X_{it}, with $t = 1, \ldots, T$; τ and ξ are constants, and a_i is the error term with a normal distribution, $a_i | Xi \sim \text{Normal}(0, \sigma^2_a)$. However, because the household time-average CRE terms most likely to be correlated with household-level unobserved heterogeneity – such as head's age, head's education, spouse's education, maximum adult education, total landholding and asset values – may be affected by mortality shocks, we instead use household characteristics as observed in 2000 (i.e., prior to recent mortality shocks for 2004 and 2008 observations, and prior to past period shocks for 2008). While the measure of each of these household characteristics in 2000 is not the 'long-term time-average', all of them except for total landholding and asset values are essentially constant over time (in the absence of the death or departure of the head or spouse). We further assume that these variables are likely to be correlated with unobserved time-constant factors such as the household's schooling preference.

We estimate a reduced form of the pooled CRE probit model in which τ is absorbed into the intercept term and the X_{i2000} terms are added to the set of explanatory variables. Using the full sample with the pooled CRE probit, we perform an adjusted Wald test and reject the hypothesis of zero correlation ($\xi = 0$) between unobserved heterogeneity and the X_{i2000} terms ($p=0.000$), indicating that the CRE approach is superior to the traditional pooled or random effects estimators. To facilitate interpretation of the results, we compute average partial effects[4] (APE) for each regressor using Stata's *margins* command. We use survey sampling weights in each regression in accordance with the complex survey design of the Zambia rural household surveys.

After running these models on the entire sample of children age 7-12 in 2004 and 11-16 in 2008, we then rank the sample by the household's asset wealth level in 2000 (where asset wealth is defined as the total value of farm equipment and livestock), and stratify the households into two groups; households which are in the bottom half of total asset value (i.e., poor) and those which are in the top half (i.e., less poor). We then run separate regressions by asset wealth category to see if mortality effects vary by the household's initial wealth level. Next, we stratify the sample by gender of the child and run separate regressions by gender to see if mortality and morbidity shocks affect girls differently than boys. Information on the number of cases (children) in each of these separate regressions is presented in Appendix Tables 1 and 2. Finally, we estimate equation (2) but with further disaggregation of the dummies for adult mortality and adult chronic illness; we disaggregate these dummies first by the household position of the deceased or ill adult (head/spouse or non-head spouse), and then by gender of the deceased or ill adult.

[4] Because the effect of an explanatory variable in a nonlinear equation depends on the level of all explanatory variables, not just its own coefficient, analysts typically compute the marginal effects for a given variable using the mean of all regressors. By contrast, we compute the partial effect for each household, and then take the average partial effect across the entire sample (or subsample), and compute bootstrapped standard errors for inference (Wooldridge 2002).

4.3.2. Measuring the Effect of Orphan Status on Child School Attendance

To measure the effect of orphan status on child attendance, we also estimate equation (2) in which we drop the mortality/morbidity shock information and add three separate binary variables: the first =1 if the child is a maternal orphan in time t (and zero otherwise), the second =1 if the child is a paternal orphan in time t, and the third =1 if the child is a double-parent orphan in time t. Because orphan status is not collected for children over age 11 in either year, this means that while we are able to track the age and education level of individual children from 2004 to 2008 (assuming they stay in their households over time), we do not have panel information on orphan status. Thus, in order to analyze the effect of orphan status on schooling outcomes, we use two pooled cross-sections of children age 7-11 (for both 2004 and 2008) who are members of panel households. Information on the number of cases (children) in each of these orphan-education regressions is presented in Appendix Table 3.

While these child-level observations are not longitudinal, they come from households which are, which allows us to control for household characteristics in 2000. However, information on orphan status does not tell us when a given orphan's parent fell ill or died, thus we don't know for certain what the household's wealth level was prior to the morbidity/mortality shock. We also do not know for certain that the orphan resides in the same household where his/her parent(s) died, although literature from other countries has typically found that paternal orphans are very likely to stay in the same household after their father's death. Assuming that the parental death occurred after 2000, we are able to control for pre-death household wealth, head's education, etc.

4.4. School Advancement Model

We next undertake an additional set of analyses focusing on schooling or grade advancement, that is, the successful progression of children from one grade to the next. This analysis provides an alternative indication of the effects of WA adult mortality on child schooling, as it is possible that the effects of mortality/morbidity shocks could be better captured in school advancement than in school attendance, due to grade repetition and/or late enrollment. One measure of school advancement is the ratio of the child's grade actually achieved over the grade that would be achieved under normal grade progression without repetition and assuming the child begins primary school at age 7. We measure school advancement as SA_{it} = (the highest grade attained) / (age – 6).

Because not attending school at all results in a SP score of zero, and a significant portion of our cases (20%) have school advancement of zero, which combined with a distribution of cases where GP >0 can result in positive skewness that may create problems for OLS. While we could simply drop the zero cases, it is likely that this dependent variable is more appropriately modeled as a corner solution, because the cases of zero are not simply missing data but rather reflect the economic decision of the household not to send a given child to school at all. We therefore estimate the following school advancement model using Tobit:

$$SA_{it} = f(PPD_{it}, RD_{it}, I_{it}, HIV_{t-j}, C_{it}, X_{i2000}, V_{k2000}, DR_{kt}, DIST_{im}, YEAR_t) \qquad (3)$$

This model uses the same regressors as in equation (3), including household characteristics measured in 2000 which we assume should control for time-invariant unobserved household heterogeneity as CRE terms (described above). As with our attendance analysis, we restrict analysis of school advancement to children who have either not started school or who have not yet completed primary school. In practice, this means that we use a sample of children who were of age 7-12 in 2004 and age 11-16 in 2008 (and who are still in primary school). While three partial effects may be computed from the Tobit model, we focus on the unconditional effect, which is the effect of the regressor of interest on school advancement of any given student, whether or not they attend school (i.e., whether or not their school advancement is >0). If we find any negative impacts of WA adult mortality on school attendance, then we would anticipate finding similar results on school advancement, assuming that lower school attendance leads to delayed school advancement. The reverse does not necessarily hold: even if we do not find any negative impacts on school attendance, we may still find negative impacts in schooling progress.

$$SA_{it} = f(PPD_{it}, RD_{it}, I_{it}, HIV_{t-j}, C_{it}, YEAR) \qquad (4)$$

To check the robustness of our Tobit results, we also estimate equation (4) on all observations (i.e., including those with zero grade progress) using OLS with household fixed effects. Use of household fixed effects causes the time-constant household, village, and district-level variables to drop out of the school advancement model.

4.5. Highest Grade Completed Model

We also undertake an additional set of analyses focusing on the highest grade completed. Like the analysis of grade progression, this analysis provides an alternative indication of the effects of WA adult mortality on child schooling, as it is possible that the effects of mortality and morbidity shocks could be better captured by highest grade completed rather than school attendance, due to potential grade repetition.

Highest grade completed is a count variable, for which a Poisson or Negative Binomial regression is typically used. We find that there is some over-dispersion in this dependent variable (using the over-dispersion test outlined in Long 1997), and that a Vuong test indicates that the Negative Binomial fits the data better than a Poisson (Vuong 1989). We therefore estimate a Negative Binomial model of the highest grade completed using the CRE terms (i.e., household characteristics in 2000) and then separately with household fixed effects.

5. RESULTS

5.1. Determinants of Reinterview

We first discuss results of the reinterview model (Appendix Table 4). The joint tests for significance of the enumerator team dummies and of all household characteristics show these variables to be highly significant, thus indicating that attrited households appear to differ from nonattrited households. For example, households with larger numbers of adults, landholding, and farm assets have a higher probability of reinterview, as well as households with lower head's education level. In addition, a household 'whose head was related to the village headman when first allocated land' is 23% (25%) more likely to be reinterviewed in 2004 (2008). These results suggest that households with ties to the local leadership, greater farming assets (land, livestock, farm equipment) and larger number of adults are more likely to stay together as a family and stay in the village. By contrast, those with lower farming assets and fewer adults are more likely to dissolve the household and/or migrate, which is consistent with higher levels of head's education (in a rural economy where education likely has higher returns to non-farm relative to farm activities). There is also some evidence that morbidity and mortality shocks may cause household attrition. For example, households headed by a single female, or which have two or more chronically ill adults are less likely to be reinterviewed in 2004, while those with a recent adult death are less likely to be reinterviewed in 2008. One exception to this is that the lagged district-level HIV prevalence is not significantly associated with reinterview.

However, this evidence alone does not mean that there is necessarily significant attrition bias with respect to the dependent and independent variables in our child schooling regressions. We therefore run regression-based attrition tests (described above) using all the observations from the year 2004. We find that there is evidence of attrition bias in only one of the 10 principal regressions (Table 1) – that for school advancement among girls. We thus present results in the next few sections from models which do not apply attrition corrections to the sampling weights (with the exception of the school advancement models using the sub-sample of girls).

Table 1. Attrition Bias Test Results

Dependent variable	Estimator	p-value for test of H_0: $\beta_{reinterview i,t+1} = 0$ vs H_1: $\beta_{reinterview i,t+1} = 1$
Primary school attendance of children age 7-12 (in 2004)		
All children age 7-12	Probit	0.244
children from poor households	Probit	0.920
children from less poor households	Probit	0.158
girls	Probit	0.224
boys	Probit	0.564
School advancement of children age 7-12 (in 2004)		
All children age 7-12	Tobit	0.233
children from poor households	Tobit	0.766
children from less poor households	Tobit	0.269
girls	Tobit	0.044
boys	Tobit	0.809

Notes: based on regressions using all households in 2004

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

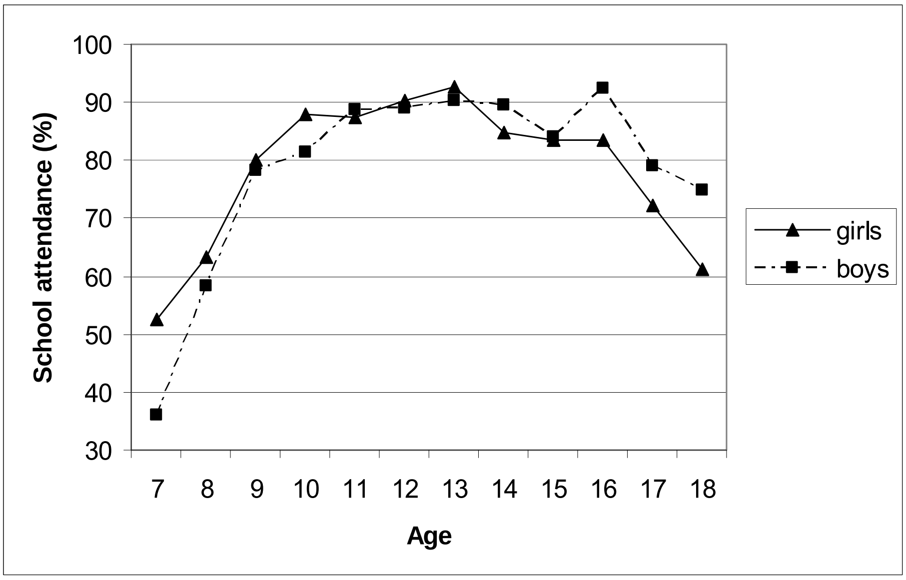
5.2. Descriptive Statistics

The means and standard deviations of each of the variables used in our child schooling models are shown in Table 2 and 3. Because we are tracking the same children over time (who are 4 years older in the second survey wave), the fact that mean attendance increases from 67% in 2004 to 87% in 2008 indicates that many children do not start primary school at the age of seven (Table 3). Likewise, mean school advancement is 0.356 in 2004 and 0.518 in 2008, which also suggests that many children do not start school at age 7 and/or advance grades on schedule. For example, if a child starts school on time (at the age of 7), stays in school, and advances one grade for each year, their school advancement ratio would consistently be 1.0 from one year to the next. Delayed initiation of primary schooling is also clear from Figure 1, which shows that mean attendance ranges from 30 to 63% for ages 7-8, then gradually climbs to around 90% by age 12, before eventually dropping again after age 16.

Three key descriptive statistics indicate the necessity of controlling for various household and child-level factors when testing for differentials in child schooling. First, attendance increases in age, thus any comparison of schooling outcomes across different categories of children must first control for the child's age (Figure 1). Second, average school attendance differs considerably by the wealth level of the household, as 64% of children age 7-12 (in 2004) from poorer households attended school, compared with 71% of children in the same age range from wealthier households (Table 2). This pattern is also clear from Figure 2, which shows that mean attendance of children age 10-11 increases in household farm assets. Average years of schooling achieved and school advancement for this age group in 2004 is also lower for children from poorer households (1.15 / 0.32) relative to those from wealthier households (1.44 / 0.39). Together, these schooling results suggest that children from poorer households start school later than those from wealthier households, and/or progress more slowly from one grade to the next.

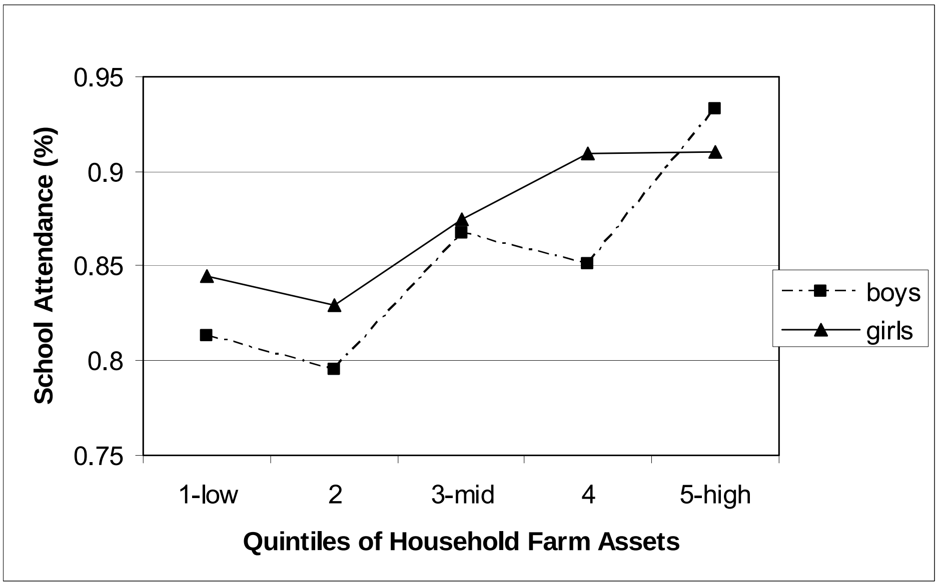
Third, school attendance and grade completion differ by the gender of the child. As shown in Figure 1, girls tend to start school earlier than boys, yet mean attendance is roughly the same for boys and girls between ages 11-15. At age 15-16, girls begin to drop out of school more quickly than boys. We next move to multivariate regression analysis, which enables us to measure the effect of adult mortality or orphan status on child schooling while controlling for the various child- and household-specific attributes which influence child school attendance and grade completion.

Figure 1. School Attendance of Children Age 7-18, Rural Zambia, 2008



Source: Author's calculations using CSO Supplemental Survey 2008.

Figure 2. School Attendance of Children Age 10-11, by Quintiles of Household Farm Assets, Rural Zambia, 2008



Source: Author's calculations using CSO Supplemental Survey 2008.

Table 2. Descriptive Statistics of Children in Primary School Age 7-12 in 2004 and 11-16 in 2008, by Wealth Category

	All children				Poor				Less Poor			
	2004		2008		2004		2008		2004		2008	
	mean	SE	mean	SE	mean	SE	mean	SE	mean	SE	mean	SE
Dependent variables												
Primary school attendance (=1)	0.678	0.010	0.876	0.007	0.640	0.014	0.867	0.010	0.717	0.014	0.884	0.010
School advancement	0.356	0.009	0.518	0.006	0.322	0.012	0.498	0.008	0.392	0.013	0.537	0.008
Highest grade completed	1.291	0.031	3.732	0.044	1.152	0.040	3.566	0.062	1.440	0.047	3.892	0.060
Explanatory Variables												
Lagged HIV prevalence rate	13.473	0.086	13.965	0.123	12.912	0.105	13.227	0.146	14.133	0.129	14.837	0.199
Household-level adult mortality/morbidity shocks												
Past WA adult mortality (4-8 years ago)	0.086	0.007	0.054	0.006	0.083	0.010	0.051	0.008	0.091	0.010	0.057	0.009
Recent WA adult mortality (0-4 years ago)	0.053	0.006	0.045	0.005	0.050	0.008	0.043	0.006	0.056	0.008	0.048	0.007
Chronically ill adult (in previous 3 months)	0.139	0.009	0.035	0.005	0.133	0.012	0.038	0.007	0.144	0.013	0.031	0.008
Child characteristics												
Age	9.240	0.027	13.165	0.027	9.183	0.035	13.116	0.036	9.301	0.037	13.218	0.038
Age squared	88.014	0.504	175.874	0.735	86.878	0.671	174.511	0.978	89.245	0.703	177.366	1.017
Girl (= 1)	0.487	0.009	0.483	0.009	0.497	0.012	0.493	0.012	0.477	0.013	0.473	0.013
Household characteristics (in 2000)												
ln(Total landholding)	0.676	0.033	0.682	0.033	0.484	0.038	0.488	0.038	0.835	0.044	0.846	0.043
ln(Total farm asset value)	9.278	0.156	9.220	0.160	6.022	0.197	5.974	0.199	12.769	0.061	12.759	0.062
Head's years of education	5.722	0.093	5.666	0.089	5.653	0.119	5.591	0.118	5.784	0.127	5.734	0.120
Spouse's years of education	4.560	0.079	4.487	0.078	4.405	0.099	4.341	0.097	4.715	0.116	4.635	0.113
Maximum years of education (of adults in HH)	7.112	0.084	7.028	0.080	6.937	0.105	6.861	0.102	7.289	0.116	7.198	0.110
Head's age	44.136	0.348	43.991	0.342	43.336	0.436	43.262	0.437	44.983	0.527	44.759	0.525
Head is polygamous (= 1)	0.113	0.009	0.113	0.009	0.091	0.010	0.091	0.010	0.135	0.015	0.135	0.016
Village characteristics												
Distance to nearest vehicular transport	8.316	0.648	8.037	0.673	8.150	0.714	8.233	0.820	8.382	0.809	7.745	0.816
Distance to nearest feeder road (in 2000)	3.264	0.170	3.280	0.174	3.486	0.219	3.501	0.224	3.017	0.187	3.031	0.191
Distance to administrative town (in 2000)	34.536	1.216	34.712	1.213	34.986	1.481	35.051	1.489	34.110	1.284	34.401	1.272
ln(Main season cumulative rainfall)	6.831	0.008	7.015	0.007	6.871	0.010	7.058	0.009	6.783	0.012	6.965	0.010
# of main-season drought shocks	2.135	0.044	1.165	0.035	1.838	0.053	0.981	0.043	2.491	0.061	1.382	0.047
No. of children	3,777		3,581		1,802		1,702		1,912		1,801	

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

Table 3. Descriptive Statistics of Children in Primary School Age 7-12 in 2004 and 11-16 in 2008, by Gender

	Girls				Boys			
	2004		2008		2004		2008	
	mean	SE	mean	SE	mean	SE	mean	SE
Dependent variables								
Primary school attendance (=1)	0.705	0.013	0.870	0.010	0.652	0.014	0.881	0.010
School advancement	0.369	0.011	0.535	0.007	0.343	0.011	0.501	0.008
Highest grade completed	1.335	0.041	3.852	0.056	1.250	0.040	3.619	0.055
Explanatory Variables								
Lagged HIV prevalence rate	13.465	0.109	13.898	0.141	13.480	0.103	14.027	0.151
Household-level adult mortality/morbidity shocks								
Past WA adult mortality (4-8 years ago)	0.094	0.009	0.053	0.008	0.079	0.008	0.055	0.007
Recent WA adult mortality (0-4 years ago)	0.051	0.007	0.045	0.007	0.054	0.007	0.046	0.006
Chronically ill adult (in previous 3 months)	0.134	0.011	0.038	0.006	0.143	0.011	0.031	0.006
Child characteristics								
Age	9.257	0.038	13.163	0.039	9.223	0.040	13.167	0.040
Age squared	88.374	0.716	175.834	1.035	87.671	0.742	175.912	1.073
Household characteristics (in 2000)								
ln(Total landholding)	0.640	0.038	0.649	0.038	0.710	0.038	0.713	0.038
ln(Total farm asset value)	9.176	0.178	9.087	0.184	9.375	0.175	9.344	0.178
Head's years of education	5.741	0.112	5.641	0.111	5.705	0.112	5.689	0.109
Spouse's years of education	4.534	0.092	4.423	0.093	4.585	0.093	4.547	0.090
Maximum years of education (of adults in HH)	7.141	0.101	7.010	0.099	7.084	0.104	7.044	0.101
Head's age	44.379	0.409	44.259	0.400	43.905	0.433	43.741	0.434
Head is polygamous (= 1)	0.115	0.011	0.115	0.011	0.112	0.010	0.111	0.010
Village characteristics								
Distance to nearest vehicular transport	8.400	0.739	7.860	0.709	8.235	0.630	8.202	0.704
Distance to nearest feeder road (in 2000)	3.324	0.184	3.350	0.188	3.206	0.173	3.215	0.176
Distance to administrative town (in 2000)	34.403	1.288	34.722	1.282	34.662	1.271	34.702	1.275
ln(Main season cumulative rainfall)	6.832	0.009	7.012	0.008	6.829	0.009	7.019	0.008
# of main-season drought shocks	2.119	0.051	1.177	0.039	2.149	0.047	1.154	0.040
No. of children	1,850		1,739		1,927		1,842	

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

5.3. School Attendance and Adult Mortality/Morbidity Shocks

We estimate a pooled Probit of child attendance on the full sample and find that children experiencing WA adult mortality and morbidity are not less likely to attend school than other children during the same period (Table 4). We then stratify the sample by household asset wealth in 2000 and find that mortality and morbidity shocks do not have significant negative effects on child schooling for children in either poor or less poor households (Table 4). However, when we stratify the sample by gender of the child (to consider whether mortality/morbidity shocks affect girls and boys differently), we find that a recent WA adult death (0-4 years ago) reduces girls' probability of attending school by -6.2 points (Table 5, column A). Given that average attendance over the two survey waves is 78%, this means that a recent WA death reduces the probability of attendance by approximately 7.9%. This effect is stronger among girls from poor households, where a recent WA death reduces attendance by -10 points or roughly 12.8% (Table 5, column C). There are no significant effects of WA mortality on boys' school attendance, which suggests that households which face mortality shocks respond with systematic bias against the schooling of girls, on average.

There are no significant effects of chronic adult illness on attendance, using either the full sample or the samples stratified by wealth or child's gender. Community-level effects of high HIV prevalence are also not significant for any of the regressions. There are two counter-intuitive results, which are that a past period WA death (which occurred 4-8 years ago) appears to increase the attendance of children from *ex ante* poor households by 6.2 points and of girls from poor households by 8.7 points.

As we would expect, the level of household assets and the education levels of adults in the household have positive and significant effects on child school attendance. However, the magnitude of the effect of household assets on attendance is negligible (a 10% increase in household farm assets increases attendance by 0.02%), while those for adult education are relatively small (Table 4). For example, a one-year increase in spouse's education increases child attendance by 0.6 points (i.e., 0.7%), while a one-year increase in the maximum adult education in the household increases attendance by 1.0 points (1.2%). Access to road infrastructure and distance from the nearest administrative town also have significant effects on child school attendance. For example, reducing the distance from the village to the nearest feeder road by one kilometer increases child school attendance by 0.5 points (or 0.6%) (Table 4). Assuming that primary schools are built near roads, this is consistent with the literature on schooling demand in developing countries which has consistently found a negative relationship between distance to the nearest school and probability of attendance.

We next disaggregate the mortality and morbidity shock variables to account for potential differences in household responses to the illness or death of a head or spouse relative to non-head/spouse members (i.e., other adults). We find that the presence of a chronically ill head/spouse in the household reduces child school attendance by -3.2 points (or 4.1%), while the death of a non-head/spouse adult lowers attendance of girls by -8.6 points (or 11%) (Table 6).

Table 4. Pooled Probit of Child School Attendance, by Wealth Category, 2004-2008

Covariates	All (A)	Poor (B)	Less Poor (B)
Lagged HIV prevalence rate	-0.004	-0.006	-0.003
	(-0.800)	(-1.083)	(-0.416)
Household-level adult mortality/morbidity shocks			
Past WA adult mortality (4-8 years ago)	0.032	0.062+	-0.004
	(1.533)	(1.916)	(-0.153)
Recent WA adult mortality (0-4 years ago)	-0.033	-0.054	-0.017
	(-1.274)	(-1.374)	(-0.533)
Chronically ill adult (in previous 3 months)	-0.005	-0.010	0.025
	(-0.315)	(-0.389)	(1.023)
Child characteristics			
Age	0.296**	0.326**	0.262**
	(21.752)	(17.624)	(13.714)
Age squared	-0.011**	-0.013**	-0.010**
	(-18.914)	(-14.881)	(-12.339)
Girl (= 1)	0.015	-0.008	0.042**
	(1.565)	(-0.585)	(3.289)
Household characteristics (in 2000)			
ln(Total landholding)	0.003	-0.008	0.009
	(0.566)	(-1.106)	(1.110)
ln(Total farm asset value)	0.002+	0.001	-0.007
	(1.826)	(0.424)	(-1.177)
Head's years of education	0.002	0.005	-0.001
	(0.914)	(1.494)	(-0.307)
Spouse's years of education	0.006**	0.007*	0.004
	(2.714)	(2.074)	(1.334)
Maximum years of education (of adults in HH)	0.010**	0.013**	0.010**
	(3.783)	(3.041)	(2.951)
Head's age	-0.000	0.000	-0.001+
	(-0.931)	(0.465)	(-1.876)
Head is polygamous (= 1)	0.005	0.035	-0.014
	(0.264)	(1.235)	(-0.536)
Village characteristics			
Distance to nearest vehicular transport	-0.000	-0.001	0.001
	(-0.341)	(-1.376)	(1.561)
Distance to nearest feeder road (in 2000)	-0.005**	-0.005*	-0.004
	(-2.584)	(-2.434)	(-1.324)
Distance to administrative town (in 2000)	-0.001**	-0.001*	-0.001**
	(-3.352)	(-2.116)	(-3.157)
ln(Main season cumulative rainfall)	0.051	0.025	0.072
	(1.031)	(0.358)	(1.042)
# of 20-day periods with <40 mm rain	-0.005	-0.006	-0.003
	(-0.587)	(-0.485)	(-0.247)
District dummies and time dummy included	Yes	Yes	Yes
No. of children	7,358	3,522	3,713

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

Table 5. Pooled Probit of Child School Attendance, by Gender and Wealth, 2004-2008

	All		Poor		Less Poor	
	Girls	Boys	Girls	Boys	Girls	Boys
Covariates	(A)	(B)	(C)	(D)	(E)	(F)
Lagged HIV prevalence rate	0.001	-0.008	0.004	-0.006	-0.007	0.001
	(0.209)	(-1.365)	(0.679)	(-0.728)	(-1.027)	(0.150)
Household-level adult mortality/morbidity shocks						
Past WA adult mortality (4-8 years ago)	0.033	0.030	0.087+	0.035	-0.030	0.012
	(1.161)	(1.175)	(1.903)	(0.968)	(-0.899)	(0.339)
Recent WA adult mortality (0-4 years ago)	-0.062+	-0.001	-0.100+	-0.020	-0.049	0.022
	(-1.726)	(-0.047)	(-1.673)	(-0.489)	(-1.302)	(0.551)
Chronically ill adult (in previous 3 months)	-0.012	-0.003	-0.018	0.009	0.027	0.011
	(-0.560)	(-0.136)	(-0.529)	(0.274)	(0.942)	(0.315)
Child characteristics						
Age	0.301**	0.284**	0.361**	0.298**	0.240**	0.279**
	(15.682)	(15.101)	(13.454)	(11.781)	(9.424)	(11.021)
Age squared	-0.012**	-0.011**	-0.014**	-0.011**	-0.010**	-0.010**
	(-13.818)	(-12.768)	(-11.400)	(-9.835)	(-8.823)	(-9.370)
Household characteristics (in 2000)						
ln(Total landholding)	0.007	-0.002	-0.011	-0.011	0.004	0.009
	(0.895)	(-0.216)	(-1.054)	(-1.005)	(0.344)	(0.919)
ln(Total farm asset value)	0.003*	0.001	-0.000	0.001	0.003	-0.018*
	(2.085)	(0.684)	(-0.008)	(0.378)	(0.404)	(-2.399)
Head's years of education	0.002	0.003	0.007	0.001	-0.006	0.004
	(0.499)	(0.870)	(1.453)	(0.253)	(-1.491)	(0.958)
Spouse's years of education	0.007*	0.006*	0.008+	0.009*	0.009*	0.003
	(2.293)	(2.076)	(1.697)	(1.980)	(2.303)	(0.696)
Maximum years of education (of adults in HH)	0.011**	0.010**	0.013*	0.013*	0.013**	0.007
	(2.715)	(2.969)	(2.285)	(2.310)	(2.786)	(1.586)
Head's age	-0.001	0.000	-0.001	0.002*	-0.001	-0.000
	(-1.544)	(0.699)	(-1.032)	(1.973)	(-0.915)	(-0.377)
Head is polygamous (= 1)	-0.006	0.015	0.045	0.015	-0.062+	0.026
	(-0.254)	(0.592)	(1.312)	(0.441)	(-1.883)	(0.745)
Village characteristics						
Distance to nearest vehicular transport	-0.001	0.001	-0.002*	-0.000	0.000	0.001
	(-1.495)	(1.329)	(-2.086)	(-0.098)	(0.427)	(1.608)
Distance to nearest feeder road (in 2000)	-0.002	-0.008**	-0.003	-0.006**	0.001	-0.009*
	(-0.677)	(-4.006)	(-0.962)	(-2.639)	(0.329)	(-2.053)
Distance to administrative town (in 2000)	-0.001**	-0.001*	-0.001	-0.001+	-0.001	-0.001
	(-2.662)	(-2.235)	(-1.399)	(-1.904)	(-1.577)	(-1.392)
ln(Main season cumulative rainfall)	0.035	0.080	-0.052	0.043	0.095	0.082
	(0.522)	(1.220)	(-0.567)	(0.502)	(1.050)	(0.861)
# of 20-day periods with <40 mm rain	-0.009	0.004	-0.007	0.002	-0.007	0.012
	(-0.793)	(0.298)	(-0.422)	(0.149)	(-0.498)	(0.638)
District dummies and time dummy included	Yes	Yes	Yes	Yes	Yes	Yes
No. of children	3,589	3,769	1,743	1,779	1,787	1,926

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

Table 6. Pooled Probit of Child School Attendance, by Household Position of Deceased/Ill Adult, and by Gender and Wealth, 2004-2008

Covariates	All (A)	Poor (B)	Less Poor (C)	Girls (D)	Boys (E)
Lagged HIV prevalence rate	-0.004	-0.006	-0.003	0.002	-0.008
	(-0.767)	(-1.089)	(-0.437)	(0.250)	(-1.367)
Household-level adult mortality/morbidity shocks					
Past WA adult mortality (4-8 years ago) - Head/spouse	0.014	0.023	0.002	0.025	0.010
	(0.319)	(0.353)	(0.025)	(0.418)	(0.193)
Past WA adult mortality (4-8 years ago) - Other	0.033	0.087*	-0.017	0.021	0.041
	(1.422)	(2.335)	(-0.666)	(0.672)	(1.341)
Recent WA adult mortality (0-4 years ago) - Head/spouse	0.002	-0.049	0.088	0.008	-0.008
	(0.049)	(-0.812)	(1.331)	(0.151)	(-0.145)
Recent WA adult mortality (0-4 years ago) - Other	-0.043	-0.056	-0.033	-0.086*	0.002
	(-1.529)	(-1.285)	(-0.933)	(-2.272)	(0.069)
Chronically ill adult (last 3 months) - Head/spouse	-0.032+	-0.029	-0.024	-0.040	-0.023
	(-1.695)	(-0.892)	(-0.840)	(-1.531)	(-0.842)
Chronically ill adult (last 3 months) - Other	0.046+	0.030	0.102**	0.041	0.034
	(1.787)	(0.817)	(2.766)	(1.291)	(0.987)
Child characteristics					
Age	0.297**	0.326**	0.265**	0.302**	0.285**
	(21.904)	(17.693)	(13.819)	(15.823)	(15.106)
Age squared	-0.011**	-0.013**	-0.010**	-0.012**	-0.011**
	(-19.034)	(-14.926)	(-12.409)	(-13.926)	(-12.806)
Girl (= 1)	0.015	-0.009	0.042**		
	(1.544)	(-0.629)	(3.318)		
Household characteristics (in 2000)					
ln(Total landholding)	0.003	-0.007	0.007	0.006	-0.001
	(0.604)	(-0.950)	(0.891)	(0.804)	(-0.121)
ln(Total farm asset value)	0.002+	0.001	-0.008	0.003*	0.001
	(1.826)	(0.322)	(-1.332)	(2.106)	(0.636)
Head's years of education	0.003	0.006+	-0.001	0.002	0.003
	(1.041)	(1.717)	(-0.290)	(0.468)	(1.117)
Spouse's years of education	0.006**	0.007*	0.004	0.007*	0.006*
	(2.747)	(2.186)	(1.200)	(2.260)	(2.173)
Maximum years of education (of adults in HH)	0.010**	0.011**	0.010**	0.011**	0.009**
	(3.638)	(2.703)	(3.052)	(2.688)	(2.619)
Head's age	-0.000	0.000	-0.001*	-0.001	0.000
	(-0.986)	(0.449)	(-1.981)	(-1.510)	(0.604)
Head is polygamous (= 1)	0.007	0.036	-0.012	-0.005	0.016
	(0.328)	(1.312)	(-0.458)	(-0.213)	(0.638)
Village characteristics				0	0
Distance to nearest vehicular transport	-0.000	-0.001	0.001	-0.001	0.001
	(-0.407)	(-1.381)	(1.594)	(-1.490)	(1.177)
Distance to nearest feeder road (in 2000)	-0.005*	-0.005*	-0.004	-0.002	-0.008**
	(-2.565)	(-2.422)	(-1.271)	(-0.713)	(-3.848)
Distance to administrative town (in 2000)	-0.001**	-0.001*	-0.001**	-0.001**	-0.001*
	(-3.347)	(-2.064)	(-3.211)	(-2.686)	(-2.188)
ln(Main season cumulative rainfall)	0.052	0.030	0.079	0.038	0.082
	(1.053)	(0.419)	(1.121)	(0.575)	(1.248)
# of 20-day periods with <40 mm rain in main season	-0.006	-0.006	-0.002	-0.009	0.004
	(-0.617)	(-0.508)	(-0.161)	(-0.808)	(0.293)
				0	0
District dummies and time dummy included	Yes	Yes	Yes	Yes	Yes
No. of children	7,353	3,520	3,710	3,587	3,766

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

However, we also find two results that are counter-intuitive: the presence of an ill non-head spouse *increases* the probability of attendance by 4.6 points (5.8%), while a past period non-head/spouse WA death *increases* attendance by 8.7 points (11%). This latter result is difficult to explain and requires further investigation. By contrast, the apparent positive effect of a chronically ill adult on attendance may not be causal, but rather might be due to a wealth effect not currently controlled for by our wealth proxies. That is, if wealthier rural households in Zambia are more likely to have high-return non-farm income and remittances from migrant members (as has been found in other Sub-Saharan African countries by Reardon (1997)), then it's possible that the 'chronically ill – other adult' dummy is picking up the effect of non-farm income (wealth) that may not be invested in farm equipment and livestock. For example, given that recent research finds that temporary migrant laborers in rural Zambia have higher disease-related adult mortality rates (Chapoto et al. 2009), these ill adults may well be migrants who have returned home to relatively wealthy households for caregiving; because the household is relatively wealthy, this does not result in adverse effects on child schooling. This potential explanation is consistent with the fact that we find a positive association between ill adults and child schooling among households with higher farm assets and not among poorer ones (Table 6, columns B and C).

We next disaggregate the mortality and morbidity shock variables to test for potential differences in household responses by gender of the ill or deceased adult. We find that the presence of a chronically ill male adult in the household reduces the probability of child school attendance by 3.7 points (or 4.9%) (Table 7). This effect is somewhat stronger among children from poorer households, where it reduces attendance by 6.4 points (8.5%). There are no other significant negative effects of adult mortality or morbidity on child schooling, although we note that sign of the effects of both recent male and female deaths are negative and their magnitudes are considerably larger among poorer households relative to less poor households.

Finally, we stratify the sample by child's gender and find that the negative effect of chronically ill male adults on school attendance appears to be borne primarily by girls, whose probability of schooling declines by 6.2 points (8.2%) (Table 8). This effect is somewhat stronger among poorer households, where it reduces girls' schooling by 9.4 points (12.5%). There are no other significant negative effects of adult mortality or morbidity on attendance by child's gender, although we note that sign of the effects of recent male and female deaths are negative and their magnitude considerably larger among girls (especially poor girls) relative to boys.

5.4. School Advancement, Highest Grade Completed, and Adult Mortality/Morbidity Shocks

Given that we have found negative effects of WA adult mortality and morbidity on school attendance (primarily for girls), we would expect to find similar results in the school advancement model, in the event that intermittent school attendance results in delayed grade advancement. We estimate a pooled Tobit regression of school advancement, the results of which are largely consistent with those from the attendance model (Table 9). First, we do not find significant negative effects of past mortality, recent mortality or current morbidity shocks on school advancement, using either the full sample or the samples stratified by *ex ante* household wealth.

Table 7. Pooled Probit of Child School Attendance, by Gender of Deceased/Ill Adult, and by Wealth, 2004-2008

Covariates	All (A)	Poor (B)	Less Poor (C)
Lagged HIV prevalence rate	-0.004	-0.006	-0.003
	(-0.850)	(-1.140)	(-0.409)
Household-level adult mortality/morbidity shocks			
Past WA adult mortality (4-8 years ago) - MALE	0.038	0.099+	-0.008
	(1.223)	(1.955)	(-0.224)
Past WA adult mortality (4-8 years ago) - FEMALE	0.028	0.054	-0.016
	(1.035)	(1.337)	(-0.489)
Recent WA adult mortality (0-4 years ago) - MALE	-0.022	-0.051	0.003
	(-0.632)	(-1.048)	(0.070)
Recent WA adult mortality (0-4 years ago) - FEMALE	-0.041	-0.058	-0.028
	(-1.175)	(-1.027)	(-0.633)
Chronically ill adult (last 3 months) - MALE	-0.037+	-0.064*	0.010
	(-1.796)	(-2.289)	(0.340)
Chronically ill adult (last 3 months) - FEMALE	0.029	0.042	0.032
	(1.373)	(1.233)	(1.053)
Child characteristics			
Age	0.296**	0.326**	0.262**
	(21.726)	(17.582)	(13.652)
Age squared	-0.011**	-0.013**	-0.010**
	(-18.904)	(-14.861)	(-12.294)
Girl (= 1)	0.016	-0.008	0.042**
	(1.613)	(-0.555)	(3.282)
Household characteristics (in 2000)			
ln(Total landholding)	0.004	-0.007	0.009
	(0.741)	(-0.907)	(1.155)
ln(Total farm asset value)	0.002+	0.000	-0.007
	(1.816)	(0.301)	(-1.171)
Head's years of education	0.003	0.006+	-0.001
	(1.077)	(1.770)	(-0.345)
Spouse's years of education	0.006**	0.007*	0.004
	(2.766)	(2.091)	(1.300)
Maximum years of education (of adults in HH)	0.010**	0.012**	0.010**
	(3.654)	(2.778)	(3.000)
Head's age	-0.000	0.000	-0.001+
	(-0.805)	(0.639)	(-1.838)
Head is polygamous (= 1)	0.005	0.035	-0.015
	(0.236)	(1.263)	(-0.541)
Village characteristics			
Distance to nearest vehicular transport	-0.000	-0.001	0.001
	(-0.399)	(-1.444)	(1.570)
Distance to nearest feeder road (in 2000)	-0.005*	-0.005*	-0.004
	(-2.532)	(-2.366)	(-1.353)
Distance to administrative town (in 2000)	-0.001**	-0.001*	-0.001**
	(-3.272)	(-2.024)	(-3.141)
ln(Main season cumulative rainfall)	0.053	0.028	0.073
	(1.077)	(0.394)	(1.052)
# of 20-day periods with <40 mm rain in main season	-0.005	-0.006	-0.003
	(-0.593)	(-0.486)	(-0.251)
District dummies and time dummy included	Yes	Yes	Yes
No. of children	7,353	3,520	3,710

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels: ** p<0.01; * p<0.05; + p<0.10

Table 8. Pooled Probit of Child School Attendance, by Gender of Deceased/Ill Adult, and by Child Gender and Wealth, 2004-2008

	All		Poor		Less Poor	
	Girls	Boys	Girls	Boys	Girls	Boys
Covariates	(A)	(B)	(C)	(D)	(E)	(F)
Lagged HIV prevalence rate	0.001	-0.008	0.004	-0.006	-0.008	0.001
	(0.125)	(-1.382)	(0.644)	(-0.729)	(-1.095)	(0.124)
Household-level adult mortality/morbidity shocks						
Past WA adult mortality (4-8 years ago) - MALE	0.006	0.057	0.088	0.075	-0.056	0.023
	(0.146)	(1.327)	(1.344)	(1.203)	(-1.354)	(0.456)
Past WA adult mortality (4-8 years ago) - FEMALE	0.046	0.012	0.092+	0.028	-0.037	-0.005
	(1.248)	(0.411)	(1.703)	(0.622)	(-0.804)	(-0.110)
Recent WA adult mortality (0-4 years ago) - MALE	-0.048	0.002	-0.118	-0.007	0.009	-0.004
	(-1.012)	(0.046)	(-1.481)	(-0.116)	(0.151)	(-0.071)
Recent WA adult mortality (0-4 years ago) - FEMALE	-0.073	-0.006	-0.093	-0.034	-0.080	0.037
	(-1.538)	(-0.165)	(-1.172)	(-0.673)	(-1.574)	(0.692)
Chronically ill adult (last 3 months) - MALE	-0.062*	-0.017	-0.094*	-0.030	-0.009	0.030
	(-2.235)	(-0.552)	(-2.337)	(-0.683)	(-0.220)	(0.686)
Chronically ill adult (last 3 months) - FEMALE	0.035	0.019	0.036	0.052	0.060	-0.020
	(1.207)	(0.605)	(0.790)	(1.183)	(1.512)	(-0.456)
Child characteristics						
Age	0.302**	0.284**	0.362**	0.298**	0.240**	0.279**
	(15.826)	(15.120)	(13.533)	(11.760)	(9.434)	(10.953)
Age squared	-0.012**	-0.011**	-0.014**	-0.011**	-0.010**	-0.010**
	(-13.976)	(-12.776)	(-11.486)	(-9.810)	(-8.857)	(-9.272)
Household characteristics (in 2000)						
ln(Total landholding)	0.007	-0.000	-0.011	-0.008	0.004	0.009
	(0.958)	(-0.049)	(-1.028)	(-0.791)	(0.381)	(0.876)
ln(Total farm asset value)	0.003*	0.001	-0.000	0.000	0.003	-0.019*
	(2.131)	(0.653)	(-0.032)	(0.239)	(0.412)	(-2.469)
Head's years of education	0.002	0.004	0.007	0.004	-0.007	0.004
	(0.470)	(1.180)	(1.462)	(0.796)	(-1.639)	(0.940)
Spouse's years of education	0.007*	0.007*	0.008+	0.009*	0.009*	0.003
	(2.309)	(2.189)	(1.735)	(2.040)	(2.272)	(0.705)
Maximum years of education (of adults in HH)	0.011**	0.009**	0.013*	0.011+	0.013**	0.007
	(2.721)	(2.599)	(2.359)	(1.855)	(2.887)	(1.599)
Head's age	-0.001	0.000	-0.001	0.002*	-0.001	-0.000
	(-1.517)	(0.840)	(-1.042)	(2.200)	(-0.833)	(-0.371)
Head is polygamous (= 1)	-0.009	0.015	0.042	0.018	-0.064+	0.027
	(-0.360)	(0.611)	(1.246)	(0.534)	(-1.923)	(0.769)
Village characteristics						
Distance to nearest vehicular transport	-0.001	0.001	-0.002*	-0.000	0.000	0.001
	(-1.513)	(1.243)	(-2.071)	(-0.148)	(0.389)	(1.578)
Distance to nearest feeder road (in 2000)	-0.002	-0.008**	-0.003	-0.005*	0.001	-0.009*
	(-0.729)	(-3.824)	(-1.111)	(-2.441)	(0.291)	(-2.093)
Distance to administrative town (in 2000)	-0.001**	-0.001*	-0.001	-0.001+	-0.001	-0.001
	(-2.595)	(-2.226)	(-1.320)	(-1.877)	(-1.506)	(-1.420)
ln(Main season cumulative rainfall)	0.040	0.082	-0.049	0.047	0.101	0.080
	(0.600)	(1.250)	(-0.541)	(0.540)	(1.115)	(0.836)
# of 20-day periods with <40 mm rain	-0.010	0.004	-0.006	0.003	-0.008	0.012
	(-0.823)	(0.319)	(-0.405)	(0.185)	(-0.537)	(0.635)
District dummies and time dummy included	Yes	Yes	Yes	Yes	Yes	Yes
No. of children	3,587	3,766	1,743	1,777	1,785	1,925

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

Table 9. Pooled Tobit of Child Schooling Progress, by Gender and Wealth, 2004-2008

				All		Poor		Less Poor	
	All	Poor	Less Poor	Girls	Boys	Girls	Boys	Girls	Boys
Covariates	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)	(I)
Household-level adult mortality/morbidity shocks									
Past WA adult mortality (4-8 years ago)	0.009	0.021	-0.004	0.044	0.007	0.101 *	-0.011	-0.041	0.025
	0.231	0.439	0.022	1.963	0.068	4.816	0.102	1.426	0.347
Recent WA adult mortality (0-4 years ago)	-0.027	-0.033	-0.020	-0.030	-0.009	-0.063	0.024	-0.006	-0.039
	1.708	0.990	0.341	0.917	0.081	1.989	0.326	0.017	0.639
Chronically ill adult (in previous 3 months)	0.012	-0.014	0.047 +	0.004	0.010	-0.036	-0.011	0.079 *	0.038
	0.410	0.234	2.737	0.024	0.139	1.210	0.074	4.605	0.887
All other child, household & village co-variates included[1]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District dummies and time dummy included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of children	7,358	3,522	3,713	3,589	3,769	1,743	1,779	1,787	1,926

Notes: 1) Each regression includes all child, household and village co-variates from the school attendance models (Table 5.2). Reported coeffcients are Average Partial Effects (APE) on the unconditional grade progress; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

Second, when we stratify the sample by child's gender and *ex ante* household wealth, we find that the unconditional effect of a recent WA death on school advancement is negative and nearly significant for girls from poor households (p=0.15). For example, a recent WA adult death in the household reduces the school advancement of girls by -6.3 (Table 9). Given that the average school advancement score for girls in 2004 and 2008 is 0.45, this means that a recent WA adult death reduces school advancement by 14% among girls from poor households (though we note this effect is not significant at the 10% level).

To check the robustness of these results, we also estimate the school advancement model using OLS with household fixed effects (and note that the large number of cases with zero school advancement may not be appropriate for OLS). While we find that the effects of adult mortality tend to be negative for girls, none of the effects are significant in any of the various FE regressions. The same results obtain when we estimate the school advancement model using OLS with household fixed effects using only observations with non-zero school advancement ratios.

We then estimate the highest grade completed model using a Negative Binomial regression, first with CRE terms and then with household FE. In none of these regressions do we find significant negative effects of mortality/morbidity shocks on highest grade completed. We note that when estimating the Negative Binomial with household FE, Stata does not enable us to apply population weights to the negative binomial with FE, which may well affect the results, given that results from other models demonstrate that use or not of population weights significantly affects the results.

In summary, the results from the school advancement and highest grade completed models suggest that although we find evidence that mortality and morbidity shocks result in lower attendance for some children – namely, girls and especially girls from poor households – that reduced attendance does not appear to result in delayed grade progression. The exception to this is that the effect of a recent WA death on girls from poor households is sizeable (8.4% loss) and nearly significant.

5.5. School Attendance and Orphan Status

We next investigate the extent to which orphan status affects child schooling outcomes. Using pooled cross-sectional samples of children from 2004 and 2008 who are age 7-11, we estimate the primary school attendance model using pooled Probit and find that there are no significant negative effects of orphan status on child school attendance (Table 10). Likewise, if we stratify the sample by wealth status of the household (Table 10), or by gender of the child (Table 11), we also find no significant negative effects of orphan status on attendance.

Given that we found evidence that adult mortality significantly reduces schooling of girls, while chronic illness of a head/spouse can result in schooling losses, it is surprising that we do not find evidence of schooling losses due to orphan status. There are at least two explanations for the lack of significant negative effects of orphanhood on school attendance. First, it is possible that the lack of orphanhood effects on attendance indicates that these orphans no longer reside in the

Table 10. Pooled Probit of Child School Attendance, by Orphan Status, Gender, and Wealth, 2004-2008

Covariates	All (A)	Poor (B)	Less Poor (B)
Lagged HIV prevalence rate	0.002	0.012+	-0.008
	(0.334)	(1.767)	(-0.876)
Child characteristics			
Paternal orphan (= 1)	0.058**	0.081**	0.035
	(2.731)	(2.585)	(1.301)
Maternal orphan (= 1)	-0.016	-0.075	0.039
	(-0.432)	(-1.311)	(0.823)
Double-parent orphan (= 1)	0.026	0.030	0.026
	(0.911)	(0.684)	(0.653)
Age	0.485**	0.430**	0.555**
	(8.701)	(5.872)	(7.158)
Age squared	-0.021**	-0.018**	-0.026**
	(-6.825)	(-4.344)	(-5.908)
Girl (= 1)	0.044**	0.034*	0.057**
	(3.993)	(2.252)	(3.918)
Household characteristics (in 2000)			
ln(Total landholding)	-0.002	-0.014	0.006
	(-0.254)	(-1.485)	(0.683)
ln(Total farm asset value)	0.004**	0.002	0.001
	(2.940)	(0.915)	(0.098)
Head's years of education	0.009**	0.016**	-0.000
	(3.108)	(3.683)	(-0.073)
Spouse's years of education	0.011**	0.012**	0.008*
	(4.336)	(3.465)	(2.360)
Maximum years of education (of adults in HH)	0.005+	0.001	0.011**
	(1.673)	(0.278)	(2.837)
Head's age	0.000	0.001	-0.001
	(0.281)	(1.355)	(-1.302)
Head is polygamous (= 1)	0.019	0.055+	-0.013
	(0.898)	(1.690)	(-0.476)
Village characteristics			
Distance to nearest vehicular transport	0.000	0.000	0.001
	(0.619)	(0.265)	(0.947)
Distance to nearest feeder road (in 2000)	-0.006**	-0.005*	-0.007*
	(-2.906)	(-2.165)	(-2.080)
Distance to administrative town (in 2000)	-0.001**	-0.001**	-0.001
	(-2.801)	(-3.069)	(-1.279)
ln(Main season cumulative rainfall)	0.039	0.004	0.113
	(0.663)	(0.054)	(1.437)
# of 20-day periods with <40 mm rain	0.005	-0.012	0.026*
	(0.599)	(-1.062)	(2.155)
District dummies and time dummy included	Yes	Yes	Yes
No. of children	7,759	3,759	3,870

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

Table 11. Pooled Probit of Child School Attendance, by Orphan Status, Gender, and Wealth, 2004-2008

	All		Poor		Less Poor	
	Girls	Boys	Girls	Boys	Girls	Boys
Covariates	(A)	(B)	(C)	(D)	(E)	(F)
Lagged HIV prevalence rate	0.003	0.001	0.018*	0.000	-0.008	0.004
	(0.328)	(0.098)	(1.975)	(0.024)	(-0.951)	(0.362)
Child characteristics						
Paternal orphan (= 1)	0.052+	0.061*	0.074+	0.090*	0.040	0.042
	(1.819)	(2.283)	(1.823)	(2.386)	(1.040)	(1.229)
Maternal orphan (= 1)	0.050	-0.059	-0.017	-0.098	0.125	-0.026
	(0.964)	(-1.204)	(-0.227)	(-1.416)	(1.628)	(-0.447)
Double-parent orphan (= 1)	0.051	-0.003	0.050	-0.004	0.041	0.012
	(1.238)	(-0.091)	(0.817)	(-0.067)	(0.719)	(0.221)
Age	0.417**	0.525**	0.543**	0.310**	0.363**	0.701**
	(5.287)	(6.535)	(5.195)	(2.964)	(3.473)	(6.271)
Age squared	-0.018**	-0.023**	-0.024**	-0.011+	-0.016**	-0.033**
	(-4.074)	(-5.091)	(-4.188)	(-1.844)	(-2.692)	(-5.269)
Household characteristics (in 2000)						
ln(Total landholding)	0.002	-0.006	-0.013	-0.016	0.002	0.004
	(0.225)	(-0.689)	(-1.031)	(-1.194)	(0.153)	(0.354)
ln(Total farm asset value)	0.004*	0.004*	-0.000	0.003	0.010	-0.010
	(2.099)	(2.446)	(-0.162)	(1.136)	(1.202)	(-1.085)
Head's years of education	0.008*	0.010**	0.013*	0.019**	0.001	0.001
	(2.269)	(2.976)	(2.375)	(3.528)	(0.293)	(0.110)
Spouse's years of education	0.011**	0.012**	0.014**	0.015**	0.006	0.010*
	(3.254)	(3.507)	(2.644)	(3.298)	(1.524)	(2.031)
Maximum years of education (of adults in HH)	0.003	0.007+	0.002	-0.002	0.008	0.015**
	(0.759)	(1.715)	(0.352)	(-0.257)	(1.529)	(3.151)
Head's age	-0.001	0.001	-0.000	0.003**	-0.001	-0.001
	(-0.896)	(1.504)	(-0.408)	(2.684)	(-0.989)	(-0.678)
Head is polygamous (= 1)	-0.014	0.052+	0.011	0.082*	-0.050	0.037
	(-0.509)	(1.907)	(0.266)	(2.094)	(-1.369)	(1.057)
Village characteristics						
Distance to nearest vehicular transport	0.000	0.000	0.000	0.000	0.000	0.000
	(0.091)	(0.726)	(0.069)	(0.090)	(0.643)	(0.456)
Distance to nearest feeder road (in 2000)	-0.001	-0.010**	-0.002	-0.007**	-0.000	-0.013**
	(-0.423)	(-4.325)	(-0.535)	(-2.648)	(-0.073)	(-3.214)
Distance to administrative town (in 2000)	-0.001**	-0.001+	-0.001*	-0.001*	-0.001	-0.000
	(-2.617)	(-1.768)	(-2.478)	(-2.003)	(-1.384)	(-0.614)
ln(Main season cumulative rainfall)	0.084	-0.007	-0.036	0.086	0.200*	-0.045
	(1.062)	(-0.106)	(-0.316)	(0.991)	(2.059)	(-0.410)
# of 20-day periods with <40 mm rain	-0.001	0.010	-0.011	-0.016	0.016	0.036+
	(-0.081)	(0.809)	(-0.707)	(-1.131)	(1.042)	(1.841)
District dummies and time dummy included	Yes	Yes	Yes	Yes	Yes	Yes
No. of children	3,832	3,927	1,854	1,905	1,911	1,959

Notes: Coeffcients are Average Partial Effects (APE) on probability of school attendance; numbers in parentheses are absolute robust z-scores; significance levels indicated by: ** p<0.01; * p<0.05; + p<0.10

household where their parent died (i.e., they were possibly relocated to live with relatively wealthier relatives, in a household free of mortality/morbidity shocks). However, this potential explanation is not supported by the fact that orphans are just as likely to be found in *ex ante* poor households as in less poor ones. Second, it's also possible that orphan status is simply not a good indicator of schooling disadvantage because it doesn't indicate when the mortality/ morbidity shock occurred. That is, given that our attendance models in the previous section found that the negative effects of adult mortality on attendance occur mostly during the illness and immediate post-death period, then if many of the orphans happen to be children who lost one or both parents more than 4 years ago (we don't know this one way or the other), then it is not surprising that they may not have lower attendance now. If these orphans experienced schooling losses in the past, we would more likely detect evidence of this in the regressions on grade progress (which we address in the next section).

Returning to the orphan-attendance results, there is evidence that paternal orphans – both boys and girls – are 5.2% and 6.1% *more* likely to attend school than non-orphans (Table 11). One potential explanation for this counter-intuitive result is that results from other studies have found that some orphans actually experience improved schooling outcomes where such orphans are moved to the households of relatively wealthy relatives. That is, once the child loses one or both parents, they may be sent to live with relatives who may live closer to a primary school and/or have more financial resources and stronger preferences for child schooling relative to the child's immediate family – all of which could result in an improvement for the orphan's probability of attending school (notwithstanding the likely negative effects of emotional trauma on the child's schooling). However, this explanation does not seem to fit the Zambia situation, as we find that the positive effect of parental orphan status is found in the regression of relatively poor households. Thus, whether these paternal orphans migrated to a different household or not, they managed to have better school attendance than non-orphans, although they currently live in households which are relatively poor (as measured by household farm assets in 2000).

5.6. School Advancement, Highest Grade Completed, and Orphan Status

While we did not find any significant negative effects of orphan status on current school attendance, it is still possible that orphans may have incurred negative schooling effects from their parent's death at some point in the past. If this is the case, then we would more likely detect such effects in orphan's school advancement ratio.

For each of the samples analyzed in the attendance section (i.e., all children, those from poor/less poor households, etc.), we estimate a Tobit with CRE terms on school advancement (and then OLS with household fixed effects).[5] In none of these regressions do we find significant negative effects of orphan status on school advancement. We then estimate the highest grade completed model using Negative Binomial regression with CRE terms (and then with household fixed effects), again for each of the subsamples studied above. In none of these regressions do we find significant negative effects of orphan status on highest grade completed.

In summary, the evidence from our survey data from 2004 and 2008 suggests that orphans age 7-11 in rural Zambia – whether paternal, maternal, or double-parent – do not have lower school attendance, school advancement or grade completion relative to non-orphans. These results are contrary to those from Ainsworth and Filmer (2006), who used household survey data from a 2003 LCMS and estimated orphan school enrollment differentials using regressions controlling for current household wealth and other household and child-level factors. They found that while

[5] For the household fixed effect regressions, instead of estimating the model for girls separately from boys, we interact the girl binary indicator variable with the mortality/morbidity shock dummies and estimate the model with boys and girls together.

paternal orphans did not have significantly lower school enrollment than non-orphans, maternal and double-parent orphans had 9.6% and 7.0% lower enrollment, respectively.

There are at least two potential explanations for the disparity in orphan-schooling results between this study and that by Ainsworth and Filmer (AF). First, while both studies are based on observations of child schooling during a period with no primary school fees, it is possible that orphan schooling in rural Zambia has improved in the latter part of this decade (i.e., when our survey data was collected in 2004 and 2008) given the continued investment over time by the Zambian government in school infrastructure. Second, the methodologies used by the two papers to estimate orphan schooling deficits are somewhat different. Because AF used a cross-sectional dataset, they are not able to control for household characteristics prior to the parental death. By contrast, our three-wave panel dataset enables us to control for household characteristics in the year 2000 (which still may or may not be prior to the parental death of children who are reported as orphans in 2004 and 2008). However, if adult mortality results in diminished household assets (i.e., lower wealth), and poorer households are less likely to send their children to school, then one would more likely find evidence of orphan schooling deficits using panel and not cross-sectional data (and yet AF's analysis found orphan schooling deficits while ours did not). A more likely source of methodological difference is the fact that AF's analysis was not able to control for education of the household head or other adults in the household (proxies of household preferences for child schooling) or for distance to the nearest road (a proxy for distance to primary school). Either of these factors could be correlated with adult mortality, and thus potentially bias the results from a cross-sectional analysis which did not control for them. Nevertheless, our earlier analysis found evidence that recent adult death and current chronic illness of either a male or head/spouse adult can significantly lower attendance among girls (especially those from poor families), thus it is not necessarily inconsistent for AF to find orphan schooling deficits while we do not.

6. CONCLUSIONS

During the last decade, the Zambian government has dramatically increased expenditures on primary and secondary schooling, and enrollment rates have risen dramatically. At the same time, Zambia has faced the challenge of rising HIV prevalence and the possibility that recent gains in long-term human capital development could be eroded if households which suffer the death of a WA adult pull their children out of school due to family labor shortages or financial constraints. This paper uses panel survey data from rural Zambia to measure the impact of WA adult mortality and morbidity on primary school attendance and school advancement, and separately tests the extent to which orphan status affects these schooling outcomes. There are five principal findings from our analysis.

First, we find that a homogenous conceptualization of WA adult mortality and morbidity shocks are not by themselves a reliable indicator of poor child schooling outcomes. For example, while we find that the effect of WA adult mortality and morbidity does not have a significant negative effect on primary school attendance using the full sample of children, we do find significant negative effects in some cases when we consider the gender of the child, the pre-death wealth level of the household, and/or the gender and household position of the deceased or ill adult.

Second, we find that effects of chronic adult illness on child school attendance depend upon the household position and gender of the ill adult. For example, when we disaggregate morbidity shocks by household position of the ill adult, we find that the presence of a chronically ill head or spouse reduces attendance by 4.1%. We also find that the presence of a chronically ill male adult reduces attendance of girls by 8.5%.

Third, we find that households in rural Zambia are more likely to respond to adult mortality or morbidity shocks by reducing the attendance of girls relative to boys. For example, when we stratify the sample by gender of the child, we find a recent WA adult death (0-4 years ago) reduces girls' probability of attending school by 7.9%, while the presence of a chronically ill male adult in the household reduces girls' school attendance by 8.5%. The fact that negative impacts of WA mortality on girls' schooling are larger in magnitude than those for boys, and are significant for girls from both poor and less-poor households (while insignificant for boys), suggests that there is a clear gender bias in rural Zambia in how households respond to the death or chronic illness of a WA adult.

Fourth, we find that the effects of adult mortality and morbidity on girls' attendance are of larger magnitude and more likely to be significant for girls from poorer households. For example, while a recent WA adult death reduces girls' probability of attending school by 7.9%, this effect is stronger among girls from poorer households where a recent WA death reduces attendance by -10 points or roughly 12.8%. Likewise, while we find that the presence of a chronically ill male adult reduces girls' attendance by 8.5%, this effect is stronger among poorer households, where it reduces girls' schooling by 12.5%. The fact that we find significant or larger impacts among children from poor households suggests that the opportunity costs of children in such households become high during the illness or following the death of a WA adult. It is likely that the financial constraints and increased labor demands faced by poorer households who suffer a WA adult death leads them to reallocate the time of children from school to family labor following the death of a WA adult.

Fifth, although we find evidence that mortality and morbidity shocks reduce attendance for some children – namely, for girls, and especially girls from poor households – this negative effect on attendance does not appear to result in delayed grade progression, as we do not find evidence that these shocks result in significant losses for either school advancement or highest grade completed. An exception to this is that the effect of a recent WA death on girls from poor households is sizeable – indicating an 8.4% loss in school advancement – and nearly significant. It is possible that lower attendance does not result in delayed grade progression in the event that children are effectively graduated from one grade to the next automatically, due to the surge in schooling demand following the abolition of primary school fees. Nevertheless, the magnitude of the reductions in attendance for girls due to mortality and morbidity shocks are large enough to warrant the concern of policymakers, though measuring the potential effects of these shocks on a child's actual learning would require a much different and more in-depth methodological approach.

Sixth, we find that there are no significant negative effects of orphan status (parental, maternal or double-parent orphans) on either child school attendance or school advancement, regardless of whether we use the full sample or samples stratified by household wealth or gender of the child. Because orphans in our sample are just as likely to be found in relatively *ex ante* poor or wealthy households, this appears to rule out the possibility that insignificant effects of orphan status on schooling outcomes is due to the orphans' migration from their original household. Rather, this suggests instead that orphan status is simply not a good indicator of potential schooling disadvantage, perhaps because information on a child's orphan status by itself doesn't indicate when the mortality or morbidity shock occurred. However, if these orphans suffered schooling losses at some point in the past, these negative effects should theoretically show up in the regressions of school advancement, and yet we do not find evidence of significant negative effects of orphan status on school advancement.

There are several policy implications from these results. First, because the extent to which childrens' schooling outcomes are affected by adult mortality or morbidity is specific to the gender of the child, the household's wealth level, characteristics of the deceased or ill adult, and the timing of the adult death, it is inappropriate to categorize all children in Zambia who are directly or indirectly affected by HIV/AIDS-related morbidity and mortality as being especially vulnerable and in need of targeted school subsidies. Policymakers should therefore resist the temptation to borrow a 'best practice' model of HIV mitigation strategy from other SSA countries, given that results from Zambia and several other countries demonstrate that the effects of adult mortality and morbidity vary considerably across both countries and household wealth levels.

Second, it follows that social protection and education policymakers concerned with primary school under-enrollment in Zambia need to tailor mitigation measures to the specific needs and situation of children in rural Zambia. The evidence in this paper suggests that girls from households with a currently ill head/spouse or male adult, as well as girls from households with a recent WA adult death (i.e., within in the past 0-4 years) – especially those from poorer households – are most likely to face losses in school attendance and advancement. Mitigation measures appropriate for rural Zambia may therefore include conditional cash transfers targeted to girls from poorer households which have incurred these mortality/morbidity shocks. Such assistance might not only ensure that these girls attend school but could also enable poorer households to hire additional labor rather than pulling other children from school to meet family labor demands.

Third, although Zambia abolished primary school fees over a decade ago, there may still be barriers to enrollment such as continued household demand for child labor, additional educational expenses for transport, school uniforms and books, and declining school quality if enrollment outpaces new school construction and teacher hiring. These additional barriers to enrollment may explain why we have found evidence of negative effects of adult mortality and morbidity on girls' schooling, even in a time period after the government had abolished primary school fees. In addition, targeted schooling subsidies alone may not reduce schooling deficits of some orphans, in the event that their poor schooling progress is due to the emotional and psychological trauma of losing one or both parents or a lack of interest by their adult guardians in their schooling.

Fourth, Zambia should continue to provide universal free primary schooling, as this policy has been found in a number of countries to improve the enrollment and schooling progress of those children most likely to suffer from poor schooling – namely children from poorer households, both orphan and non-orphan alike. For example, evidence from Malawi and Uganda suggest that improvements in enrollments among the poor through universal abolition of primary school fees can substantially raise the enrollment of orphans, even to the point of eradicating orphan schooling deficits (Ainsworth and Filmer 2006). Finally, it should be noted that because of the well-established positive correlation between educational attainment and safer sexual behavior (World Bank 1999), Education for All is itself an important policy that can help reduce the spread of HIV/AIDS and thus the potential for negative shocks to child schooling (Ainsworth and Filmer 2006).

APPENDICES

Appendix Table A1. Number of Cases of Children Age 7-12 in 2004 and 12-16 in 2008 by Mortality/Morbidity Shock, Gender, and Household Wealth

	No. of cases	
	2004	2008
All children age 7-12 in 2004 (12-16 in 2008)		
Unafflicted households	2800	3102
HH with past period WA death (4-8 years ago)	341	211
past period WA head/spouse death	59	42
past period WA other death	282	182
HH with recent WA death (0-4 years ago)	223	170
recent WA head/spouse death	41	71
recent WA other death	194	99
HH with current WA chronically ill adult	533	129
WA chronically ill adult, head/spouse	370	73
WA chronically ill adult, other	222	62
Children in poor households (bottom 50% of farm assets/AE)		
Unafflicted households	1353	1490
HH with past period WA death (4-8 years ago)	157	91
past period WA head/spouse death	33	22
past period WA other death	124	78
HH with recent WA death (0-4 years ago)	96	80
recent WA head/spouse death	19	36
recent WA other death	84	44
HH with current WA chronically ill adult	246	72
WA chronically ill adult, head/spouse	177	42
WA chronically ill adult, other	99	34
Children in less poor households (top 50% of farm assets/AE)		
Unafflicted households	1397	1556
HH with past period WA death (4-8 years ago)	177	115
past period WA head/spouse death	25	17
past period WA other death	152	102
HH with recent WA death (0-4 years ago)	122	88
recent WA head/spouse death	19	33
recent WA other death	108	55
HH with current WA chronically ill adult	277	57
WA chronically ill adult, head/spouse	186	31
WA chronically ill adult, other	120	28

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

Appendix Table A2. Number of Cases of Children Age 7-12 in 2004 and 12-16 in 2008 by Mortality/Morbidity Shock, Gender, and Household Wealth

	No. of cases	
	2004	2008
Girls, age 7-12 in 2004 (12-16 in 2008)		
Unafflicted households	1372	1503
HH with past period WA death (4-8 years ago)	173	100
past period WA head/spouse death	35	23
past period WA other death	138	83
HH with recent WA death (0-4 years ago)	106	81
recent WA head/spouse death	23	36
recent WA other death	89	45
HH with current WA chronically ill adult	249	67
WA chronically ill adult, head/spouse	166	40
WA chronically ill adult, other	109	29
Boys, age 7-12 in 2004 (12-16 in 2008)		
Unafflicted households	1428	1599
HH with past period WA death (4-8 years ago)	168	111
past period WA head/spouse death	24	19
past period WA other death	144	99
HH with recent WA death (0-4 years ago)	117	89
recent WA head/spouse death	18	35
recent WA other death	105	54
HH with current WA chronically ill adult	284	62
WA chronically ill adult, head/spouse	204	33
WA chronically ill adult, other	113	33

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

Appendix Table A3. Number of Cases of Children Age 7-11 in 2004 and 7-11 in 2008 by Orphan Status, Gender, and Household Wealth

	No. of cases	
	2004	2008
All children age 7-11 in 2004 (7-11 in 2008)		
Both parents alive	4366	3524
Paternal orphan	541	320
Maternal orphan	183	93
Double-parent orphan	278	113
Children in poor households (bottom 50% of farm assets/AE)		
Both parents alive	1610	1652
Paternal orphan	197	153
Maternal orphan	60	38
Double-parent orphan	78	51
Children in less poor households (top 50% of farm assets/AE)		
Both parents alive	1692	1595
Paternal orphan	210	148
Maternal orphan	80	47
Double-parent orphan	129	53
Girls		
Both parents alive	2181	1737
Paternal orphan	264	158
Maternal orphan	90	44
Double-parent orphan	141	61
Boys		
Both parents alive	2185	1787
Paternal orphan	277	162
Maternal orphan	93	49
Double-parent orphan	137	52

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

Appendix Table A4. Probit Regressions of Household Reinterview in 2004 and in 2008

	Dependent Variable = 1 if:	
Household and village-level covariates	HH is in the 2001 sample & was reinterviewed in 2004	HH is in the 2001 & 2004 samples & was reinterviewed in 2008
1=HH head is a single female	-0.128*	-0.07
	(2.51)	(1.10)
head's age	0.01	0.022*
	(1.30)	(2.19)
head's age squared	0	0
	(0.63)	(1.17)
head's education level	-0.025**	-0.026**
	(3.18)	(2.69)
spouse's education level	-0.007	0.006
	(0.93)	(0.68)
maximum education level among those 12 and older	0.016+	0.001
	(1.75)	(0.08)
# of male adults age 15-59	0.040*	0.155*
	(1.98)	(2.14)
# of female adults age 15-59	0.047*	0.396**
	(2.13)	(5.05)
# of children age 0 to 5	0.060**	0.039+
	(2.93)	(1.65)
# of children age 6 to 11	0.025*	0.041*
	(2.09)	(1.98)
# of adults age 60+	-0.041	-0.09
	(0.82)	(1.16)
1=HH suffered a head death in last 4 years	-0.19	-0.217*
	(1.00)	(2.11)
1=HH suffered a spouse death in last 4 years	-0.09	-0.068
	(0.87)	(0.45)
1=HH suffered a non-spouse working-age adult death in last 4 years	0.049	0.062
	(0.73)	(0.74)
1=HH has a chronically ill adult	0.083	0.062
	(0.75)	(0.48)
1=HH has 2 or more chronically ill adults	-0.189*	-0.004
	(1.96)	(0.04)
1=HH has land forcibly removed in last 10 years, 2001	-0.003	0.116
	(0.04)	(1.33)
1=head related to headman when first allocated land, 2001	0.255**	0.240**
	(6.25)	(4.66)
1=spouse related to headman when first allocated land, 2001	-0.063	-0.049
	(1.09)	(0.67)
total land area owned	0.015**	0.071**
	(3.01)	(3.78)
total land area owned, squared	-0.000*	-0.002*
	(2.07)	(2.50)

Appendix Table A4, continued

ln(total value of farm equipment and livestock)	0.012**	0.003
	(3.63)	(0.75)
1=HH owns a vehicle	0.077	0.156
	(0.58)	(0.72)
1=house has a good floor	-0.102+	-0.061
	(1.81)	(0.85)
1=house has a good roof	-0.135+	-0.043
	(1.72)	(0.42)
1=house has good walls	0.082	-0.048
	(1.40)	(0.61)
% of village HHs who say their area's education has improved in past 10 years, 2001	-0.483**	-0.329
	(3.18)	(1.61)
% of village HHs who say their area's education has gotten worse in past 10 years, 200'	-0.312	-0.125
	(1.62)	(0.47)
distance from village to rail, 2001	-0.082	0.027
	(0.90)	(0.25)
distance to nearest tarred/main road (km) from village, 2001	0.001	0.003**
	(0.90)	(3.37)
distance to feeder road from village (km), 2001	-0.014*	-0.009
	(2.41)	(1.23)
distance to nearest district town (km) from village, 2001	-0.001	-0.002
	(1.24)	(1.59)
district average HIV prevalence, lagged five years	0.012	0.018*
	(0.97)	(2.06)
Constant	0.298	-0.074
	(0.38)	(0.21)
Province dummies included	Yes	Yes
Joint test for enumerator team dummies	14671.3	170.7
	(0.000)***	(0.000)***
Joint test for household & village characteristics	219.5	170.8
	(0.000)***	(0.000)***
Observations	6914	5418

Robust z statistics in brackets; + significant at 10%; * significant at 5%; ** significant at 1%. Unadjusted coefficients are presented, with standard errors below

Source: Author's calculations using CSO Supplemental Surveys 2004 and 2008.

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