

Does Prime-Age Mortality Reduce Per-Capita Household Income? Evidence from Rural Zambia¹

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Abstract

Evidence on the impoverishing effects of HIV/AIDS is still scarce. This paper evaluates the impact of prime-age mortality on per-adult equivalent incomes of surviving household members. The analysis uses a nationally representative sample of rural households from Zambia where prime-age mortality is strongly associated with AIDS. We employ difference-in-difference matching techniques and control for spillover effects by excluding households from the control group if members departed or joined for reasons related to AIDS. The latter is required because non-afflicted households hosting new members, e.g. orphans, incur considerable reductions in per-adult equivalent incomes, pointing to the relevance of spillover effects. We find that the death of a prime-age member has no significant short-run effect on per-adult equivalent household income but markedly reduces income in the medium run. A likely explanation for this finding is that surviving household members pursue a mix of income and demographic coping strategies that stabilize income but cannot be sustained over a longer period of time.

Keywords: HIV/AIDS, prime-age mortality, per-adult equivalent income, spillover effects, difference-in-difference, propensity score matching, Zambia

JEL classification: I31, J19, C14, C23

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1 Introduction

Within two decades, AIDS has become the leading cause of death on the African continent and the sixth highest cause of mortality worldwide (WHO, 2008; UNAIDS, 2007). Despite the increasing availability of anti-retroviral treatment, sub-Saharan Africa recorded about 1.3 million AIDS-related deaths in 2009 alone. In the same year, another 1.8 million Africans got infected with HIV. With 23 million, or two thirds, of the global total of 33 million people infected with HIV, sub-Saharan Africa is the epicenter of the AIDS epidemic (UNAIDS, 2010). The consequences of AIDS-related deaths are not comparable to those from most other diseases because AIDS hits adults at the peak of their productivity and earning capacity. It disables and kills the people on whom families rely for their livelihood. Consequently, it has been widely conjectured that AIDS may constitute a severe impediment to Africa's socio-economic development. In turn, development agencies, international organizations and national governments are spending billions of dollars each year to respond to the epidemic. The arrival and scaling-up of antiretroviral treatment has dramatically increased health expenditures in highly affected countries. From 2005 to 2009 alone, global spending on HIV/AIDS programs doubled from US\$ 7.9 billion to US\$ 15.9 billion (UNAIDS, 2009; UNAIDS 2010) with half of the resources being provided by donor governments.

Yet, the impact of AIDS-related mortality on household welfare as measured by per-capita income or expenditure is not clear a priori. While AIDS-related mortality may lower total household income through the loss of productive family members, the size of the household is also reduced by the death of a member. *Ceteris paribus*, the direction of the effect depends on whether the deceased member was a net consumer or a net producer for the household. The death of a net producer would reduce per-capita household income of surviving members, whereas the death of a net consumer would actually raise per-capita income. In addition, AIDS-afflicted households may cushion adverse welfare effects through adjusting the income-generating activities of remaining members (income coping) or the composition and size of the household (demographic coping).

Income-coping strategies include changes in the supply and allocation of household labor. In rural areas, non-farm employment may be given up to fill labor shortages on the farm. Case studies for Rwanda (Donovan et al., 2003) and Kenya (Yamano and Jayne, 2004) show that off-farm income of vulnerable and asset-poor households is at particular risk if

an adult member dies. By contrast, Beegle (2005) finds no significant changes in labor supply of individuals in households having experienced a prime-age death for the Kagera region in Tanzania. Another income-coping strategy is the sale of assets. Yamano and Jayne (2004) as well as Chapoto and Jayne (2008) find that prime-age mortality has only a minor impact on livestock, but greatly reduces the value of small animals such as goats and sheep in rural Kenya and Zambia, respectively.

Demographic coping may involve attempts to attract additional net producers, e.g. by letting migrants return to support the household, or to reduce the number of net consumers, e.g. by sending children away to members of the extended family. Demographic coping has been shown to exist in the African context, with a strong variation across countries as well as the position and sex of the deceased (Chapoto and Jayne, 2008; Yamano and Jayne, 2004; Mather et al., 2004). At one extreme, afflicted households in Kagera, Tanzania were able to maintain their household size (Ainsworth et al., 1995). In Uganda, by contrast, household size declined by about one person after a prime-age death, indicating that no new members could be attracted (Menon et al., 1998). Based on Demographic and Health Surveys from 21 countries across Africa, Beegle et al. (2010b) also show that many single orphans do not stay with the remaining parent, but are rather sent away to live with other relatives.

Despite the theoretical ambiguity and the policy relevance of the topic, surprisingly little empirical knowledge exists about the effects of AIDS deaths on *monetary per-capita* welfare of African households (Beegle and de Weerd, 2008).³ Beegle et al. (2008) for the Kagera region in Tanzania as well as Mather and Donovan (2008) for Mozambique are notable exceptions. It is important to look at the *per-capita* implications of prime-age mortality, as the impact on *total* household income is theoretically not ambiguous. Besides, in a poor context where food consumption shares are high and the asset base low, consumption is mostly of private rather than public good nature, rendering a per-capita measure a more meaningful welfare indicator. Examining the causal relationship between HIV/AIDS and poverty is essential for a reasonable cost-benefit analysis of current efforts to tackle the epidemic and prioritization of public health interventions.

This paper aims to make a contribution to filling the existing gap by evaluating the changes in per-adult equivalent household incomes associated with AIDS-related

³ This is not to deny that various studies have examined the impact of HIV/AIDS on non-monetary dimensions of welfare, in particular educational outcomes of orphans (e.g. Beegle et al., 2010a; Case and Ardington, 2006).

mortality in rural Zambia. We distinguish between the deaths of household heads and spouses, who are likely to be net producers, as well as other adult household members, who rather tend to be net consumers. The analysis draws on a large nationally representative longitudinal survey that tracks about 5420 Zambian households over the period 2001-2004. To deal with the potentially endogenous selection of households into experiencing prime-age mortality, we employ a difference-in-difference propensity score matching (DID-PSM) estimator. While PSM removes the selection bias due to observed differences between afflicted and non-afflicted households, the DID estimator differences out the time-invariant unobserved heterogeneity between the two groups.

The second major contribution of the paper is the consideration of spillover effects. To the extent that demographic coping plays a significant role, spillover effects may cause a bias in the estimated impact of prime-age mortality. For instance, households without a deceased member may experience welfare losses when taking care of children sent away by households that experienced prime-age mortality. These effects have been neglected in the previous literature. We assess their relevance by testing whether non-afflicted households with demographic changes related to prime-age mortality fare worse in per-capita income terms than their counterparts without such changes. In estimating the welfare effects of prime-age mortality, we exclude households from the control group if members departed or joined for reasons related to prime-age mortality.

Our paper is related to two previous empirical studies by Beegle et al. (2008) and Chapoto and Jayne (2008). Beegle et al. (2008) share our objective of evaluating the impact of prime-age mortality on the per-capita welfare of surviving household members. Yet they do so for one specific region characterized by high HIV prevalence, Kagera district in Tanzania, which makes it difficult to extrapolate their results to the national level. We use the same dataset as Chapoto and Jayne (2008), but their analytical focus is different from our study. Most notably, they look at how prime-age mortality affects various (on-farm and off-farm) income sources rather than per-capita household income. Furthermore, Beegle et al. (2008) as well as Chapoto and Jayne (2008) use parametric approaches as compared to the semi-parametric PSM applied here.

2 Data and Setting

We use a nationally representative longitudinal dataset of 5420 rural farm households from Zambia who were surveyed in 2001 and 2004. As a very poor country with high

HIV prevalence rates, Zambia makes an interesting case to study the effects of AIDS. Three in four rural Zambians live below the national poverty line (World Bank, 2007) and about 14 percent of adults aged between 15 and 49 are HIV-positive (UNAIDS, 2010). As a result, life expectancy at birth has fallen to less than 47 years, making it one of the lowest in the world (UNDP, 2010). Recently, availability of anti-retroviral treatment has greatly improved and reaches 44 percent of the population in need. Still, Zambia reported about 45,000 AIDS-related deaths in 2009 (UNAIDS, 2010). These numbers naturally raise the question to what extent AIDS has contributed to the low levels of socio-economic outcomes in Zambia.

The household survey was conducted by the Central Statistical Office in conjunction with the Ministry of Agriculture and Cooperatives and Michigan State University. In addition to standard socio-economic information of the household and its members, the survey paid particular attention to capturing a household's income generation process and its demographic development including the death of members. It not only recorded all deaths of household members after 2001, but also asked whether households had experienced the loss of a member between 1996 and 2001. To the best of our knowledge, the survey design makes it one of the most representative and comprehensive longitudinal household surveys available from Africa to examine the socio-economic effects of prime-age mortality. In particular, the combination of the large sample size and relatively high HIV prevalence rates in Zambia provides a sufficiently large subsample of households that experienced adult mortality. It allows examining the impact of a low-frequency event such as prime-age death with meaningful statistical power.

Although the surveys collected detailed information on mortality, epidemiological information on the cause of death is not available. For this reason, we follow the literature and use disease-related prime-age (15-59 years) mortality as a proxy for AIDS mortality. Comparing provincial HIV prevalence rates from antenatal clinics with adult mortality rates from the survey also used in this study, Chapoto and Jayne (2008) illustrate that the large majority of prime-age deaths are indeed likely to be AIDS-related (Pearson correlation coefficient of 0.84). Oster (2010) also suggests the use of adult mortality data to estimate HIV prevalence rates in Africa.

Around ten percent of the households re-interviewed in 2004 experienced at least one prime-age adult death between the two survey years. We refine this sample in various ways to arrive at treatment and control groups that are best suitable to identify the effects of adult mortality. First, households are only included if no member had died before the

baseline survey was conducted, and if no chronically ill members were present in the household in 2001, because AIDS-related death is typically preceded by prolonged illness.⁴ This correction is made to ensure that 2001 incomes were not contaminated in the sense of already being lowered by AIDS-related adjustments. It should be noted that only 16 percent of the households that reported a prime-age death before the baseline survey experienced another prime-age death between 2001 and 2004. The relatively low proportion of multiple deaths does not suggest that our sample systematically excludes households that were hit particularly hard. Nevertheless, in an extension of the baseline model, we also evaluate the income effects of deaths that occurred before 2001.

Second, we only consider the death of individuals who were recorded as household members in the baseline survey, eliminating from the sample the group of more than 150 households that experienced the death of a member who returned to the household to seek terminal care after the baseline survey. The main reason for doing so is that in households suffering the death of a long-term resident the deceased member contributed to initial income in 2001, whereas this was not necessarily the case for households incurring the death of a member who had not been a resident at the time of the baseline survey. As a consequence, the impact of prime-age mortality on welfare may be more severe for the former than for the latter (Chapoto and Jayne, 2008). However, given the difficulty to draw a clear line between resident and non-resident members in the Southern African context, we also test whether the inclusion of these households affects our results.

We end up with a treatment group of 221 households, which are fairly equally distributed across income quartiles (Table 1). Among these households, 210 reported one dead member, nine reported two dead members, and two reported three dead members. Another 465 households incurred the death of a prime-age adult between 1996 and 2001. Again, afflicted households are fairly evenly spread over the income distribution.

When it comes to evaluating the impact of prime-age mortality on household welfare, either consumption or income can be employed as an indicator. While consumption is usually measured with a higher degree of precision, income changes may give a better indication of the household's future prospects and the sustainability of a household's adopted strategies to cope with the loss of an adult member. For instance, households

4 Overall, 548 households reported a prime-age death in the period 1996-2001. The total number of households with a chronically ill member was 580 in 2001.

may be able to temporarily smooth consumption by selling assets, but lower their income-generating capacity through such measures. In our case, consumption data are not available, but the survey offers very detailed information on the income sources of a household. Total yearly income is computed as the sum of the value of agricultural production (including production for home consumption), livestock income, and off-farm income including remittances. Per-adult equivalent incomes are computed using total household income and the adult equivalent conversion factors provided by the Ministry of Finance and National Planning. In principle, welfare could also be measured in per-capita terms. However, we prefer the use of per-adult equivalent units in order to capture age and sex-specific differences in need, but also conduct a robustness check with per-capita income. On average, yearly per-adult equivalent incomes in AIDS-afflicted households slightly fell over the period 2001-2004 (Table 1).

TABLE 1: NUMBER OF HOUSEHOLDS REPORTING PRIME-AGE MORTALITY

	Total	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Income change	
						2001-2004 (in ZMK)	
						Mean	SD
<i>Period 2001-2004</i>							
any prime-age death	221	57	60	44	60	-26379	1059819
death of head/spouse	124	28	35	28	33	-7852	1195724
death of other members	105	30	26	18	31	-58859	833998
death of male members	101	18	32	22	29	27930	866045
death of female members	130	40	31	24	35	-75022	1157807
<i>Period 1996-2001</i>							
any prime-age death	465	138	101	109	117	-36684	770837
death of head/spouse	96	27	23	20	26	-140071	775322
death of other members	375	112	82	89	92	-9133	764037
death of male members	218	66	51	46	55	-90790	828569
death of female members	277	77	61	69	70	1887	692275

All monetary values are computed in 2004 Zambian Kwacha (ZMK). Mean 2001 per-adult equivalent incomes of the four income quartiles are ZMK 91,669; ZMK 217,212; ZMK 422,337; and ZMK 1,347,059.

As argued above, the income effect of a death also depends on whether the deceased was a net consumer or a net producer for the household. However, the dataset does not allow identifying individual income and consumption levels. Instead, we try to account for the previous position of the deceased in the household by distinguishing between the deaths of a household head or his/her spouse and other adult members as well as between the deaths of male and female adults.⁵

⁵ To keep the number of observations for each kind of prime-age death large enough, we abstain from further disaggregation. Note that the ratio of head's/spouse's and other members' death is different for the

3 Empirical Strategy

In estimating the causal impact of prime-age mortality on household welfare with panel data three problems have to be taken into account. First, with sexual transmissions accounting for the vast bulk of HIV infections in Africa (Schmid et al., 2004; Walker et al., 2003), AIDS is a behavioral disease. This may give rise to endogenous selection into treatment (prime-age mortality being the treatment).⁶ Second, to the extent that the chances of re-interviewing households in 2004 are systematically affected by prime-age mortality, our estimates might suffer from attrition bias. Third, if demographic coping leads to negative spillover effects on the control group of households without a deceased member, the estimated effect of AIDS-related deaths will be downward biased.

3.1 Estimation Method

As a solution to the problem of endogenous selection, we employ the DID-PSM estimator. While PSM removes the selection bias due to observed differences between afflicted and non-afflicted households, the DID estimator controls for time-invariant unobserved characteristics such as risk attitudes and sexual behavior.

The general idea of matching is to find a comparison group that shares the same characteristics as the treatment group, but did not receive treatment. However, there is a trade-off between the potentially wide range of characteristics one might want to match on and the chances of finding matches which would be identical across all characteristics. In addition, it is not clear how each characteristic should be weighted. Rosenbaum and Rubin (1983) suggest the use of PSM to solve this dimensionality problem. Performing PSM involves matching treated to non-treated households based on similarity of their predicted probability of receiving the treatment (their ‘propensity scores’):

$$P(X_i) = Pr(D_i = 1 | X_i) \tag{1}$$

where $D_i = 1$ refers to households i receiving a treatment, X_i is a vector of pre-exposure characteristics, and $0 < P(X_i) < 1$. PSM matches pairs on the basis of how close the

two periods because for the period 2001-2004 the reference was made to the head at the time of the death, while for the period 1996-2001 the reference was made to the head at the time of the interview in 2001. No deaths of heads should therefore be reported for the period 1996-2001, but some households nevertheless did so, probably confounding current with former (deceased) heads.

⁶ Some authors (e.g. Gisselquist et al., 2003) argue that other factors, in particular medical transmissions, have also played an important role in spreading HIV in sub-Saharan Africa. Nevertheless, non-sexual modes of transmission, e.g. infection through contaminated injection needles or blood transfusions, are potentially also subject to endogenous selection into treatment.

propensity scores $P(X_i)$ are across the two samples of treated and non-treated observations. Through the use of the propensity scores, PSM balances the observable covariates X between the treatment and the control group and thus selects similar non-treated observations for each of the treated observations.

The assumption needed for the treatment effect to be identified is that selection into treatment is random conditional on the observable covariates X (conditional independence assumption). In other words, all factors which simultaneously determine treatment and outcomes of interest should be observed. If data quality is not sufficient to capture important components of X , the presence of these unobserved characteristics implies that PSM will be biased.

To more convincingly argue that the assumption of conditional independence is satisfied, we combine PSM with DID. Hence, we compare changes in the levels of the outcome variable rather than the levels themselves which allows us to purge all time-invariant unobservables from the analysis. Yet, unobservable effects may still bias our results if they are time-varying and apply differently to treated and non-treated observations. For instance, a bias might occur if the death of a household member from AIDS prompts other members to discover their HIV status and change their behavior, e.g. by adjusting income-generating activities. While this kind of bias cannot be ruled out completely, we are confident that our dataset is rich enough to control for important time-varying factors.

The resulting DID-PSM estimator for the average treatment effect on the treated households (ATT) can be expressed as

$$ATT = \sum_i \lambda_i [(y_{i1} - y_{i0})/y_{i0} - \sum_{j \in c(i)} \mu_{ij} (y_{j1} - y_{j0})/y_{j0}] \quad (2)$$

where the λ_i s are sample weights used to construct the mean impact PSM estimator for the afflicted households i , $(y_{i1} - y_{i0})/y_{i0}$ is the growth rate in these households' per-adult equivalent income before and after the death of a member, and $(y_{j1} - y_{j0})/y_{j0}$ is the growth rate in per-adult equivalent income for households j of the control group over the same period. The μ_{ij} s are the weights applied in calculating the average outcome of the matched non-afflicted households. In our case, these weights are based on Kernel matching, which is a non-parametric approach that uses weighted averages of all households in the control group to construct the counterfactual outcome.⁷ The average treatment effect is only

⁷ Note, however, that our results are robust to using caliper matching.

defined in the region of common support $c(p_i)$, which implies that all observations whose propensity score is lower than the minimum and larger than the maximum of the opposite groups are excluded.

Matching gives us counterfactual growth rates of per-adult equivalent income, which we apply to observed 2001 per-adult equivalent incomes to compute counterfactual 2004 incomes. We then estimate the ATT by comparing actual with counterfactual 2004 incomes. Even in a very poor context like rural Zambia it is likely that growth rates depend on initial income levels. The poorest households might, for example, be caught in poverty traps with severely limited growth prospects. To avoid matching households with very different income levels, we enforce exact matches within income quartiles.

The propensity score for each observation in the treatment and control group is estimated by using the predicted values from a logit model. Only variables which have an effect on both treatment and outcome and are not affected by the treatment should be included. Building on the existing literature on the determinants of prime-age mortality and HIV infection, we identify a set of variables simultaneously affecting a household's probability to experience the death of a prime-age member and its income-generation capacity. These variables are constructed using pre-treatment data from the baseline survey. At the household level, we include variables capturing the demographic composition of the household, also accounting for the sex and educational levels of prime-age adults and the household's head (compare Hargreaves and Glynn, 2002; Chapoto and Jayne, 2006). We also control for the importance of different income sources, as involvement in local non-farm activities or migration not only has an effect on income levels, but also increases the risk of infection with HIV (Ainsworth and Semali, 1998; Lurie et al., 2003). As a proxy for social capital we add a dummy indicating whether a household has a relation to the village head (compare Pronyk et al., 2008). The effect of household wealth is captured through land endowments and a broader asset index⁸ (compare Gillespie et al., 2007). We also include a dummy for radio ownership to proxy access to information. At the community level, we control for the distance of the village to the district town and nearest tarred main road (compare Buvé et al., 2002; Tanser et al.,

⁸ The asset index is based on a principal component analysis of the following variables: Dummy for iron roof, dummy for brick walls, dummy for modern door, dummy for cement floor, and the value of agricultural productive assets.

2000). Besides, we add population density and rainfall levels and variability.⁹ The latter variable is thought to account for the general exposure of communities to weather risks and related agricultural income shocks. Finally, by introducing provincial dummies, we also control for the observed regional variety in HIV prevalence. Table A1 presents a complete list of the explanatory variables included in the logit model to estimate the propensity score.

A formal requirement for a reliable estimation with PSM is that, under the conditional independence assumption, the relevant covariates are balanced between the treatment group and the comparison group. Lack of balance points to a possible misspecification of the propensity score model. Hence, it is important to verify that the balancing condition is satisfied by the data. We perform three different balancing tests.

The first balancing test (Sianesi, 2004) re-estimates the propensity score on the matched sample, i.e. only on afflicted households and matched non-afflicted households, and compares the pseudo-R² before and after matching. The pseudo-R² indicates how well the regressors explain the probability of being affected by prime-age mortality. After matching there should be no systematic differences in the distribution of covariates between both groups and hence the pseudo-R² should be fairly low. As shown in the upper part of Table A3, the pseudo-R² indeed approaches zero after matching.

The second balancing test (Rosenbaum and Rubin, 1985) calculates the standardized bias before and after matching. For each covariate it is defined as the difference in the means of the treated and (matched) control group as a percentage of the square root of the mean of variances in both groups. In the matched sample the standardized bias should be reduced considerably and be close to zero percent. In our case, PSM reduces the mean bias by about 70 percent, bringing the remaining mean bias down to about five percent (lower part of Table A3).

The third balancing test (Dehejia and Wahba, 2002) first divides the observations into strata based on the estimated propensity scores. These strata are chosen so that no statistically significant difference in the mean of the estimated propensity scores remains between the treatment and comparison group observations within each stratum. Then, t-tests are used to test within each stratum for mean differences in the regressors between

⁹ Community variables are taken from supplementary datasets: Distance and rainfall data for the period 1990-2004 were kindly provided by Michigan State University's Food Security Group. Population density is derived from the 2000 Census.

the observations in the treatment and comparison group. In our propensity score model, mean differences turn out to be insignificant across the board, suggesting that the balancing conditions are satisfied.¹⁰

3.2 Attrition

To the extent that it reflects household dissolution caused by the death of an adult member, attrition may lead to underreporting of prime-age mortality and bias estimations of the socio-economic consequences: households that dissolve are likely to be hit harder by adult mortality than those that remain intact. At first sight, the re-interview rate of 78 percent (6922 households were interviewed in 2001) appears to be fairly low. It should be noted, however, that the re-interview rate rises to 89 percent if one excludes attrition caused by enumerators simply not revisiting several enumeration areas in 2004 that were included in the 2001 survey. And the re-interview rate increases further to 95 percent if one disregards attrition that can be attributed to the absence of adult household members at the enumeration period and interview refusals (Chapoto and Jayne, 2008). Attrition is therefore largely related to factors other than household dissolution.

Nevertheless, we use the method of inverse probability weights (IPW) to deal with potential attrition bias (Wooldridge, 2002). This method involves estimating a model where the probability of being re-interviewed in 2004 is predicted with a set of household characteristics measured in 2001 and dummies describing enumerator quality. The probabilities are based on a Probit model which includes the following household-level variables: (i) 2001 household composition (number of individuals 0-14, 15-59, 60-99 years, number of migrants, sex and education of head, relationship to village head), (ii) income sources (agricultural income, income from self-employment, remittances, wages), and (iii) wealth (land size, radio ownership, asset index). We then compute the inverse probability of being re-interviewed for each household and use it as a weight in all estimations in this paper. Throughout, the use of IPW to control for possible attrition bias has little effect on the estimated impact of prime age mortality, which suggests that attrition bias does not constitute a major problem in our data (compare Chapoto and Jayne, 2008).

3.3 Spillover Effects

Demographic coping, the relocation of members between afflicted and non-afflicted households following a prime-age death, may generate spillover effects on the control

¹⁰ Also compare Table A4, which shows simple t-tests for the matched sample within different income quartiles.

group of households without a deceased member. These households may see their per-capita income decrease when they host net consumers from afflicted households or sent net producers to support afflicted households. In such cases, the estimated effect of AIDS-related deaths will be downward biased. A unique characteristic of the Zambian dataset is that it allows us to link demographic changes in the composition of households to AIDS-related mortality. The survey included specific questions on why a member (12 years or older) joined or left a household between the two waves, and on whether a household hosted orphans (11 years or younger). We define the following three groups of individuals who may give rise to an AIDS-related demographic burden for households without a deceased member, thus reducing their per-adult equivalent income:

1. *New members related to prime-age mortality* (12 years and older). They only include individuals who (i) had not been in the household in the first wave, but were so in the second wave and (ii) joined the household for one of the following reasons: having lost parents/being fostered, having been widowed, having returned to the household because of sickness¹¹, having needed help without having being sick, and having married a household member¹². Not included as AIDS-related cases are individuals who joined the household to help with activities, to work for the household, because of old age, and because of divorce or separation.
2. *Departed members related to prime-age mortality* (12 years and older). They only include individuals who (i) had been a household member in the first wave, but were no longer so in the second wave and (ii) departed from the household to live with other relatives. Individuals who departed to find a job, to establish a new home, to enter marriage, and because of divorce or separation are not counted as AIDS-related cases.
3. *Orphans* (11 years and younger), who had lost at least one parent and did not live with the remaining parent. Information for orphans is only available for the 2004 wave, i.e., we do not know whether they had already been members of the household in the first (2001) wave.

11 Death due to AIDS is normally preceded by prolonged and severe illness. Given the transmission nature of HIV, it is likely that the partner of a deceased is also infected. Using the same dataset, Chapoto and Jayne (2006) provide evidence that this is the case in Zambia.

12 Beegle and Krutikova (2008) show for Tanzania that adult death affects the timing of marriage in affected households. In particular girls who lose their father marry at significantly younger ages. In addition, Ueyama and Yamauchi (2009) demonstrate for the case of Malawi that women's marriage age is lowered in general if the local marriage market is characterized by excess adult mortality.

Employing this demographic information, we check in various ways whether spillover effects can be observed in the data. First, based on Dzekedzeke and Fylkesnes (2006), who use DHS and antenatal care clinic data to estimate the HIV prevalence in 2001, we compare households without a deceased member in low-prevalence and high-prevalence provinces.¹³ Using the same estimation method as described above, we test whether the number of members who joined or left a household due to AIDS-related reasons is higher in regions with high HIV prevalence. Obtaining a significant difference across regions would not only point to spillovers but also suggest that the above definitions indeed provide a reliable proxy for AIDS-related demographic changes. Second, we examine whether households that are indirectly affected by AIDS through demographic changes fare worse in terms of income per-capita than their unaffected counterparts. Finally, we control for spillover effects in the estimation of the impact of prime-age mortality on per-adult equivalent incomes by excluding households from the control group if members departed or joined for AIDS-related reasons. We also check robustness of our results using a more conservative definition of demographic adjustments that is less likely to overstate the extent to which adjustments are indeed AIDS-related.

Of course, per-adult equivalent incomes of households in the control group are not only subject to demographic spillovers. Income levels are also affected through economic spillovers. High adult mortality rates may alter communities' economic structure, e.g. through general equilibrium effects on local labor markets or the availability of land for cultivation. AIDS may also lower the resilience of communities' social networks to other shocks (de Waal and Whiteside, 2003). However, the indirect nature of these effects renders them not only difficult to identify but also likely to be small. Besides, they should affect afflicted and non-afflicted households in a similar way. Using the same dataset, Jayne et al. (2006) find only minor effects of prime-age mortality on Zambian communities' crop output, mean income, and income per capita. We are thus confident that economic spillovers do not seriously impinge on the validity of our control group.

13 Provinces with relatively low HIV prevalence are Northern and Northwestern (8.3 and 9.0 percent, respectively), while the three provinces with the highest prevalence are Copperbelt, Lusaka, and Southern (19.9, 22.0 and 17.6 percent, respectively).

4 Results

4.1 Base Estimates

Before turning to the role of spillover effects, we first present PSM-DID estimates based on the unrefined control group (Table 2). This facilitates comparison with previous studies where spillovers were not taken into account. Note that the number of observations in Table 2 is slightly lower than in Table 1 because a few cases are off the region of common support. Standard errors are computed using bootstrapping with 400 replications.¹⁴ At the aggregate level, we find a very small negative and clearly insignificant effect of prime-age mortality on household per-adult equivalent income. Disaggregating impacts by the gender and position of the deceased person leaves the estimated treatment effects insignificant throughout, rendering it impossible to identify any pattern that would point to differences between net producers and net consumers.

TABLE 2: SHORT-RUN EFFECTS OF PRIME-AGE MORTALITY (2001-2004) ON PER-ADULT EQUIVALENT HOUSEHOLD INCOME

Treatment	Observations	ATT (in ZMK)	Standard error	p-value
Any prime-age death	217	-4520	83369	0.96
Death of head/spouse	119	15816	120089	0.90
Death of other members	96	-92295	124476	0.46
Death of male members	98	-16848	110574	0.88
Death of female members	124	-27168	112457	0.81

Chapoto and Jayne (2008) provide detailed evidence on the coping mechanisms that may lie behind this ‘non-finding’. As concerns income coping, they show that the death of a male prime-age household head is associated with a modest fall in the value of cattle, while the death of other adult household members has no significant effect on cattle assets. After the death of male and female adults, the value of small animals declines by 36 percent and 18 percent, respectively. Selling small animals thus appears to be a quantitatively important coping strategy used by AIDS-afflicted households. In addition, demographic coping plays a role. While households experiencing the death of the head are partially able to replenish their household size by attracting additional members, the death of other members is associated with a more than one-person decline in household

¹⁴ The reason for using bootstrapping is that conventional standard errors tend to be biased because the estimated variance of the treatment effect should also include the variance due to the estimated propensity score and the imputation of the common support (Caliendo and Kopeinig, 2008). The validity of bootstrapped standard errors in matching estimators has been questioned by Abadie and Imbens (2008), but their critique does not apply to the kernel-based matching employed here.

size due to a significant reduction in the numbers of boys and girls. (Chapoto and Jayne, 2008) According to Chapoto and Jayne (2008, p. 344), the latter suggests that “in many cases, the deceased adult’s family lived with him or her while the sick person received terminal care at a relative’s home (possibly the older parents) and then left after the person passed away.” When re-estimating the relationship for the present sample that excludes sick persons seeking terminal care, we find a less than one-person decline in household size both for aggregate deaths (Table 3) and for all subgroups including other members. In combination, the attraction of new household members and the selling of productive assets may have prevented incomes from falling over the period 2001-2004.

Our core result is corroborated by Mather and Donovan (2008) for Mozambique, a neighboring country that provides a comparable setting of predominantly poor rural households. Mather and Donovan (2008) also find insignificant effects of prime-age mortality on rural households’ per-adult equivalent income irrespective of whether household heads or other members died, and identify a loss of assets and changes in household composition as major factors contributing to this outcome.

Two further studies investigating the welfare impact of prime-age mortality come to different conclusions. For the case of Indonesia, Grimm (2010) shows that coping effects more than offset the direct losses caused by the death of an adult household member, leading to overall gains in consumption per adult equivalent. Notably, surviving members of Indonesian households experiencing prime-age adult deaths are found to rely on increases in labor supply as one coping strategy, a scenario that is unlikely to happen in the rural African context as a result of limited employment opportunities. By contrast, Beegle et al. (2008) estimate for the Kagera region in Tanzania that within the first five years after the death of an adult per-adult equivalent consumption of the surviving household members grew by about seven percent less than consumption levels of non-afflicted households. Nonetheless, average consumption growth continued to be positive for households affected by prime-age mortality for the period under consideration (2000-2004). Positive economic conditions may thus have enabled afflicted households to uphold or even increase their consumption levels over time without resorting to desperation-led selling of assets.

4.2 Controlling for Spillover Effects

Demographic coping of households that experienced prime-age mortality may translate into significant spillover effects and thereby affect the welfare estimates reported above.

Overall, we obtain strong evidence for the existence of considerable spillover effects. We find that on average individuals (12 years and older) more frequently join and leave households without a deceased member for reasons related to prime-age mortality when the households reside in regions with high HIV prevalence (Table 3). The arrival of a new member related to prime-age mortality is on average associated with a fall in per-adult equivalent income of the receiving household by more than US\$ 20. Having to care for orphans is estimated to exhibit an even stronger impact on yearly average household income per adult equivalent, reducing it by about US\$ 25. Given the low income levels characterizing rural Zambia, this is a quantitatively important income drop of about 25 percent.

TABLE 3: SPILLOVER EFFECTS OF RECENT PRIME-AGE MORTALITY CHANGE HEADINGS

<i>a) Change in adult equivalent size of households with a deceased member</i>				
Treatment	Observations	ATT	Standard error	p-value
Any prime-age death	220	-0.42**	0.18	0.02
<i>b) Individuals (12 years and older) joining households without a deceased parent for AIDS-related reasons in regions with high HIV prevalence</i>				
Treatment	Observations	ATT	Standard error	p-value
High-prevalence region	498	0.08**	0.04	0.05
<i>c) Individuals (12 years and older) leaving households without a deceased parent for AIDS-related reasons in regions with high HIV prevalence</i>				
Treatment	Observations	ATT	Standard error	p-value
High-prevalence region	499	0.08*	0.04	0.07
<i>d) Changes in per-adult equivalent incomes of households without a deceased member but with AIDS-related demographic changes</i>				
Treatment	Observations	ATT (in ZMK)	Standard error	p-value
New members (12 and older)	436	-116650**	50327	0.02
Hosting orphans (11 and younger)	248	-151558***	47524	0.00
Departed members (12 and older)	398	-38320	65803	0.56

***/**/* denote significance at the 1/5/10 percent level respectively.

In a subsequent step, we therefore re-run the DID-PSM regressions using only households without AIDS-related demographic changes as a control group. The results are shown in Table 4. For aggregate deaths, the estimated impact of prime-age mortality on household incomes rises from -4000 ZMK to -78000 ZMK when controlling for spillovers. It continues to be insignificant, but the p-value drops markedly. Disaggregating households by the position or sex of the member who died also produces strongly negative but insignificant coefficients, with the death of male members coming closest to having a statistically significant impact. We also check for impact heterogeneity across the

initial poverty status of a household. Examining the income effects for each income quartile separately, however, yields no significant effects (results available upon request).

TABLE 4: SHORT-RUN INCOME EFFECTS OF PRIME-AGE MORTALITY (2001-2004) ON PER-ADULT EQUIVALENT HOUSEHOLD INCOME CONTROLLING FOR SPILLOVERS¹⁵

Treatment	Observations	ATT (in ZMK)	Standard error	p-value
Any prime-age death	207	-77931	82334	0.34
Death of head/spouse	118	-71837	124654	0.56
Death of other members	86	-104236	129440	0.42
Death of male members	93	-117203	109710	0.29
Death of female members	120	-87217	127889	0.50

4.3 Extensions

Table 5 reports several modifications and extensions of our basic specification. For the sake of comparison, our main finding is replicated in column 1. In column 2, we further disaggregate mortality impacts and consider the death of male household heads separately. This is because previous work has shown that the effects of prime-age mortality on variables such as the size of the cultivated land area and net crop income are particularly severe if the deceased was the male household head (e.g. Mather and Donovan, 2008; Yamano and Jayne 2004; Chapoto and Jayne, 2008). We do not find a significant impact of the death of a male household head on per-adult equivalent incomes, however, which is in accordance with Chapoto and Jayne’s (2008) finding that losing a male member substantially reduces household size in rural Zambia.

In the next two specifications, we modify the definition of our welfare indicator. First, income per adult equivalent is replaced by income per capita. This could change estimated impacts, the direction being inconclusive a priori. If an adult member of a family with children dies, *ceteris paribus* adult equivalent household size declines relatively more than household size. In such a case it possible that per-capita income falls while per-adult equivalent income rises. If the household response involves sending away children, the net outcome might be the opposite. As shown in column 3, the impact of prime-age mortality remains insignificant when using income per capita, but the (negative) coefficient becomes much lower, pointing to the importance of demographic coping. Second, we employ total household income as an indicator that should unambiguously decrease after the death of an adult member. Interestingly, it only does so in the case of

¹⁵ The respective number of observations in Table 4 is slightly different from the one in Table 2 because matching is confined to the region of common support.

male deaths (column 4). The insignificant results for other positions of the deceased in the household might reflect the specific context of a very poor rural economy with high subsistence shares, where in a Lewis-type fashion surplus labor on farms implies that only minor income losses occur when the household loses a member. In a similar vein, the distinction between net producers and net consumers may get blurred in such a setting because all household members tend to be involved in the same set of low-return activities.

TABLE 5: EXTENSIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Base estimates	Death of male head	Per-capita income	Total household income	Including death of return members	Conservative spillovers	Medium-run effects
Treatment	ATT/ standard error	ATT/ standard error	ATT/ standard error	ATT/ standard error	ATT/ standard error	ATT/ standard error	ATT/ standard error
Any prime-age death	-77931 (82334)	-	-23701 (83165)	-734652 (496783)	-44973 (59747)	-76314 (74421)	-99018* (51748)
Death of head/spouse	-71837 (124654)	-	21319 (127231)	-925455 (669374)	-9005 (116746)	-72494 (116057)	-129933 (202963)
Death of male head	-	-27348 (126685)	-	-	-	-	-
Death of other members	-104236 (129440)	-	-94312 (120657)	-1068161 (860152)	-75796 (69356)	-84666 (125923)	-50971 (47883)
Death of male members	-117203 (109710)	-	-99502 (99867)	-1137649* (631767)	-91379 (104509)	-108381 (106535)	-96619 (70534)
Death of female members	-87217 (127889)	-	3607 (127793)	-733570 (746195)	-38058 (178249)	-86671 (138356)	-7687 (54779)
Death of former members	-	-	-	-	-49366 (78025)	-	-
Death of returned members	-	-	-	-	-17596 (96656)	-	-

* denotes significance at the 10 percent level.

Our sample has so far excluded more than 150 households that experienced the death of a member who had come to the household to seek terminal care. Since these returned members were not in the household in 2001, we cannot account for any direct income effects of their death, but we can capture income losses incurred by the household if it no longer receives remittances. Including the death of returned members does not alter the main finding of a negative but insignificant impact of prime-age mortality on per-adult equivalent income (column 5).

Our estimates of spillover effects are based on a rather broad definition of members joining or leaving households for reasons related to prime-age mortality and may thus provide a biased account of their true importance. Yet, employing a more conservative definition, no longer including members who joined the household because of sickness, needing help without being sick, and marrying a household member, does leave results largely unaffected (column 6).

Finally, since we do not find evidence for significant short-run income effects, even after controlling for spillovers, we can also examine the medium-run income effects for the period 2001-2004 resulting from prime-age deaths that occurred before 2001. As noted above, the household survey also recorded the death of a member between 1996 and 2001. Our identifying assumption, which is supported by the preceding analysis, is that 2001 incomes are not contaminated by short-run welfare losses induced by prime-age mortality. As shown in column 7, the medium-run effect of prime-age mortality on per-adult equivalent incomes turns out to be significantly negative and of considerable quantitative importance, with losses amounting to about US\$ 20.

5 Concluding Remarks

Finding no immediate negative impact of prime-age mortality on the per-capita incomes of surviving household members may appear counterintuitive, in particular if it was the household head or the spouse who died. Yet, through income and demographic coping, which has been shown to exist on a significant scale, households affected by prime-age mortality may well be able to preserve their per-capita income levels over the short term as our analysis and a similar study for rural Mozambique suggest. Nevertheless, our medium-term results point to possible limits of sustaining this strategy. In a poor setting such as rural Zambia, members of the extended family may, for example, only temporarily be able to care for children, and the sale of assets may only provide short-run relief.

In addition to the medium-run income losses identified in this paper, the potentially negative impact of prime-age mortality on human capital formation and its transmission across generations has to be taken into account (Beegle et al., 2010a; Bell et al., 2006). Besides, our analysis focuses on AIDS-related mortality, not morbidity. Yamano and Jayne (2005) and Linnemayr (2010) present evidence that AIDS may already lower household welfare well before the infected household member dies.

As concerns policymaking, the general purpose of evaluating the welfare effects of prime-age mortality is to define priority areas for government interventions in the context of tight budgets. From the specific results obtained in this paper we conclude that affected households need assistance especially in the medium to longer run. Yet, the finding that surviving household members are able to avoid a drop in per-capita incomes shortly after a prime-age death does not render immediate public support unnecessary if coping involves actions with severe long-run consequences, such as depleting the only assets a household owns or taking children out of school. In addition, our analysis of spillover effects suggests that the targeting of AIDS-related interventions should extend to households not experiencing a death but hosting orphans and other new members as they are shown to incur considerable reductions in per-adult equivalent incomes.

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Appendix

TABLE A1: SUMMARY STATISTICS

	Treated (n=221)		Control (n=3,289)		t-statistics
	Mean	SD	Mean	SD	
income change, 2001-2004 (in ZMK)	-26,379	71,291	-23,500	12,004	0.06
no 0-14 years	3.11	2.36	2.96	2.02	1.07
no 15-59 male skilled	0.50	0.85	0.39	0.68	2.38
no 15-59 male unskilled	1.18	1.31	0.94	0.93	3.61
no 15-59 female skilled	0.38	0.79	0.21	0.58	4.06
no 15-59 female unskilled	1.51	1.23	1.17	0.84	5.60
no 60 years and older	0.40	0.68	0.24	0.52	4.41
no migrants	0.39	0.83	0.37	0.80	0.39
male head	0.81	0.40	0.82	0.39	0.45
skilled head	0.22	0.42	0.23	0.42	0.31
relationship with village head	0.37	0.48	0.36	0.48	0.18
crop income (%)	69.12	33.87	71.77	31.88	1.19
self-employment income (%)	13.15	25.25	12.89	24.79	0.15
remittances income (%)	3.74	11.08	3.15	9.72	0.86
wage income (%)	10.58	24.75	9.14	23.56	0.88
land per adult equivalent	0.72	0.89	0.83	1.46	1.15
radio	0.38	0.49	0.37	0.48	0.07
asset index	-0.13	1.45	-0.20	1.33	0.78
distance to main road	26.83	39.80	24.89	35.30	0.78
distance to district town	32.74	22.23	34.37	22.41	1.05
population density	22.46	37.92	24.92	44.58	0.80
mean rainfall	963.02	178.49	998.84	181.99	2.84
rainfall variability	155.06	41.88	158.27	38.44	1.19
Central	0.15	0.36	0.10	0.30	2.47
Copperbelt	0.04	0.19	0.07	0.25	1.86
Eastern	0.21	0.41	0.22	0.42	0.31
Luapula	0.14	0.34	0.13	0.34	0.21
Lusaka	0.05	0.22	0.02	0.15	2.51
Northern	0.12	0.33	0.16	0.37	1.56
Northwestern	0.03	0.18	0.07	0.26	2.34
Southern	0.13	0.33	0.12	0.33	0.29
Western	0.14	0.34	0.10	0.30	1.50

Treatment is any prime-age death between 2001 and 2004.

TABLE A2: LOGIT ESTIMATION OF THE PROPENSITY SCORE (TREATMENT IS ANY PRIME-AGE DEATH BETWEEN 2001 AND 2004)

	coef	se
no 0-14 years	-0.070**	0.028
no 15-59 male skilled	0.189**	0.095
no 15-59 male unskilled	0.201***	0.053
no 15-59 female skilled	0.389**	0.164
no 15-59 female unskilled	0.351***	0.038
no 60 years and older	0.497***	0.179
no migrants	-0.139	0.109
male head	-0.085	0.176
skilled head	0.075	0.194
relationship with village head	-0.024	0.110
crop income	-0.302	0.544
self-employment income	-0.106	0.668
remittances income	0.178	0.556
wage income	0.143	0.671
land per adult equivalent	-0.023	0.041
radio	-0.066	0.098
asset index	-0.084	0.068
distance to main road	0.002	0.002
distance to district town	-0.005	0.005
population density	0.001	0.001
mean rainfall	0.000	0.001
rainfall variability	-0.004	0.003
Central	0.620**	0.293
Copperbelt	-0.541***	0.141
Eastern	0.017	0.270
Luapula	0.217	0.145
Lusaka	0.857**	0.366
Northwestern	-0.502***	0.096
Southern	-0.024	0.379
Western	0.455	0.320

***/**/* denote significance at the 1/5/10 percent level respectively.

TABLE A3: PSEUDO R2 AND MEAN BIAS REDUCTION

	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Pseudo R2 before matching	0.136	0.145	0.133	0.118
Pseudo R2 after matching	0.016	0.019	0.02	0.022
Mean bias reduction (%)	78.93	68.57	67.76	63.61
Mean bias after matching (%)	3.19	4.07	5.24	4.49

Treatment is any prime-age death between 2001 and 2004.

TABLE A4: T-TESTS FOR MATCHED SAMPLE AFTER PSM

2001 Characteristics	Quartile 1				Quartile 2				Quartile 3				Quartile 4			
	Mean (matched sample)		t-statistics	p-value	Mean (matched sample)		t-statistics	p-value	Mean (matched sample)		t-statistics	p-value	Mean (matched sample)		t-statistics	p-value
	Treated	Control			Treated	Control			Treated	Control			Treated	Control		
no 0-14 years	2.87	2.96	-0.24	0.82	2.97	3.02	-0.16	0.88	3.40	3.18	0.37	0.72	3.02	3.06	-0.10	0.92
no 15-59 male skilled	0.20	0.25	-0.52	0.60	0.36	0.34	0.10	0.92	0.53	0.52	0.09	0.93	0.82	0.83	-0.06	0.95
no 15-59 male unskilled	1.33	1.31	0.09	0.93	0.98	1.01	-0.14	0.89	1.33	1.05	0.93	0.35	0.93	1.02	-0.46	0.65
no 15-59 female skilled	0.18	0.16	0.27	0.79	0.44	0.40	0.25	0.80	0.37	0.35	0.10	0.92	0.43	0.58	-0.79	0.43
no 15-59 female unskilled	1.56	1.52	0.24	0.81	1.41	1.49	-0.49	0.63	1.58	1.33	0.80	0.43	1.33	1.40	-0.33	0.74
no 60 years and older	0.42	0.39	0.26	0.80	0.36	0.34	0.10	0.92	0.28	0.29	-0.08	0.94	0.45	0.40	0.37	0.72
no migrants	0.22	0.26	-0.35	0.73	0.36	0.36	-0.03	0.98	0.53	0.54	-0.02	0.99	0.48	0.49	-0.01	0.99
male head	0.73	0.72	0.05	0.96	0.76	0.79	-0.35	0.73	0.88	0.87	0.15	0.88	0.85	0.87	-0.36	0.72
skilled head	0.11	0.12	-0.14	0.89	0.20	0.18	0.34	0.73	0.23	0.25	-0.17	0.87	0.33	0.38	-0.48	0.63
relationship with village head	0.44	0.43	0.10	0.92	0.36	0.39	-0.41	0.69	0.37	0.39	-0.17	0.87	0.30	0.30	0.05	0.96
crop income (%)	81.05	81.94	-0.17	0.87	76.34	76.51	-0.03	0.97	71.63	70.23	0.20	0.84	48.10	48.01	0.01	0.99
self-employment income (%)	3.38	3.81	-0.15	0.88	12.73	12.01	0.18	0.86	13.31	13.37	-0.01	0.99	22.52	24.38	-0.31	0.76
remittances income (%)	7.34	7.13	0.06	0.95	2.74	3.02	-0.25	0.81	5.01	6.02	-0.36	0.72	0.77	0.94	-0.28	0.78
wage income (%)	5.72	4.93	0.24	0.81	6.70	6.18	0.16	0.87	7.35	7.70	-0.08	0.94	21.86	21.29	0.09	0.93
land per adult equivalent	0.36	0.38	-0.26	0.80	0.80	0.92	-0.42	0.68	0.76	0.79	-0.16	0.87	0.95	0.99	-0.16	0.88
radio	0.13	0.14	-0.24	0.81	0.29	0.30	-0.15	0.88	0.47	0.42	0.43	0.67	0.63	0.64	-0.06	0.95
asset index	0.63	-0.61	-0.14	0.89	-0.43	-0.41	-0.11	0.91	0.23	-0.28	0.19	0.85	0.69	0.85	-0.47	0.64
distance to main road	30.70	29.72	0.12	0.90	23.64	24.15	-0.08	0.94	28.82	28.21	0.07	0.95	24.18	22.95	0.19	0.85
distance to district town	32.68	32.87	-0.05	0.96	31.66	32.81	-0.27	0.79	35.46	34.40	0.23	0.82	32.02	31.64	0.10	0.92
population density	18.75	19.67	-0.20	0.84	18.93	19.25	-0.06	0.95	20.21	21.18	-0.19	0.85	31.66	30.37	0.12	0.91
mean rainfall	962.62	969.33	-0.20	0.84	1,008.70	1,000.80	0.23	0.82	968.56	978.38	-0.24	0.81	922.90	921.08	0.06	0.95
rainfall variability	151.53	152.11	-0.07	0.94	150.67	149.79	0.12	0.90	150.24	151.91	-0.22	0.83	166.23	165.62	0.08	0.94
Central	0.05	0.06	-0.17	0.87	0.10	0.12	-0.24	0.81	0.21	0.19	0.26	0.80	0.23	0.19	0.52	0.61
Copperbelt	0.04	0.04	-0.18	0.86	0.02	0.02	-0.29	0.78	0.05	0.05	-0.03	0.98	0.05	0.05	-0.03	0.98
Eastern	0.20	0.21	-0.10	0.92	0.19	0.19	-0.04	0.97	0.21	0.21	-0.05	0.96	0.25	0.23	0.27	0.79
Luapula	0.16	0.17	-0.10	0.92	0.20	0.21	-0.07	0.94	0.14	0.14	0.04	0.97	0.05	0.06	-0.17	0.86
Lusaka	0.02	0.01	0.21	0.84	-	-	-	-	0.12	0.10	0.27	0.79	0.08	0.08	0.11	0.91
Northwestern	0.02	0.03	-0.44	0.66	0.03	0.04	-0.17	0.86	0.02	0.03	-0.28	0.78	0.05	0.04	0.24	0.81
Southern	0.16	0.16	0.05	0.96	0.07	0.09	-0.42	0.68	0.07	0.07	-0.06	0.95	0.18	0.21	-0.31	0.76
Western	0.20	0.18	0.23	0.82	0.19	0.15	0.55	0.58	0.07	0.07	-0.09	0.93	0.07	0.08	-0.36	0.72

Treatment is any prime-age death between 2001 and 2004.