AIDS Treatment and Household Spillover Benefits: Children's Nutrition and Schooling in Kenya[*]

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February 2007

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[*] This project would not have been possible without the support of the Academic Model for Prevention and Treatment of HIV/AIDS (AMPATH) and members of the IU-Kenya partnership. We are grateful to Kathleen Beegle, Michael Boozer, David Cutler, T. Paul Schultz, Christopher Udry, and seminar participants at the NBER Health Care Program Meeting, the NEUDC Conference, the University of Pittsburgh, and Yale University for comments and suggestions. Many individuals contributed to the implementation of the household survey under the direction of the authors and Mabel Nangami. Giovanna d'Adda assisted in managing the second round of the survey and the data collection was facilitated by the field supervision of Irene Muhunzu. We also acknowledge the tremendous contributions of Andrew Anyembe, Caroline Amuyunzu, Jayne Chaina, Norbert Ketter, James Mungai, June Ochanda, and Jacklyne Tetee for administering questionnaires; and Chelimo Cherono, David Marende, Maurice Mungai, Florence Oduor, and Martha Simiyu for computer entry of questionnaires. Financial support for this project was received from the Economic and Social Research Council (UK), Pfizer, Inc., The World Bank, Yale University's Center for Interdisciplinary Research on AIDS (CIRA) through a grant from the National Institute of Mental Health to Michael Merson, M.D. (No. P30 MH 62294), the Social Science Research Council, and the Calderone Program at Columbia University. The views expressed here do not necessarily reflect those of the World Bank or its member countries. All errors and opinions are our own.

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Abstract

The provision of life-saving antiretroviral (ARV) treatment has emerged as a key component of the global response to HIV/AIDS, but very little is known about the impact of this intervention on the welfare of children in the households of treated persons. We estimate the impact of ARV treatment on children's schooling and nutrition outcomes using longitudinal household survey data collected in collaboration with a treatment program in western Kenya. We find that children's weekly hours of school attendance increase by 20 to 35 percent within six months after treatment is initiated for the adult household member. For boys in treatment households, these increases closely follow their reduced market labor supply. Similarly, young children's short-term nutritional status—as measured by their weight-for-height Z-score—also improves dramatically. These results illustrate how the intrahousehold allocation of time and resources are altered in response to significant health improvements. Since the improvements in children's schooling and nutrition at these critical early ages will affect their socio-economic outcomes in adulthood, the widespread provision of ARV treatment is likely to generate significant long-run macroeconomic benefits.

1. Introduction

Health and education are the primary forms of human capital, critical for individual welfare and societal economic development. The threat posed to children's human capital is therefore among the most negative and far-reaching consequence of AIDS-related morbidity and mortality in sub-Saharan Africa. While the provision of life-saving antiretroviral (ARV) treatment has emerged as a central part of the medical and policy response to HIV/AIDS,[2] very little empirical research has investigated the welfare impacts of this important intervention on the children living with treated patients. The long-term macroeconomic consequences of HIV/AIDS and treatment in afflicted countries depends critically on our understanding of these intergenerational effects (Bell, Gersbach, and Devarajan, 2003; Young, 2005). Such information also lies at the heart of the debate about the merits of prevention versus treatment policies (Canning, 2006). This paper provides insights into these issues by studying how household behavior changes in response to the provision of ARV treatment to an infected adult in the household. Using a novel longitudinal dataset, we document large improvements in the nutrition and schooling outcomes of children living with treated patients.

Children living in households with HIV-infected adults are likely to be impacted by the disease in a number of ways. As the labor productivity of infected adults diminishes in response to disease progression, income and substitution effects can result in a change in the level of human capital investment in children, as well as a change in their labor supply.[3] In low income settings with imperfect credit and insurance markets, these reallocations of time and resources can be important consumption-smoothing mechanisms; and several studies have documented their use in response to other health and income shocks (see, for example, Pitt and Rosenzweig, 1990; Strauss and Thomas, 1995; Kochar, 1995; and Jacoby and Skoufias, 1997). Progression to late-stage HIV/AIDS is also accompanied by considerable morbidity, generating increased caregiving needs for the infected adult. This care is likely to be provided by household

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[2] The World Health Organization reports that as of December 2005, 810,000 HIV-positive individuals were estimated to be receiving ARV treatment in sub-Saharan Africa (WHO, 2006). However, this number represents only 17 percent of the 4.7 million HIV-positive individuals who currently need treatment.

[3] A large theoretical and empirical literature examines the role of income and substitution effects in individual time allocation decisions (beginning with Becker, 1965), family labor supply (beginning with Ashenfelter and Heckman, 1974) and household investment decisions.

members, thereby reducing the amount of time available for other activities and lowering household income.

Finally, in the case of AIDS, the health 'shock' is both large and—absent treatment—permanent, implying that investment in child human capital and child time allocation patterns could be altered to a greater extent than they would be in response to transitory shocks.[4] A growing empirical literature finds that orphaned children in Africa suffer setbacks in their schooling in the years after they lose their parents (Yamano and Jayne, 2005; Case and Ardington, 2006; Evans and Miguel, 2007), although the severity of these schooling impacts does vary substantially across low-income countries (Ainsworth and Semali, 2006). The small literature examining the effects of orphanhood on health finds small declines in the short-run (Ainsworth and Semali, 2000), but substantial negative impacts in the long-run (Beegle, De Weerdt, and Dercon, 2005). Together, the labor productivity, caregiving, and mortality effects of disease, point towards substantial negative impacts on children's nutrition and schooling outcomes. These impacts are important, as health and education have large economic returns (for reviews, see Strauss and Thomas, 1998 and Schultz, 1999) and drive economic growth in developing countries.

ARV treatment, which has been shown to dramatically improve patient health and market labor supply (Thirumurthy, Graff Zivin, and Goldstein, 2005), has the potential to reverse these impacts of AIDS-related morbidity and mortality. This paper provides direct evidence on how the provision of treatment affects household decisions to invest in children's health and schooling. This analysis also sheds light on how the negative shocks of AIDS, prior to treatment, affected investments in children's human capital and the extent to which those investment decisions were irreversible. Examining this response to treatment deepens our understanding of intrahousehold dynamics and human capital formation in the developing world, particularly in the context of the AIDS epidemic.

In order to measure the effects of treatment, we use household survey data we collected in collaboration with an ARV treatment program in western Kenya. Over the course of one year, longitudinal socio-economic data were collected from HIV-positive adult patients who had AIDS and received free ARV treatment. The survey data include information on the schooling and

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[4] There is, however, virtually no evidence on what happens to the health and education of children when an HIV-infected adult in their household becomes sick and approaches death. This is largely due to the lack of adequate survey information collected from individuals known to be HIV-positive.

nutritional outcomes of children residing in the patients' households. We examine changes over time in these outcomes, focusing on the school enrollment and weekly hours of school attended for children between the ages of 8-18 years and the anthropometric status of children under the age of 5 years. The effect of ARV therapy on children's outcomes is identified by examining outcomes at several points in time, both before and after adults in the children's households receive treatment. Our identification strategy is also strengthened by variation in the length of time that adult patients had been receiving treatment prior to the start of the survey. Using data collected simultaneously from a large random sample of non-patient households in the survey area, we also control for time-varying factors that could bias the estimates. The approach is tantamount to a difference-in-difference estimation strategy in which the treatment group consists of children in households of ARV recipients and the comparison group consists of children in the survey area.[5]

Since children in the random sample are unlikely to represent the counterfactual scenario under which adults with AIDS remain untreated, we make use of several additional comparisons to better capture the true impact of treatment on the treated. In particular, we compare children in treatment households to (a) orphans in the random sample; as well as to (b) children in random sample households where at least one adult self-reports a moderate or high risk of being HIV-positive. We also use propensity score matching to construct better comparison groups between the ARV and random sample households.

The results in this paper indicate that treating adult AIDS patients with ARV treatment results in substantial improvements in the welfare of children living with the patients. Our results indicate that there is a significant increase in the children's weekly hours of school attendance. These increases generally occur within six months after treatment is initiated for adult patients, and they are experienced by boys and girls living with the patients. Relative to a random sample of children, weekly school hours attended rise by over 20 percent during this period, with boys experiencing an even larger rise of 30 percent. After six months of treatment, the increases in school attendance are maintained with no significant drop off over the time period of our study. Moreover, we find that for boys in particular, the increase in hours of attendance is almost exactly equal to the treatment-driven decrease in their hours of market labor

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[5] Given the clinical effectiveness of ARV therapy, the construction of a randomly chosen control group of children living with HIV-positive adults who are medically qualified for ARV treatment but do not receive it is ethically infeasible.

supply. The treatment effect is considerably larger when we compare children in treatment households to control groups that better represent the counterfactual scenario of no treatment. Overall attendance rises by approximately 35 percent within the first six months of treatment, with smaller gains made in the period that follows. Again, results are larger for boys, who experience a 50 percent increase in weekly hours of school attended within six months and substantial additional gains thereafter. These results suggest that pre- and post-treatment comparisons that do not consider the consequences of continued health deterioration and premature death among untreated adults with AIDS significantly underestimate the full impact of ARV treatment on schooling outcomes.

Our analysis of the short-term nutritional status of young children in adult patients' households also reveals significant improvements in children's welfare due to ARV treatment. At baseline, children in ARV households are more malnourished than their random sample counterparts. Within six months after treatment is initiated for the adult patient, this difference is completely erased. When we focus exclusively on the severely malnourished, we find similar results, with the likelihood that a child is wasted declining by 9.8 percentage points across all treatment households. As in the case of schooling outcomes, the improvements in nutritional status are much larger when children in ARV households are compared to alternative control groups that better represent the counterfactual. These gains in nutritional status can be expected to improve the physical and cognitive abilities of children and eventually, their post-school productivity levels (Alderman, Behrman, Lavy, and Menon, 2001; Glewwe, Jacoby, and King, 2001; Alderman, Hoddinott, and Kinsey, 2006). Together these results serve as evidence that there are substantial long-term benefits from providing ARV treatment.

The next section provides background on the treatment intervention that we study, as well as the household survey data. This is followed by a discussion in Section 3 of our strategy for estimating the impact of ARV treatment on children's outcomes. Regression results showing the effect of treatment on children's schooling and nutrition are presented in Sections 4 and 5, respectively. Section 6 concludes and discusses policy implications of this research.

2. Background and Data

This paper uses data from a household survey that we conducted in Kosirai Division, a rural region near the town of Eldoret, in western Kenya. The survey has been described in detail in

Thirumurthy, Graff Zivin, and Goldstein (2005). In this section, we provide a brief review of the literature on ARV treatment followed by an overview of the survey and details on the schooling and nutrition data.

2.1. Treatment of HIV/AIDS with Antiretroviral Therapy

Almost all HIV-infected individuals experience a weakening of the immune system and progress to developing AIDS. This later stage is very often associated with substantial weight loss (wasting) and opportunistic infections such as pneumonia and tuberculosis. Once individuals develop AIDS, death is highly imminent without treatment. Median survival times are estimated to be between 5.1 months and 9.2 months (Chequer et al., 1992; Morgan et al., 2002). Opportunistic infections are generally the cause of death in AIDS cases.

Highly active antiretroviral therapy[6] has been proven to reduce the likelihood of opportunistic infections and prolong the life of HIV-infected individuals. According to WHO guidelines, ARV therapy should be initiated around the time that individuals progress to AIDS (WHO, 2002).[7] After several months of treatment, patients are generally asymptomatic and have improved functional capacity. Numerous studies in various countries and patient populations have reported positive results.[8,9] In Haiti, patients had weight gain and improved functional capacity within one year after the initiation of ARVs (Koenig, Leandre, and Farmer, 2004). In Brazil, median survival time after developing AIDS rose to 58 months with ARV therapy (Marins et al., 2003). ARV therapy has also been highly effective at the HIV clinic where our study took place (see Wools-Kaloustian et al., 2006, as well as the results in Thirumurthy, Graff Zivin, and Goldstein, 2005).

While the effect of ARV therapy on the health of treated patients has been widely documented, much less is known about the broader impact that treatment interventions can have

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[6] In this paper, we use the terms “ARV therapy” and “ARV treatment” to refer to highly active antiretroviral therapy (HAART), which was introduced in 1996. HAART consists of three ARV medications, with a common first-line regimen of nevirapine, stavudine, and lamivudine. Generic medications that combine three ARVs in one pill (such as Triomune) have recently become available.

[7] Many treatment programs in developing countries, including the one that we collaborated with, have followed these guidelines. See Grubb, Perriens, and Schwartlander (2003) and Mamlin et al. (2004).

[8] For evidence from western countries where ARV therapy first became available, see Hammer et al. (1997), Hogg et al. (1998), and Palella et al. (1998).

[9] Since placebo-controlled randomized trials of ARV therapy are ethically infeasible, these studies are either observational cohort studies or randomized trials that compare regimens composed of different antiretroviral medications.

on the social and economic outcomes of patients and their families. Our survey in western Kenya was designed to examine these impacts.

2.2. Household Survey Data

Households in the survey area are scattered across more than 100 villages where crop farming and animal husbandry are the primary economic activities and maize is the major crop. The largest health care provider in the survey area is a government-run health center that offers primary care services. The health center also contains a clinic that provides free medical care (including ARV therapy) to HIV-positive patients. This rural HIV clinic (one of the first in sub-Saharan Africa) was opened in November 2001 by the Academic Model for the Prevention and Treatment of HIV/AIDS (AMPATH).[10] Since late-2003, AMPATH has had adequate funding to provide ARV therapy to all patients who are eligible according to the WHO guidelines.[11]

We conducted two rounds of interviews between March 2004 and March 2005, with an interval of roughly six months between rounds (round 1 was between March and August 2004; round 2 was between September 2004 and March 2005). The survey sample contains three different groups of households: 503 households chosen randomly from a census of households in Kosirai Division without an AMPATH patient (random sample households), 206 households with at least one known HIV-positive adult who began receiving ARV therapy at the AMPATH clinic prior to the round 2 interview (ARV households), and 60 households with at least one known HIV-positive adult who is an AMPATH patient who was not yet sick enough to require ARV therapy (HIV households).[12] Our analysis in this paper excludes the HIV households, and instead focuses on the ARV households since they had experienced significant health changes before and after receiving the treatment intervention of interest. We also exclude all children known to be HIV-positive, as we are primarily interested in studying how uninfected family members are affected by the provision of ARV treatment to an adult. While we do not know the

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[10] AMPATH is a collaboration between the Indiana University School of Medicine and the Moi University Faculty of Health Sciences (Kenya). Descriptions of AMPATH's work in western Kenya can be found in Mamlin, Kimaiyo, Nyandiko, and Tierney (2004) and Cohen et al. (2005).

[11] In response to evidence that individuals with AIDS have higher caloric needs (WHO, 2003), AMPATH also began distributing food to ARV patients during our study period. Quantities were small, but nearly two thirds of our patients received some food prior to our round 2 interview. All of the results presented in this paper remain unchanged when we control for the provision of food to treatment households.

[12] Since AMPATH did not face funding constraints during the survey period, the HIV-positive patients in our sample who were not receiving ARV therapy were not sick enough to need treatment (according to the WHO guidelines mentioned in Section 2). Using the AMPATH Medical Records System (AMRS), we have verified that these patients in fact do have a significantly higher CD4 count and body mass index (BMI) than the ARV recipients.

HIV status of all household members of AMPATH patients, it is likely that most young children of adult AMPATH patient have been brought in for HIV testing. These children are provided the required prophylaxis and/or treatment at the HIV clinic. As we discuss in the next section, we use the data from the random sample households to control for a range of confounding factors that would influence our interpretation of the longitudinal data for children in ARV households.

The ARV households were chosen according to the following criteria. First, they include *all* non-pregnant patients who entered the Mosoriot HIV clinic before April 2004 and resided in Kosirai Division. Second, to obtain a larger sample size, we also conducted in-clinic interviews with non-pregnant patients who entered the clinic before April 2004 but resided outside Kosirai Division (too far away from the clinic to be visited at home).[13]

The random sample was chosen from a list of households (excluding those that included patients at the HIV clinic) based on a census that we conducted with village elders in all 107 villages within Kosirai Division.[14] The total number of households recorded this way (6,215) corresponded very closely to the 6,643 households recorded by the 1999 Population Census (Central Bureau of Statistics, 1999), with the discrepancy most likely due to changes in the boundaries of Kosirai Division. The random sample of households should therefore provide representative information on the disease burden and socio-economic issues for non-patient households in the survey area.

In each round of the survey, information was obtained from the household head and spouse on a range of household and individual outcomes. This included data on asset sales and purchases, housing characteristics, hours of market labor supply in the past week by each household member, children's current and past school enrollment, and children's hours of school attendance in the past week.[15] For households within Kosirai Division, all of which were visited at home, measurement of the heights and weights of all resident children under the age of 5 years was also obtained.[16] In this paper, we focus on two key outcomes recorded in the survey: children's education and nutrition.

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[13] The identical strategy was used to construct the HIV sample. In total, 81 percent of all survey households were visited at home.

[14] In the random sample, the HIV status of respondents is usually unknown, unless the respondent gives a self-report of having gone for an HIV test and testing HIV-positive or HIV-negative.

[15] In the household visits, teams of male and female enumerators interviewed the household head and spouse as well as a youth in the household. For in-clinic interviews, all information was obtained from the AMPATH patient.

[16] In the second round, we also measured children who became older than five years *between* the first and second rounds.

2.3. Children's Education

When analyzing schooling outcomes, we focus on children who were between the ages of 8 and 18 in round 1. The reasons for studying this particular age group are two-fold. First, there is substantial variation in the age at which children in Kenya begin primary school. Typically, this occurs between the ages of 6 and 8, with considerable variation in the exact starting age. Since we will be examining how schooling outcomes change between rounds 1 and 2, restricting the sample to children older than 8 years allow us to focus on children who are very likely to be of school-going age during the survey period. Likewise, children above the age of 18 are much less likely to be enrolled in school since it is not common for children in rural areas to obtain a university education. Older children are also more likely to leave the parental home for reasons of employment or marriage, thereby creating a selected sample of over-18 children who are household members.[17]

In the random sample households and ARV households, there are a total of 1,245 children (from 527 households) between the ages of 8 and 18 in round 1—consisting of 902 children in 368 random sample households and 343 children in 159 ARV households.[18] Attrition of children in round 2 is minimal: information is available on 876 children from random sample households and 327 children in ARV households, representing attrition rates of 3 percent and 5 percent, respectively.[19] In the random sample, attrition generally is due to relocation of the entire household.[20] In the sample of ARV households, attrition generally occurs because the adult patient was not found at the clinic in round 2.

The two schooling outcomes we examine are enrollment in school and hours of school attended in the week prior to the interview. Information about these outcomes was obtained from the primary female respondent in each household (typically the spouse of the male

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[17] For this reason, we do not focus on the schooling outcomes of children older than 18 years.

[18] These figures indicate that a non-trivial fraction of households in our sample do not have any children between the ages of 8 and 18 in round 1: 27 percent of households in the random sample and 23 percent of households in the ARV sample.

[19] These figures do not include children who were household members in round 1 but moved out before round 2. For these children, the primary respondent was asked about school enrollment. School attendance information, however, is generally unavailable since the primary respondent would not know how many hours of school the child attended in the past week.

[20] Refusal rates in the second round of the survey were below 1 percent in the random sample.

household head; or the female household head in the case of single-headed households).[21] It is important to distinguish between the two schooling outcomes since children can often enroll in a school at the beginning of the school term but not attend on a regular basis (and therefore spend very little time in school).[22] Our enrollment measure indicates whether or not a child was enrolled in a school during the term in which the interview occurred. School attendance is measured as the primary female respondent's report of the number of hours of school attended by the child in the seven days prior to the interview (excluding travel time to and from the school).[23] Respondents were also asked follow-up questions about whether the reported hours of school attendance for the child was unusual, and if so, the reason why it was unusual. Since some respondents were interviewed during (or shortly after) school holiday periods, it is important to control for the presence of holidays during the recall period. To address this aspect of the data, our analysis below is based on a restricted sample of children in households for which interviews occurred during non-holiday periods.[24] Since the survey did not collect information on the *number* of days in the past week that were school holidays, we do not pursue an alternate strategy of keeping all the observations of unusual hours of attendance due to holidays and including a dummy variable that indicates whether the past week contained school holidays.[25] This sample restriction limits the sample size to 480 children who are studied in our data analysis.

In the context of primary schooling, it is noteworthy that Kenya's Ministry of Education abolished primary school fees beginning in January 2003. Given that the new policy took effect more than one year prior to start of our study, it is unlikely that the comparisons of schooling outcomes in round 1 and round 2 will be affected by this policy change. Although enrolling in

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[21] In cases where the respondent is an HIV-positive patient who was interviewed at the clinic, information on schooling outcomes is not necessarily obtained from the primary female in the household. For example, the respondent may be a male patient, or a female patient who is not the head or spouse.

[22] Primary and secondary schools in Kenya have three terms in each year. The first term begins in January and ends in April, the second term begins in May and ends in August, and the third term begins in September and ends in December.

[23] To emphasize the distinction between the discrete measure of school enrollment and the continuous measure of school attendance, the latter variable is generally referred to with the label "hours of school attendance".

[24] We also exclude cases in which respondents reported that children did not go to school because Class 8 exams were being held in late November. This is the nationwide exam taken to enter secondary school, and most primary schools in the survey area did not hold school for children below Class 8 during the day of these exams.

[25] The inclusion of month-of-interview (or week-of-interview) indicators is also an inadequate control for the effect of school holidays on weekly hours attended. This is because there appears to be variation in school holidays across schools and locations. The use of children from the random sample as a control group to absorb the effects of such variation is not feasible, as the required set of location-date indicators would leave us with inadequate power to control for these effects.

and attending primary school entails many costs other than school fees (as we discuss below), this policy may provide some explanation for the high levels of school enrollments that we observe in the survey area. It should be kept in mind, however, that secondary school fees still exist and can represent a substantial fraction of household income. Secondary school attendance may also be lower and more variable since the effective 'price' of spending time in school is greater for older kids who are more productive in the labor market. As a result, our analyses will generally distinguish between the schooling outcomes of young and old children.

2.4. Children's Nutrition

Anthropometry is widely recognized to be an important tool for assessing children's nutritional status (Waterlow et al., 1977; WHO Working Group, 1986; WHO, 1995). Two anthropometric indices, with different biological and statistical interpretations, are typically considered in the literature: weight-for-height and height-for-age. The former is a measure of thinness (or wasting) while the latter is measure of shortness (or stunting).[26] Weight-for-height is particularly sensitive to short-term growth disturbances caused by factors such as inadequate food and illnesses. As such, it represents a current estimate of nutritional status and can exhibit considerable variation over short periods of time. Height-for-age represents the cumulative effects of previous growth disturbances. Since growth in height is a much slower process than growth in body mass, a shortfall (or catch-up) in height-for-age will generally be slow to emerge, especially for children older than 2 years (Gorstein et al., 1994; WHO Working Group, 1986). For these reasons, weight-for-height is particularly well-suited for examining the short-term nutritional changes of interest in this paper. The primary outcome we examine is the weight-for-height Z-score, which is based on comparisons to the NCHS/CDC reference population of children in the U.S.[27] For a child with a given weight and height, the Z-score is calculated by subtracting the median weight of children in the reference population with the same height (as well as same age group and sex) and then dividing by the standard deviation in the reference population. The anthropometric indicators of children in the study population thereby remain comparable to each other and can also be compared to the reference population.

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[26] A third index, weight-for-age, is not widely used since it is primarily a composite of weight-for-height and height-for-age. As a result, it cannot distinguish between acute and chronic malnutrition. See Waterlow et al. (1977) and Gorstein et al. (1994) for further discussion.

[27] The comparison to well-nourished children in the U.S. is a common practice when analyzing anthropometric data from developing countries, where reference standards based on data from well-nourished children are generally not available. For a discussion of this reference population, see Gorstein et al. (1994) and WHO (1995).

As noted above, the heights and weights of children less than 5 years of age were measured during all household visits in each round. We conducted household visits for all non-pregnant HIV-positive patients that resided within Kosirai Division, which yielded panel data on 41 uninfected children from 30 households. The random sample contains 349 children (from 238 households) who were measured in both rounds.[28] In Section 5 we use the anthropometric data from both of these samples to examine what happens to the nutritional status of children as a result of providing ARV treatment to adults in their households.

3. Empirical Strategy

This section describes how we identify the effect of treatment on children's schooling and nutritional status. In particular, we discuss the methods used to analyze the longitudinal survey data on the outcomes of children living with HIV-positive adults who receive ARV treatment.

3.1. Estimating the Responsiveness of Schooling to Treatment

The reduced form treatment effect is identified by comparing schooling outcomes of children in ARV households in round 1 and 2. When attributing changes in enrollment and hours of attendance between rounds to the provision of treatment to an adult household member, however, it is also necessary to control for other time-varying factors that influence schooling outcomes. In the rural setting that we study, these factors include seasonal fluctuations in weather, labor demand, and food availability. We control for these factors by using data from children in the randomly selected households and by including a full set of month-of-interview indicators in the schooling equations. We also include individual fixed effects to control for time-invariant characteristics of children and their households that might influence *levels* of school enrollment and attendance. Specifically, the following equation is estimated with longitudinal data for children in the ARV and random sample households:

$$S_{iht} = \alpha_{ih} + \beta_1 (ARVHH_h * ROUND2_t) + \beta_2 ROUND2_t + \sum_{\tau=1}^{10} \gamma_\tau MONTH_t^\tau + \varepsilon_{iht} . \qquad (1)$$

S_{iht} is the schooling outcome of interest for child i in household h at time t (round 1 or 2), α_{ih} is a fixed effect for individual i in household h, $ARVHH_h$ is an indicator variables equal to 1 if

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[28] These sample sizes are smaller than those reported in Table 1 for several reasons. In the random sample and ARV sample, absence on repeated visits and attrition from the sample are among the reasons why the number of children used in our analysis is smaller than the number of children between 0-5 years in round 1. In the ARV sample, many children were not measured because households were outside the survey area of Kosirai Division and therefore not visited at home. Children who are themselves HIV-positive and are receiving care at the HIV clinic are also excluded from our analysis.

household *h* has an adult who began ARV therapy at any time before round two, and $ROUND2_t$ indicates whether the observation is from round 2. The round 2 indicator along with the ten month-of-interview indicator variables (with one month from each round omitted to avoid collinearity with the round 2 indicator) control for monthly fluctuations in schooling outcomes in the entire community. The coefficient of interest, β_1, measures the change in schooling levels (between round 1 and round 2) that is due to the adult patient being treated. This strategy is tantamount to a difference-in-difference estimation strategy in which the treatment group consists of children in ARV households and the "control" group consists of children in the random sample. The key identification assumption here is that the trends in schooling for children in the random sample accurately represent the effects of time-varying factors such as seasonality and other aggregate events. If schooling trends due to such factors are different for children in ARV and random sample households, we would obtain biased estimates of β_1. Differences in landholdings between the two types of households may be one source of bias, since the amount of land owned by a household could affect not only the levels of labor supplied by children but also changes in labor supply between planting and harvest seasons. We therefore estimate modified versions of equation 1 that allow trends in schooling between rounds to depend on land owned. We also estimate equation 1 for a sample that excludes households without land.

Earlier work has documented a highly non-linear temporal pattern in the health status and labor supply of patients after ARV treatment is initiated, with the largest impacts occurring within the first six months of treatment (Thirumurthy, Graff Zivin, and Goldstein, 2005). As such, we take advantage of variation in the treatment initiation date within our sample to estimate heterogeneous treatment effects on schooling.[29] We divide the sample of children in ARV households into two sub-samples of (a) children living with adult patients who had been receiving treatment for more than 100 days in round 1, and (b) children living with adult patients who had been receiving treatment for less than 100 days in round 1, including those who began receiving treatment between round 1 and round 2. Using individual fixed effects and month-of-interview controls, we then estimate the following equation to examine whether children in the two sub-samples have different changes in enrollment and attendance levels between rounds:

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[29] Among the adults who began receiving ARV therapy before round 2, roughly half began treatment more than 100 days before round 1. The other half began treatment less than 100 days before round 1 or between round 1 and round 2.

$$S_{iht} = \alpha_{ih} + \beta_1(ARVHH_{<100,h} * ROUND2_t) + \beta_2(ARVHH_{>100,h} * ROUND2_t) + \beta_3 ROUND2_t + \sum_{\tau=1}^{10} \gamma_\tau MONTH_t^\tau + \varepsilon_{iht}\,. \qquad (2)$$

$ARVHH_{<100,h}$ and $ARVHH_{>100,h}$ are indicator variables equal to 1 if household *h* has an adult who was receiving ARV therapy for less than or more than 100 days, respectively, at the time of the round 1 interview. The coefficient β_1 would indicate whether a treatment effect occurs soon after treatment is initiated, while the coefficient β_2 would indicate whether an effect is evident in the later stages of treatment.

The identification of a temporal response to ARV treatment based on the division of ARV households into early-stage and late-stage treatment households relies on the assumption that the only important difference between these two groups is the time they have been "exposed" to ARV treatment. Of particular concern is the scenario under which patients who began treatment earlier are better informed or better connected than those who began treatment just prior to round 1. This does not appear to be the case, as the AMPATH medical records reveal no significant differences in the health status of patients in the two groups (as revealed by the CD4 count) at the time of treatment initiation. We also find no significant differences in other important characteristics of households in the two groups. Both are indistinguishable with respect to household size and demographic characteristics, distance to clinic, and wealth measures such as land and livestock holdings.

The results from estimating the reduced form equations above will reveal how ARV treatment ultimately affects the schooling outcomes of children in treated patients' households. Since the survey recorded information on the hours of market labor performed by children in the week prior to interview, we can also examine how the effect of treatment on hours of school attended compares to the effect of treatment on market labor supply. Thus, as an extension of our analysis, we estimate equations 1 and 2 with the children's labor supply, rather than their school attendance, as the primary dependent variable.

3.2. Estimating ARV Treatment Impacts on Schooling with Alternate Comparison Groups

The estimation strategy above is unlikely to reveal the average effect of treatment on the treated because our data do not contain a control group of households with adults who are known to have AIDS but *do not* receive ARV treatment. Children's schooling outcomes under the counterfactual scenario of no treatment are therefore unobserved. The counterfactual is especially important here because we know that without ARV treatment, the health of adults with AIDS would have declined below the baseline levels measured in Round 1 of our survey. If this continued deterioration of health would have worsened children's schooling and nutritional outcomes, the strategy of examining changes in outcomes over the duration of treatment will yield an underestimate of the treatment effect on the treated. If children's outcomes would have improved, perhaps because parental death is more manageable than parental illness, the bias would be in the opposite direction.

In this section, we describe alternative strategies that can be used to approximate the average treatment effect on the treated. Rather than examining changes in the outcomes of "treated" children and controlling for the effects of seasonality through a comparison group of children in the random sample (as described in the previous section), we compare the trends in treated children's outcomes to those of children that share some similar characteristics to ARV households, with the key difference being that none of the latter children live with adult ARV recipients. The two different groups of children are a) orphans in the random sample; and b) children in the random sample who live in a household where an adult respondent self-reported high or moderate likelihood of having HIV/AIDS. We also use propensity score matching (PSM) techniques to estimate treatment effects. The rationale for considering these "control" groups is described below.

As discussed earlier, there is an extensive medical literature showing that untreated individuals with AIDS have extremely low life expectancy, as well as growing evidence that children in Africa experience declining school attendance in the periods before and after they become orphans. As a step toward comparing the outcomes of children in treatment households to the relevant counterfactual group, we first make use of data from the children in the random sample who are orphans. While information about their parent's death is generally limited, the schooling trends of orphans in the random sample may be a close representation of what would happen to children in treatment households under the "no treatment scenario." We therefore estimate equations 1 and 2 for a restricted sample consisting only of children in treatment households and orphaned children in the random sample. One potential weakness of this approach relates to the absence of information on the timing of parental death. If the parents of orphans in the random sample died long ago, any effect of parental death on schooling trends may have run its course before round 1 of the survey and the data on orphans would therefore be a poor representation of the counterfactual in changes, if not eventual levels.[30]

While orphans are likely to have lived with an adult who died of AIDS, it is more difficult to identify children in the random sample who currently live with untreated AIDS-afflicted adults. Although identification of adults in the random sample who have AIDS is not possible because survey respondents were not tested for HIV, we can take advantage of self-reported information from them on beliefs about their own HIV status. All respondents were asked to respond to a question about their chances of currently having HIV/AIDS (ranking them as high, moderate, low, or zero). For adult respondents in the random sample, it is likely that those who have engaged in riskier behavior or are having symptoms of AIDS would be more likely to report that they have a moderate or high chance of being HIV-positive.[31] As such, we also estimate equations 1 and 2 using the samples of children in ARV households and children living in random sample households in which at least one adult respondent reported a moderate or high chance of having HIV/AIDS. While not all of these adults are necessarily experiencing increasing morbidity due to AIDS (or are even HIV-positive for that matter), to the extent that this group is likely to contain a greater number of the counterfactual cases, we can expect the children in their households to be a better control group than children in the entire random sample.

Finally, we address the challenge of estimating the treatment effect on the treated using propensity score matching methods. Households in the random sample are matched to ARV households on the basis of shared characteristics that include wealth, household head's marital status, and self-reported HIV risk of adult household members. More specifically, the control group is determined by creating propensity scores from a probit equation that identifies characteristics strongly associated with being an ARV household. Comparisons are then made between members of each group with similar scores. The changes in schooling outcomes between rounds using this approach is analogous to the strategies discussed above, except in this

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[30] In our sample, the school attendance levels of random sample orphans are similar to those of children in ARV households at baseline and declining between survey rounds, providing suggestive evidence that parental death was relatively recent.

[31] Nearly all ARV recipients in our sample reported a high chance of having HIV/AIDS. We include those reporting a moderate chance in the random sample since the absence of a confirmatory test along with the stigma associated with the disease is likely to lead to an understatement of self-reported probabilities. In separate work, we have found that risk perception is, indeed, correlated with HIV risk factors, such as having a sexual partner who is unfaithful (Thirumurthy et al., 2006).

instance where the control group is identified using more than one covariate. Since, in addition to other household characteristics, an indicator of moderate or high self-reported HIV risk among adult respondents and an indicator of single-headed household are both included in our first stage regression, this approach can be viewed as a hybrid of our earlier comparisons. Using the propensity scores for households in our sample, we present estimates of the treatment effect on the treated using nearest neighbor and kernel matching methods.

3.3. Estimating the Responsiveness of Children's Nutrition to Treatment

To analyze the anthropometric data, we follow the standard practice of constructing weight-for-height standard deviation scores (Z-scores) using the 1978 NCHS/CDC reference population of children in the U.S. This index compares the weight of a boy or girl to the median weight of boys or girls in the reference population with the same height. Thus, a negative Z-score indicates that a child is thinner than the median child in the US population.

The effect of ARV treatment on the anthropometric outcomes of children living with treated patients is estimated by an empirical strategy similar to the one outlined above for schooling outcomes. In particular, we use the longitudinal data to estimate an equation with age controls and individual (child) fixed effects:

$$\begin{aligned} WHZ_{iht} = \alpha_{ih} + \theta_1 AGEmths_{iht}^{6-12} + \sum\nolimits_{\kappa=2}^{6} \theta_\kappa AGEyrs_{iht}^{\kappa} + \beta_1 (ARVHH_h * ROUND2_t) \\ + \beta_2 ROUND2_t + \sum\nolimits_{\tau=1}^{10} \gamma_\tau MONTH_t^{\tau} + \varepsilon_{it}\,. \qquad (3) \end{aligned}$$

WHZ_{iht} is the weight-for-height Z-score of child i in household h at time t (round 1 or round 2), $ARVHH_h$ indicates whether household h has an adult ARV recipient, and $ROUND2_t$ is an indicator for observations from round 2.[32] This contrasts the trend in nutritional status of children in ARV households with that of children in the random sample of households. Data from the latter group of children allows us to control for the sensitivity of the weight-for-height Z-score to age and to seasonal patterns in food availability. We also estimate a revised version of equation 3 that allows for heterogeneous treatment effects among children in early-stage and later-stage treatment households.

Finally, we also examine how estimated treatment effects change when we compare children in ARV households to sub-samples of children within the random sample that more closely resemble the ideal counterfactual group (children living with untreated, AIDS-afflicted

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[32] Following guidelines in Waterlow et al. (1977) for samples of our size, we use a set of one-year age indicators for children older than 1 year and six-month age indicators for children younger than 1 year.

adults), as outlined in the previous section for the case of schooling outcomes. In particular, we focus on what happens to the nutritional status of two groups of young children in the random sample households: (a) orphans; and (b) those living in households with an adult who self-reports a moderate or high risk of having HIV/AIDS. Due to the relatively small sample size of children with anthropometric measures in both rounds, we are unable to estimate the treatment effect using propensity score matching methods.

4. Results for Children's Schooling

Table 1 compares the main characteristics of households in the random sample and ARV sample in round 1. The statistics are only reported for the 170 random sample households and 76 ARV households that have children between the ages of 8-18 years and that were interviewed during non-holiday periods in *both* rounds (for reasons discussed in Section 2.3, this is the sample we use in the data analysis). On average, households in the random sample have 7.1 members. ARV households are significantly smaller, with about 6.4 members on average. There are also differences in the demographic composition of households in the two samples. ARV households are much more likely to be headed by single (and often widowed) women, whereas random sample households are generally headed by married men. Not surprisingly, ARV households also tend to consist of significantly more orphans. When we examine wealth measures for households, i.e. land holdings, land value, and the value of livestock, we do not find significant differences between ARV and random sample households.

School enrollment rates in the survey area are high, as indicated by the summary statistics in Table 2. Among all 352 children in the random sample between the ages of 8 and 18 years in round 1, 89 percent were reported as being enrolled in school.[33] However, this conceals significant differences in enrollment rates between primary and secondary school-aged children. There is nearly universal enrollment among children between the ages of 8 and 14 years, but enrollment rates decline for older children. For the 128 children in ARV households, enrollment rates are lower than those in the random sample of households, but the differences are not statistically significant in either round 1 or round 2.

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[33] The sample used excludes a small number of children for whom enrollment information is available but attendance information is unavailable. This is likely to be the case for children in boarding schools, since the respondents are unlikely to know the number of hours attended in the past week.

The high enrollment rates for younger children are similar to findings from earlier surveys of school enrollment in Kenya (Yamano and Jayne, 2005; Evans and Miguel, 2007).[34] These rates are also consistent with the more recent figures from the nationally representative 2003 Demographic and Health Survey (Central Bureau of Statistics, 2004). The DHS data indicate that in rural Kenya nearly 90 percent of children between the ages of 6 and 15 were attending school in 2003. The high enrollment rates found in the DHS may partly be driven by the nationwide abolition of primary school fees shortly before the survey was conducted. The absence of fees makes enrollment inexpensive, but the costs of regular attendance can be much more substantial. These include the variable costs of school materials, daily transportation to and from school in some cases, and most importantly, the opportunity costs of time spent in school. Thus, a better measure of schooling, particularly for primary school children, is likely to be school attendance.

As discussed in Section 2.3, school attendance is measured as the total number of hours that the child spent in school during the seven days prior to the interview. Table 2 reports the summary statistics for hours of attendance in our study sample. The summary statistics indicate that in round 1, there are significant differences in hours of attendance between children in random sample and ARV households. Children in the random sample of households attend school for an average of 34 hours in round 1 (unconditional on being enrolled in school), whereas children in ARV households attend for an average of 30.8 hours. In round 2, however, the differences in hours of school attended are not statistically significant. The summary statistics also indicate that average hours of school attended are lower in round 2, for both the random sample and the ARV sample. The most likely explanation for this stems from the fact that round 2 was conducted during the harvest period, when children generally spend more time working on the farm. Regardless of the reason for this decline in hours of school attendance, the advantage of our empirical approach, which makes use of a comparison group of children in the random sample, is that secular patterns in hours of school attended will not result in biased estimates of the response to ARV treatment.

For each household member older than 8 years, the survey also recorded information on hours of work devoted to three types of activities: wage and salaried jobs, farming on the

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[34] Both studies report very high average enrollment rates for children under 14 years of age. The study by Evans and Miguel (2005) also took place in western Kenya and found enrollment rates of 98 percent in 1998.

household's owned or rented land, and non-farm self-employed work. Our measure of market labor supply is defined as the total hours devoted to these the three activities. As we will discuss below, the temporal patterns in children's labor supply provide a useful comparison to the trends in hours of school attendance. Table 2 indicates that children's average hours of weekly labor supply are typically around 10 hours.[35] In round 1, children in ARV household work nearly 4 more hours per week than children in the random sample of households, a difference that is statistically significant. In round 2, the difference in labor supply between children in the two groups of households is no longer significant.

To identify the major correlates of school enrollment and hours of attendance, we estimate cross-sectional regressions on our sample of children between the ages of 8 and 18 years in round 1. All of the regressions also include a full set of month-of-interview indicators to control for seasonality. The hours of attendance regressions are not conditional on enrollment.[36] The results in Table 3 show that there are no significant differences in schooling between boys and girls.[37] Compared to children who are 18 years old in round 1, younger children are significantly more likely to be enrolled. Hours attended (unconditional on enrollment) are also higher for younger children, with the peak occurring for children between 11-14 years.

Parental education and family background are recognized as being strongly associated with children's education (e.g. Strauss and Thomas, 1995; Behrman, Foster, Rosenzweig, and Vashishta, 1999). The results in Table 3 verify that this association holds in our survey data as well. The amount of land owned by the household in which the child lives is positively associated with enrollment and hours of attendance. Years of schooling completed by the child's father and mother also has a positive association, with father's schooling being more strongly related than mother's schooling.[38] Consistent with the evidence on orphans cited earlier, we find that children whose mother is not a household member are at a significant educational disadvantage.

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[35] These average figures, of course, mask substantial variation by age and gender. Also, the definition of market labor supply does not include time devoted to household chores, which can be substantial for some children (particularly girls).

[36] It is not instructive to examine school attendance conditional on enrollment in *both* rounds of data since changes from no attendance to some attendance (or vice versa) could represent important treatment effects. Very few children are reported to be not enrolled in *both* rounds.

[37] The sample sizes in Table 3 are smaller than 480 due to missing information on land ownership for 18 children.

[38] Since the survey did not collect information on parents' schooling for orphans and foster children, we use an imputed value equal to the mean of father's or mother's schooling in the entire sample. We also include dummy variables indicating whether an imputed value is used.

Finally, we focus on the outcomes of children living with ARV recipients. Table 3 shows that in round 1, children in households of patients who have just begun ARV treatment attend fewer hours of school than children in the random sample, but the difference is not statistically significant. In round 2, the pattern is reversed but again not statistically significant. The next section examines the magnitude and significance of changes in schooling patterns by using child fixed effects to control for confounding factors.

4.1. Responsiveness of Schooling to ARV Treatment Provision, using Child Fixed Effects

Since the estimates in Table 3 may be biased due to omitted variables that are correlated with the indicator of whether the child lives in a household with an HIV-positive adult receiving ARV treatment, we estimate equations 1 and 2 using longitudinal data for the 480 children in our sample.

Columns 1 and 3 of Table 4a show the average treatment effect on school enrollment and hours of school attendance, respectively, for children in all ARV households. There is no significant change in these children's enrollment rates during the six months between rounds 1 and 2, but there is a large and significant increase of 4.06 hours in weekly school attendance. This represents a 12 percent increase relative to the average weekly hours attended by these children in round 1. This result is thus the first indication that the provision of ARV treatment has a positive effect on school outcomes of children living in treated patients' households.

Table 4a also presents results from estimating equation 2, which tests for heterogeneous effects that correspond to the length of time that patients have been receiving ARV treatment.[39] Again, no significant effect on school enrollment is found (column 2), a result that is perhaps not surprising given the low costs of enrollment and the high levels of enrollment for all children at baseline. For hours of attendance however, there is significant heterogeneity in the treatment effects. As column 6 shows, the increase in hours of attendance between rounds is particularly large for children in households of adults who are just beginning treatment in round 1 ($ARHH_{<100}$). The average increase in weekly hours attended is 6.39 hours for these children (representing a 21 percent increase relative to their average attendance level in round 1[40]). For children in households with an adult who started treatment at least 100 days prior to round 1

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[39] Since there are typically multiple children in each household, standard errors are clustered at the household level in each round.

[40] The average hours attended is 30.47 for children in households with adult ARV recipients who had been on treatment for less than 100 days in round 1 or who began treatment shortly after round 1 (represented by $ARVHH_{<100}$ in equation 2).

($ARHH_{>100}$), there is no significant change in weekly hours attended, although when we disaggregate by gender we do find that hours of attendance for boys continue to increase (discussed below). Taken together, these results suggest that children in ARV households experience the largest increase in hours of attendance within six months after treatment is initiated for the adult. In subsequent periods, they experience no additional changes, and they continue to maintain their initial increase in attendance. A striking feature of these results is their consistency with the large health and labor supply response to ARV treatment among adult AIDS patients (see Thirumurthy, Graff Zivin, and Goldstein, 2005). Given the previous finding that adult patients' health improves and labor supply increases soon after the initiation of treatment, these results suggest that the resulting income effect and decrease in care-giving burden allows children to spend more time in school.[41]

In Kenya, as in many developing countries, work and household responsibilities are frequently gender- and age-specific. Thus, when an adult becomes healthier and returns to work, the magnitude of the income and substitution effects that operate on household members and the extent to which they translate into changes in schooling outcomes may depend on their age and sex.[42] In Table 4a, Columns 3, 4, 7, and 8 show the results from testing for heterogeneous effects by sex. When we examine all ARV households together (columns 3 and 4), the results suggest that only boys experience a significant increase in attendance (of 6.51 hours). However, when we control for the length of time that the adult patient has been on treatment (columns 7 and 8), we find that there are significant increases in hours of attendance for *both* boys and girls in households where the adults are just beginning treatment in round 1. The increase in school attendance of 8.67 hours for boys is especially large, representing a 29 percent increase relative to their average attendance level in round 1.[43] Girls also experience a large and significant increase of 6.51 hours of school attended in the past week. In both cases, the increase in hours of attendance occurs within roughly six months after the initiation of treatment. Furthermore, there is some evidence that the increase in boys' attendance may spill over into the later periods of

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[41] The absence of any effect on school enrollment rates (columns 1 and 2 in Table 4a) suggests the provision of treatment results in improved school attendance among children *already* enrolled in school.

[42] We might also expect the effect of treatment to depend on the gender of the treated patient, but preliminary analysis indicates that this is not the case. However, given that nearly 75 percent of adult ARV recipients in our sample are women, we may not have large enough sample sizes to detect significantly different effects between male and female patients.

[43] The average hours attended are 30.15 for boys in households with adult ARV recipients who had been on treatment for less than 100 days in round 1 or who began treatment shortly after round 1.

treatment. As column 7 show, the boys' attendance increases by 4.9 hours for patients who have been on treatment over 100 days in round 1. This more persistent effect could be due to the fact that boys' labor supply shows a more drawn out response to treatment, as we will see in table 5.

As discussed in Section 3, the estimates of the treatment response will be biased if ARV and random sample households are affected differently by seasonal patterns and other community-level influences, such as weather shocks. Differences in land ownership could be one reason for this, although the summary statistics in Table 1 indicate no significant differences in standard measures of wealth. In results that are not reported here, we find that the treatment effects are not dependent on the amount of land owned by the household. When equations 1 and 2 are estimated for a sample that excludes households owning no land, we again find that the results are identical to those reported in Table 4a.

In Table 4b, we test for further heterogeneity in treatment effects by reporting the results for boys and girls of different age groups, focusing on primary school age children (ages 8-14 in round 1) and older children (14-18 in round 1).[44] We again find no significant changes in school enrollment rates of children in ARV households. Reported school enrollment is nearly universal for young girls and there is no variation between rounds, making it impossible to estimate a treatment effect.[45] For hours of school attended, we find that young boys and girls experience the bulk of the increases stemming from treatment provision. Within roughly six months after the initiation of treatment, there is an increase of 10.31 hours in weekly attendance for young boys (column 2) and an increase of 8.76 hours in weekly attendance for young girls. Taken together these education results represent an important benefit from the provision of ARV treatment and suggest that this health intervention has a dramatic effect on children's educational outcomes.

To put these results in perspective, it is worth comparing the magnitudes of the treatment effect on hours of school attendance (particularly those reported in Table 4a) to the treatment effect on children's weekly hours of market labor supply. Table 5 presents the results from estimating equations that identify the effect of ARV treatment on the children's labor supply. The equations estimated are similar in form to equations 1 and 2, with hours of market labor

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[44] The results below are robust to different definitions of these young and old age groups (with cutoffs at 12 and 13 years). The cutoff of 14 years is chosen since it is typically the age when children complete primary school.

[45] It is important to note, however, that our power to detect significant changes in hours of attendance is somewhat compromised by the fairly small sample sizes of children in each age-sex group.

performed in the week prior to interview as the dependent variable. Data from children in the random sample are again used to control for aggregate seasonal effects on labor supply.

Column 1 of Table 5 shows that on average, there is a large and significant reduction of 3.86 hours between round 1 and round 2 in the weekly market labor supply of children living in households of ARV recipients.[46] This is remarkably close to the estimated *increase* of 4.06 hours when the analogous equation is estimated for weekly hours of school attendance (see column 3 of Table 4a), suggesting a near-perfect crowd-out of work for school in response to improvements in adult health in the household. These results are particularly striking since for each child, the information on hours of school attendance in the past week and hours of market labor supply in the past week was obtained from the household's primary female and male respondent, respectively, in separate interviews.[47] Turning to the relationship between labor supply changes and duration of ARV treatment, column 4 of Table 5 shows that the average change in the labor supply of all children in ARV households occurs soon after treatment is initiated for the adult patient, and that reductions in labor supply continue to occur in the later stages of treatment as well.

Looking beyond average effects for all children in ARV households, columns 2, 3, 5, and 6 reveal larger and significant reductions in market labor supply for boys and no significant changes for girls. For boys in all ARV households, there is an average decrease in market labor supply of 7.46 hours per week in the six months between survey rounds (column 2 of Table 5). This is quite close to the estimated increase of 6.51 hours in weekly school attendance (column 4 or Table 4a). For boys in households of patients who are in the early stages of treatment, the estimated decrease in market labor supply (7.45 hours) also remarkably similar to the estimated increase in weekly school attendance (8.67 hours). The continued decline in the labor supply of boys in the later stages of adult treatment is also accompanied by a corresponding increase in hours of school attendance. The lack of any treatment effect on the market labor supply of girls should be interpreted with caution. Girls in the survey area spend significantly more time in non-market labor activities (such as household chores and care-giving) than market ones. If girls experience decreases in their non-market labor supply when an adult household member becomes healthier due to ARV treatment, this will not be captured in the market labor supply

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[46] The results presented here differ only slightly from those in Thirumurthy, Graff Zivin, and Goldstein (2005), as our analysis here is restricted to children in households that were interviewed during non-holiday periods.

[47] In addition, the respondents were also interviewed by different interviewers.

measures reported in Table 5. The evidence in this paper on girls' school attendance is consistent with such a time re-allocation pattern for girls (although we lack data on non-market labor supply to test whether this is actually the case).[48]

In summary, the results in this section indicate that the provision of ARV treatment to adults results in significant increases in hours of school attendance for children living with the patients. For boys, these results are strikingly consistent with the evidence that treatment also results in a decrease in their market labor supply. In particular, the magnitude of the effects on hours of attendance mirror closely the reductions in market labor supply. For girls, the schooling increases do not appear to be driven by changes in their market labor supply, suggesting a reallocation of time from non-market labor supply and/or leisure.

4.2. ARV Treatment Impacts: Results Based on Alternate Comparison Groups

As discussed in Section 3.2, estimating the average treatment effect on children in treated households requires making comparisons to children who lived in households with AIDS-infected adults that did not receive treatment. In the absence of such a comparison group, there are several strategies that can be employed to estimate this effect. In this section we present estimates of the treatment effect based on comparisons of children in ARV households to three alternative comparisons groups: (a) orphan children in the random sample of households; (b) children in random sample households that include an adult with moderate or high self-reported HIV risk (high-risk households); and (c) a control group based on propensity score matching.

Table 6 presents the results from estimating equation 2 with the alternative comparison groups for all children between 8-18 years, as well as separately for boys and girls. The main finding is that the average effect of ARV treatment on hours of attendance is much larger than the treatment responses reported in the previous section. The increase in the treatment effect when we use comparison groups that better represent the no-treatment counterfactual scenario is consistent with the hypothesis that outcomes of children living with AIDS-infected adults would worsen as the adults' health continued to deteriorate.

Column 1 of Table 6 shows that for all children in ARV households, hours of attendance increase significantly between round 1 and round 2 when compared to the attendance patterns of orphaned children. The increase in weekly hours of school attended is larger for children in

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[48] The survey did not collect information on non-market labor supply in round 2, thereby making it impossible to examine how time allocation to non-market activities was affected by the provision of ARV treatment.

households of patients who are in the early stages of treatment—the increase of 10.68 hours is nearly two times the effect estimated previously and it represents a 35 percent increase relative to these children's average attendance level in round 1 of 30.47 hours. Columns 3 and 5 show that the increase in hours of attendance is significant for both boys and girls, respectively. The point estimates for boys and girls in households of patients in the early stages of treatment are again considerably larger than before, at 15.69 and 10.8 hours respectively (compare to 9.14 and 6.36 hours in Table 4a). In contrast to the previous results, a positive and significant effect on hours of attendance is also observed for children in households of patients in later-stages of treatment in round 1. This seems to be driven by the larger effects observed for boys, as the results for girls are not significant in the later-stage treatment group.[49]

Table 6 also reports the results from comparing children in ARV households to children in high-risk households within the random sample. We find that the effect of treatment is again considerably larger than the treatment response estimated earlier. When we examine all children between 8-18 years of age (column 2), the average increase in weekly hours among ARV households is 10.79 hours. Significant increases in weekly hours of attendance are also found for boys and girls (columns 4 and 6, respectively). Reassuringly, the magnitudes of these changes are quite close to those found using orphans as the comparison group.

The results in Table 6 therefore illustrate that the effect of ARV treatment on school attendance of children in treated households is considerably larger when we compare them to children in the random sample who better resemble the true counterfactual group. Another method of estimating the average treatment effect on the treated, as discussed in Section 3.2, involves the use of propensity score matching to identify households in the random sample that closely resemble ARV households but do not receive ARV treatment. In Table 7a, we present results from estimating a probit model of whether or not a household is an ARV household. The key characteristics associated with a household being under treatment with ARVs are indicators of whether the head of household is single (which includes never married, divorced, or separated individuals) and whether the household has an adult respondent who self-reports a moderate or

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[49] As noted earlier, a large fraction of adult patients at the HIV clinic are women who have lost their husbands (most likely due to AIDS) and as a result, many children in the ARV households have already been orphaned. Since orphans in the random sample may not represent a good counterfactual comparison group for those children in ARV households who are already orphans, we can instead compare the schooling outcomes of *non-orphans* in treatment households to those of orphans in the random sample. The results in this case are very similar to those reported in Table 6.

high risk of being HIV-positive. Using the propensity scores calculated with the coefficients from estimating the probit model, Table 7b presents the average treatment effect on the treated (ATT) under different matching methods. Based on matching to the 2 nearest neighbors, the ATT is positive and significant, with an increase of 7.28 hours between rounds. When matching is conducted using a kernel, the ATT is smaller, with an increase of 4.12 between rounds. Given the small sample sizes used in the analysis, it is not possible to separately estimate the ATT for children in households that are in earlier and later stages of treatment. This likely explains why the average treatment effects are smaller than the treatment effects estimated in Table 6, which show a larger increase in school attendance between round 1 and round 2 for children in early-stage ARV households.

The striking feature of these results, however, is that regardless of the comparison groups used, we find a statistically significant and meaningful improvement in children's schooling outcomes as a result of treatment provision. While our ability to employ PSM techniques is constrained by the relatively small sample sizes, the results based on comparisons to orphan children and children in high-risk households suggest that the pre- and post-treatment comparisons of schooling outcomes (as presented in Section 4.1) significantly underestimate the full impact of ARV treatment.

5. Results for Children's Nutrition

As ARV treatment improves the health and employment outcomes of adult HIV-positive patients, outcomes other than time allocation of children (to labor and schooling) are also likely to be affected. In particular, an income effect from the increased labor supply of the adult patient may affect the nutritional status of household members. We look at this through an examination of the nutritional status of very young children (age 0-5 years) residing in the households of adult ARV recipients.

Table 8 presents summary statistics for the household characteristics and anthropometric status of all children measured in both rounds of the survey. Measurements were taken for children below the age of 5 years and residing in households that were visited by interviewers. In the analysis below, we again focus on the outcomes of children in ARV households who are not known to be HIV-positive (the very small number of children who are HIV-positive tend to also be patients at the AMPATH's pediatric HIV clinic). Comparing the ARV households and random sample households that contained children measured in both rounds, we find significant differences in the characteristics of the household head. ARV households are again much more likely to be headed by single women than the random sample of households. Notably, we do not find significant differences in household size or wealth measures (land and livestock ownership) between the two types of households.

Turning to the weight and height measurements of the children themselves, we find that those in ARV households have relatively low weight-for-height Z-scores in round 1 (average of -0.39). However, the mean weight-for-height Z-score of these children is not statistically different from that of children in the random sample of households (in either of the two survey rounds). This focus on means, however, masks important differences in the tails of the distribution. The simple cross-sectional comparisons show that 12 percent of children in ARV households exhibit wasting (Z-score below –2.0) in Round 1, significantly more than the 4 percent in the random sample. These differences disappear in Round 2, suggesting that the ARV treatment improves the nutritional status of wasted children.[50]

Some of these patterns are also evident in Table 9, which reports results from regressing the weight-for-height Z-score (for each round separately) on a set of individual and household characteristics.[51] The non-linear growth pattern for children is reflected in the various age coefficients, which indicate that Z-scores are initially high but decline in the first year after birth. Column 1 shows that in the first round, children living in households with an ARV recipient have significantly lower weight-for-height than children in the random sample of households. The shortfall in Z-score of 0.44 is substantial and suggests relatively low living standards in ARV households. We then divide the sample of children in ARV households as before, on the basis of treatment duration when the round 1 interview occurred. Column 2 shows that children in *both* treatment groups have lower Z-scores in round 1, but the differences are statistically significant for only the children living with late-stage ARV recipients. Examining the nutritional status of the same children in round 2 (columns 3 and 4 of Table 9), we find that children in ARV households are relatively better off in comparison to round 1. Column 4 shows that this is true for children in both groups of ARV households, although in terms of the difference in point

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[50] Table 8 also reports substantial variation in Z-scores by age group, as has been reported in other studies (Waterlow et al., 1977).

[51] Seasonal variations in weight-for-height are common (WHO Working Group, 1986), so we control for seasonality here and when analyzing changes in weight-for-height.

estimates between rounds 1 and 2, the largest improvements are experience by children living with early-stage ARV recipients.

To investigate changes in nutritional status while controlling for time-invariant unobserved characteristics that might be correlated with living in an ARV household, we use the longitudinal data to estimate equation 3 with child fixed effects Children in the random sample of households are again used as a comparison group to control for the effects of seasonality and aggregate events that influence nutritional status. We also include fixed effects for the interviewers who measured the children.[52,53] As column 1 of Table 10 indicates, children in ARV households have higher Z-scores in round 2, but the point estimate of 0.315 is not statistically significant. However, previous work has found that patients experience the largest clinical and labor market impacts soon after the initiation of ARV treatment (Wools-Kaloustian et al., 2006; Thirumurthy, Graff Zivin, and Goldstein, 2005). If these improvements translate into increased family income, there is reason to expect that short-term nutritional status should improve most for children in households of adult patients beginning to receive treatment in round 1. As column 2 of Table 10 indicates, this is exactly the pattern we observe. Children residing with patients who began ARV therapy less than 100 days prior to round 1 have a large and significant increase in their weight-for-height Z-score between rounds. The magnitude of the point estimate is worth emphasizing. Weight-for-height of children living with early-stage treatment recipients improves by 0.57 standard deviations in the six months between rounds 1 and 2, which more than erases the "pre-treatment" discrepancy in nutritional status that was observed in round 1. Children in later-stage treatment ARV households, on the other hand, have no significant change in Z-scores. The large magnitude of the improvement in weight-for-height soon after initiation of ARV treatment is consistent with the fact that it is a measure of current nutritional status and is known to be sensitive to short-term changes in the availability of food and other factors that affect growth.[54,55]

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[52] Following the recommendations in WHO (1995), the 9 observations with weight-for-height Z-score or height-for-age Z-score larger than 6 or smaller than -6 are excluded from the analysis.

[53] Interviewer fixed effects are included here because anthropometry, particularly measuring heights in small children, is challenging and can vary with individual skills and experience.

[54] The estimates can also be compared to the estimated effect of South Africa's Old Age Pension program on the nutritional status of children living with pension recipients. In this program, women older than 60 years and men older than 65 years receive a large monthly pension. Duflo (2003) finds that pensions received by women increased the weight-for-height of girls by 1.19 standard deviations.

While the results in columns 1 and 2 inform us about the average change in weight-for-height Z-score among children in treatment households, the effect of treatment on children with extremely low Z-scores in round 1 is of special interest given the potential long term effects of extreme malnutrition. We examine this by looking at how the fraction of children at the bottom of the distribution (those with Z-scores below -2.0, i.e. wasting) responds to the provision of treatment. In columns 3 and 4 of Table 10, we define the dependent variable as an indicator variable of whether a child's Z-score is below -2.0. The results show that there is indeed a significant decline in wasting among young children living in households of ARV treatment recipients – in column 3, we see that the likelihood that a child is wasted declines by 9.8 percentage points across all treatment households. As column 4 shows, the treatment effect is slightly more pronounced for children residing with patients who began ARV therapy more than 100 days prior to round 1. For children in early-stage treatment households, the decline in the likelihood of wasting is not statistically significant. These latter results suggest that, for children in the bottom of the distribution – those that have a larger amount of catching up to do – the effects can manifest well into the course of a household member's treatment.

Columns 5-8 of Table 10 show that the effect of treatment on weight-for-height Z-scores are considerably larger when we compare children in ARV households to sub-samples of the children in random sample households. In columns 5 and 6, the comparison group is made up of orphans in the random sample. The point estimates are much larger than those in columns 1 and 2, which compare children in treatment households to all children in the random sample, but the very small number of orphans less than five years old in the random sample (i.e. 7 children) limits our statistical power to the point where these results are not significant. More interesting are the results in columns 7 and 8, which are based on comparisons of children in treatment households to children in random sample households with an adult that reports moderate/high HIV risk. Between round 1 and round 2, the provision of ARV treatment to newly treated HIV-positive adults (those on treatment for less than 100 days as of round 1) results in a significant increase of 0.77 in the weight-for-height Z-score for children living in the adults' households. For all children in ARV households, we find a positive but insignificant change in Z-scores between round 1 and round 2 (column 7). Taken together, these results, much like the results on

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[55] Since height-for-age is an anthropometric index that changes slowly, children are unlikely to experience large changes over the course of six months. When examined as an outcome variable, it is reassuring that we find no significant changes in the height-for-age Z-scores of children living with ARV recipients.

children's schooling, highlight the fact that simple pre- and post-treatment comparisons of outcomes (which do not account for consequences of the morbidity and mortality that would occur among AIDS-afflicted adults without ARV treatment) generate underestimates of the effect of ARV treatment.

Considering the growing evidence that early childhood nutrition affects cognitive abilities later in life (Alderman, Behrman, Lavy, and Menon, 2001; Glewwe, Jacoby, and King, 2001; Alderman, Hoddinott, and Kinsey, 2006), the results in Table 10 are particularly noteworthy. Increases in the weight-for-height Z-score of children at early ages are capable of dramatically improving their economic and social well-being throughout the life cycle and are likely to have broader consequences for society. Thus, the impact of treatment on the nutritional status of children in treated patients' household may well equate to being the most long-lasting social benefit from providing ARV treatment.

6. Conclusion

The morbidity and mortality associated with AIDS poses a significant threat to family well-being among those infected. Our results suggest that the diminished earning capacity of HIV-infected adults along with the additional caregiver burden associated with their illness hastens the participation of children in the labor force and significantly reduces their school attendance. Furthermore, children living in AIDS-affected households experience significant malnutrition at a critical stage in their development. ARV treatment, which dramatically improves the health of infected individuals, reverses these effects. Children work less and spend more time in school; young children are better nourished. In contrast to the literature that examines more obviously temporary health and income shocks, we find the impacts on children's schooling and nutrition to be especially large. Indeed, the magnitude of the effects found here suggest that the mechanisms that households use to cope with transient shocks are of little assistance in the face of what is initially perceived as a severe permanent shock – the imminent death of a household member.

Of course, our results also have important implications for how one should value investments in ARV treatment. Most research in this area denominates the returns to treatment in some metric of health, measures that are focused on morbidity and mortality impacts for patients. Even the use of quality- (or disability-) adjusted-life-years saved, which under certain conditions can capture patient income effects, still misses the important non-patient impacts described in this paper. These impacts are not small. Within six months after the initiation of treatment for HIV-infected adults, weekly hours of school attendance for children in the treated adults' households increases by over 20 percent. When these children are compared to a group that better represents the counterfactual scenario of no treatment, the schooling impacts are even larger: weekly hours of school attendance increases by 35 percent in the first six months and continues to climb thereafter. The impacts on the nutritional status of very young children are equally impressive. Given the high returns to these two forms of children's human capital, our results suggest that ARV treatment has benefits that extend well beyond those experienced directly by treated patients.

These intergenerational impacts are not only important for family welfare, they have potentially important implications for economic growth. The increases in school attendance and improvements in the nutrition of very young children are likely to translate into higher levels of educational attainment for kids in HIV-infected households. As a result, treating the *current* generation of infected adults will contribute to economic growth in future years. Conversely, the absence of treatment will lead to an extended economic contraction as the loss of skills and labor supply of currently infected adults is compounded by the significantly lower levels of human capital amongst the next generation. Importantly, these intertemporal economic consequences of not providing treatment would be experienced even if we divert current treatment expenditures to disease prevention efforts. Thus, the discussions of any such diversion must weigh these costs against the benefits derived from reducing prevalence rates in the future. The HIV/AIDS epidemic and our response to it exert a profound influence on household investment decisions today, creating an inextricable link between the welfare of current and future generations in countries heavily impacted by the disease.

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Table 1. Characteristics of ARV Households and Random Sample Households

	Random Sample		ARV Sample		
	Mean	Std.Dev.	Mean	Std.Dev.	P-value
Number of households	170		76		
Household Structure (Round 1)					
Household size	7.1	2.9	6.4	2.3	0.06
Number of children (0-18 years)	4.0	2.0	3.6	1.6	0.10
Number of children (0-5 years)	0.87	0.94	0.72	0.91	0.24
Number of children (8-18 years)	2.6	1.4	2.5	1.3	0.37
Number of orphans (0-18 years)	0.38	0.88	1.3	1.6	0.00
Number of extended family members	1.2	1.4	1.4	1.6	0.37
Household Head Characteristics					
Age	49.3	13.9	46.8	11.2	0.16
Male	76%		49%		0.00
Single	21%		53%		0.00
Widowed	13%		37%		0.00
Asset Ownership (Round 1)					
Quantity of land owned (acres)	7.2	9.1	6.3	11.7	0.48
Percent landless	17%		16%		0.91
Value of land owned (1,000 Shillings)	697	928	625	1510	0.62
Value of livestock owned (1,000 Sh.)	62	67	75	96	0.36

Notes: P-value from t-test for equality of means for households in random sample and ARV sample. The summary statistics are calculated after excluding individuals who joined the household between round 1 and round 2. Individuals who left the household between round 1 and round 2 are included. The ARV sample consists of households that have an adult patient who began receiving ARV treatment sometime before round 2. Statistics are reported for only those households that a) have children between the ages of 8-18 years; and b) were interviewed on dates that did not contain any school holidays in the past week.

Table 2. Summary Statistics for Children's Schooling Outcomes

	Random Sample		ARV households		
	Mean	Std. Dev.	Mean	Std. Dev.	P-val.
N (children 8-18 in round 1)	352		128		
Enrolled in School					
Round 1	0.89		0.84		0.15
Round 2	0.83		0.77		0.15
Hours of school attended in past week (unconditional on enrollment)					
Round 1	34.0	(15.8)	30.8	(17.6)	0.06
Round 2	28.0	(15.8)	27.4	(17.0)	0.70
Hours of school attended in past week (conditional on enrollment)					
Round 1	38.1	(11.0)	36.5	(12.5)	0.23
Round 2	33.7	(10.5)	33.4	(9.4)	0.15
Did any work in past week					
Round 1	69%		69%		0.93
Round 2	78%		69%		0.06
Hours worked in past week (includes farm labor)					
Round 1	9.2	(12.2)	13.1	(20.8)	0.01
Round 2	8.9	(11.3)	7.4	(12.4)	0.22
Enrollment rates by age group (random sample in round 1)					
8-10.99	99%				
11-13.99	99%				
14-17.99	86%				

Notes: P-value from t-test for equality of means for children in ARV households and the random sample of households. Statistics are reported for only those households that a) have children between the ages of 8-18 years; and b) were interviewed on dates that did not contain any school holidays in the past week.

Table 3. Determinants of Children’s Schooling Outcomes in Round 1 and 2

	(1)	(2)	(3)	(4)
Dependent Variable:	Enrollment	Attendance	Enrollment	Attendance
	Round 1		Round 2	
Female	0.003	1.742	0.007	1.018
	(0.027)	(1.366)	(0.030)	(1.254)
Age 8-10.99 years	0.445	8.783	0.571	17.676
	(0.049)***	(2.523)***	(0.054)***	(2.280)***
Age 11-13.99 years	0.439	14.985	0.556	19.318
	(0.049)***	(2.497)***	(0.054)***	(2.262)***
Age 14-17.99 years	0.292	10.652	0.349	14.353
	(0.046)***	(2.345)***	(0.050)***	(2.109)***
Amt of land owned (acres)	0.003	0.347	0.003	0.226
	(0.001)**	(0.066)***	(0.001)**	(0.062)***
ARV household (<100 days)	-0.028	-2.391	-0.045	1.501
	(0.043)	(2.199)	(0.049)	(2.043)
ARV household (>100 days)	-0.033	-1.707	-0.060	-0.035
	(0.043)	(2.194)	(0.047)	(1.966)
Yrs school - father	0.005	0.866	0.007	0.676
	(0.005)	(0.270)***	(0.006)	(0.245)***
Yrs school - mother	0.001	0.463	0.004	0.574
	(0.005)	(0.242)*	(0.005)	(0.220)***
Missing father's schooling	0.015	2.270	0.031	-0.084
	(0.029)	(1.488)	(0.032)	(1.345)
Missing mother's schooling	-0.177	-9.711	-0.172	-4.775
	(0.037)***	(1.894)***	(0.042)***	(1.747)***
Month indicators	Yes	Yes	Yes	Yes
Constant	0.344	9.227	0.384	6.894
	(0.067)***	(3.432)***	(0.072)***	(3.008)**
Observations	462	462	462	462
R-squared	0.32	0.27	0.38	0.37

Notes: Standard errors in parentheses are clustered at the household level in each round (* significant at 10%; ** significant at 5%; *** significant at 1%). Dependent variable *Enrollment* indicates whether the child is enrolled in a school during the time of interview and *Attendance* is the total number of hours the child spent in school during the week prior to interview. Observations for which school attendance was reported to be below normal because of school holidays during the past week are dropped from the sample. A total of 18 observations are dropped due to missing information on landholdings of the children’s households.

Table 4a. Responsiveness of Schooling to ARV Treatment (with Child Fixed Effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Enrollment		Hours of school attended					
	All kids	All kids	All kids	Boys	Girls	All kids	Boys	Girls
ARV hh * Rd. 2	-0.011		4.064	6.507	2.916			
	(0.036)		(2.294)*	(2.787)**	(3.056)			
ARV hh (<100 days)*Rd. 2		-0.010				6.393	8.673	6.513
		(0.042)				(2.792)**	(3.854)**	(3.241)**
ARV hh (>100 days)*Rd. 2		-0.011				1.893	4.902	-1.035
		(0.045)				(2.548)	(2.770)*	(3.941)
Round 2	0.078	0.078	0.044	2.913	-2.199	-0.365	2.570	-2.789
	(0.048)	(0.048)	(3.698)	(6.604)	(3.277)	(3.621)	(6.417)	(3.298)
Constant	0.725	0.725	23.771	23.290	23.679	23.649	23.527	22.643
	(0.057)***	(0.057)***	(3.053)***	(3.809)***	(4.108)***	(3.097)***	(3.885)***	(4.144)***
Observations	960	960	960	518	442	960	518	442
R-squared	0.88	0.88	0.83	0.83	0.85	0.83	0.83	0.85

Notes: Standard errors in parentheses are clustered at the household level in each round (* significant at 10%; ** significant at 5%; *** significant at 1%). All regressions include child fixed effects as well as ten month-of-interview indicators (with one month from each round omitted to avoid collinearity with the round 2 indicator). Dependent variable *Enrollment* indicates whether the child is enrolled in a school during the time of interview and *Hours of school attended* is the total number of hours the child spent in school during the week prior to interview. Observations for which school attendance was reported to be below normal because of school holidays during the past week are excluded from the sample.

Table 4b. Responsivenes of Schooling to ARV Treatment, by Age and Gender (with Child Fixed Effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	Enroll	Attend	Attend	Enroll	Attend	Enroll	Attend
	Young Boys		Young Girls	Old Boys		Old Girls	
ARV hh (<100 days)*Rd. 2	0.020	10.307	8.761	0.014	5.027	0.014	5.093
	(0.017)	(4.593)**	(5.183)*	(0.112)	(5.079)	(0.081)	(3.811)
ARV hh (>100 days)*Rd. 2	0.028	4.114	0.161	0.075	3.814	-0.086	-4.319
	(0.021)	(2.820)	(4.101)	(0.120)	(4.346)	(0.122)	(5.379)
Round 2	-0.011	1.029	-10.686	0.208	0.790	0.036	1.519
	(0.010)	(7.101)	(6.568)	(0.141)	(8.062)	(0.065)	(3.565)
Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.993	24.007	43.684	0.411	19.615	0.535	14.481
	(0.008)***	(4.453)***	(4.026)***	(0.169)**	(6.192)***	(0.079)***	(3.757)***
Observations	274	274	192	244	244	250	250
R-squared	0.60	0.76	0.77	0.87	0.86	0.89	0.88

Notes: Standard errors in parentheses are clustered at the household level in each round (* significant at 10%; ** significant at 5%; *** significant at 1%). All regressions include child fixed effects as well as ten month-of-interview indicators (with one month from each round omitted to avoid collinearity with the round 2 indicator). Dependent variable *Enroll* indicates whether the child is enrolled in a school during the time of interview and *Attend* is the total number of hours the child spent in school during the week prior to interview. Young children are defined as children between the ages of 8 and 14 in round 1, old children are defined as children between the ages of 14 and 18 in round 1. Observations for which school attendance was reported to be below normal because of school holidays during the past week are excluded from the sample.

Table 5. Responsiveness of Market Labor Supply to ARV Treatment (with Child Fixed Effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Hours worked in past week					
Sample:	All 8-18	Boys	Girls	All 8-18	Boys	Girls
	Child Fixed Effects					
ARV hh * Rd. 2	-3.855	-7.464	0.145			
	(1.625)**	(2.426)***	(2.213)			
ARV hh (<100 days) * Rd. 2				-4.475	-7.452	-0.812
				(2.046)**	(3.185)**	(2.740)
ARV hh (>100 days) * Rd. 2				-3.276	-7.473	1.192
				(1.996)	(2.897)**	(2.833)
Round 2	-4.681	-9.939	-0.021	-4.584	-9.940	0.142
	(3.234)	(5.034)**	(4.246)	(3.242)	(5.048)*	(4.262)
Month Indicators	Yes	Yes	Yes	Yes	Yes	Yes
Constant	21.714	27.512	12.833	21.776	27.513	13.159
	(2.973)***	(4.008)***	(4.555)***	(2.978)***	(4.019)***	(4.595)***
Observations	916	502	414	916	502	414
R-squared	0.78	0.82	0.68	0.78	0.82	0.68

Notes: Standard errors in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%). All regressions include child fixed effects as well as ten month-of-interview indicators (with one month from each round omitted to avoid collinearity with the round 2 indicator). The dependent variable is the total number of hours devoted to income-generating activities in the past week. Observations for which school attendance was reported to be below normal because of school holidays during the past week are excluded from the sample. The number of observations is smaller than the 960 observations used in the analysis of schooling outcomes because labor supply information is missing for 44 children.

Table 6. Impact of ARV Treatment on Schooling: Alternate Comparison Groups (with Child Fixed Effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Hours of school attended in past week					
Comparison group:	Orphans in Random sample	High/Mod. HIV Risk households	Orphans in Random sample	High/Mod. HIV Risk households	Orphans in Random sample	High/Mod. HIV Risk households
	All kids (8-18 years)		All boys (8-18 years)		All girls (8-18 years)	
ARV hh (<100 days)*Rd. 2	10.675	10.787	15.686	14.561	10.805	10.397
	(3.262)***	(2.720)***	(4.877)***	(3.832)***	(4.676)**	(3.979)**
ARV hh (>100 days)*Rd. 2	5.808	5.316	10.930	9.302	2.503	1.652
	(3.133)*	(2.638)**	(4.467)**	(3.513)***	(4.566)	(4.036)
Round 2	0.069	1.904	5.530	10.914	-5.653	-5.704
	(5.665)	(4.828)	(9.624)	(7.144)	(7.795)	(6.890)
Constant	14.723	15.836	13.073	8.307	17.526	23.553
	(5.583)***	(4.753)***	(6.510)**	(5.693)	(10.406)*	(7.712)***
Observations	334	424	164	210	170	214
R-squared	0.86	0.85	0.84	0.87	0.90	0.86

Notes: Standard errors in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%). All regressions include child fixed effects as well as ten month-of-interview indicators (with one month from each round omitted to avoid collinearity with the round 2 indicator). Dependent variable is the total number of hours the child spent in school during the week prior to interview. Observations for which attendance was reported to be below normal because of school holidays during the past week are dropped from the sample.

Table 7a. Probit Regression of Households with an Adult on ARV Treatment

	Coefficient	z-value
Single-headed household	0.8917932	3.06
Amt of land owned (acres)	-0.0153242	-0.83
Household size	0.0060359	0.12
Value of livestock owned (shillings)	9.36E-07	0.4
Travel time to main road (mins.)	0.0034674	1.4
Value of durables owned (shillings)	-9.35E-08	-0.01
House with tin roof	0.2535599	0.58
House with non-mud roof	0.2180698	0.7
Household with respondent who reported high/moderate risk of having HIV/AIDS	2.76405	6.88
Constant	-3.250733	-4.87
Observations	225	
Pseudo R-squared	0.5151	

Notes: Sample includes households in the random sample and ARV households. Dependent variable is a dummy variable indicating whether the household contains an adult ARV recipient. Observations for which children's attendance was reported to be below normal because of school holidays during the past week are dropped from the sample.

Table 7b. Average Treatment Effect on Treated Using Propensity Score Matching

	Mean change between rounds 1 and 2			
	Random Sample	ARV households	Difference	T-stat
Hours of school attendance				
Nearest neighbor matching neighbors=2	-10.97	-3.69	7.28	1.94
Kernel matching bandwidth=.06	-7.82	-3.69	4.12	1.65

Notes: The average treatment effect on the treated is calculated using nearest neighbor matching and kernel matching of propensity scores. A common support of propensity scores is imposed by dropping observations in the treatment group whose propensity score is higher than the maximum or less than the minimum propensity score of observations in the control group, and also by trimming 2 percent of the treatment observations at which the propensity score density of the control observations is the lowest. For kernel matching, a biweight kernel is used.

Table 8. Summary Statistics of Children's Anthropometric Status and their Households Characteristics

	Random Sample		ARV households		
	Mean	Std. Dev.	Mean	Std. Dev.	P-value
Number of households	238		30		
Household size	7.0	2.8	6.7	2.4	0.61
Number of extended family members	1.0	1.5	1.4	2.0	0.15
Age of household head	41.5	12.4	45.4	12.9	0.11
Male household head	92%		67%		0.00
Single household head	12%		40%		0.00
Quantity of land owned (acres)	5.5	7.8	5.4	11.4	0.95
Percent landless	15%		17%		0.70
Value of land owned (1,000 Shillings)	503.2	65.5	655.1	1346.2	0.31
Value of livestock owned (1,000 Sh.)	52.8	62.8	48.8	102.7	0.76
N (children 0-5 years in Round 1)	349		41		
Weight-for-height Z-score					
Round 1	-0.08	(1.34)	-0.39	(1.77)	0.17
Round 2	0.03	(1.20)	-0.12	(1.43)	0.47
Percent with Weight-for-height Z<-2 (wasting)					
Round 1	4%		12%		0.03
Round 2	2%		5%		0.17
Height-for-age Z-score					
Round 1	-0.62	(1.48)	-1.38	(1.39)	0.00
Round 2	-0.80	(1.21)	-1.52	(1.52)	0.00
Mean Weight-for-height Z-scores (random sample in round 1)					
0-6 months	0.72	(1.47)			
6-12 months	0.60	(1.79)			
1-2 years	0.26	(1.53)			
2-3 years	-0.21	(0.92)			
3-4 years	-0.32	(0.91)			
4-5 years	-0.67	(0.97)			

Notes: P-value from t-test for equality of means for ARV households and the random sample of households.

Table 9. Determinants of Weight-for-Height Z-score in Round 1 and 2

	(1)	(2)	(3)	(4)
Dependent variable:	Weight-for-Height Z-score			
	Round 1		Round 2	
Age 6-12 months	0.099	0.069		
	(0.302)	(0.305)		
Age 1-2 years	-0.470	-0.476	-1.530	-1.530
	(0.251)*	(0.251)*	(0.216)***	(0.215)***
Age 2-3 years	-0.883	-0.894	-1.565	-1.581
	(0.259)***	(0.260)***	(0.218)***	(0.217)***
Age 3-4 years	-0.954	-0.953	-1.691	-1.685
	(0.259)***	(0.259)***	(0.212)***	(0.212)***
Age 4-5 years	-1.380	-1.375	-2.033	-2.017
	(0.248)***	(0.248)***	(0.222)***	(0.221)***
Age 5-6 years	-2.849	-2.633	1.212	1.193
	(1.435)**	(1.465)*	(0.222)***	(0.221)***
Female	-0.347	-0.348	-0.215	-0.213
	(0.139)**	(0.139)**	(0.111)*	(0.110)*
Orphan child	-0.173	-0.222	-0.067	-0.130
	(0.387)	(0.393)	(0.298)	(0.298)
Household variables				
with patient on ARVs	-0.441		-0.008	
	(0.255)*		(0.208)	
with patient on ARVs < 100 days in Round 1		-0.255		0.412
		(0.360)		(0.298)
with patient on ARVs > 100 days in Round 1		-0.597		-0.306
		(0.333)*		(0.257)
Constant	0.672	0.681	1.212	1.193
	(0.257)***	(0.258)***	(0.222)***	(0.221)***
Observations	384	384	389	389
R-squared	0.18	0.18	0.28	0.29

Notes: Standard errors in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%). *Weight-for-Height Z-score* is calculated from the measured weights and heights of children and based on comparison to a well-nourished reference population of children in the U.S. All regressions include month-of-measurement indicators and fixed effects for the interviewer who measured the child. The omitted age category is 0-6 months. Observations with weight-for-height Z-score or height-for-age Z-score larger than 6 or smaller than -6 are excluded from the analysis.

Table 10. Responsiveness of Weight-for-Height Z-score to ARV Treatment (with Child Fixed Effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Weight-for-Height Z-score		WHZ<=-2		Weight-for-Height Z-score			
Sample:	All children 0-5 in round 1				RS Orphans		RS Mod/High Risk	
ARV household * Round 2	0.315		-0.098		1.038		0.521	
	(0.202)		(0.043)**		(0.733)		(0.327)	
ARV household (<100 days in rd 1)		0.570		-0.071		1.195		0.768
* Round 2		(0.277)**		(0.058)		(0.785)		(0.392)*
ARV household (>100 days in rd 1)		-0.003		-0.111		0.773		0.220
* Round 2		(0.252)		(0.053)**		(0.859)		(0.419)
Round 2	-0.185	-0.166	0.018	0.019	-1.753	-1.598	-0.235	-0.167
	(0.321)	(0.321)	(0.067)	(0.067)	(1.382)	(1.422)	(0.557)	(0.559)
Age 6-12 months	0.628	0.636	-0.056	-0.055	1.241	1.284	0.814	0.825
	(0.179)***	(0.179)***	(0.038)	(0.038)	(0.801)	(0.813)	(0.347)**	(0.346)**
Age 1-2 yrs	0.467	0.436	-0.061	-0.064	0.101	0.008	0.044	-0.043
	(0.270)*	(0.270)	(0.057)	(0.058)	(1.016)	(1.039)	(0.524)	(0.528)
Age 2-3 yrs	0.281	0.245	-0.024	-0.027	0.525	0.371	0.191	0.072
	(0.367)	(0.367)	(0.078)	(0.078)	(1.409)	(1.448)	(0.745)	(0.751)
Age 3-4 yrs	0.487	0.421	-0.027	-0.033	0.596	0.236	0.339	0.094
	(0.466)	(0.467)	(0.099)	(0.100)	(1.851)	(1.964)	(1.002)	(1.023)
Age 4-5 yrs	0.408	0.324	0.046	0.039	0.158	-0.206	0.254	0.009
	(0.562)	(0.564)	(0.119)	(0.120)	(2.292)	(2.395)	(1.210)	(1.227)
Age 5-6 yrs	0.507	0.422	0.008	0.001	-0.623	-1.065	0.246	-0.035
	(0.646)	(0.647)	(0.137)	(0.138)	(2.583)	(2.713)	(1.385)	(1.404)
Constant	-0.498	-0.481	0.076	0.077	0.864	0.904	-0.339	-0.314
	(0.386)	(0.386)	(0.082)	(0.082)	(1.567)	(1.588)	(0.819)	(0.818)
Observations	772	772	772	772	96	96	250	250
R-squared	0.87	0.87	0.70	0.70	0.92	0.92	0.88	0.88

Notes: Standard errors in parentheses (* significant at 10%; ** significant at 5%; *** significant at 1%). Dependent variable *Weight-for-Height Z-score* (WHZ) is calculated from the measured weights and heights of children and based on comparison to a well-nourished reference population of children in the U.S. In columns 3 and 4, the dependent variable is an indicator of whether *WHZ* is less than or equal to -2 (meaning the child is classified as exhibiting wasting). All regressions include child fixed effects, fixed effects for the interviewer who measured the child, and ten month-of-interview indicators (with one month from each round omitted to avoid collinearity with the round 2 indicator). The omitted age category is 0-6 months. Observations with weight-for-height Z-score or height-for-age Z-score larger than 6 or smaller than -6 are excluded from the analysis.