Worms at Work: Long-run Impacts of Child Health Gains*

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Abstract: The question of whether – and how much – child health gains improve adult living standards is of major intellectual interest and public policy importance. We exploit a prospective study of deworming in Kenya that began in 1998, and utilize a new dataset with an effective tracking rate of 83% over a decade, at which point most subjects were 19 to 26 years old. Treatment individuals received two to three more years of deworming than the comparison group. Among those with wage employment, earnings are 21 to 29% higher in the treatment group, hours worked increase by 12%, and work days lost to illness fall by a third. A large share of the earnings gains are explained by sectoral shifts, for instance, through a doubling of manufacturing employment and a drop in casual labor. Small business performance also improves significantly among the self-employed. Total years enrolled in school, test scores and self-reported health improve significantly, suggesting that both education and health gains are plausible channels. Deworming has very high social returns, with conservative benefit-cost ratio estimates ranging from 24.7 to 41.6.

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1. Introduction

The question of whether – and how much – child health gains improve adult living standards is of major intellectual interest and public policy importance. The belief that childhood health investments have large payoffs in terms of adult living standards underlies government and foreign aid donor school health and nutrition programs in many less developed countries. In the absence of such public subsidies, standard models imply that productive child health investments might go unexploited in settings where infectious diseases are widespread, if treatment generates broader social benefits that households do not internalize. Yet the well-known methodological challenges to studying this issue – including the scarcity of both experimental variation in health investments and panel datasets tracking children into adulthood, and research designs that do not allow for the estimation of epidemiological externalities – have limited research progress.

We exploit a prospective experiment that provided deworming treatment to children in rural Kenyan schools starting in 1998, and utilize a new longitudinal dataset with an effective tracking rate of 83% among a representative subset of individuals enrolled in these schools over a decade (to 2007-09), at which point most subjects were young adults between 19 to 26 years of age. The combination of prospective variation in child health investments with a long-term panel dataset featuring high tracking rates, together with our ability to estimate spillover benefits of deworming treatment, sets this study apart from most of the existing literature.

Intestinal worm infections – including hookworm, whipworm, roundworm and schistosomiasis – are among the world's most widespread diseases, with roughly one in four people infected (Bundy 1994, de Silva *et al*. 2003). School age children have the highest infection prevalence of any group, and baseline infection rates in our Kenya study area are over 90%. Although light worm infections are often asymptomatic, more intense infections can lead to lethargy, anemia and growth stunting. Fortunately, worm infections can be treated infrequently (once to twice per year) with cheap and safe drugs. There is a growing body of evidence that school-based deworming in African settings can generate immediate improvements in child appetite, growth and physical fitness (Stephenson *et al.* 1993), and large reductions in anemia (Guyatt *et al.* 2001, Stoltzfus *et al.* 1997). Treating worm infections also appears to strengthen children's immunological response to other infections, potentially producing much broader health benefits in regions with a high tropical disease burden. For instance, a recent double-blind placebo controlled randomized trial among Nigerian preschool children finds that children who received deworming treatment for 14 months show reduced infection prevalence with *Plasmodium*, the malaria parasite (Kirwan *et al.* 2010), and other authors have hypothesized that deworming might even provide some protection against HIV infection (e.g., see Fincham *et al.* 2003, Hotez and Ferris 2006, Watson and John-Stewart 2007).

Due to the experimental design, deworming treatment group individuals in our sample received two to three more years of deworming than the control group. Previous work in this sample shows that deworming treatment led to large medium-run gains in school attendance and health outcomes, and, due to worms' infectious nature, that sizeable externality benefits accrued to the untreated within treatment communities and to those living near treatment schools (Miguel and Kremer 2004), as well as to the younger siblings of the treated (Ozier 2010).

In this paper, we generate unbiased estimates of the average impact of deworming on long-run outcomes by comparing the program treatment and control groups during 2007 to 2009. Among those with wage employment, we find that earnings are 21 to 29% higher in the deworming treatment group, while hours worked increase by 12% and work days lost to illness fall by a third. There is suggestive evidence that deworming also generated positive externalities on labor market outcomes, although these spillover effects are relatively imprecisely estimated.

These labor market gains are accompanied by marked shifts in employment sector for the treatment group, with more than a doubling of well-paid manufacturing jobs (especially among males) and declines in both casual labor and domestic services employment. Changes in the subsector of employment account for nearly all of the earnings gains in deworming treatment group in a Oaxaca-style decomposition. This pattern indicates that health investments not only boost productivity and work capacity in existing activities, but, by leading individuals to shift into more lucrative economic activities (like manufacturing employment), may also contribute to the structural transformation of the economy a whole. Understanding how to promote this transition has long been a central theme within development economics (see Lewis 1954, among many others), and our results provide a piece of suggestive evidence that health investments may speed this transition.

Measuring labor productivity is more challenging for the majority of our subjects who were either self-employed or working in subsistence agriculture, rather than working for wages, although even in these groups there is evidence of positive impacts. The estimated impacts on the small business performance of the self-employed, namely measures of profits and employees hired, are also positive and relatively large in magnitude. Total hours worked in any occupation was significantly higher in the treatment group, with particularly large gains in hours worked among the self-employed. The number of meals the respondent ate yesterday is also significantly higher in the treatment group, consistent with higher living standards.

We present a simple model (building on Bleakley 2010) to illustrate the conditions under which health and education gains might drive higher earnings. We find empirically that the total years enrolled in school increased, by approximately 0.3 years, some test scores rose, and self-reported health improved in the treatment group. Although we cannot convincingly decompose how much of our earnings gains are working through education versus health without imposing considerably stronger assumptions, these findings suggest that both channels are likely playing some role.

Deworming appears to have very high social returns. Considering only the earnings gains among the subset of wage earners, and taking into account the costs of drug treatment, the opportunity cost of additional time spent in school rather than working, and implicit congestion costs on the educational system from higher school attendance, conservative estimates of the benefit-cost ratio for deworming investments range from 24.7 to 41.6, depending on whether only wage productivity gains (per hour worked) are considered or if total earnings are assumed to capture benefits, respectively. The latter approach may be appropriate if better health improves the capacity to work longer hours, as in the original formulation of health capital in Grossman (1972), who argues that it is precisely this increase in "non-sick" time that distinguishes health investments from other types of human capital investment.

Our findings contribute to several strands of existing work. The most closely related studies are by Bleakley (2007a, 2007b, 2010), who examines the impact of a large-scale deworming campaign in the U.S. South during the early 20th century on schooling and adult earnings, by comparing heavily infected versus lightly infected regions over time in a difference-in-difference design. He finds that deworming raised adult income by roughly 17%, and, extrapolating these findings to the even higher worm infection rates found in tropical Africa, estimates that deworming in Africa could lead to income gains of 24%. Remarkably, given the gap in time and space between his setting and ours, this falls squarely in the range (21 to 29%) of our estimated earnings gains. Early work by Schapiro (1919) using a simpler first-difference research design found wage gains of 15-27% on Costa Rican plantations after workers received deworming treatment. Taken together, these findings lend credence to the view that treating intestinal worm infections can substantially increase labor productivity.[1] As Bleakley (2010) notes, the fact that deworming reduces morbidity but has negligible effects on mortality means it is particularly likely to boost per capita living standards.

[1] There remains a lively debate in the public health and nutrition literatures about the cost-effectiveness of deworming treatment (as surveyed in Taylor-Robinson *et al.* 2007). In earlier work in economics, Weisbrod et al (1973) document relatively weak cross-sectional correlations between worm infections and labor productivity, test scores, fertility, and mortality in St. Lucia. Bundy *et al*. (2009) argue that many existing studies understate the deworming's benefits since they fail to consider treatment externalities (and so understate true treatment effects) by

Beyond deworming, our findings contribute to the growing literature on the long-run economic impacts of early life health and nutrition shocks. The well-known INCAP experiment in Guatemala described in Hodinott *et al.* (2008), Maluccio *et al.* (2009), and Behrman *et al.* (2009) provided nutritional supplementation to two villages while two others served as a control, and finds gains in male wages of one third, improved cognitive skills among both men and women, and positive intergenerational effects on the nutrition of beneficiaries' children. Beyond the small sample size of four villages, a limitation of the INCAP studies is their relatively high attrition rate over the approximately 35 years of follow-up surveys, at roughly 40%. A series of other influential studies have shown large long-run economic impacts of *in utero* or child health and nutrition shocks resulting from natural experiments, including the worldwide influenza epidemic of 1918 (Almond 2006), war-induced famine in Zimbabwe (Alderman *et al.*, 2006a), and economic shocks driven by rainfall variation in Indonesia (Maccini and Yang, 2009).[2] While many studies argue that early childhood health gains *in utero* or before age three have the largest impacts (World Bank 2006 and Hodinott *et al.* 2008 are but two examples), our findings show that even health investments made in school aged children can have important effects.[3]

The rest of the paper is organized as follows. Section 2 presents background on the school deworming project and the follow-up survey. Section 3 lays out the estimation strategy and describes the impacts of deworming on labor market outcomes, while Section 4 focuses on effects on education and health. Section 5 computes the social returns to deworming investment, and the final section concludes, discussing external validity and implications for ongoing research and for public policy.

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using designs that randomize within schools; focus almost exclusively on biomedical criteria and ignore cognitive, education and income gains that are a key component of overall benefits; and do not deal adequately with high rates of attrition. The current paper attempts to address these three concerns. Beyond Miguel and Kremer (2004) and the current paper, Alderman *et al.* (2006b) and Alderman (2007) also use a cluster randomized controlled design and find large positive child weight gains in Uganda.

[2] Other studies in less developed countries that attempt to address the issue of long-run impacts of child health are those that deal with low birthweight (Sorenson *et al.*, 1997; Conley and Bennett, 2000); iodine deficiency *in utero* (Xue-Yi *et al.*, 1994; Pharoah and Connolly, 1991; Field *et al.*, 2007) and in early childhood (Fernald and Grantham-McGregor, 1998); whether children were breastfed (Reynolds, 2001); early childhoold malaria prophylazis, and early childhood under nutrition (Alderman *et al.*, 2003; Mendez and Adair, 1999; Glewee *et al.*, 2001), among many others. Though these studies are generally non-experimental (Jukes *et al.*, 2006 is an exception), taken together they provide considerable evidence that adult cognitive performance may be affected by nutrition in the womb and early childhood. Related work on the long-run benefits of child health and nutrition investments in the U.S. include Currie and Thomas (1995), Currie, Garces and Thomas (2002), and Case and Paxson (2010). Other noteworthy micro-empirical contributions on nutrition, health and productivity include Schultz (2005), Alderman (2007), Thomas *et al.* (2008), and Pitt, Rosenzweig and Hassan (2011), and recent contributions in macroeconomics on health and economic growth include Acemoglu and Johnson (2007), Ashraf, Lester and Weil (2009), and Aghion, Howitt and Murtin (2010).

[3] As discussed below, we do not find evidence of heterogeneous treatment effects by age or gender, but there is suggestive evidence that gains in work hours are larger among those who were younger when they received deworming, perhaps because the resulting health gains are somewhat larger earlier in life.

2. Background on the Primary School Deworming Program and Kenya Life Panel Survey

This section describes the study site, the deworming experiment, and follow-up survey, including our respondent tracking approach. We then present sample summary statistics.

2.1 The Primary School Deworming Program (PSDP)

In 1998, the non-governmental organization ICS launched the Primary School Deworming Program (PSDP) to provide deworming medication to individuals enrolled in 75 primary schools in Busia District, a densely-settled farming region of rural western Kenya adjacent to Lake Victoria. The schools participating in the program consisted of 75 of the 89 primary schools in Budalangi and Funyula divisions in southern Busia (with 14 town schools, all-girls schools, geographically remote schools, and program pilot schools excluded), and contained 32,565 pupils at baseline. Parasitological surveys conducted by the Kenyan Ministry of Health indicated that these divisions had high baseline helminth infection rates at over 90%. Using modified WHO infection thresholds (described in Brooker *et al.* 2000a), over one third of children in the sample had “moderate to heavy” infections with at least one helminth at the time of the baseline survey, a high rate but one not atypical in African settings (Brooker *et al.* 2000b). The 1998 Kenya Demographic and Health Survey indicates that 85% of 8 to 18 year old children in western Kenya were enrolled in school, indicating that our school-based sample is broadly representative of western Kenyan children as a whole.

Busia is close to the Kenyan national mean along a variety of economic and social measures. The 2005 Kenya Integrated Household Budget Survey shows that 96% of children aged 6 to 17 in Busia had “ever attended” school compared to 93% nationally, the gross enrollment rate was 119 compared to 117 nationally, while 75% of Busia adults were literate versus 80% nationally. However, Busia is poorer than average: 62% of Busia households fall below the poverty line compared to 41% nationally. Given that Kenyan per capita income is somewhat above the sub-Saharan African average (if South Africa is excluded), the fact that Busia is slightly poorer than the Kenyan average probably makes the district more representative of rural Africa as a whole.

The 75 schools involved in this program were experimentally divided into three groups (Groups 1, 2, and 3) of 25 schools each: the schools were first stratified by administrative sub-unit (zone), listed alphabetically by zone, and were then listed in order of enrollment within each zone, and every third school was assigned to a given program group; supplementary appendix A contains a detailed description of the experimental design. The groups are well-balanced along baseline demographic and educational characteristics, both in terms of mean differences and distributions, where we assess the latter with the Kolmogorov-Smirnov test of the equality of distributions (Table 1).[4] The same balance is also evident among the subsample of respondents currently working for wages (see Supplementary Appendix Table A1).

Due to the NGO's administrative and financial constraints, the schools were phased into the deworming program over the course of 1998-2001 one group at a time. This prospective and staggered phase-in is central to this paper's econometric identification strategy. Group 1 schools began receiving free deworming treatment in 1998, Group 2 schools in 1999, while Group 3 schools began receiving treatment in 2001; see Figure 1. The project design implies that in 1998, Group 1 schools were treatment schools while Group 2 and 3 schools were the comparison schools, and in 1999 and 2000, Group 1 and 2 schools were the treatment schools and Group 3 schools were comparison schools, and so on. The NGO typically requires cost sharing, and in 2001, a randomly chosen half of the Group 1 and Group 2 schools took part in a cost-sharing program in which parents had to pay a small positive price to purchase the drugs, while the other half of Group 1 and 2 schools received free treatment (as did all Group 3 schools). Kremer and Miguel (2007) show that cost-sharing led to a sharp reduction in deworming treatment rates by 60 percentage points, introducing further exogenous variation in deworming treatment that we can exploit. In 2002 and 2003, all sample schools received free treatment.

Children in Group 1 and 2 schools thus were assigned to receive 2.41 more years of deworming than Group 3 children on average (Table 1), and these early beneficiaries are what we call the deworming treatment group below. We focus on a single treatment indicator rather than separating out effects for Group 1 versus Group 2 schools since this simplifies the analysis, and because we find few statistically significant differences between Group 1 and 2 (not shown).

The fact that the Group 3 schools eventually did receive deworming treatment will tend to dampen any estimated treatment effects relative to the case where the control group was never phased-in to treatment. In other words, a program that consistently dewormed some children throughout childhood while others never received treatment might have even larger impacts. However, persistent differences between the treatment and control groups are plausible both because several cohorts "aged out" of primary school (i.e., graduated or dropped out) before treatment was phased-in to Group 3, and to the extent that more treatment simply yields greater benefits..

[4] Miguel and Kremer (2004) present balance along a fuller set of baseline covariates for the treatment and control groups. Deaton (2010) critiques the "list randomization" approach in Miguel and Kremer (2004), Chattopadhyay and Duflo (2004), and several other recent field experiments.

Deworming drugs for geohelminths (albendazole) were offered twice per year and for schistosomiasis (praziquantel) once per year in treatment schools.[5] We focus on intention-to-treat (ITT) estimates, as opposed to actual individual deworming treatments, in the analysis below. This is natural as compliance rates are high. To illustrate, 81.2% of grades 2-7 pupils scheduled to receive deworming treatment in 1998 actually received at least some treatment. Absence from school on the day of drug administration was the leading cause of non-compliance. The ITT approach is also attractive since previous research showed that untreated individuals within treatment communities experienced significant health and education gains (Miguel and Kremer 2004), complicating estimation of treatment effects on the treated. Miguel and Kremer (2004) show that deworming treatment improved self-reported health and reduced school absenteeism by one quarter during 1998-1999. Large externality benefits of treatment also accrued to individuals attending other schools within 6 kilometers of program treatment schools. There were no statistically significant academic test score or cognitive test score gains during 1998-2000.

2.2 Kenya Life Panel Survey (KLPS)

The first follow-up survey round of the PSDP sample, known as the Kenyan Life Panel Survey Round 1 (KLPS-1), was launched in 2003. Between 2003 and 2005, the KLPS-1 tracked a representative sample of approximately 7,500 individuals who had been enrolled in primary school grades 2-7 in the 75 PSDP schools at baseline in 1998. The second round of the Kenyan Life Panel Survey (KLPS-2) was collected during 2007-2009, and tracked this same sample of individuals. The KLPS-2 includes detailed questions on the employment and wage history of respondents (with questions based on Kenyan national surveys), as well as education, health, demographic and other life outcomes.

A notable strength of the KLPS is its respondent tracking methodology. In addition to interviewing individuals still living in Busia District, survey enumerators traveled throughout Kenya and Uganda to interview those who had moved out of local areas; one respondent was even surveyed in London (in KLPS-1). Searching for individuals in rural East Africa is an onerous task, and migration of target respondents is particularly problematic in the absence of information such as

[5] Following World Health Organization recommendations (WHO 1992), schools with geohelmith prevalence over 50% were mass treated with albendazole every six months, and schools with schistosomiasis prevalence over 30% mass treated with praziquantel annually. All treatment schools met the geohelminth cut-off while roughly a quarter met the schistosomiasis cut-off. Medical treatment was delivered to the schools by Kenya Ministry of Health public health nurses and ICS public health officers. Following standard practices at the time, the medical protocol did not call for treating girls thirteen years of age and older due to concerns about the potential teratogenicity of the drugs.

forwarding addresses or home phone numbers, although the recent spread of mobile phones has been helpful. The difficulty in tracking respondents is especially salient for the KLPS, which follows young adults in their late teens and early twenties, when many are extremely mobile due to marriage, schooling, and job opportunities. Thus, it is essential to carefully examine survey attrition. If key explanatory variables, and most importantly deworming treatment assignment, were strongly related to attrition, then resulting estimates might suffer from bias.

The 7,500 individuals sampled for KLPS-2 were randomly divided in half, to be tracked in two separate waves. KLPS-2 Wave 1 tracking launched in Fall 2007 and ended in November 2008. During the first part of Wave 1, all sampled individuals were tracked.[6] In August 2008, a random subsample containing approximately one-quarter of the remaining unfound target respondents was drawn. Those sampled were tracked "intensively" (in terms of enumerator time and travel expenses) for the remaining months, while those not sampled were no longer actively tracked. We re-weight those chosen for the "intensive" sample by their added importance to maintain the representativeness of the sample. The same two phase tracking approach was employed in Wave 2 (launched in late 2008). As a result, all figures reported here are "effective" tracking rates (ETR), calculated as a fraction of those found, or not found but searched for during intensive tracking, with weights adjusted properly. The effective tracking rate (ETR) is a function of the regular phase tracking rate (RTR) and intensive phase tracking rate (ITR) as follows:

(eqn. 1) $$\text{ETR} = \text{RTR} + (1 - \text{RTR})*\text{ITR}$$

This is closely related to the tracking approach employed in the Moving to Opportunity project (Kling *et al.* 2007, Orr *et al.* 2003).

Table 2, Panel A provides a summary of tracking rates in KLPS-2. Over 86% of respondents were located by the field team, with 82.5% surveyed while 3% were either deceased, refused to participate, or were found but were unable to be surveyed. These are very high tracking rates for any age group over a decade, and especially for a highly mobile group of adolescents and young adults, and they are on par with some of the best-known panel survey efforts in less developed countries, such as the Indonesia Family Life Survey (Thomas *et al*. 2001, 2010). To our knowledge, these are among the highest tracking rates among a young adult population in any African panel survey data

[6] After 12 months of tracking, 64% of the Wave 1 sample (2,404 pupils) had been successfully surveyed, refused, or had died. Among the remaining 1,341 respondents, for budgetary reasons a representative one quarter were "intensively" tracked. As expected, individuals found during the intensive phase were much more likely to be living outside of Busia, are somewhat older, and are also less likely to work in agriculture, see supplementary Appendix Table A2. Baird, Hamory and Miguel (2008) has a more detailed discussion of the KLPS tracking approach as well as its impact on several treatment effect estimates of interest.

collection effort carried out over a decade-long timeframe.[7] Reassuringly, tracking rates are nearly identical in the treatment and control groups.

We have information on where surveyed respondents were living at the time of KLPS-2 survey in 2007-09 (Table 2, Panel B); the locations of residence (for at least four consecutive months at any point during 1998-2009) are presented in the map in Appendix Figure A2. There is considerable migration out of Busia District, at nearly 30%, which once again is balanced between the treatment and control groups. Since the approximately 14% of individuals we did not find, and thus did not obtain residential information for, are plausibly even more likely to have moved out of the region, these figures almost certainly understate true out-migration rates. Nearly 8% of individuals had moved to neighboring districts, including just across the border into the Ugandan districts of Busia and Bugiri, while 22% of those with location information were living further afield, with most in Kenya's major cities of Nairobi, Mombasa or Kisumu. While there are some significant differences in the migration rates to Nairobi versus Mombasa across the treatment and control groups, they are relatively minor in magnitude.

We focus on the KLPS-2 data, rather than KLPS-1, in this paper since it was collected at a more relevant time point for us to assess adult life outcomes: the majority of sample respondents are adults by 2007-09 (with median age at 22 years as opposed to 18 in KLPS-1), have completed their schooling, many have married, and a growing share are engaging in wage employment or self-employment, as shown graphically in Figure 2. While most individuals' main economic occupation is farming, as expected in rural Kenya, 16% worked for wages in the last month and 24% at some point since 2007, while 11% were currently self-employed outside of farming (Table 2, Panel C). The rates of wage work and self-employment are nearly identical across the deworming treatment and control groups, as discussed further below. This pattern simplifies the interpretation of the deworming earnings impacts we estimate below, although they are somewhat surprising given the large deworming impacts we estimate on other labor market dimensions, including the large shifts across employment sectors among wage earners. The issue of selection into the wage earning subsample is critical for interpretation of the results, and we discuss it extensively below.

3. Deworming impacts on labor market outcomes

This section lays out the estimation and describes deworming impacts on labor outcomes.

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[7] Other successful recent longitudinal data collection efforts among African youth are described in Beegle *et al.* (2010) and Lam et al (2008). Pitt, Rosenzweig and Hassan (2011) document high tracking rates in Bangladesh.

3.1 Estimation strategy

The econometric approach relies on the PSDP's prospective experimental design, namely, the fact that the program provided individuals in treatment (Group 1 and 2) schools two to three additional years of deworming treatment. We also adopt the approach in Miguel and Kremer (2004) and estimate the cross-school externality effects of deworming. Exposure to spillovers is captured by the number of pupils attending deworming treatment schools within 6 kilometers; conditional on the total number of primary school pupils within 6 kilometers, the number of treatment pupils is also determined by the experimental design, generating credible estimates of local spillover impacts.

In the analysis below, the dependent variable is a labor market outcome (such as wage earnings), $Y_{ij,2007\text{-}09}$, for individual i from school j, as measured in the 2007-09 KLPS-2 survey:

(eqn. 2) $$Y_{ij,2007\text{-}09} = a + bT_j + X_{ij,0}'c + d_1N_j^T + d_2N_j + e_{ij,2007\text{-}09}$$

The labor market outcome is a function of the assigned deworming program treatment status of the individual's primary school (T_j), a vector $X_{ij,0}$ of baseline individual and school controls, the number of treatment school pupils (N_j^T) and the total number of primary school pupils within 6 km of the school (N_j), and a disturbance term $e_{ij,2007\text{-}09}$, which is clustered at the school level.[8] The $X_{ij,0}$ controls include school geographic and demographic characteristics used in the "list randomization", the student gender and grade characteristics used for stratification in drawing the KLPS sample, the pre-program average school test score to capture school academic quality, as well as controls for the month and wave of the interview.

The main coefficients of interest are b, which captures any gains accruing to deworming treatment schools, and d_1, which captures any spillover effects of treatment for nearby schools. Bruhn and McKenzie (2009) argue for including variables used in the randomization procedure as controls in the analysis, which we do, although as shown below, the coefficient estimates on the treatment indicator are robust to whether or not the baseline individual and school characteristics are included as regression controls, as expected given the baseline balance across the treatment and control groups. Results are also robust to accounting for the cross-school spillovers. In fact, accounting for externalities tends to increase the b coefficient estimate; in other words, a failure to account for the program treatment "contamination" generated by spillovers dampens the "naïve" difference between

[8] Miguel and Kremer (2004) separately estimate effects of the number of pupils between 0-3 km and 3-6 km. Since the analysis in the current paper does not generally find significant differences in externality impacts across these two ranges, we consider the 0-6 km range as a whole for simplicity. The externality results are unchanged if we focus on the proportion of local primary school pupils who were in treatment schools as the key spillover measure (i.e., N_j^T / N_j, results not shown). Several additional econometric issues related to estimating externalities are discussed in Miguel and Kremer (2004).

treatment and control groups (and also potentially leads the researcher to miss a second dimension of program gains, the spillovers themselves). Certain specifications explore heterogeneity by interacting individual demographic characteristics with the deworming treatment indicator.

3.2 Deworming Impacts on Labor Earnings, Hours and Wages

The distribution of wage earnings, as represented in kernel densities, is shifted sharply to the right in deworming treatment schools (Figure 3, panel A), a first piece of evidence that deworming improved labor market outcomes. Here and below we present real earnings measures that account for the higher prices found in the urban areas of Nairobi and Mombasa.[9] The distribution of hours worked for wages or in-kind (among those with at least some wage earnings) is also shifted to the right in the treatment group (panel B), with a noticeably larger share of treatment individuals working approximately full-time (roughly 40 hours per week) and fewer working part-time.

We next turn to the regression analysis described above to assess the magnitude and statistical significance of these effects in Table 3, and find that deworming treatment leads to higher earnings in: log transformations of earnings (columns 1-3) and linear specifications (columns 4-6); with and without regression controls; and when cross-school externalities are accounted for. In the specification without the local externality controls (column 2), the estimated impact is 18.7 log points (s.e. 7.6, significant at 95% confidence), or roughly 21 percent. In our preferred log specification with the full set of regression controls (column 3), the impact is 25.3 log points (standard error 9.3, 99% confidence), or approximately 29 percent, a large effect.

While the coefficient estimate on the local density of treatment pupils (in thousands) is not significant at traditional confidence levels (19.9 log points, s.e. 16.8), it reassuringly has the same sign as the main deworming treatment effect, and a substantial magnitude: an increase of one standard deviation in the local density of treatment school pupils (917 pupils), which Miguel and Kremer (2004) found led to large drops in worm infection rates, would boost labor earnings by roughly (917/1000)*(19.9 log points) = 18.2 log points, or 20 percent.

We also include an indicator for inclusion in the randomly chosen group of 2001 cost-sharing schools in all specifications; recall that cost-sharing was associated with much lower deworming take-up in 2001. Consistent with this drop, the point estimate on the cost-sharing indicator in the regression shown in Table 3, column 3 is negative and marginally significant at -15.9 log points (s.e., 8.8). This provides further evidence that deworming treatment is associated with higher earnings.

[9] We collected our own comparable price surveys in both rural western Kenya and in urban Nairobi during the administration of the KLPS-2 surveys, and base the urban price deflator on these data.

The main earnings result is almost unchanged to trimming the top 1% of earners, so the result is not driven by outliers (Table 4, Panel A). The earnings result is also robust to including a full set of gender-age fixed effects (estimate 0.270, s.e. 0.093, significant at 99% confidence), to including fixed effects for each of the "triplets" of Group 1, Group 2 and Group 3 schools from the list randomization, and considering cross-school cost-sharing externalities (not shown). The next set of results in Table 4 summarizes a wider set of labor market outcomes among wage earners, using our preferred specification with the full set of regression controls (as in columns 3 and 6 in Table 3). Log wages (computed as earnings per hour worked) rise 16.5 log points in the deworming treatment group, although the effect is not significant at traditional confidence levels (t-stat=1.4), and trimming the top 1% of wages leads to similar results (not shown).

Positive wage earnings impacts are similar in the larger group of individuals, 24% of the sample, who have worked for wages at any point since 2007, where we use their most recent monthly earnings if they are not currently working for wages. The mean impact on log earnings is 0.211 (s.e. 0.072), and there is once again suggestive evidence of positive externality effects (Table 4, Panel B).

Hours worked also increase in the deworming treatment group. Considering the full sample first, hours worked (in any occupation) increased by 1.76 hours (s.e. 0.97, Table 5) on a control group mean of 15.3 hours, a 12% increase in the full sample that is significant at 90% confidence. The increase in hours worked is even more pronounced among the 66.2% of the sample that worked at all in the last week, at 2.40 hours (s.e. 1.16). Note that equal proportions of treatment and control group individuals worked in the last week, with a small and insignificant difference of just 1.0 percentage points between the groups. Hours worked for wages or in-kind in particular increases substantially in the deworming treatment group by 5.2 hours (significant at 90% confidence), an increase of 12% on a base of 42.2 hours worked on average in the control group. There is also a large, positive and significant coefficient estimate on the term capturing local deworming treatment externalities, at 6.6 (s.e. 2.9). Some of these gains appears to be the direct result of improved health boosting individual work capacity among wage earners: days lost to poor health in the last month falls by a third, or 0.499 of a day (s.e. 0.235) in the treatment group.

We find no significant evidence that deworming earnings gains differ by gender (Table 6, column 1), individual age at baseline (column 2) or the local level of serious worm infections at baseline (column 3). The relatively weak worm infection interaction effect may be due to use of an average zonal-level baseline worm infection rate, rather than individual-level data (which was not collected at baseline for the control group for ethical reasons); using zonal averages is likely to introduce measurement error and attenuation bias. While there is no evidence of differential gender impacts on hours worked for wages (column 4), it is notable that the gain in work hours is somewhat higher among individuals who were initially younger at baseline (in grades 2-4), with an average gain of 10.4 (s.e. 4.9) hours worked, while among those initially in grades 5-7 the effect is positive but close to zero (column 5). The gains in hours worked are no higher in areas with higher worm infection rates at baseline (column 6).

3.3 Selection into Wage Earning

The degree of selection into the wage earner subsample is a key issue in assessing the validity of the earnings results. For example, estimates could be biased downward if deworming led some individuals with relatively low labor productivity to enter the wage earner sample. While there is no single ideal solution to this issue, we present several different types of evidence – including demonstrating that (i) there is no differential selection into wage earning subsamples, (ii) the observable characteristics of wage earners in the treatment and control groups are indistinguishable, (iii) there are significant impacts on certain labor market outcomes in the full sample, (iv) results are robust to a standard Heckman selection correction model, (v) and to restricting analysis to a subsample where labor market participation is substantially higher than average – all of which indicate that selection bias is unlikely to be driving our results.

Confirming the result in Table 2, we again find no evidence that deworming treatment individuals are more likely to be working for wages or in-kind in the last month (Table 4, Panel A, estimate -0.015, s.e. 0.018), making it less likely that differential selection is driving the results. There is similarly no differential selection into the subsample who have worked for wages at any point since 2007 by treatment group (Panel B, estimate 0.000, s.e. 0.021). While it remains possible that deworming led certain types of individuals to enter wage earning and others to leave while leaving the overall proportions unchanged, the lack of deworming impacts on the proportion of individuals working in both self-employed and agriculture as well makes this appear even less likely.

We further confirm that there is no differential selection into the wage earner sample by gender (Table 6, column 7) or age (column 8). There is some evidence of greater selection into the wage earner subsample among deworming treatment individuals in zones with high worm infection rates at baseline (column 9), but the coefficient is only marginally significant and its magnitude is quite small. To illustrate, a one standard deviation increase in the baseline local moderate-heavy worm infection rate is 0.2, and thus an increase of this magnitude leads to a (0.2) x (0.028) = 0.0056 increase in the likelihood that individuals are wage earners, a small 3.4 percent increase on the base of 0.166 in the control group. Baseline characteristics, including academic performance measures, are also indistinguishable across the treatment and control groups in the wage earner subsample (Appendix Table A1).

We next focus on the full sample in Table 4, Panel C (before turning to more detailed analysis of the self-employed and agriculture subsamples below). Total labor earnings are significantly higher in the treatment group at the 95th percentile in a quantile regression, and the same is true for other percentiles above the 90th (not shown). As noted above ,there is also a significant increase in total hours worked either for wages, self-employment or in agriculture of 1.76 hours (s.e. 0.97, Table 5), a 12% increase in the full sample that is significant at 90% confidence.

The widely used Heckman (1979) approach explicitly models the process of selection into wage earning. We use a marital status indicator and marital status interacted with gender as variables that predict selection into earning but are excluded from the earnings regression; marital status is strongly positively (negatively) correlated with any wage earning among males (females), results not shown. This approach yields an almost unchanged estimated impact of deworming on log wage earnings in the last month, at 0.285 (s.e., 0.108), as shown in Table 4, Panel C, and similar impacts on the larger subsample that had earnings since 2007 (0.181, s.e. 0.078).

An additional approach that partially addresses selection concerns restricts the analysis to males in our sample, who have a much higher rate of participation in wage employment since 2007, at 32%, than females (15%), and thus for whom selection bias is potentially less severe. The estimated treatment effect in this subsample among those currently working for wages is 0.217 (s.e. 0.117), and among those working since 2007 is 0.196 (s.e. 0.101), with both effects statistically significant at 90% confidence.

3.4 Impacts on employment sector

The increased earnings in the deworming treatment group can largely be accounted for by pronounced shifts in the sector of employment, out of relatively low-skilled and low wage sectors into better paid sectors. We present the share of control group individuals working in each of the major employment sectors in the first column of Table 7, where the sectors presented taken together account for over ninety percent of the entire wage earning subsample. The largest sectors are services, accounting for 41.7% of the wage earner subsample, with domestic work and food services being the largest subsectors; agriculture and fishing (21.0%); retail (at 15.3%); trade contractors (9.2%); casual labor or construction labor (2.9%); manufacturing (2.9% overall and 5.7% among males); and wholesale trade (2.7%). We then present the deworming treatment effect and the estimated externality impacts in the next two columns, respectively, and in the final two columns present average earnings and hours worked in this sector in the control group.

The most striking impacts are a large increase in manufacturing work for deworming treatment individuals, with a point estimate of 0.072 (s.e. 0.024, Table 7), signifying a tripling of manufacturing employment overall. The gains among males are particularly pronounced at 0.090 (s.e. 0.030). The two most common types of manufacturing jobs in our sample are in food processing and textiles, with establishments ranging in size from small local corn flour mills up to large blanket factories. On the flip side, casual labor employment falls significantly (-0.038, s.e. 0.018), as does domestic service work for females (-0.174, s.e. 0.110), although this latter effect is only marginally significant. Local deworming spillover effects have a consistent sign in all of these cases, and are significant for domestic employment among females (-0.435, s.e. 0.180). Not surprisingly given these shifts, a somewhat larger proportion of treatment group wage earners live in urban areas.

Manufacturing jobs tend to be quite highly paid, with average real monthly earnings of 5,311 Shillings (roughly US$68), compared to casual labor (2,246 Shillings) and domestic services (3,047 Shillings). Manufacturing jobs are also characterized by somewhat longer work weeks than average at 53 hours per week. A decomposition along the lines of Oaxaca (1973) indicates that 97% of the increase in labor earnings for the treatment group, and 30% of the increase in hours worked, can be explained by the sectoral shifts documented in Table 7. While there are standard errors around these estimates and thus the exact figures should be taken with a grain of salt, they indicate that the bulk of the earnings gains are driven by sectoral shifts.

3.5 Impacts on self-employment and agricultural outcomes

As with wage earning, there is no evidence of differential selection into self-employment or own agricultural work among deworming treatment individuals (Table 8, Panels A and B), simplifying the interpretation of the estimated impacts in these subsamples. Unfortunately, reliable measures of productivity are much harder to generate among the self-employed and those working on their own farms relative to wage work, making it difficult to assess whether deworming had positive living standards impacts on these individuals. For instance, it is unclear how the self-employed are pricing their time (and the time of the family members and friends who assist them) when reporting their profits. Similarly, measuring the on-farm productivity of an individual worker in the context of a farm where multiple household members (and sometimes hired labor) are all contributing to different facets of the production process is notoriously difficult, and our survey instrument did not even attempt to disentangle individuals' separate contributions. As a result, we focus on a set of standard but imperfect proxies in this subsection.

Business outcomes improved considerably among the self-employed. The estimated deworming treatment effect on the profits of the self-employed (as directly reported in the survey) is positive (343 Shillings, s.e. 306, Panel A), although this 19% gain is not significant at traditional confidence levels, and there are similarly positive but not significant impacts on reported profits in the last year, on a profit measure based directly on revenues and expenses reported in the survey, as well as on the total number of employees hired (0.446, s.e. 0.361). The mean effect size of the three profit measures and the total employees hired taken together is positive, relatively large and statistically significant at 95% confidence at 0.175 (s.e., 0.089), where the magnitude is interpretable as 0.175 standard deviations of the normalized control group distribution, a sizeable effect.

There is also a large increase in hours worked among the self-employed, with a gain of 8.9 hours (s.e. 3.0) on a base of 33.9 hours worked on average in the control group, a 26% increase. There is also a large externality effect on self-employed hours worked (8.0, s.e. 3.0), further indication that deworming appears to boost work hours.

Among the majority of the total sample that continues to work primarily on their own farm (rather than for wages or as self-employed), there is no indication that deworming led to higher crop sales in the past year, greater hours worked in agriculture in the last week, or to higher adoption of "improved" agricultural practices including fertilizer, hybrid seeds or irrigation (Table 8, Panel B). The failure to find increased crop sales may, in part, be due to the fact that households are consuming more grain of the grain they produced, as suggested by the increase in meals eaten. Again, these results should be read with a grain of salt as we are unable to credibly measure individual on-farm productivity, but taken together, there are no clear impacts on agricultural outcomes we can measure.

3.6 Impacts on consumption

Household consumption expenditures are a standard tool for assessing living standards in rural areas of less developed countries, where much of the population engages in subsistence agriculture rather than wage work. One consumption measure, the number of meals consumed by the respondent yesterday, is narrower than total consumption expenditures but has the advantage that it was collected for the entire sample. Deworming treatment individuals consume 0.096 more meals (s.e. 0.028, significant at 99% confidence, Panel C) than the control group, and the externality impact is also large and positive (0.080, s.e. 0.023, 99% confidence). This suggests that deworming led to living standard gains in the full sample.

A standard LSMS-style consumption expenditure module was collected for roughly 5% of the KLPS-2 sample during 2007-09, for a total of 254 complete surveys. Such surveys are time-consuming and project budget constraints prevented us from collected a larger number of surveys. Note, too, that such data faces an important limitation in practice since consumption expenditures are best captured at the household level, and thus any productivity gains among KLPS respondents would be "diluted" if other household members do not experience similar gains, making them more difficult to detect in per capita household consumption measures. The data indicate that per capita average consumption levels in the control group are reasonable for rural Kenya, at US$580 (in exchange rate terms, Table 8, Panel C), and that food constitutes roughly 64% of total consumption. The estimated treatment effect for total consumption is near zero and not statistically significant at traditional confidence levels (-$14, s.e. $66), though it is worth noting that the confidence interval is quite large and includes large gains. The estimated deworming effect on a wealth measure, total household durable asset ownership, is also close to zero and not significant at traditional levels.

Thus taken together, there is some suggestive evidence that deworming improved living standards in the full sample as captured by meals eaten, with the important caveat that impacts on a broader consumption measure are unfortunately quite imprecisely estimated.

4. Deworming impacts on education and health

We first work through the comparative statics of a simple textbook model of health, educational investment and income to illustrate the channels through which deworming is likely to affect labor market outcomes (in subsection 4.1), and then estimate deworming impacts along educational (4.2) and health (4.3) dimensions.

4.1 Understanding the impact of health gains on educational investments and lifetime income

While many existing studies focus on educational attainment as the most likely channel linking child health gains to higher adult earnings, Bleakley (2010) rightly points out that standard models do not necessarily imply that education is the key mechanism. In this sub-section, we present a simple model related to Bleakley's to illustrate this point and generate further hypotheses.

We consider a model in which individuals choose how much education (denoted e below) to obtain to maximize discounted lifetime earnings, y, and examine how these schooling investments change as a function of child health (denoted h). The discounted future income benefits to schooling are b(e,h), and the costs (including both direct tuition costs and the opportunity cost of time spent in school rather than working) are c(e,h). Both the benefits and costs are increasing in education and health (b_e, b_h, c_e and c_h are all positive), but the marginal benefit of schooling declines with more education ($b_{ee} < 0$) while costs are convex ($c_{ee} > 0$). Both benefits and costs increase mechanically with health status if "non-sick" time increases, thus expanding the effective time budget. An individual's optimal educational investment level e^* is determined by the first order condition $y_e(e^*,h) = 0$, and equates marginal benefits to marginal costs, $b_e(e^*,h) = c_e(e^*,h)$.

The first relevant question for our analysis is how optimal educational investment levels change as child health improves. It is straightforward to show that:

(eqn. 3) $$\frac{de^*}{dh} = -\frac{b_{eh} - c_{eh}}{b_{ee} - c_{ee}}$$

By the usual assumptions above, the denominator is negative, but the numerator is more difficult to sign. Both derivatives are likely to be positive, in other words, improved child health boosts the marginal benefit of both school learning ($b_{eh} > 0$) and the opportunity cost of time (as labor productivity improves, $c_{eh} > 0$), but *a priori* there is no obvious sign on the difference. To the extent that the additional marginal benefits and costs are similar, there will be no change at all in the amount of schooling obtained, and it is even possible for schooling to fall after a positive health shock if the gains in current labor productivity outweigh the future gains from schooling. To the extent that the foregone earnings accruing to better health rise with age (i.e., good health is more relevant to the labor market success of an 18 year old than an 8 year old, whose current labor productivity is probably near zero regardless of his health status), we would expect optimal educational investments to respond most positively to improved health at younger ages.

We next derive the change in discounted lifetime income with respect to improved child health. There are two main channels, the direct labor benefits of better health (the first term in eqn. 4) and effects through education (the second term):

(eqn. 4) $$\frac{dy^*}{dh} = \left.\frac{\partial y}{\partial h}\right|_{e*} + \left.\frac{\partial y}{\partial e}\right|_{e*} \times \frac{de^*}{dh}$$

In an application of the envelope theorem, the change in lifetime income with respect to educational investment at optimal investment is zero, implying that the second term is zero. To the extent that individuals are making optimal educational investment choices, then, schooling gains will not be able to account for later income gains, and we certainly cannot use an exogenous change in health as an instrumental variable to identify the returns to schooling. Rather, it is the direct effects of health on adult productivity (for instance, if healthier people are stronger), and on other forms of human capital accumulation (for instance, more learning per unit of time spent in school, as captured by the test score, say, rather than school attainment per se), that drives any later income gains.

However, there are some conditions under which increased educational investment generated by child health gains might be a key channel, for instance, when educational investment choices are not being made optimally in the sense described above. While there are many reasons why $e \neq e^*$ is possible, a leading explanation is that child disease morbidity constrains educational investment below the optimal level. This is plausible in a setting like ours with high levels of baseline intestinal worm infection levels. Imagine a case in which children are simply too sick to attend school once every s days, and thus school attendance is 1/s lower than children would choose in the absence of poor health. If a health intervention like deworming reduced sickness-induced school absenteeism from 1/s to 1/s′, where s′ > s, it would allow children to get closer to their ideal educational investment level, yielding first-order welfare gains.[10] Miguel and Kremer (2004) found large school attendance gains among deworming treatment pupils, especially among younger children.

In assessing the welfare impacts of increased adult earnings, a further application of the envelope theorem would imply that these are best captured in wage (productivity) gains rather than in increased hours worked. However, this only holds if individuals with poor health are already at or near their optimal labor supply. To the extent that they are not, and better health improves the capacity to work longer hours, then the total gain in earnings (rather than just gains generated by higher wages per hour worked) is a more appropriate welfare metric; we return to this issue in section 5 below in our discussion of the returns to deworming investment.[11] The seminal model of health capital developed in Grossman (1972) argues that the fundamental difference between health capital and other forms of human capital, such as those created through education, is precisely the fact that

[10] Bleakley (2010) makes a similar observation about child school attendance gains. In the framework laid out above, this attendance effect is consistent with either the health investment allowing children to avoid some sickness-induced absenteeism, or with deworming shifting the marginal benefits of education more than the marginal costs ($b_{eh} > c_{eh}$). An alternative explanation for suboptimal educational investment could be agency problems or imperfect altruism within the household that leads parents to place too little weight on future child labor market gains from education. Note that in such a setting, improving child health (and thus labor productivity) today might instead boost current school drop-out rates.

[11] The relevant expression is $\frac{du^*}{dh} = \left.\frac{\partial u}{\partial h}\right|_{L^*} + \left.\frac{\partial u}{\partial L}\right|_{L^*} \times \frac{dL^*}{dh}$,

where L denotes hours worked and u is individual utility, in the context of a model where individuals face a labor-leisure trade-off. To the extent that individuals in poor health are working the optimal number of hours (L^*) then the second term equals zero, implying that increased hours worked should not be considered in assessing the welfare gains from better health, but this does not hold if poor health constrains labor supply below L^*.

better health status increases “the total amount of time [one] can spend producing money earnings and commodities” (p. 224). It is worth noting that the increases in adult hours worked and reduction in work days lost due to sickness (Table 5) among deworming treatment individuals, reported above are consistent with the view that healthier adults have greater work capacity and are thus better able to attain their ideal labor supply, leading to first-order welfare gains.

4.2 Impacts on education

We examine school enrollment and attendance using two different data sources in Table 9. In Panel A, the dependent variable is school enrollment as reported by the respondent in the KLPS-2 survey, which equals one if the individual was enrolled for at least part of a given year. These show consistently positive effects from 1999 to 2007 both on the deworming treatment indicator and the externalities term, and the total increase in school enrollment in treatment relative to control schools over the period is 0.279 years (s.e. 0.147, significant at 90% confidence). Note that there is no treatment effect estimate for 1998 since all students were enrolled at some point in 1998, as a criterion for inclusion in the KLPS sample. The treatment effect estimates are largest during 1999-2003 before tailing off during 2004-07, as predicted in the optimal educational investment framework above since the current opportunity cost of time is rising relative to the later benefits of schooling as individuals age.

The data in Panel B is school participation, namely, being found present in school by survey enumerators on the day of an unannounced school attendance check. This is our most objective measure of actual time spent at school, and was a main outcome measure in Miguel and Kremer (2004). The enrollment measure in Panel A misses much of the attendance variation captured in this measure. However, two important limitations of the school participation data are that it was only collected during 1998-2001, and only at primary schools in the study area; the falling sample size between 1998 to 2001 is mainly driven by students graduating from primary school. School participation rates also rise significantly in the deworming treatment group, by 0.074 (s.e. 0.023) and 0.068 (s.e. 0.023) in 1998 and 1999, respectively, before dropping off somewhat in later years (particularly in 2000). Total school participation gains are 0.129 of a year of schooling (s.e. 0.064, significant at 95% confidence). Given that the school enrollment data misses out on attendance impacts, which are sizeable, a plausible lower bound on the total increase in time spent in school induced by the deworming intervention is the 0.129 gain in school participation from 1998-2001 plus the school enrollment gains from 2002-2007, which works out to 0.304 years of schooling.[12]

Despite the sizeable gains in years of school enrollment, there are no significant impacts on either total grades of schooling completed (0.153, s.e. 0.143 – Table 10, Panel A) or attending at least some secondary school (0.032, s.e. 0.035), although note that both of these point estimates are positive. The likely explanation is that the increased years in school are accompanied by increased grade repetition (0.060, s.e. 0.017, significant at 99% confidence). To summarize, deworming treatment individuals attended school more and were enrolled for more years on average, but do not attain significantly more grades in part because repetition rates rise substantially.

Test score performance is another natural way to assess deworming impacts on human capital and skills. While the impact of deworming on primary school academic test score performance in 1999 is positive but not statistically significant (Table 10, Panel B), there is suggestive evidence that the passing rate did improve on the key primary school graduation exam, the Kenya Certificate of Primary Education (point estimate 0.046, s.e. 0.031). There is also some evidence that English vocabulary knowledge (collected during the 2007-09 survey) is somewhat higher in the deworming treatment group (impact of 0.076 standard deviations in a normalized distribution, s.e., 0.055). The mean effect size of the 1999 test score, the indicator for passing the primary school leaving exam, and the English vocabulary score in 2007-09 taken together does yield a normalized point estimate of 0.112 that is statistically significant at 90% confidence (s.e. 0.067), providing suggestive evidence of moderate human capital gains in the treatment group.

As expected, there is no effect on the Raven's Matrices cognitive exam, which is designed to capture general intelligence rather than acquired skills (Panel B). While many would argue that nutritional gains in the first few years of life could in fact generate improved cognitive functioning as captured in a Raven's exam – as Ozier (2010) indeed does find among younger siblings of the deworming beneficiaries – it was apparently already "too late" for such gains among the primary school age children in our study.

4.3 Impacts on health and nutrition

There is evidence that adult health also improved as a result of deworming. Respondent self-reported health (on a normalized 0 to 1 scale) improved by 0.041 (s.e. 0.018, significant at 95% confidence, Table 11, panel A). Many studies have found that self-reported health reliably predicts actual

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[12] The impacts of deworming on years enrolled in school are somewhat larger in the wage earner subsample, though we cannot reject the hypothesis that effects are the same for this subgroup as for the full sample (not shown).

morbidity and mortality even when other known health risk factors are accounted for (Idler and Benyamini 1997, Haddock *et al.* 2006, Brook *et al.* 1984). Note that it is somewhat difficult to interpret this impact causally since it may partially reflect health gains driven by the higher adult earnings detailed above, in addition to the direct health benefits of earlier deworming. Yet the fact that there were similar positive and statistically significant impacts on self-reported health in earlier periods, namely, in surveys administered in 1999 before most in sample individuals were working (see Table 11, panel C and Miguel and Kremer 2004), suggests that at least part of the effect is directly due to deworming.

In terms of other health outcomes, there is no evidence that deworming improved self-reported happiness or wellbeing or reduced major health shocks. Total health expenditures by the respondent in the last month are significantly higher in the treatment group (91.1 Shillings, s.e. 30.0), suggesting that they may have greater ability or willingness to make health investments, but interpretation is again complicated by the fact that such spending also reflects health needs. Despite the finding that the number of meals consumed is larger for deworming treatment individuals (in Table 8), deworming did not lead to higher body mass index (Table 11, Panel B). Nor are there detectable height gains, and these non-impacts hold even when we restrict attention to younger individuals (those in grades 2-4 in 1998, regression not shown).

It is difficult to disentangle the precise contributions of the education versus health gains we document in driving deworming's impact on labor market earnings, as the causal impacts on earnings of schooling attainment, other measures of skill (like our test of English vocabulary), self-reported health and our other measures are themselves not very well-understood, and interactions among these channels are also possible. We are able to show in the cross-section, however, that the education and health factors we focus on are correlated with higher earnings among the control group. For instance, a Mincerian regression indicates that the return to a year of schooling is between 6 to 12 log points (and highly significant, not shown), and both academic test scores and self-reported health are also associated with higher earnings. At a minimum, these associations establish as plausible the claim that the education and health channels that we focus on are contributing to higher earnings in the deworming treatment group.

The growing evidence that deworming improves immunological resistance to other infections, such as malaria (i.e., Kirwan *et al.* 2010), also implies that deworming might generate health benefits beyond those captured solely in anthropometric measures. Fortunately, we are able to assess the claim about malaria with the data from the original deworming program, in particular, a 1999 survey conducted among a representative subsample of pupils. To use our standard econometric specification, we focus on those who were also sampled for the KLPS follow-up, and find that self-reported malaria in the last week fell in the treatment group by 3.2 percentage points (s.e. 1.8, significant at 90% confidence, Panel C), with an externality effect that is similar in magnitude though not significant. This 3.2 percentage point effect is a large reduction of 14% given the self-reported malaria rate of 22.8 percentage points in the control group, suggesting that deworming led to broader childhood health benefits in the treatment group.

5. Assessing the Social Returns to Deworming as a Human Capital Investment

We next consider deworming as a human capital investment, comparing the benefits in terms of measured earnings gains versus the costs of treatment, and find very large positive returns, with a conservative benefit-cost ratio ranging from 24.7 to 41.6, and a conservative internal rate of return ranging from 17.7% to 22.1% per annum.

On the benefits side, we consider the earnings gains estimated (as in Table 3, column 3) over 40 years of an individual's work life. We assume that earnings first rise and then gradually fall over the life cycle in an inverted-U shaped manner, as documented by Knight, Sabot and Hovey (1992) for Kenyan labor markets, with earnings increasing proportionally in the deworming treatment group. We make several conservative assumptions. The most important is the fact that we only consider income gains when assessing welfare benefits. There may be a variety of benefits to child health gains that are not reflected in earnings, for instance, the utility gains that result from simply feeling better after worm infections are eliminated.

A second important assumption is that only the subset of wage earners (16% of the sample) will experience improved living standards as a result of deworming. We also ignore the fact that a growing proportion of individuals are likely to work for wages in the future as more of them enter the labor market (Figure 2), and disregard any gains in living standards experienced by non-wage earners, which is again conservative (given that the number of meals eaten rose in the full sample as well as the improved small business performance measures among the self-employed, for instance). In our main analysis we also ignore the suggestive evidence of positive externality gains to deworming treatment (accruing to those living near treatment schools) in terms of adult earnings, although we do discuss the magnitude of these benefits below. This analysis also ignores any broader community-wide benefits to deworming among those not of school age, for example, among the younger siblings of the treated.[13]

Under these assumptions, the average gain in total lifetime earnings (undiscounted) from deworming treatment is $1,258 (Table 12, Panel A). Note that the externality benefits to deworming treatment could also be very large (at $2,213 per 1000 additional treatment pupils within 6 km, which is roughly equivalent to an increase of one standard deviation in this density, result not shown), and would thus substantially boost the rates of return reported below.

We next derive an estimate of benefits only considering higher wages (earnings per hour), ignoring the greater number of hours worked by deworming treatment group individuals. As discussed above, the implicit assumption made when focusing only on wage gains in assessing welfare is that control group individuals are near their optimal labor supply level, and thus the greater hours worked by the treatment group will, to a first order approximation, have zero utility benefits. In contrast, if better health allows individuals to attain something closer to their optimal labor supply by reducing undesired illness-induced absenteeism and increasing work capacity, then the additional hours worked can legitimately be considered welfare gains. The true welfare gains thus probably lie in between the gains derived from focusing on total earnings versus wages alone. Focusing on wage gains alone, the lifetime benefits are $746.

There are three main social costs to deworming. The most obvious is the direct cost of deworming pill purchase and delivery through the schools. We use current estimates of per pupil mass treatment costs (provided by the NGO DewormTheWorld) of $0.59 per year. This cost incorporates the time of personnel needed to administer drugs through a mass school-based program, and accounts for the fraction of our sample that requires treatment with the more expensive drug for schistosomiasis (praziquantel). The total direct deworming cost then is the 2.41 years of additional deworming in the treatment group times $0.59, times the drug compliance rate in treatment schools, or $0.65 (Table 12, Panel B).

The second component is the opportunity cost of time spent in school rather than doing something else, presumably working. We calculate the maximum number of potential extra work days that children could gain (given the long school vacation periods in Kenyan schools), namely 185 days. We then compute the increased school participation (for 1998-2001, years where this data is available) and school enrollment (for 2002-2007) among treatment school individuals, at each individual age (in an analysis similar to Table 9); we disaggregate effects by child age since

[13] Ozier (2010) shows that children 0-3 years old when the deworming program was launched who had older siblings in treatment schools themselves show large nutritional and cognitive gains ten years later.

schooling gains are often concentrated among younger children. We then use recent data on the average unskilled agricultural wage in western Kenya (reported in Suri 2009), at $1.26 per day, as a benchmark. We assume that out-of-school children work approximately 20 more hours per week than those enrolled in school, which is conservative given recent time-use survey data from sub-Saharan Africa (Bardasi and Wodon 2006, Akabayashi and Psacharopoulos 1999). Finally, we make the assumption that foregone wage earnings would be zero for children at age 8 and would increase linearly up to 100% of the local unskilled wage for 18 year olds. This implies that, say, 13 year olds are roughly half as productive as adults per hour worked.

While some of these assumptions are difficult to validate given the well-known difficulties in measuring both agricultural and home productivity, and the particular rarity of such data for children, we feel these are likely to be conservative (and in any case, the returns to deworming remain large with even more conservative assumptions). The average per capita opportunity cost of time generated by deworming treatment under these assumptions is $23.29.

The third main social cost would be incurred if governments responded to the increased school participation induced by the deworming program by hiring more teachers, in order to maintain class sizes at the same average level as before the program. This would necessitate increasing the number of teachers by the same proportion as the rise in school participation. To compute these costs we employ official data on current Kenyan primary school and secondary school teacher salaries, and also assume that a deadweight loss of 20% would be incurred on the government revenue raised to fund this expansion (Auriol and Walters 2009).[14] The total social cost (including the deadweight loss) of hiring a sufficient number of teachers is thus $6.33 per treatment individual.

A graphical depiction of these various benefits and costs is presented in Figure 4. It is immediate that the undiscounted lifetime benefits of deworming far outweigh the costs, even when just considering the income gains that result from higher wages alone. The results are also presented in Table 12, Panel C, both for the total earnings case, where the ratio of benefits to costs is 41.6, and for wages alone it is 24.7.

These estimates ignore the externality benefits of deworming treatment among those located within 6 kilometers of treatment schools. While estimated externality effects (Table 3) are not significant at traditional confidence levels, if we take their magnitude seriously and consider productivity (wage) gains, the externality benefits of deworming alone outweigh the costs (of drug

[14] Busia District Education Office statistics indicate that average total primary school teacher annual compensation is $2,861 and average secondary school compensation is $4,060.

delivery and the opportunity cost of child time) by a ratio of 5.84:1. These gains might justify full public subsidies for deworming treatment.

We have so far focused on wage earners because their productivity gains are much more accurately measured than those working in self-employment or agriculture. If we were to abandon the assumption that earnings and wage gains were only experienced by those with wage earnings, and assumed that the full sample experienced analogous living standard gains, then the social benefit-cost ratio for deworming investment would be massive: 245.9 for the returns in terms of earnings and 145.9 in terms of wage productivity. While it is impossible for us to accurately assess just how much productivity did increase for those not working for wages given our data, the point here is that any living standards gains among the non-wage earning group would drastically increase the social returns to deworming investments.

An alternative approach to comparing the future benefits and costs of an investment is by calculating its internal rate of return (IRR). The IRR for deworming when we consider total earnings is 22.1% per annum, and it is 17.7% when we focus on wage productivity gains alone (Table 12, Panel D). Once again, these are quite high returns. The interpretation is that a social planner with an annual discount rate or cost of capital of less than 22.1% would choose to invest in deworming as a human capital investment. As a point of reference, at the time of writing, nominal commercial interest rates in Kenya are 10-12% per annum, the rate on long-term sovereign debt is 11% and inflation is 3% (according to the Central Bank of Kenya website).[15] Thus deworming appears to be an attractive investment given the real cost of capital in Kenya.[16]

6. Conclusion

We exploit an unusually useful setting for estimating the impact of child health gains on adult earnings and other life outcomes. The Kenya Primary School Deworming Program was experimentally phased-in across 75 rural schools between 1998 and 2001 in a region with high rates of intestinal worm infections, one of the world's most widespread diseases, especially among

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[15] This figure was obtained at: http://www.centralbank.go.ke/ (accessed November 1, 2010). Note that the analogous internal rate of return for the Indonesia primary school construction program studied in Duflo (2001) was 4 to 10%.

[16] A fuller social benefit-cost calculation would consider general equilibrium effects in the labor market of boosting productivity among younger cohorts, for instance, on the outcomes of older cohorts. The general equilibrium effects will depend on the degree and speed of aggregate physical capital accumulation in response to human capital gains (Duflo 2004), as well as the magnitude of any positive human capital spillovers across neighbors and coworkers (Moretti 2004, Mas and Moretti 2009). Duflo (2004) finds mixed impacts on the cohorts too old to have directly benefited from the 1970's school construction program in Indonesia, with positive gains in labor market participation but some moderate drops in wages among those working.

children in poor countries. As a result the treatment group exogenously received an average of two to three more years of deworming treatment than the control group. A representative subset of the sample was followed up for roughly a decade, through 2007-09 in the Kenya Life Panel Survey, with high survey tracking rates, and the labor market outcomes of the treatment and control groups are compared to assess impacts.

Among those working for wages, average adult earnings rise by approximately 21 to 29% as a result of deworming. These gains are accompanied by large increases in average hours worked (by 12%), a reduction in work days lost to sickness, and sharp shifts in employment towards high-paying manufacturing sector jobs (especially for males) and away from casual labor and domestic services employment (for females). The finding that shifts into different employment sectors account for the bulk of the earnings gains suggests that characteristics of the broader labor market – for instance, sufficient demand for manufacturing workers – may be critical for translating better health into higher living standards. While a simple model of optimal educational investment gives ambiguous predictions about the relative roles played by education versus health channels, there are significant deworming impacts on total years of school enrollment, test scores and self-reported health, suggesting that both may be important. The social returns to child deworming treatment are very high, with conservative estimates of the benefit-cost ratio ranging from 24.7 to 41.6.

These findings build on and complement Bleakley's work on historical deworming programs in the U.S. South in the early 20th century. It is remarkable that his estimated earnings gains in the U.S. South line up so closely with our findings: while Bleakley's (2007a) U.S. estimates imply that the treatment of worm infections at rates commonly found in Africa would raise earnings by 24%, we estimate gains of 21 to 29%. The correspondence between these two sets of results – using distinct research designs and data from different time periods – increases confidence in the external validity of both findings.

The main implication of this paper is that childhood health investments like school-based deworming can substantially boost adult earnings. It goes without saying that deworming alone, and its associated increase in earnings, cannot make more than a small dent in the large gap in living standards between poor African countries like Kenya and the world's rich countries. Yet that obvious point does not make deworming any less attractive as a public policy option given its extraordinarily high social rates of return, and the fact that boosting income by one quarter would have major welfare impacts for households living near subsistence.

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Table 1: Baseline (1998) summary statistics and PSDP randomization checks

	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)	Kolmogorov-Smirnov p-value
Female	0.470	0.469	0.473	-0.004	--
				(0.019)	
Grade (1998)	4.23	4.22	4.25	-0.03	0.450
	(1.68)	(1.70)	(1.66)	(0.05)	
Age (1998)	11.9	11.9	12.0	-0.04	0.258
	(2.6)	(2.6)	(2.6)	(0.11)	
Assignment to the deworming treatment group	0.678	1	0	--	--
Years of assigned deworming treatment, 1998-2003	3.31	4.09	1.68	2.41***	--
	(1.82)	(1.52)	(1.23)	(0.08)	
Primary school located in Budalangi division	0.370	0.364	0.381	-0.017	--
				(0.137)	
Population of primary school	476	494	436	58	0.307
	(214)	(237)	(146)	(54)	
School average test score (1996)	0.019	0.024	0.038	-0.013	0.310
	(0.427)	(0.436)	(0.406)	(0.109)	
Total treatment (Group 1, 2) primary school students within 6 km	3,180	3,085	3,381	-296	0.206
	(917)	(845)	(1,022)	(260)	
Total primary school students within 6 km	4,709	4,698	4,732	-34	0.119
	(1,337)	(1,220)	(1,555)	(389)	

Notes: The data are from the PSDP, and includes all individuals surveyed in the KLPS2. There are 5,084 observations for all variables, except for Age (1998) where there are 5,072 observations due to missing survey data. All observations are weighted to maintain initial population proportions. All variables are 1998 values unless otherwise noted. Years of assigned deworming treatment is calculated using the treatment group of the respondent's school and their grade, but is not adjusted for the treatment ineligibility of females over age 13 or assignment to cost-sharing in 2001. Those individuals who "age out" of primary school are no longer considered assigned to deworming treatment. The average school test score is from the 1996 Busia District mock exam, and has been converted to units of normalized individual standard deviations. The "Treatment – Control" differences are derived from a linear regression of the outcome on a constant and the treatment indicator, but results are similar if we include further controls (for survey wave, 1998 administrative zone of residence, cost sharing school indicator, and baseline 1998 population of the individual's primary school). Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. The Kolmogorov-Smirnov p-values are only presented for the non-binary variables, where it is informative.

Table 2: Attrition and residential location patterns, KLPS2 (2007-09)

	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)
Panel A: Sample attrition, KLPS2 I-module				
Found[a]	0.862	0.860	0.867	-0.007
				(0.017)
Surveyed	0.825	0.824	0.827	-0.003
				(0.018)
Not surveyed, dead	0.017	0.018	0.014	0.004
				(0.004)
Not surveyed, refused	0.015	0.014	0.017	-0.003
				(0.005)
Panel B: Residential location information				
Have residential location information (2007-09)	0.824	0.823	0.826	-0.003
				(0.018)
Among those with residential location information:				
Residence in Busia district	0.705	0.708	0.700	0.007
				(0.022)
Residence in districts neighboring Busia district[b]	0.078	0.082	0.069	0.013
				(0.011)
Residence outside of Busia and neighboring districts	0.217	0.210	0.230	-0.020
				(0.020)
In Nairobi	0.102	0.093	0.120	-0.027*
				(0.014)
In Mombasa	0.037	0.043	0.024	0.019**
				(0.008)
In Kisumu	0.018	0.018	0.017	0.002
				(0.006)
Residence outside of Kenya	0.052	0.056	0.043	0.012
				(0.010)
Panel C: Employment patterns				
Worked for wages or in-kind in last month[c]	0.158	0.154	0.166	-0.013
				(0.016)
Self-employed in the last month[d]	0.107	0.110	0.100	0.010
				(0.013)
Worked in agriculture in the last week[e]	0.519	0.513	0.531	-0.018
				(0.023)

Notes: The sample used in Panel A and for the variable "have residential location information" includes all individuals surveyed, found deceased, refused participation, found but unable to survey, and not found but sought in intensive tracking during KLPS2, a total of 5,569 individuals (3,686 treatment and 1,883 control). The remainder of Panels B and C include all individuals surveyed in the KLPS2. All observations are weighted to maintain initial population proportions. The "Treatment – Control" differences are derived from a linear regression of the outcome on a treatment indicator, but results are similar if we include further controls (for survey wave, 1998 administrative zone of residence, cost sharing school indicator, and baseline 1998 population of the individual's primary school). Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. The Kolmogorov-Smirnov p-values are not presented since all variables in this table are binary variables.

[a] The proportion "Found" is the combination of pupils surveyed, found deceased, refused and found but unable to survey. [b] Districts neighboring Busia include Siaya, Busia (Uganda), and other districts in Kenya's Western Province. [c] Employment includes only those who earned a positive salary or payment in kind. [d] Self-employment includes only those who earned positive profits, and excludes household farming activities. [e] Agriculture includes both farming and pastoralist activities.

Table 3: Deworming impacts on labor earnings (2007-2009)

	Dependent variable:					
	Ln(Total labor earnings, past month)			Total labor earnings, past month (in Kenya Shillings)		
	(1)	(2)	(3)	(4)	(5)	(6)
Deworming Treatment indicator	0.193**	0.187**	0.253***	611**	627**	780*
	(0.077)	(0.076)	(0.093)	(285)	(306)	(417)
Deworming Treatment pupils within 6 km (in '000s), demeaned			0.199			451
			(0.168)			(740)
Total pupils within 6 km (in '000s), demeaned			-0.098			-201
			(0.127)			(575)
Cost sharing school (in 2001)	-0.104	-0.139	-0.159*	-390	-540	-584
	(0.085)	(0.094)	(0.088)	(370)	(425)	(410)
Additional controls	No	Yes	Yes	No	Yes	Yes
R^2	0.064	0.176	0.182	0.060	0.125	0.126
Observations	710	710	710	710	710	710
Mean (s.d.) in the control group	7.86	7.86	7.86	3,739	3,739	3,739
	(0.88)	(0.88)	(0.88)	(3,744)	(3,744)	(3,744)

Notes: The sample used here includes all individuals surveyed in the KLPS2 who report positive labor earnings at the time of survey. Labor earnings include cash and in-kind, and are deflated to reflect price differences between rural and urban areas. All observations are weighted to maintain initial population proportions. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, and survey wave and month of interview. Additional controls include a female indicator variable, baseline 1998 school grade fixed effects, and the average school test score on the 1996 Busia District mock exams. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 4: Deworming impacts on labor earnings and wages

Dependent variable	Control group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming Treatment indicator	Coefficient estimate (s.e.) on deworming Treatment pupils within 6 km (in '000s), demeaned	Obs.
Panel A: Wage earner subsample				
Ln(Total labor earnings, past month)	7.86	0.253***	0.199	710
	(0.88)	(0.093)	(0.168)	
Ln(Total labor earnings, past month) – top 1% trimmed	7.83	0.269***	0.237	698
	(0.85)	(0.092)	(0.161)	
Ln(Total labor earnings, past month) – with all gender-age fixed effects	7.86	0.270***	0.197	710
	(0.88)	(0.093)	(0.159)	
Ln(Wage = Total labor earnings / hours, past month)	2.82	0.165	0.012	625
	(0.96)	(0.117)	(0.160)	
Indicator for worked for wages or in-kind in last month	0.166	-0.015	-0.002	5,081
	(0.372)	(0.018)	(0.020)	
Panel B: Wage earner since 2007 subsample				
Ln(Total labor earnings, most recent month worked)	7.88	0.211***	0.170	1,175
	(0.91)	(0.072)	(0.116)	
Indicator for worked for wages or in-kind since 2007	0.244	0.000	0.040	5,081
	(0.430)	(0.021)	(0.024)	
Panel C: Full sample				
Ln(Total labor earnings, past month) – Heckman selection correction	7.86	0.285***	0.148	5,082
	(0.88)	(0.108)	(0.170)	
Ln(Total labor earnings, most recent month worked) – Heckman selection correction	7.88	0.181**	0.006	5,082
	(0.91)	(0.078)	(0.120)	
Total labor earnings, past month, earnings=0 for non- earners	619	27	-17	5,084
	(2,060)	(81)	(97)	
Total labor earnings, past month – 95th percentile (quantile regression), earnings=0 for non-earners	619	290**	123	5,084
	(2,060)	(117)	(140)	

Notes: Each row is from a separate OLS regression analogous to Table 3, column 3, except the quantile regression in Panel C. Ln(Wage) adjusts for the different reporting periods for earnings (month) and hours (week), and is missing for those with zero earnings. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 5: Deworming impacts on hours worked

Dependent variable	Control group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming Treatment indicator	Coefficient estimate (s.e.) on deworming Treatment pupils within 6 km (in '000s), demeaned	Obs.
Hours worked (for wages, self-employed, agriculture) in last week	15.2	1.76*	1.54	5,084
	(21.9)	(0.97)	(1.16)	
Hours worked (for wages, self-employed, agriculture) in last week, among those with hours worked > 0	23.0	2.40**	2.75**	3,514
	(23.4)	(1.16)	(1.36)	
Indicator for hours worked > 0 (for wages, self-employed, agriculture) in last week	0.662	0.010	-0.007	5,084
	(0.473)	(0.022)	(0.025)	
Hours worked (for wages or in-kind) in the last week	42.2	5.19*	6.60**	693
	(24.7)	(2.74)	(2.93)	
Days of work missed due to poor health, past month (negative binomial)	1.46	-0.499**	-0.337	718
	(2.99)	(0.235)	(0.305)	

Notes: Each row is from a separate OLS regression analogous to Table 3, column 3, except the negative binominal. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 6: Deworming impacts on labor market outcomes among subgroups

	Dependent variable:								
	Ln(Total labor earnings, past month)			Hours worked (for wages or in-kind) in the last week			Indicator for worked for wages or in-kind in last month		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Deworming Treatment indicator	0.219**	0.297*	0.255***	6.0*	10.4**	4.9*	0.002	-0.018	-0.016
	(0.103)	(0.152)	(0.092)	(3.2)	(4.9)	(2.7)	(0.024)	(0.019)	(0.018)
Female	-0.473***			9.7***			-0.128***		
	(0.140)			(3.6)			(0.022)		
Female * Treatment	0.121			-2.7			-0.035		
	(0.195)			(4.5)			(0.027)		
Grades 5-7 in 1998		0.497***			7.7			0.105***	
		(0.164)			(5.3)			(0.023)	
Grades 5-7 * Treatment		-0.069			-8.2			0.004	
		(0.186)			(6.0)			(0.028)	
Moderate-heavy worm infection rate at the zonal level (1998), demeaned			-0.048			-1.0			-0.035*
			(0.084)			(2.8)			(0.018)
Moderate-heavy infection rate * Treatment			0.071			-0.8			0.028*
			(0.078)			(2.4)			(0.015)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.183	0.170	0.183	0.111	0.113	0.112	0.093	0.085	0.094
Observations	710	710	710	693	693	693	5081	5081	5081
Mean (s.d.) in the control group	7.86	7.86	7.86	42.2	42.2	42.2	0.166	0.166	0.166
	(0.88)	(0.88)	(0.88)	(24.7)	(24.7)	(24.7)	(0.372)	(0.372)	(0.372)

Notes: The sample used in columns (1)-(6) includes all individuals surveyed in the KLPS2 who report positive labor earnings at the time of survey and include data for the relevant dependent variable. The sample used in columns (7)-(9) includes all surveyed individuals with non-missing information on wage employment. Labor earnings include cash and in-kind. All observations are weighted to maintain initial population proportions. Additional controls include a gender indicator, baseline grade fixed effects, geographic zone fixed effects, the mean pre-program school test score, baseline school population, cost-sharing school in 2001 indicator, survey wave indicator, and month of interview fixed effects, as well as both the total number of deworming treatment school pupils and the total number of primary school pupils within 6 km (in '000s), demeaned (coefficient estimates not shown). Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 7: Deworming impacts on employment sector and occupation

Employment sector:	Control group proportion	Coefficient estimate (s.e.) on deworming treatment indicator	Coefficient estimate (s.e.) on deworming treatment pupils within 6 km (in '000s), demeaned	Mean (s.d.) earnings in sector, past month (Kenya Shillings), control	Mean (s.d.) hours per week worked in sector, control group
Agriculture and fishing	0.210	-0.038	-0.152*	2,872	35
		(0.059)	(0.080)	(1,804)	(25)
Retail	0.153	-0.018	0.025	2,049	39
		(0.038)	(0.043)	(1,713)	(29)
Trade contractors	0.092	-0.005	0.060	3,172	27
		(0.028)	(0.004)	(2,170)	(22)
Manufacturing	0.029	0.072***	0.041	5,311	53
		(0.024)	(0.031)	(3,373)	(24)
Manufacturing – males only	0.057	0.090***	0.031	6,277	49
		(0.033)	(0.033)	(3,469)	(20)
Wholesale trade	0.027	0.023	0.022	4,727	44
		(0.029)	(0.035)	(3,963)	(14)
Services (all)	0.417	0.032	0.037	4,694	47
		(0.054)	(0.075)	(5,013)	(24)
Domestic	0.115	-0.012	-0.026	3,047	61
		(0.032)	(0.038)	(1,754)	(18)
Domestic – females only	0.335	-0.174	-0.435***	2,795	65
		(0.110)	(0.180)	(888)	(17)
Restaurants, cafes, etc.	0.060	-0.029	0.024	4,194	53
		(0.023)	(0.034)	(3,567)	(21)
Casual/Construction laborer	0.029	-0.038**	-0.020	2,246	51
		(0.018)	(0.017)	(1,576)	(31)
Other	0.030	-0.028*	-0.013	4,600	47
		(0.015)	(0.014)	(1,740)	(13)

Notes: The sample used here includes all individuals surveyed in the KLPS2 who report working for pay (with earnings greater than zero) at the time of the survey. Each row is from a separate OLS regression analogous to Table 3, column 3. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 8: Deworming impacts on other economic outcomes

Dependent variable	Control group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming Treatment indicator	Coefficient estimate (s.e.) on deworming Treatment pupils within 6 km (in '000s), demeaned	Obs.
Panel A: Self-employed profits, hours and employees				
Indicator for self-employed earnings in last month	0.100	0.015	0.004	5,083
	(0.300)	(0.012)	(0.011)	
Total self-employed profits (self-reported) past month (among those >0)	1,766	343	-151	585
	(2,619)	(306)	(320)	
Total self-employed profits (constructed) past month (among those >0)	1,535	1,211	2,088	595
	(6,524)	(1,091)	(1,886)	
Total self-employed profits (self-reported) past year (among those >0)	12,193	1,952	-1,753	566
	(17,346)	(2,286)	(2,590)	
Total employees hired (excluding self), among the self-employed	0.188	0.446	0.044	633
	(0.624)	(0.361)	(0.492)	
Mean effect size (three profits measures, and total employees hired)	0.000	0.175**	0.014	555
	(1.000)	(0.089)	(0.097)	
Total self-employed hours last week (among those >0)	33.9	8.9***	8.0***	583
	(25.7)	(3.0)	(3.0)	
Panel B: Agricultural work, sales, hours and practices				
Indicator for respondent did agricultural work in last week	0.531	-0.010	0.005	5,080
	(0.499)	(0.025)	(0.031)	
Total value (KSh) of crop sales past year (if farm household)	576	-81	-460**	3,758
	(2458)	(148)	(206)	
Hours worked in agriculture last week (among those > 0)	9.5	0.48	-0.75	2,829
	(9.1)	(0.53)	(0.48)	
Uses "improved" agricultural practice (if farming household)	0.310	0.032	0.005	3,766
	(0.462)	(0.026)	(0.024)	
Panel C: Consumption				
Number of meals eaten yesterday	2.16	0.096***	0.080***	5,083
	(0.64)	(0.028)	(0.023)	
Household consumption expenditures per capita (2009 $US)	580	-14	33	249
	(400)	(66)	(76)	
Value of household durable assets (2009 $US)	211	-2	-7	5,034
	(202)	(11)	(10)	

Notes: Each row is from a separate OLS regression analogous to Table 3, column 3. "Agricultural work" includes both farming and pastoral activities. The average of "typical monthly" and last week recall is used for household consumption. The consumption expenditure and household durable assets results trim the top 2 and 1% of households, respectively. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 9: Impacts on school enrollment and participation

Panel A: Dep. var.: School enrollment indicator	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	Total
Deworming Treatment indicator	N/A	0.021*	0.036**	0.047**	0.046**	0.046*	0.028	0.035	0.017	0.003	0.279*
		(0.011)	(0.016)	(0.019)	(0.021)	(0.022)	(0.026)	(0.027)	(0.027)	(0.027)	(0.147)
Deworming Treatment pupils within 6 km	N/A	0.011	0.014	0.024	0.026	0.015	0.008	0.016	0.034	-0.011	0.138
(in ‘000s), demeaned		(0.013)	(0.015)	(0.017)	(0.018)	(0.025)	(0.027)	(0.027)	(0.029)	(0.031)	(0.149)
Mean in the control group		0.924	0.834	0.757	0.696	0.653	0.584	0.474	0.426	0.342	6.690
Observations		5,037	5,037	5,037	5,037	5,037	5,037	5,037	5,037	5,037	5,037
Panel B: Dep. var.: Primary school participation											
Deworming Treatment indicator	0.074***	0.068***	0.013	0.057**	N/A	N/A	N/A	N/A	N/A	N/A	0.129**
	(0.023)	(0.023)	(0.020)	(0.024)							(0.064)
Deworming Treatment pupils within 6 km	0.019	-0.008	-0.019	0.009							0.044
(in ‘000s), demeaned	(0.024)	(0.018)	(0.020)	(0.017)							(0.049)
Mean in the control group	0.839	0.709	0.686	0.586							2.513
Observations	4,900	4,821	4,342	3,831							5,037

Notes: The sample used in Panel A includes all individuals who were surveyed in KLPS2. The sample used in Panel B includes a subset of these individuals who additionally have school participation data from at least one of the years between 1998 and 2001. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, cost-sharing school in 2001 indicator, a gender indicator and pupil grade. The treatment indicator in 1998 is the Group 1 indicator. There is no estimated result for 1998 in Panel A since all individuals were enrolled in school in 1998 (as this was a study inclusion criterion). All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 10: Impacts on schooling and test score outcomes

Dependent variable	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming treatment indicator	Coefficient estimate (s.e.) on deworming Treatment school pupils within 6 km (in '000s), demeaned
Panel A: School participation, enrollment and attainment			
Total years enrolled in school, 1998-2007	6.69	0.279*	0.138
	(2.97)	(0.147)	(0.149)
Total primary school participation, 1998-2001	2.51	0.129***	0.056
	(1.12)	(0.064)	(0.048)
Grades of schooling attained	8.72	0.153	0.070
	(2.21)	(0.143)	(0.146)
Indicator for repetition of at least one grade (1998-2007)	0.672	0.060***	0.010
	(0.470)	(0.017)	(0.023)
Attended some secondary school	0.421	0.032	0.000
	(0.494)	(0.035)	(0.039)
Panel B: Test scores			
Took primary school leaving exam in grade 8 (KCPE or PLE)	0.700	0.019	0.007
	(0.460)	(0.023)	(0.025)
Academic test score (normalized across all subjects), 1999	0.026	0.059	0.158
	(1.000)	(0.090)	(0.101)
Passed primary school leaving exam	0.509	0.046	0.032
	(0.500)	(0.031)	(0.030)
English vocabulary test score (normalized), 2007-09	0.000	0.076	0.067
	(1.000)	(0.055)	(0.053)
Mean effect size (1999 test, passed primary school exam, 2007-09 English test)	0.000	0.112	0.068
	(1.000)	(0.067)*	(0.058)
Raven's Matrices cognitive test score (normalized), 2007-09	0.000	-0.011	0.055
	(1.000)	(0.048)	(0.042)

Notes: Each row is from a separate OLS regression analogous to Table 3, column 3. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence.

Table 11: Impacts on health, wellbeing and nutritional outcomes

Dependent variable	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming treatment indicator	Coefficient estimate (s.e.) on deworming Treatment school pupils within 6 km (in '000s), demeaned
Panel A: Health and wellbeing outcomes			
Self-reported health "very good", KLPS-2 (2007-09)	0.673	0.041**	0.028
	(0.469)	(0.018)	(0.022)
Self-reported currently "very happy"	0.673	0.020	0.028
	(0.469)	(0.018)	(0.023)
Index of wellbeing (0 to 1)	0.831	0.018	-0.013
	(0.290)	(0.012)	(0.012)
Respondent health expenditures (medicine, in/out-patient) in past month (KSh)	119.2	91.1***	40.7
	(389.9)	(30.0)	(55.9)
Panel B: Anthropometric outcomes			
Body mass index (BMI = Weight in kg / (height in m)^2)	27.2	0.024	0.064
	(1.3)	(0.044)	(0.053)
Height (cm)	167.3	-0.12	-0.39
	(8.0)	(0.26)	(0.33)
Panel C: Health outcomes in the 1999 pupil survey			
Malaria in the last week (self-reported), 1999	0.228	-0.032*	-0.027
	(0.420)	(0.018)	(0.023)
Falls sick often (self-reported), 1999	0.152	-0.032*	-0.004
	(0.359)	(0.019)	(0.023)

Notes: The sample includes all individuals surveyed in KLPS-2. Each row is from a separate OLS regression analogous to Table 3, column 3. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. The sample size in Panel C is 2,798, as a representative subset of pupils were surveyed for the 1999 pupil survey. Self perceived health "very good" takes on a value of one if the answer to the question "Would you describe your general health as somewhat good, very good, or not good?" is "very good", and zero otherwise. Self-reported currently "very happy" takes on a value of one if the answer to the question "Taking everything together, would you say you are somewhat happy, very happy or not happy?" is "very happy", and is zero otherwise. The underlying index of well being takes on a value of 0 to 4 where 4 implies answering no to all of the following four questions: "In the past week, have you felt tense, nervous or worried?" "In the past week have you generally not enjoyed your daily activities?" "In the past week have you felt more unhappy than usual?" "In the past week have you found it difficult to make decisions?" Indicator of no major health problem since 1998 takes on a value of one if they answer no to the following question "Have you experienced any major health problems that seriously affected your life or work, since 1998?", and is zero otherwise.

Table 12: Returns to child deworming investments

Panel A: Benefits (per pupil in the treatment group)	
Total lifetime earnings (over 40 years, no time discount), excluding externalities	$1,258
Lifetime earnings from wage productivity gains (over 40 years, no time discount), excluding externalities	$746
Panel B: Costs (per pupil in the treatment group)	
Deworming pill and delivery (2.41 additional years in treatment schools)	$0.65
Child opportunity cost of attending more school (as described in the text)	$23.29
Additional teacher wages (due to school participation increases)	$5.17
Deadweight loss of taxation (from raising revenue for teacher salaries)	$1.16
Panel C: Benefit-cost ratio	
Total lifetime earnings / All costs	41.6
Lifetime earnings from wage productivity gains / All costs	24.7
Panel D: Internal rate of return (per annum)	
Total lifetime earnings and all costs	22.1%
Lifetime earnings from wage productivity gains and all costs	17.7%

Notes: Calculated in the KLPS sample assuming only those in the wage earner subsample have earnings gains. The details of the construction of benefits and costs are in the text.

Figure 1: Project Timeline of the Primary School Deworming Program (PSDP) and the Kenya Life Panel Survey (KLPS)

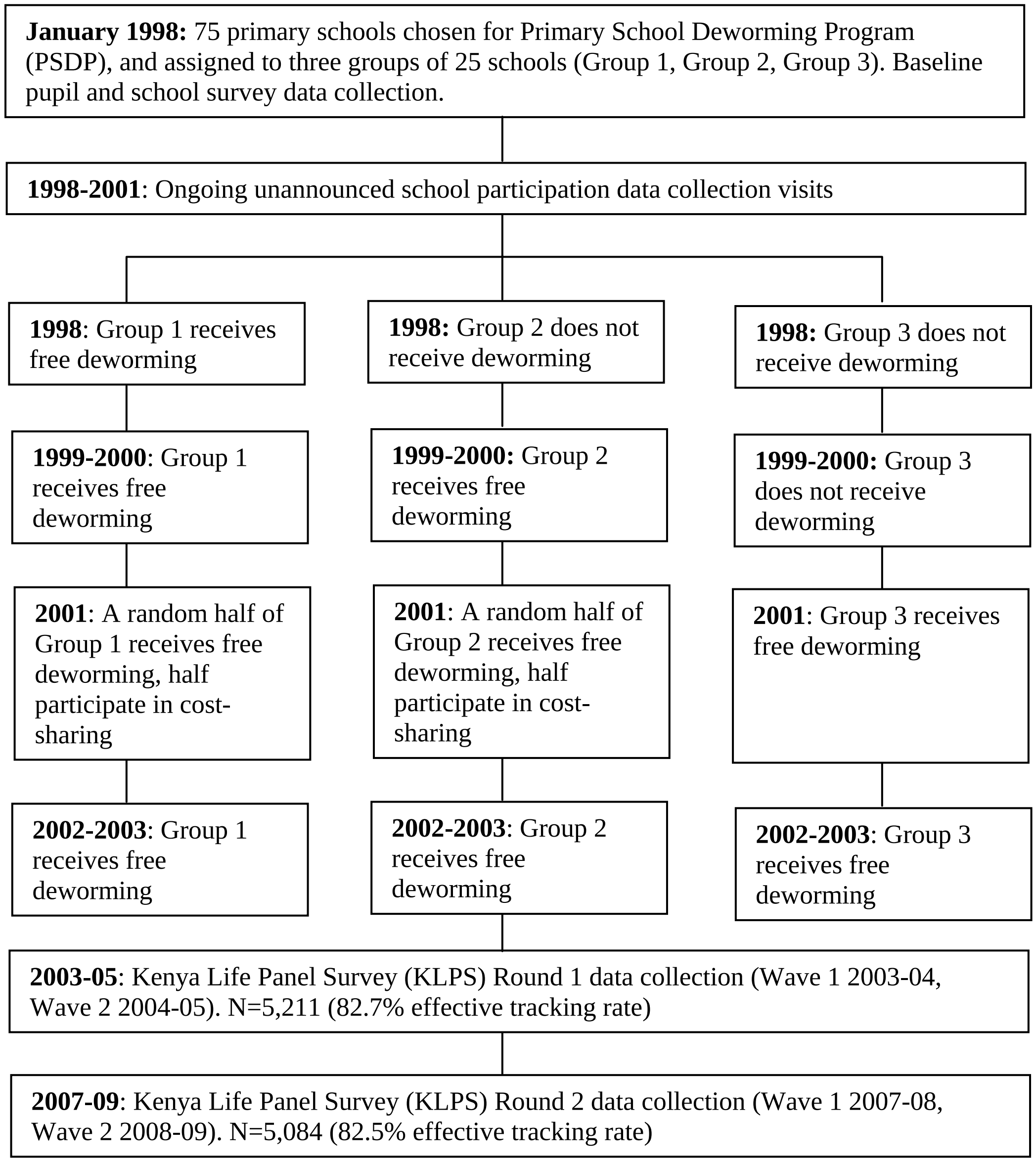
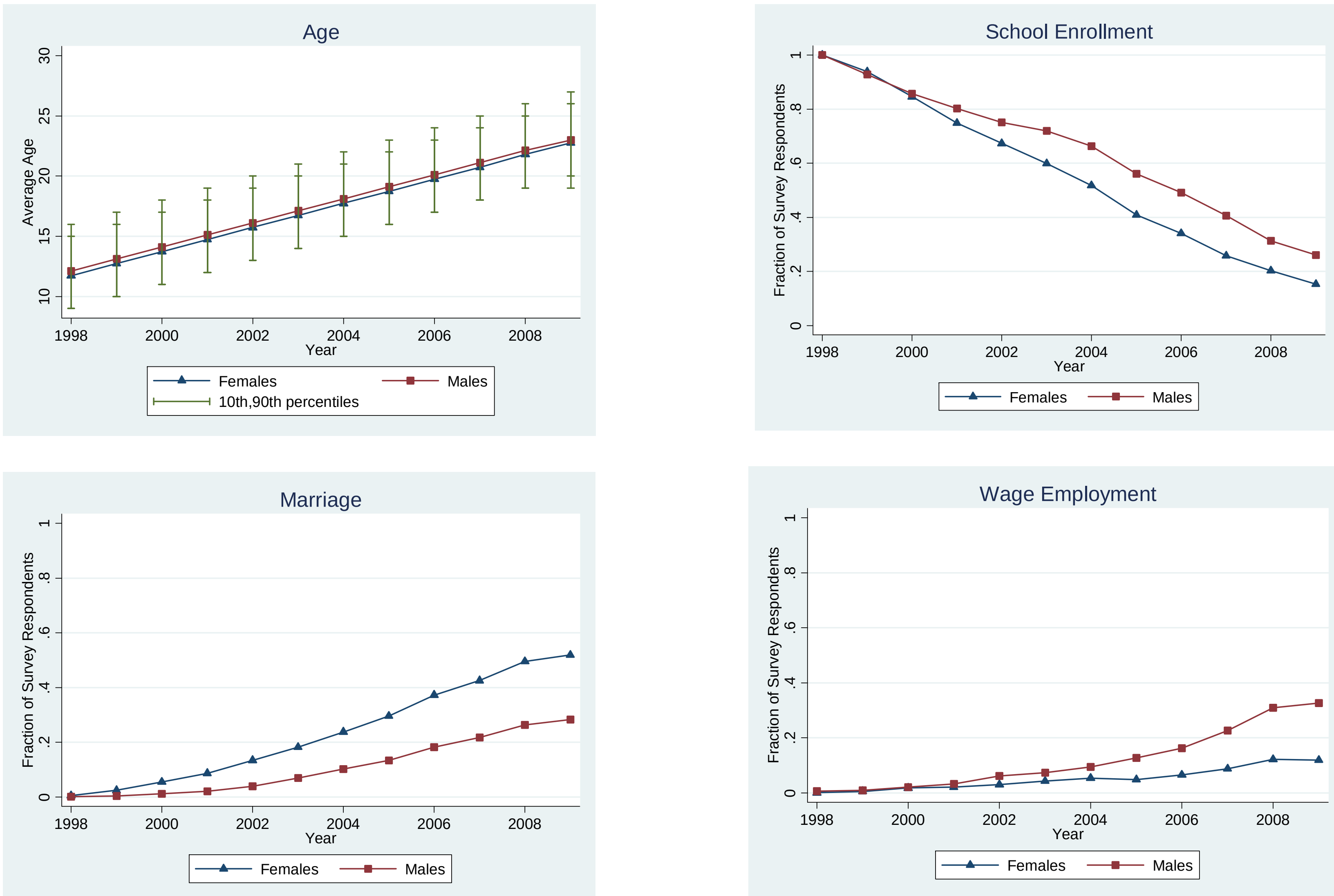


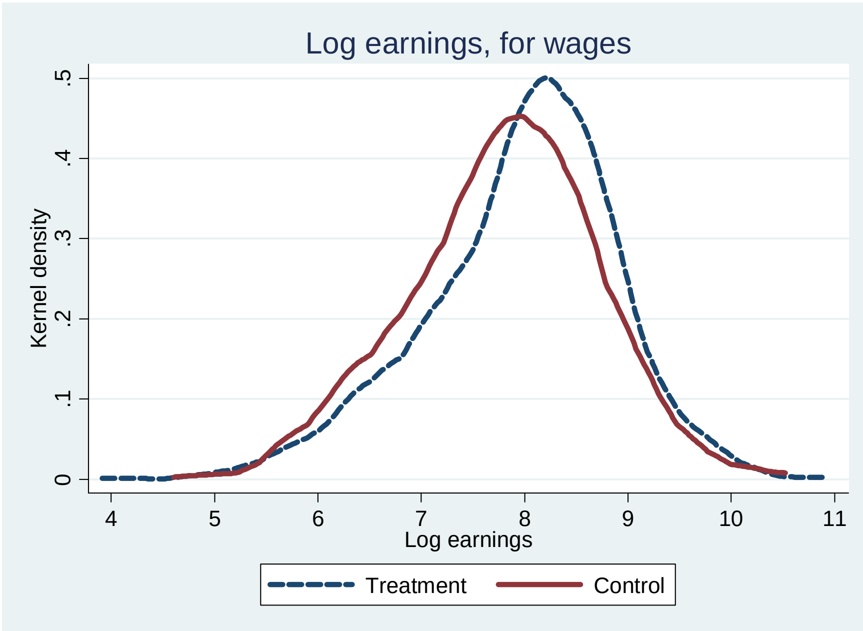
Figure 2: Age, School Enrollment, Marriage and Employment Patterns over 1998-2009



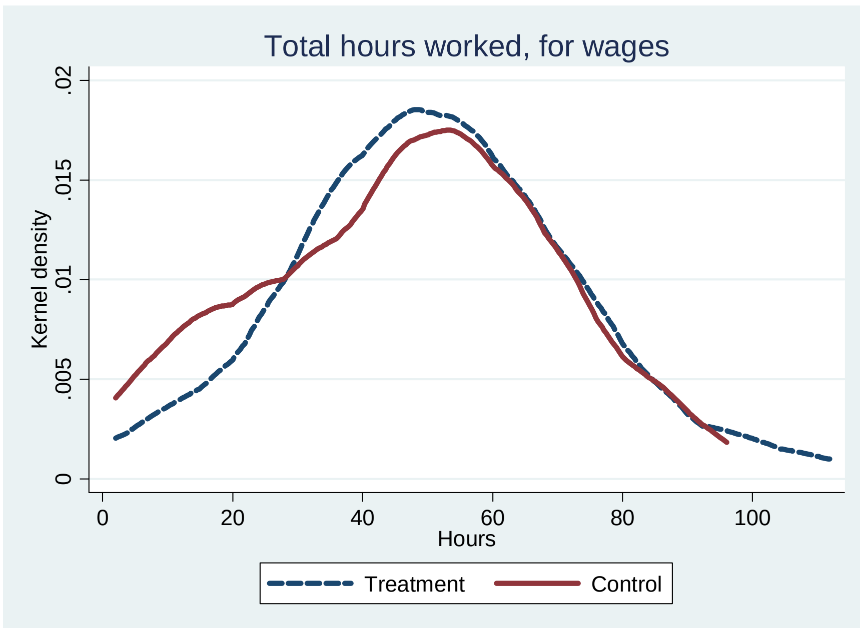
Notes: Observations are weighted to maintain initial population proportions.

Figure 3:

Panel A: The distribution of log labor earnings in the last month, deworming treatment versus control (among those with positive labor earnings)

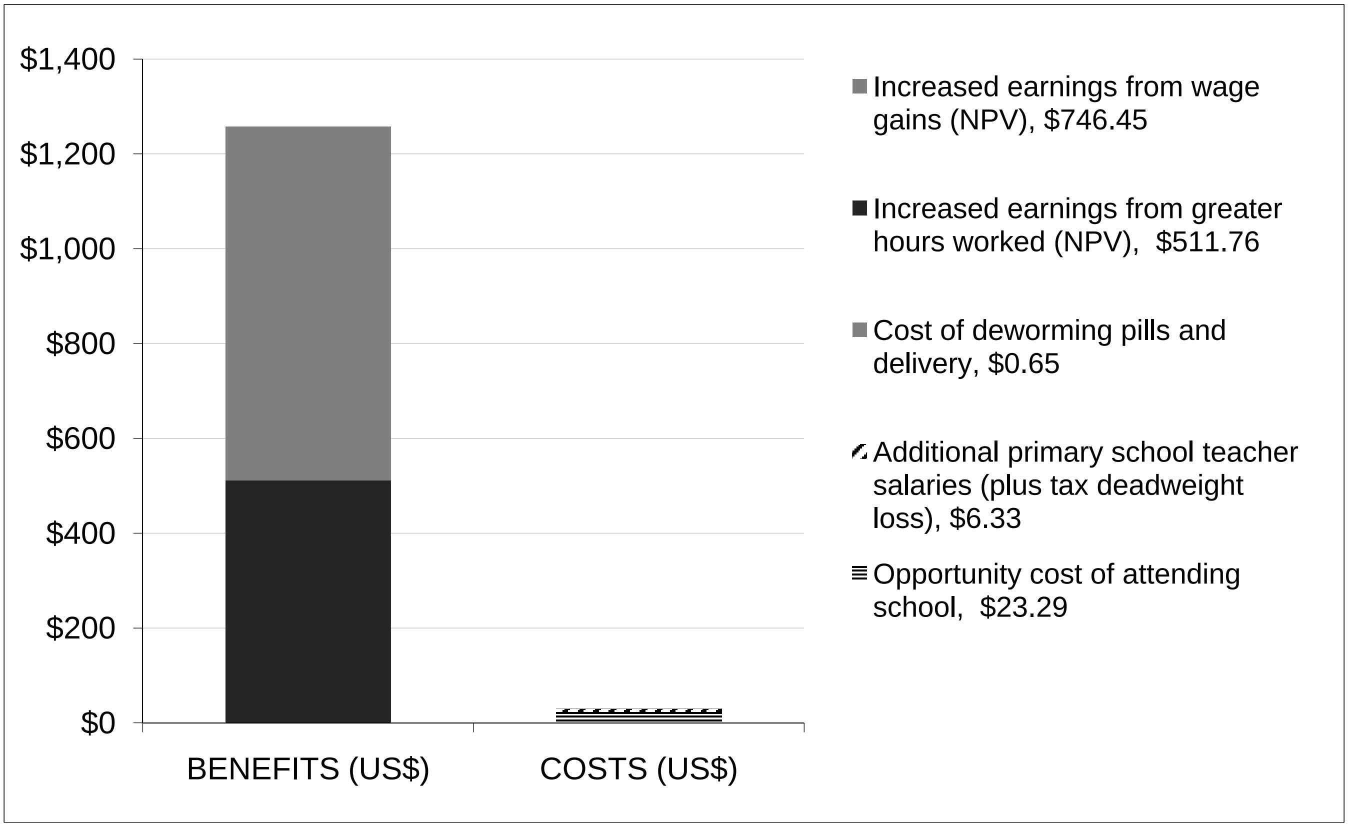


Panel B: The distribution of hours worked in the last week, deworming treatment versus control (among those working for wages)



Notes: The sample used here includes all individuals who were surveyed in KLPS-2 and reported working for wages or in-kind in the last month. All observations are weighted to maintain initial population proportions.

Figure 4: Labor market returns of childhood deworming treatment



Notes: These are the undiscounted sum of benefits and costs over the schooling years and 40 year working life of sample individuals.

Supplementary Appendix A: Research Design Appendix (not intended for publication)

A.1 Selection of Primary Schools for the PSDP Sample:
There were a total of 92 primary schools in the study area of Budalangi and Funyula divisions, across eight geographic zones, in January 1998. Seventy-five of these 92 schools were selected to participate in PSDP. The 17 excluded schools include: town schools that were quite different from other local schools in terms of student socioeconomic background; single-sex schools; a few schools located on islands in Lake Victoria (posing severe transportation difficulties); and those few schools that had in the past already received deworming and other health treatments under an earlier small-scale ICS (NGO) program.

In particular, four primary schools in Funyula Town were excluded due to large perceived income differences between their student populations and those in other local schools. In particular, Moody Awori Primary School, Namboboto Boys Primary School, and Namboboto Girls School charged schools fees well in excess of neighboring primary schools, and thus attracted the local "elite". Nangina Girls Primary School is a private boarding school, and charged even higher fees, and was similarly excluded.

Four other primary schools in Budalangi division were excluded from the sample due to geographic isolation, which introduced logistic difficulties and would have complicated deworming treatment and data collection. Three of these schools – Maduwa, Buluwani and Bubamba Primary Schools – are located on islands in Lake Victoria. The fourth, Osieko Primary School, is separated from the rest of Budalangi by a marshy area.

Two additional schools were excluded. Rugunga Primary School in Budalangi division served as the pilot school for the PSDP in late 1997, receiving deworming treatment before other local schools, and thus it was excluded from the evaluation. Finally, Mukonjo Primary School was excluded since it was a newly opened school in 1998 with few pupils in the upper standards (grades), and thus was not comparable to the other sample schools.

Seven schools had participated in the ICS Child Sponsorship Program/School Health Program (CSP/SHP). In 1998, it was felt that identification of treatment effects in these schools could be complicated by the past and ongoing activities in those schools, including health treatment (and deworming in particular), and hence they were excluded from the sample. The NGO's earlier criteria in selecting these particular seven schools (in 1994-1995) is not clear.

A.2 Prospective Experimental Procedure:
Miguel and Kremer (2004) contains a partial description of the prospective experimental "list randomization" procedure, and we expand on it here. Schools were first stratified by geographical area (division, then zone)[17], and the zones were listed alphabetically (within each division), and then within each zone they were listed in increasing order of student enrolment in the school. Table 1 shows there is no significant difference between average school populations in the treatment and control groups.

While the original plan had been to stratify by participation in other NGO programs, the actual randomization was not carried out this way. Schools participating in the intensive CSP/SHP program were dropped from the sample (as detailed above), while 27 primary schools with less intensive NGO programs were retained in the sample. These 27 schools were receiving assistance in the form of either free classroom textbooks, grants for school committees, or teacher training and bonuses. It is worth emphasizing that the randomized evaluations of these various interventions did

[17] There are two divisions (Budalangi and Funyula) containing a total of eight zones (Agenga/Nanguba, Bunyala Central, Bunyala North, Bunyala South, Bwiri, Funyula, Namboboto, Nambuku).

not find statistically significant average project impacts on a wide range of educational outcomes.[18] The schools that benefited from these previous programs were found in all eight geographic zones; the distribution of the 27 schools across the eight zones is: Agenga/Nanguba (5 schools), Bunyala Central (1), Bunyala North (4), Bunyala South (2), Bwiri (4), Funyula (5), Namboboto (1), Nambuku (5). The results in the current paper are robust to including controls for inclusion in these other NGO programs (results not shown).

The schools were "stacked" as follows. Schools were divided by geographic division, then zone (alphabetically), and then listed according to school enrolment (as of February 1997, for grades 3 through 8) in ascending order. If there were, say, four schools in a zone, they would be listed according to school enrolment in ascending order, then they would be assigned consecutively to Group 1; Group 2; Group 3; Group 1. Then moving onto the next zone, the first school in that stratum was assigned to Group 2, the next school to Group 3, and so on. Thus the group assignment "starting value" within each stratum was largely arbitrary, except for the alphabetically first zone (in the first division), which assigned the school with the lowest enrolment in its geographic zone to Group 1. Finally, there were three primary schools (Runyu, Nangina Mixed, and Kabwodo) nearly excluded from the original stacking of 72 schools that were added back into the sample for the original randomization, to bring the sample up to 75. These schools were originally excluded for similar reasons as listed above – e.g., Runyu is rather geographically isolated, and Nangina Mixed is a relatively high quality school located near Funyula Town. However, in the interests of boosting sample size, these three schools were included in the list randomization alphabetically as the "bottom" three schools in the list.

Deaton (2010) raises concerns about the list randomization approach, in the case where the first school listed in the first randomization "triplet" is different than other schools (in our case, it has lower than average school enrolment); the same concerns would apply to several other well-known recent field experiments in development economics, most notably Chattopadhyay and Duflo's 2004 paper "Women as policymakers: Evidence from a randomized policy experiment in India" in *Econometrica*. However, this is not a major threat to our empirical approach. Following Bruhn and McKenzie (2009) we include all variables used in the randomization procedure (such as baseline school enrolment) as explanatory variables in our regression specifications, thus controlling for any direct effect of school size, and partially controlling for unmeasured characteristics correlated with school size. Table 3 shows that the estimate on the deworming treatment indicator is unchanged whether or not additional explanatory variables are included, suggesting that any bias is likely to be very small. The difference in average school enrollment between the treatment and control groups is small and not statistically significant (Table 1). Moreover, even if the first school in the first randomization triplet were an outlier along some unobserved dimension (which seems unlikely), given our sample size of 75 schools and 25 randomization triplets, and the fact that school size is not systematically related to treatment group assignment for the other 24 randomization triplets (as discussed above), approximately 96% of any hypothesized bias would be eliminated. Taken together, the prospective experimental design we exploit in the current paper is likely to yield reliable causal inference.

[18] See Glewwe, Paul, Michael Kremer, and Sylvie Moulin. (2009). "Many Children Left Behind? Textbooks and Test Scores in Kenya", *American Economic Journal: Applied Economics*, 1(1): 112-135.

Supplementary Appendix Table A1: Baseline (1998) summary statistics and PSDP randomization checks, wage earner subsample

	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)	Kolmogorov-Smirnov p-value
Female	0.233	0.209	0.280	-0.071	--
	(0.423)	(0.407)	(0.450)	(0.045)	
Grade (1998)	4.87	4.86	4.91	-0.054	0.445
	(1.61)	(1.63)	(1.57)	(0.141)	
Age (1998)	13.2	13.2	13.0	0.204	0.202
	(1.8)	(1.9)	(1.7)	(0.391)	
Primary school located in Budalangi division	0.412	0.430	0.378	0.052	--
	(0.493)	(0.496)	(0.486)	(0.144)	
Population of primary school	477	504	425	78	0.342
	(218)	(246)	(136)	(56)	
School average test score (1996)	-0.010	-0.027	0.024	-0.050	0.273
	(0.408)	(0.415)	(0.391)	(0.106)	
Total treatment (Group 1, 2) primary school students within 6 km	3206	3115	3383	-267	0.172
	(908)	(802)	(1064)	(283)	
Total primary school students within 6 km	4731	4731	4730	1.72	0.342
	(1332)	(1173)	(1598)	(420)	

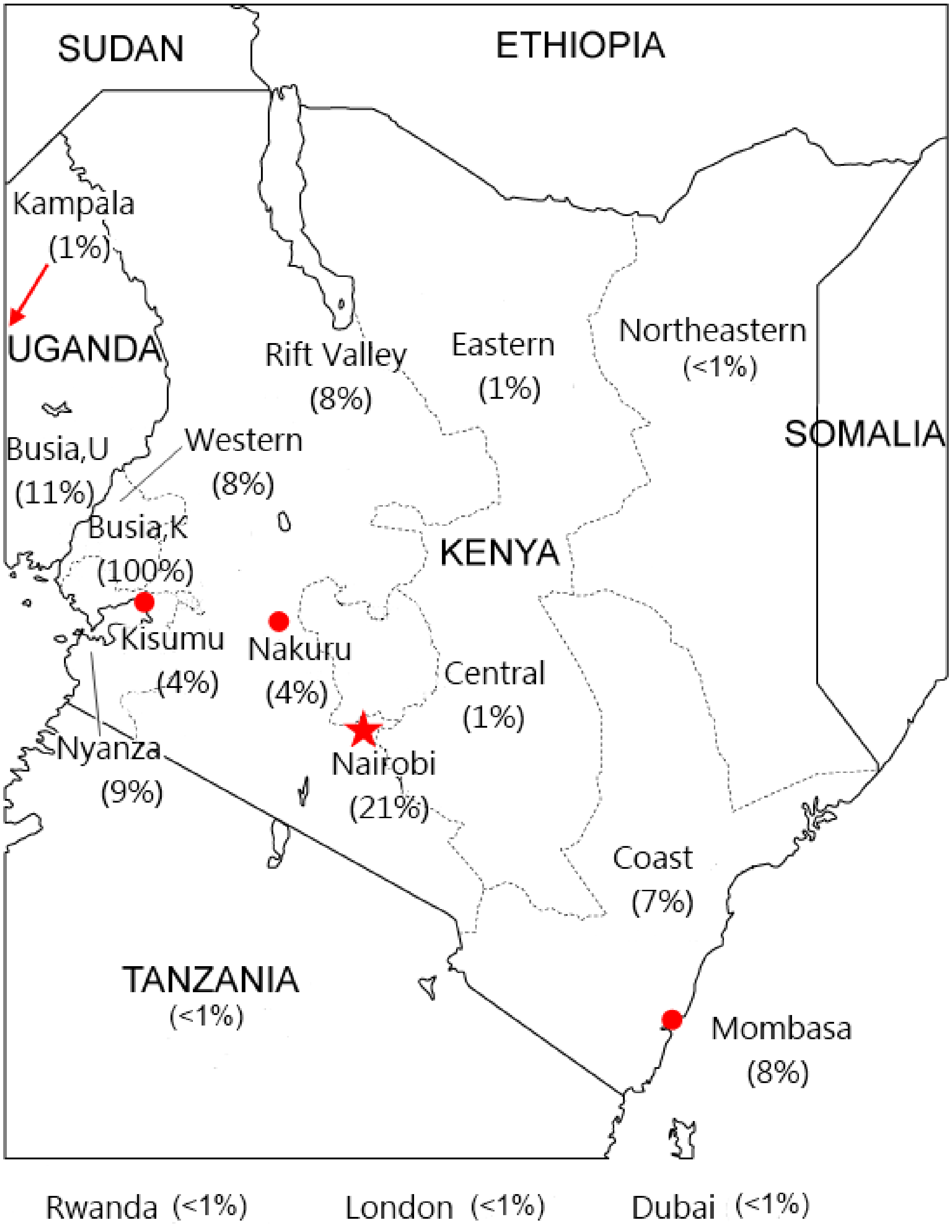
Notes: The data are from the PSDP, and includes all individuals surveyed in the KLPS2 who had worked for wages in the past month at the time of the interview. All observations are weighted to maintain initial population proportions. All variables are 1998 values unless otherwise noted. The average school test score is from the 1996 Busia District mock exam, and has been converted to units of normalized individual standard deviations. The "Treatment – Control" differences are derived from a linear regression of the outcome on a constant and the treatment indicator, but results are similar if we include further controls (for survey wave, 1998 administrative zone of residence, cost sharing school indicator, and baseline 1998 population of the individual's primary school). Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. The Kolmogorov-Smirnov p-values are only presented for the non-binary variables, where it is informative.

Supplementary Appendix Table A2: Baseline (1998) summary statistics and attrition checks

	Full KLPS Sample	Found: Regular Tracking	Found: Intensive Tracking	Not Found	Found (Regular and Intensive) – Not Found
Female	0.486	0.461	0.495	0.535	-0.072***
	(0.500)	(0.499)	(0.501)	(0.499)	(0.016)
Grade (1998)	4.26	4.24	4.24	4.32	-0.105
	(1.69)	(1.68)	(1.70)	(1.70)	(0.063)
Age (1998)	12.4	12.4	12.5	12.7	-0.37***
	(2.2)	(2.2)	(2.2)	(2.1)	(0.09)
Assignment to the deworming treatment group	0.675	0.681	0.665	0.664	0.006
	(0.468)	(0.466)	(0.473)	(0.472)	(0.020)
Group 1 school	0.357	0.355	0.354	0.362	-0.015
	(0.479)	(0.479)	(0.479)	(0.481)	(0.025)
Group 2 school	0.318	0.326	0.311	0.302	0.021
	(0.466)	(0.469)	(0.463)	(0.459)	(0.021)
Years of assigned deworming treatment during 1998-2003	3.29	3.32	3.25	3.22	0.069
	(1.83)	(1.82)	(1.83)	(1.85)	(0.090)
Primary school located in Budalangi division	0.380	0.361	0.389	0.420	-0.067***
	(0.486)	(0.480)	(0.488)	(0.494)	(0.023)
Population of primary school	484	480	465	496	-20**
	(221)	(223)	(178)	(222)	(8)
School average test score (1996)	0.043	0.035	0.023	0.066	-0.026
	(0.439)	(0.434)	(0.416)	(0.453)	(0.021)
Total treatment (Group 1 and 2) primary school students within 6 km	3171	3182	3174	3149	30
	(910)	(915)	(918)	(900)	(36)
Total primary school students within 6 km	4678	4713	4691	4602	93
	(1340)	(1342)	(1335)	(1334)	(62)
Number of observations [a]	7530	4891	421	2218	7530

Notes: The regression results (Found – Not Found) in column 5 reweights appropriately for intensive tracking. [a] The number of observations is correct except for the Age (1998) variable, which has somewhat more missing data.

Supplementary Appendix Figure A1: Migration residential location map



Notes: Percentages sum to greater than one, since they capture residential location (for at least four consecutive months) at any point during 1998-2009.