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

Sarah Baird
George Washington University

Joan Hamory Hicks
University of California, Berkeley CEGA

Michael Kremer
Harvard University and NBER

Edward Miguel
University of California, Berkeley and NBER

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We examine the impact of a child-health program on adult living standards by following participants in a deworming program in Kenya that began in 1998. The effective tracking rate was 83% over a decade. Treatment individuals received two to three more years of deworming than the comparison group. Self-reported health, years enrolled in school, and test scores improve significantly, and hours worked increase by 12% in the treatment group. These findings suggest that poor health may contribute to the relatively low labor supply previously documented among African workers. Treatment individuals report eating an average of 0.1 additional meals per day. Point estimates suggest there were substantial positive externalities among those living within 6 km of treatment schools, although significance levels vary due to large standard errors. Within the subsample working for wages, earnings are over 20% higher for the treatment group. Most of the earnings gains are explained by sectoral shifts, including a doubling of manufacturing employment. Small business performance also improves among the self-employed. A lower bound on the annualized social internal rate of return to deworming is large, at 83%. The externality benefits alone appear to justify fully subsidizing school-based deworming.

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1. Introduction

Many child public health measures – from immunization to water treatment, deworming and insecticide treated nets – have far from universal take-up in low-income countries and are not routinely provided for free by governments. There has been a lively debate between those who argue that governments should provide these goods for free, or even subsidize them, and those who argue that individuals should decide on their own whether to purchase these goods (Kremer and Miguel, 2007; Kremer and Holla, 2009; Ashraf, Berry, and Shapiro, 2010; Dupas 2011). A growing literature suggests that many people who will utilize these measures when they are free will not use them when they must pay. However, to understand whether public investments are worthwhile, it is also important to know the impact of these investments, both on the people who use the technologies and on others who may be affected by externalities from reduced transmission of infectious disease. After all, one view might be that low willingness-to-pay for these goods implies that people in poor countries have other priorities and that subsidies are not justified.

Advocates of public health spending in low-income countries often argue that, even setting aside the immediate utility benefits of improved health, such programs have high rates of return as investments because of their impact on adult living standards. Yet assessing the long-run causal impacts of public health measures has been problematic given the relative lack of both panel data sets tracking children into adulthood, and convincing causal identification from experimental variation.

We provide evidence from a prospective study on the impact of deworming of children in rural Kenyan primary schools on outcomes nearly a decade later, when most respondents were 19 to 26 years old. This analysis is based on a new longitudinal data set with an effective tracking survey rate of 83%. The combination of exogenous variation in child health investments with a long-term panel (longitudinal) 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 broader health benefits in regions with high tropical disease burdens. For instance, a recent double-blind placebo controlled randomized trial among Nigerian preschool children finds that children who received deworming treatment for 14 months showed 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). Chronic parasitic infections in childhood are known to generate inflammatory (immune defense) responses and elevated cortisol levels that lead substantial energy to be diverted from growth, and there is mounting evidence that this can produce adverse health consequences throughout the life course, including atherosclerosis, impaired intestinal transport of nutrients, organ damage, and cardiovascular disease (Crimmins and Finch 2005).

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 younger children in the treatment communities and especially the younger siblings of the treated (Ozier 2010).

In this paper, we first present a simple model (building on Bleakley 2010) to illustrate the conditions under which child health gains might affect educational investments and later income. We next find empirically that self-reported health improved, and years enrolled in school increased by approximately 0.3 years. Although we cannot decompose how much of our labor market impacts are working through health versus education without imposing strong assumptions, these patterns suggest that both channels are playing a role.

We next generate estimates of the average impact of deworming on long-run outcomes by comparing the program treatment and control groups during 2007 to 2009. Treatment individuals report eating 0.1 more meals per day, consistent with higher living standards. Hours worked increase by 12% and work days lost to illness fall by a third among wage earners. Labor supply also increases among those living within 6 km of deworming treatment schools, evidence of positive treatment externalities. The finding that better health appears to increase the capacity to work longer hours is consistent with 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.

This finding contributes to a debate on labor supply issues in less developed countries. Since the colonial era, some observers have claimed that average labor supply is surprisingly low in sub-Saharan Africa and other less developed regions – although it is worth noting that a lack of comparable cross-country data on hours worked, especially in the rural sector, makes it difficult to rigorously verify this claim. Colonial observers, true to form, resorted to racial or ethnic theories to explain what they saw as Africans' "laziness", love of leisure, and lack of ambition (see Abudu 1986 for a discussion of colonial accounts in West Africa). A growing body of work in labor economics emphasizes cultural (though not racial) differences across groups as key drivers of labor supply decisions (Fernandez and Fogli 2009). In contrast, Fafchamps (1993) argues that the low levels of labor supply he observed among peasant farmers in Burkina Faso are due not to culture, but to low marginal products of labor in the traditional rain-fed agricultural sector.

Our findings point to another explanation for low levels of labor supply in poor countries, namely the elevated disease burden in sub-Saharan Africa and other tropical regions. Our finding that those individuals who received health investments as children work significantly more hours as adults echoes existing evidence on the link between disease and work absenteeism in other African settings (Schultz and Tansel 1997), and is consistent with other findings that moderate increases in morbidity can have large impacts on labor supply. Ichino and Moretti (2009) show that workplace absenteeism peaks every 28 days for women under age 45 but not for women over age 45 or for men. They attribute this pattern in their Italian dataset to the menstrual cycle, and argue that it can explain up to two thirds of the male-female gap in absenteeism episodes, and 14% of the gender wage gap.

The changes in labor supply we document are accompanied by marked shifts in employment occupation in 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. Manufacturing jobs are among the most demanding in our dataset, with long average work weeks and little apparent tolerance for job absenteeism. It seems likely that the health gains produced by deworming helped more individuals secure and retain these high-paying jobs.

In one of our main findings, we find that earnings are over 20% higher in the deworming treatment group among those with wage employment. The changes in the occupation of employment noted above 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 some evidence of positive impacts. For instance, 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.

Deworming appears to have high social returns. A lower bound on the annualized social internal rate of return for deworming investments is high, at 83%. It is appropriate to consider total earnings and exclude the opportunity costs of child time in this calculation if better health improves schooling and work capacity by expanding the amount of non-sick time, The internal rate of return on deworming's externality gains alone are also high, accounting for most of the social benefits, and suggesting that the externalities alone justify fully subsidizing school-based deworming. The returns we present are lower bounds as they ignore non-earnings benefits of deworming, and capture only a subset of total spillovers from the intervention.

Our findings contribute to several other 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%, similar to our estimated earnings gains. Taken together, these findings lend credence to the view that treating intestinal worm infections can 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.

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%.[2] While many studies argue that early childhood health gains *in utero* or before age three have the largest impacts (World Bank 2006, Hodinott *et al.* 2008, Almond and Currie 2010 are but a few examples), our findings show that even health investments made in school aged children can have important effects.

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[1] There has been a lively debate in public health and nutrition about the cost-effectiveness of deworming (see Taylor-Robinson *et al.* 2007). Early work by Schapiro (1919) using a first-difference research design found wage gains of 15-27% on Costa Rican plantations after workers received deworming. Weisbrod et al (1973) document relatively weak cross-sectional correlations between worm infections and labor productivity, test scores, and fertility in St. Lucia. Bundy *et al*. (2009) argue that many existing studies understate deworming's benefits since they fail to consider externalities (thus understating true treatment gains) by using designs that randomize within schools; focus almost exclusively on biomedical criteria and ignore cognitive, education and income gains that are key components of overall benefits; and do not deal adequately with 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] 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). Other studies 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), Currie (2009), Smith (2009), 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).

The rest of the paper is organized as follows. Section 2 presents a simple model of health, educational investments and income. Section 3 discusses the deworming project and survey. Section 4 lays out the estimation strategy and describes the impacts of deworming on health, education, and labor market outcomes. Section 5 computes the social returns to deworming investment, and the final section concludes, discussing external validity and implications for research and policy.

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

We present the comparative statics of a simple textbook model of health, educational investment and income to illustrate the channels through which deworming may affect labor market outcomes. 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. Here we present a simple model related to Bleakley's to illustrate this and other points.

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. 1)

$$\frac{de^*}{dh} = -\frac{b_{eh} - c_{eh}}{b_{ee} - c_{ee}}$$

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 little change in schooling attainment, 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 right-hand side term in eqn. 2) and effects through education (the second term):

(eqn. 2)

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

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 or have more stamina), and on other dimensions of human capital (for instance, more learning per unit of time spent in school, as captured by the test score, say, rather than school attainment alone), 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 initially optimal 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.[3] 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 below in our discussion of the returns to deworming investment. The seminal model of health capital developed in Grossman (1972) argues that the fundamental difference between health capital and

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[3] 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 labor productivity) today might instead boost current school drop-out rates.

other forms of human capital, such as those created through education, is precisely the fact that 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 among deworming treatment individuals that we report below 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.

3. 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.

3.1 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 but not atypical rate in African settings (Brooker *et al.* 2000b, Pullan et al. 2011). The 1998 Kenya Demographic and Health Survey indicates that 85% of 8 to 18 year olds 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

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[4] Miguel and Kremer (2004) present a fuller set of baseline covariates for the treatment and control groups.

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 drop in deworming treatment, 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, Panel A), 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, as discussed below. 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.

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

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[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.

(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 reported 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.

3.2 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, and other life outcomes.

A notable feature 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 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. 3) $$\text{ETR} = \text{RTR} + (1 - \text{RTR})*\text{ITR}$$

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

Table 1, Panel B provides a summary of tracking rates in KLPS-2. Overall, the RTR in KLPS-2 is 65.0% and the ITR is 62.1%, which implies that over 86% of respondents were effectively located by

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[6] As expected, individuals found during the intensive phase were 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.

the field team, with 82.5% surveyed while 3% were either deceased, refused to participate, or were found but were unable to be surveyed. The effective survey rate among those still alive is 84%. 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), and several recent African panel surveys.[7] Reassuringly, survey tracking rates are nearly identical in the treatment and control groups (Panel B). 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 Appendix Figure A2.

4. Deworming impacts on health, education and labor market outcomes

This section lays out the estimation strategy and presents deworming impacts on health, education and labor outcomes.

4.1 Estimation strategy

The econometric approach relies on the PSDP's prospective experimental design, namely, the fact that the program exogenously 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

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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.

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

The labor market outcome is a function of the assigned deworming program treatment status of the individual's primary school (T_j), and thus this is an intention to treat (ITT) estimator; 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, the 2001 cost-sharing school indicator, as well as controls for the month and wave of the interview.

The main coefficients of interest are b, which captures 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

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[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 focus on 0-6 km for simplicity. The externality results are unchanged if we focus on the proportion of local 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).

program treatment "contamination" generated by spillovers dampens the "naïve" difference between 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.

We also use an instrumental variables approach to generate a structural estimate of the impact of eliminating an intestinal worm infection. On the representative subsample of respondents administered parasitological stool sample exams during 1999, 2001 and 2002, we first estimate the first stage relationship by regressing an indicator for individual moderate-heavy worm infection on the deworming treatment school and externality variables (and other standard controls) in a specification similar to equation 4 above.[9] We present these first stage results in Table 2 below. This generates the predicted number of years with moderate-heavy worm infections between 1998-2001 at the individual-level, which serves as the endogenous variable in the IV specifications. We then use a two-sample IV approach with bootstrapped standard errors (Angrist and Pischke 2008) to generate the estimated impact of eliminating a moderate-heavy worm infection for one year.

The IV specification imposes the condition that the labor market impacts of different interventions that affected worm loads (e.g., free treatment, cross-school spillovers, and cost-sharing) are proportional to the reduction in moderate-heavy infection. This is potentially restrictive for several reasons, for instance, if some gains are instead the result of reduced worm loads that are insufficient to meet the moderate-heavy threshold. The exclusion restriction may also not hold due to complementarities in schooling outcomes—if children are more inclined to go to school if their classmates are also in school, for instance. The IV estimates may also overstate the effects of eliminating a worm infection for another reason. As Miguel and Kremer (2004) discuss, since worm

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[9] Since the parasitological exams were collected early in each calendar year, we follow Miguel and Kremer (2004) in assuming that the worm infection measures are relevant for understanding the previous year, i.e., that the early 1999 parasitological survey captures infection levels in 1998. For ethical reasons, parasitological surveys were only collected for groups that were to be treated in that year, so Group 1 schools have parasitological data for 1998-2002, Group 2 schools for 1999-2002, and Group 3 schools for 2001-2002.

infections were measured up to a year after treatment, when many pupils will already have been reinfected with worms, the difference in infection levels between treated and untreated pupils was likely greater on average over the interval from the date of deworming treatment to the parasitological exam than it was at the time of the parasitological exam (given the documented short-term efficacy of the drugs and rapid rate of reinfection). Thus the first stage probably understates the total number of moderate-heavy infections eliminated immediately after treatment, perhaps leading us to overstate labor market impacts per infection eliminated. While these factors suggest that one should be cautious about interpreting these results as a consequence of eliminating a moderate-heavy infection alone, the IV estimates may in fact represent the most accurate estimates of the impact of a general deworming program, providing additional external validity.

4.2 Impacts on health and education

We first document that deworming led to large reductions in moderate to heavy worm infections (defined as in Miguel and Kremer 2004) during the course of the original deworming intervention, using the parasitological stool sample data from 1999 and 2001 (Table 2, Panel A). As in the earlier study, there are large direct impacts of being assigned to a treatment school (-0.245, s.e., 0.030) as well as externality benefits for those living within 6 kilometers of treatment schools (-0.075, s.e., 0.026).[10] There is weak evidence of improved hemoglobin status (1.03, s.e. 0.81). In a 1999 survey conducted among a representative subsample of pupils, there is also a significant reduction in self-reported “falling sick often”, by 3.7 percentage points (s.e. 1.5).

Adult health also improved as a result of deworming: respondent self-reported health (on a normalized 0 to 1 scale) rose by 0.041 (s.e. 0.018, significant at 95% confidence, Table 2, panel B). Many studies have found that self-reported health reliably predicts actual morbidity and mortality

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[10] The time pattern of moderate-heavy worm infections across deworming treatment groups 1, 2 and 3 are presented graphically in Appendix Figure A3.

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 below, 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 the 1999 survey before most were working, suggests that at least part of the effect is directly due to deworming.

Deworming did not lead to higher body mass index, nor are there detectable height gains, even when we restrict attention to younger individuals (those in grades 2-4 in 1998, regression not shown). This is a reassuring result since the deworming beneficiaries were already of primary school age when the program started, and thus beyond the age at which we would expect nutritional and health improvements to translate into permanent anthropometric gains.

We examine school enrollment and attendance using two different data sources in Table 2, Panel C. We first report 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), but two important limitations 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 (shown in appendix Table A3) is mainly driven by students graduating from primary school. Total school participation gains are 0.129 of a year of schooling (s.e. 0.064, significant at 95% confidence).

Another outcome 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). The treatment effect estimates are largest during 1999-2003 before tailing off during 2004-07 (Appendix Table A3), as predicted in the educational investment framework laid out above since the opportunity cost of time rises relative to the later benefits of schooling as individuals age. 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 (multiplied by average attendance conditional on enrollment), which works out to nearly 0.3 years of schooling.

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) or attending at least some secondary school (0.032, s.e. 0.035), although both estimates are positive. A likely explanation is that the increased time in school is 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. Despite the absence of significant attainment effects, the increase in time spent in school may still yield some labor market returns through improved social or other non-cognitive skills (Heckman, Stixrud, and Urzua 2006).

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 2, Panel D), there is some 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), and that English vocabulary knowledge (collected in 2007-09) is higher in the 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 yields a normalized point estimate of 0.112 that is 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. 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 these deworming beneficiaries – it was seemingly already "too late" for such gains among the primary school age children in our study.

4.3 Deworming Impacts on Living Standards and Labor Supply

Household consumption is commonly used to assess living standards in rural areas of less developed countries, where most households engage in subsistence agriculture rather than wage work. We do not have a consumption module but did collect data on the number of meals consumed. Deworming treatment individuals consume 0.096 more meals per day (s.e. 0.028, significant at 99% confidence, Table 3, Panel A) than the control group, and the externality impact is also large and positive (0.080, s.e. 0.023, 99% confidence).[11] Total health expenditures by the respondent in the last month are also significantly higher in the treatment group (91.1 Shillings, s.e. 30.0). One interpretation is that this reflects higher income levels. Other possibilities are that people in the treatment group saw positive effects of biomedical treatment through the program and that this experience led them to be more willing to invest in such treatments in the future, or perhaps that they have different health needs.

Hours worked increase substantially 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 3, Panel B) on a control group mean of 15.3 hours, a 12% increase that is significant at 90% confidence. Median regression is a natural alternative to OLS given the large share (33.8%) of individuals with zero hours

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[11] A consumption expenditure module was collected as a pilot 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. 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), and that food constitutes roughly 64% of total consumption. The estimated treatment effect for total consumption is near zero and not statistically significant (-$14, s.e. $66), though the confidence interval is large and includes substantial gains.

worked in the last week, and yields a median gain of 1.10 hours (s.e. 0.54, significant at 95% confidence). The increase can be partitioned into an increase in the propensity to work, and in hours worked conditional on positive hours. Equal proportions of treatment and control group individuals worked at all in the last week, with a small and not significant difference of just 1.0 percentage points between the groups. The increase in hours worked is concentrated among the 66.2% of the sample that worked at all in the last week, at 2.40 hours (s.e. 1.16), on a base of 23.0 hours in the control group. There is also a large, positive and significant coefficient estimate on the term capturing local deworming treatment externalities, at 2.75 (s.e. 1.36). Hours worked for wages 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. There are even larger increases in hours worked in self-employment in the last week, at 8.9 hours (s.e. 3.0) and again a large and statistically significant externality effect (8.0, s.e. 3.0). Impacts on hours worked in agriculture are small and not statistically significant.

Some of these gains appear to be the direct result of improved health boosting individual work capacity among wage earners: while impacts in the full sample of labor market participants are negative but not significant at traditional levels (point estimate -0.105, s.e., 0.136), the number of days lost to poor health in the last month falls by a third, or 0.495 of a day (s.e. 0.245), among wage earners in the treatment group.

The distributions of hours worked (in all occupations), as represented in kernel densities, for the treatment and control groups are presented in Figure 2, panel A. There are few striking differences between these two distributions, although there are somewhat fewer treatment individuals working 20 to 30 hour work weeks. In both the wage-earning subsample (panel B) and the self-employed subsample (panel C), though, a noticeably larger share of treatment individuals were working approximately full-time (roughly 40 hours per week) with fewer working part-time. The distributions of hours worked are nearly identical for those working in agriculture (Panel D).

4.4 Impacts on employment sector, occupation, and migration

A leading question in developing country labor markets is the extent to which human capital improvements lead to shifts out of particular employment sectors and occupations, and alter migration patterns. We begin by exploring impacts on the sector of employment. Improved health and education could allow people to work more hours and/or to be more productive per hour, either in the same sector (or occupation) or in a different sector. While the most common employment sector is farming (53.1% in the control group), as expected in rural Kenya, 16.7% worked for wages in the last month (and 24.4% at some point since 2007), while 10% were currently self-employed outside of farming (Table 4, Panel A). The rates of agricultural, wage work and self-employment are nearly identical across the deworming treatment and control groups, with no significant differences.

There are, however, pronounced shifts in the occupation of employment among wage earners, out of relatively low-skilled and low wage sectors into better paid sectors (Table 4, Panel B). We present the share of control group individuals working in each of the major employment occupations in the first column, where the sectors presented taken together account for over 90 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 three columns present average earnings, hours worked in this sector, and mean days lost to poor health in the last month 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 4), 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 in Nairobi. 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, in contrast to 42 hours for all wage earning jobs. Workers in manufacturing jobs also tend to have relatively few work days missed due to poor health, at just 1.1 days (in the control group), compared to 1.5 days among all wage earning jobs. One explanation for this pattern that ties into our earlier labor supply findings is that child health investments improve individuals' capacity to carry out physically demanding, characterized by long work weeks and little tolerance of absenteeism, and thus allow them to access higher paid jobs such as those in manufacturing.

We also have information on where surveyed respondents were living (Table 5); the locations of residence (for at least four consecutive months at any point during 1998-2009) are presented in the map in Appendix Figure A1. There is considerable migration out of Busia District, at nearly 30%, which is roughly 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 two major cities of Nairobi and

Mombasa. Treatment group individuals are significantly more likely to move to Mombasa, which is considerably farther away from Busia than Nairobi, and significantly more likely to live at least 500 km away from Busia by 2007-09. This tendency for treatment group individuals to live at a greater distance away from the home district may capture greater effort exerted in the job search process.

4.5 Impacts on wage earnings

For a first piece of evidence on impacts on earnings, the distribution of wage earnings is shifted sharply to the right in deworming treatment schools (Figure 3).[12] In the regression analysis, we find that deworming treatment leads to higher log earnings (Table 6), with results unchanged if we do not use the log transformation (results not shown); 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 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. The earnings gains are slightly smaller for Group 2 schools, as expected since they received one less year of deworming treatment on average, but the difference between Groups 1 and 2 (that together comprise the treatment group) is not statistically significant (column 4), and there are similarly no statistically significant differences between Group 1 and 2 for a range of other labor market outcomes, including hours worked (not shown), providing a rationale for considering Groups 1 and 2 together as the treatment group in the analysis.

A decomposition along the lines of Oaxaca (1973) indicates that over 90% of the increase in labor earnings for the treatment group (Table 6), and nearly a third of the increase in hours worked

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[12] Here and below we present real earnings measures that account for the higher prices found in the urban areas of Nairobi and Mombasa. 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. Results are unchanged without this price adjustment.

(Table 3), can be explained by the occupational shifts documented in Table 4. 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 can be accounted for by such shifts.

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, in Table 6, column 3), 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 column 3 is negative and marginally significant at -15.9 log points (s.e., 8.8). This provides further evidence that more deworming treatment is associated with higher earnings.

The earnings result is almost unchanged to trimming the top 1% of earners, so the result is not driven by outliers (Table 7, 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%), 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 7 summarizes a wider set of labor market outcomes among wage earners, using our preferred specification with the full set of regression controls (equivalent to equation 4 and as in column 3 in Table 6). Log wages (computed as earnings per hour worked) rise 16.5 log points in the deworming treatment group, and the effect is marginally significant (t-stat=1.4). 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 (0.170, s.e. 0.116, Table 7, Panel B).

We find no significant evidence that deworming earnings gains differ by gender (Appendix Table A4, 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 the zonal-level 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. There is marginally significant evidence that the gains in hours worked are larger among females (column 7), but it is notable that the gain in work hours is not larger among individuals who were initially younger at baseline (in grades 2-4, column 8). The gains in hours worked are no higher in areas with higher worm infection rates at baseline (column 9).

The degree of selection into the wage earner subsample is an 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, we present several pieces 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 similar, (iii) there are significant impacts on certain labor market outcomes in the full sample, (iv) results are robust to a 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 these results.

In Table 4, we found no evidence that deworming treatment individuals are more likely to be working for wages or in-kind in the last month (Panel A, estimate -0.015, s.e. 0.018). There is similarly no differential selection into the subsample who have worked for wages at any point since 2007 by treatment group (Table 7, Panel B, estimate 0.000, s.e. 0.021). While it remains possible that deworming led different types of individuals to enter wage earning while leaving the overall proportion unchanged, the lack of deworming impacts on the proportion of individuals working in both self-employed and agriculture as well makes this appear less likely.

We further confirm that there is no differential selection into the wage earner sample by gender (Appendix Table A4, column 4) or age (column 5). 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 6), but the coefficient is only marginally significant and quite small. A one standard deviation increase in the baseline local moderate-heavy infection rate is 0.2, so 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 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 focus on earnings in the full sample in Table 7, Panel C (before turning to more detailed analysis of the self-employed and agriculture subsamples below). While there is no effect on mean total labor earnings (setting non-wage earnings to zero for those without a job), total labor earnings are significantly higher in the treatment group at the 95th percentile in a quantile regression.

The 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. Keeping in mind the standard caveats to selection correction models, this approach yields an almost unchanged estimated impact on log wage earnings of 0.285 (s.e., 0.108, Table 7, Panel C).

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.

4.6 Instrumental Variable Estimates

We next go beyond the intention to treat approach and generate instrumental variable estimates of the impact of years of moderate-heavy worm infections on later outcomes. These estimates of gains per infection eliminated are particularly useful since they may generalize beyond the particular program we study. The first stage results are presented in Table 2 (panel A), and show that assignment to a treatment school, as well as geographic proximity to other treatment schools, both lead to significantly lower individual worm infection levels.

The two-sample IV (TSIV) results are broadly similar to the ITT estimates in terms of statistical significance levels, although magnitudes and interpretation differ (Table 8). The estimates indicate that experiencing one fewer year with a moderate-heavy worm infection during childhood increases hours worked by 3.14 hours in the last week (s.e. 1.24), with particularly large effects on labor supply among wage earners (7.96 hours, s.e. 3.65). The impact of eliminating a serious worm infection on earnings in the most recent month worked is large, at 26.6 log points (s.e. 10.8).

4.7 Impacts on self-employment and agricultural outcomes

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 more 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, Table 9, 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.

Among those who work primarily on their own farm, there is no indication that deworming led to higher crop sales in the past year or adoption of "improved" agricultural practices including fertilizer, hybrid seeds or irrigation (Table 9, Panel B). The failure to find increased crop sales may, in part, be due to the fact that households are consuming more of the grain they produced, as suggested by the increase in meals eaten, a finding that also holds in the subset of agricultural households (not shown). While these results should be read with a grain of salt as we cannot easily measure individual on-farm productivity, there are no clear impacts on agricultural outcomes. This "non-result" echoes the labor supply results reported above, where there was no evidence of increased hours worked in agriculture, in contrast to the large increases in labor supply both among wage earners and the self-employed.

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.

On the benefits side, we consider the earnings gains estimated (as in Table 6, 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 assumptions that imply that our rate of return estimates are lower bounds on the true returns to deworming. 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 in some calculations is that only the subset of current wage earners (16% of the sample) will experience improved living standards as a result of deworming. This ignores the fact that a growing proportion of individuals are likely to work for wages in the future as they age and more enter the labor market. Disregarding living standards gains experienced by non-wage earners is conservative, given that the number of meals eaten rose in the full sample and that small business performance measures improved among the self-employed, for instance, and so we also report figures below that assume that the earnings gains we estimate among wage earners also apply to the full sample.

There may also be broader community-wide benefits to deworming among those not of school age, for example, among the younger siblings of the treated. Ozier (2010) shows that children 0-3 years old when the deworming program was launched and lived in the catchment area of a treatment school themselves show large cognitive gains ten years later, with average test score gains for those who were less than one year old when their communities received mass deworming treatment of 0.4 standard deviation units, equivalent to 0.5-0.8 of a year of school learning in Ozier's sample. We conservatively ignore these gains.

Under these assumptions, the average gain in total lifetime earnings (undiscounted) from deworming treatment per pupil in the PSDP sample is $3,145 (Table 10, Panel A). The externality benefits to deworming treatment – including both the cross-school externalities presented above, and estimated within-school spillovers – are the lion's share of the gains (at $2,956 per sample pupil, or 94% of total benefits), and thus substantially boost the rates of return reported below. Miguel and Kremer (2004) estimate that the reduction in moderate-heavy worm infections experienced by treatment school individuals who did not receive deworming drugs themselves were 78% as large as those experienced by treatment school individuals who did receive the drugs (see Table 6, panel B in that paper). We apply this estimate of the magnitude of within-school externalities here. Adding in the future wage benefits to younger untreated cohorts (as in Ozier 2010) would further increase raises the share of future earnings benefits that can be attributed to externalities. The overwhelming share of benefits generated by externalities is a plausible explanation for the relatively low private demand for deworming drugs in rural Kenya, as found by Kremer and Miguel (2007).

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 additional work hours can legitimately be considered welfare gains. True welfare gains thus probably lie somewhere in between. Focusing on wage gains alone, lifetime benefits are $686, with $575 (84%) attributable to externalities.

There are two main social costs to deworming. The most obvious is the direct cost of deworming pill purchase and delivery. 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 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 per treatment pupil and $0.44 per pupil in the full sample (Table 10, Panel B). We assume a deadweight loss of 20% is incurred on the government revenue raised to fund this expansion (Auriol and Walters 2009).

The second potential component of costs is the opportunity cost of time spent in school rather than doing something else, presumably working. We consider two cost cases. We first consider opportunity costs under the strong assumption that all days of additional schooling came at the expense of days worked. This is an upper bound on the actual opportunity cost of time, if school participation instead increased at least in part because children were simply sick less often. A lower bound on opportunity costs is obtained under the assumption that all of the additional days spent in school were due to an increase in non-sick time, as in Grossman (1972).

To compute opportunity costs, we first 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 enrollment among treatment school individuals, at each individual age (in an analysis similar to Appendix Table A3); we disaggregate effects by child age since schooling gains are often concentrated among younger children. We use data from the KLPS survey on average wages in western Kenya 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, 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 (including externalities) under these assumptions is $37.22.

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 (Table 10, Panels A and B). The most natural approach to assess the relative future benefits and costs of an investment is by calculating its internal rate of return (IRR). An immediate lower bound on the IRR considers only the wage productivity benefits of deworming (for the wage earner subsample), while factoring in the opportunity costs of time spend in school, and this yields a return of 22.6% per annum (Panel C). An arguably more realistic estimate of 82.7% is obtained by considering the increase in total earnings while disregarding the opportunity cost of time spent in school. The interpretation is that a social planner with an annual discount rate or cost of capital of less than 82.7% (or 22.6% depending on assumptions) would choose to invest in deworming as a human capital investment. For 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).[13] Deworming appears to be an attractive investment given the real cost of capital in Kenya.[14]

We have so far focused on wage earners because their productivity gains are more accurately measured than those working in self-employment or agriculture. If we abandon the assumption that earnings and wage gains were only experienced by those with wage earnings, and assume that the

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[13] 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%.

[14] 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.

full sample experienced analogous living standard gains, the social internal rate of return would be much larger: a 117.1% per annum return in terms of total earnings while excluding opportunity costs, and 46.69% in terms of wage productivity and including opportunity costs of time.

The magnitude of the externality gains is central for understanding the desirability of public subsidies for school-based deworming. We next compute the social rate of return only considering externality impacts on earnings and wages. As noted above, these are lower bounds on the true externality gains since we ignore benefits experienced by many other individuals in the treatment communities who might have also gained from the improved disease environment. The estimated annualized internal rates of return in terms of deworming externalities range from 21.4% (considering only wage productivity gains among the wage earner subsample) to 115.8% for earnings gains in the full sample (Panel C). These returns alone would appear to justify full public subsidies for deworming treatment given the real cost of capital in Kenya.

Another policy concern is that real-world programs in Kenya or other countries with high worm infection levels would not be implemented as cost effectively as the program we study, leading to higher costs per child treated, for instance, if a certain share of the funds was misused or stolen. However, we find that the level of such leakage would have to be extremely large to drive the annualized IRR down even to a still respectable 10%. In particular, for the case where we consider lifetime earnings gains for the full sample and exclude child opportunity costs of time, 99.9% of funds would have to be misused to drive the IRR down to 10% per annum (from 117.1%). In the case where we only consider wage productivity gains in the wage earner subsample (and include child opportunity costs) a full 76.1% of funds would still have to be misused to drive the internal rate of return down to 10%. The immediate implication is that even heavily mismanaged real-world school based deworming programs are still likely to generate very high social rates of return.

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. 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.

There were large increases in average hours worked (by 12%), and a reduction in work days lost to sickness as a result of deworming. Among those working for wages, average adult earnings rise by over 20%, and these gains are accompanied by 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. The social returns to child deworming treatment are high, with lower bounds on the annualized social internal rate of return ranging from 22.6% to 117.1% depending on assumptions, and our estimates suggesting that externality benefits alone justify fully subsidizing school-based deworming. These findings complement Bleakley's work on historical deworming programs in the U.S. South in the early 20th century, and the correspondence between the two sets of results – using distinct research designs and data – increases confidence in 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) PSDP summary statistics and randomization checks, and KLPS (2007-09) survey attrition patterns

Panel A: Baseline summary statistics	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)	Kolmogorov-Smirnov p-value
Age (1998)	11.9	11.9	12.0	-0.04	0.258
	(2.6)	(2.6)	(2.6)	(0.11)	
Grade (1998)	4.23	4.22	4.25	-0.03	0.450
	(1.68)	(1.70)	(1.66)	(0.05)	
Female	0.470	0.469	0.473	-0.004	--
				(0.019)	
School average test score (1996)	0.029	0.024	0.038	-0.013	0.310
	(0.427)	(0.436)	(0.406)	(0.109)	
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)	
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)	
Years of assigned deworming treatment, 1998-2003	3.31	4.09	1.68	2.41^{***}	--
	(1.82)	(1.52)	(1.23)	(0.08)	
Panel B: Sample attrition, KLPS					
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)	

Notes: The data in Panel A 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 variables in Panel A 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 sample used in Panel B 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). All observations are weighted to maintain initial population proportions. 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.

[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.

Table 2: Impacts on health, nutrition and education outcomes

Dependent variable	Control 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 outcomes during 1999-2001			
Moderate-heavy worm infection (1999, 2001 parasitological surveys)	0.321	-0.245***	-0.075***
	(0.467)	(0.030)	(0.026)
Hemoglobin (Hb) level (1999, 2001 parasitological survey samples)	126.1	1.03	0.91
	(14.7)	(0.81)	(0.96)
Falls sick often (self-reported), 1999	0.154	-0.037**	0.001
	(0.361)	(0.015)	(0.014)
Panel B: Health and nutrition outcomes, KLPS (2007-09)			
Self-reported health “very good”	0.673	0.041**	0.028
	(0.469)	(0.018)	(0.022)
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: School participation, enrollment and attainment			
Total primary school participation, 1998-2001	2.51	0.129***	0.056
	(1.12)	(0.064)	(0.048)
Total years enrolled in school, 1998-2007	6.69	0.279*	0.138
	(2.97)	(0.147)	(0.149)
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 D: Test scores			
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)
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 during 1998-2007	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)
Raven’s Matrices cognitive test score (normalized), 2007-09	0.000	-0.011	0.055

	(1.000)	(0.048)	(0.042)

Notes: The sample size in Panel A is 2,720 for worm infection, 1,765 for Hb, and 3,861 for health self-reports. Representative subsets of pupils in all schools were surveyed for these 1999 and 2001 pupil surveys. The sample in Panel B includes all individuals surveyed in KLPS-2. Each row is from a separate OLS regression analogous to equation 4. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview, a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator. 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.

Table 3: Deworming impacts on living standards and labor supply

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: Living standards				
Number of meals eaten yesterday	2.16	0.096***	0.080***	5,083
	(0.64)	(0.028)	(0.023)	
Respondent health expenditures (medicine, in/out-patient) in past month (KSh)	119.2	91.1***	40.7	5.083
	(389.9)	(30.0)	(55.9)	
Panel B: Labor supply				
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 – median regression	15.2	1.10**	0.66	5,084
	(21.9)	(0.54)	(0.63)	
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, self-employed, agriculture) in last week, if hours worked > 0	23.0	2.40**	2.75**	3,514
	(23.4)	(1.16)	(1.36)	
Hours worked (for wages) in the last week, if hours worked > 0	42.2	5.19*	6.60**	693
	(24.7)	(2.74)	(2.93)	
Hours worked (as self-employed) in last week, if hours worked > 0	33.9	8.9***	8.0***	583
	(25.7)	(3.0)	(3.0)	
Hours worked (in agriculture) in last week, if hours worked > 0	9.5	0.48	-0.75	2,829
	(9.1)	(0.53)	(0.48)	
Work days missed due to poor health, past month (negative binomial)	1.67	-0.105	0.020	1,935
	(3.16)	(0.136)	(0.172)	
Work days missed due to poor health (if work for wages), past month (negative binomial)	1.48	-0.495**	-0.023	626
	(3.10)	(0.245)	(0.340)	
Work days missed due to poor health (if self-employed), past month (negative binomial)	1.46	-0.086	0.070	478
	(3.07)	(0.319)	(0.392)	
Work days missed due to poor health (in agriculture), past month (negative binomial)	1.57	0.060	0.127	1,132
	(3.32)	(0.146)	(0.146)	

Notes: Each row is from a separate OLS regression analogous to equation 4, except the median regression specification and the negative binominal specification. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview**,** a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator. In the “work days missed due to poor health” regressions, the sample is restricted to individuals who worked at least 10 hours in the last week, so that this is a meaningful measure.

Table 4: Deworming impacts on employment sector and occupation

	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	Mean (s.d.) days of work lost to poor health in last month, control group
Panel A: Employment Sector[a]						
Agriculture (for own household)	0.531	-0.010	0.005	--	10	1.5
		(0.025)	(0.031)		(9)	(2.8)
Self-Employment	0.100	0.015	0.004	--	34	1.8
		(0.012)	(0.011)		(26)	(4.4)
Wage Employment	0.167	-0.015	-0.002	3739	42	1.5
		(0.018)	(0.020)	(3744)	(25)	(3.0)
Panel B: Occupation in Wage Employment						
Agriculture and fishing	0.210	-0.038	-0.152*	2,872	35	2.1
		(0.059)	(0.080)	(1,804)	(25)	(4.0)
Retail	0.153	-0.018	0.025	2,049	39	1.0
		(0.038)	(0.043)	(1,713)	(29)	(2.0)
Trade contractors	0.092	-0.005	0.060	3,172	27	0.8
		(0.028)	(0.004)	(2,170)	(22)	(2.5)
Manufacturing	0.029	0.072***	0.041	5,311	53	1.1
		(0.024)	(0.031)	(3,373)	(24)	(1.8)
Manufacturing – males only	0.057	0.090***	0.031	6,277	49	1.0
		(0.033)	(0.033)	(3,469)	(20)	(1.9)
Wholesale trade	0.027	0.023	0.022	4,727	44	0.7
		(0.029)	(0.035)	(3,963)	(14)	(1.9)
Services (all)	0.417	0.032	0.037	4,694	47	1.3
		(0.054)	(0.075)	(5,013)	(24)	(2.6)
Domestic	0.115	-0.012	-0.026	3,047	61	1.5
		(0.032)	(0.038)	(1,754)	(18)	(2.5)
Domestic – females only	0.335	-0.174	-0.435***	2,795	65	1.6
		(0.110)	(0.180)	(888)	(17)	(2.6)
Restaurants, cafes, etc.	0.060	-0.029	0.024	4,194	53	1.2
		(0.023)	(0.034)	(3,567)	(21)	(2.5)
Casual/Construction laborer	0.029	-0.038**	-0.020	2,246	51	0.4
		(0.018)	(0.017)	(1,576)	(31)	(1.0)
Other	0.030	-0.028*	-0.013	4,600	47	6.0
		(0.015)	(0.014)	(1,740)	(13)	(4.8)

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 equation 4. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview**,** a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator. For Panel A, when computing means in columns 4-6, we do not include zeros among those not working in the relevant sector.

[a] Note that we only have days of work missed in total, not separated by sector, so among those who work in multiple sectors, there is some overlap.

Table 5: Deworming impacts on residential migration

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.
Residence in Busia district	0.700	0.014	-0.019	5,075
	(0.458)	(0.022)	(0.025)	
Residence in districts neighboring Busia district[b]	0.069	-0.002	0.011	5,075
	(0.254)	(0.011)	(0.017)	
Residence outside of Busia and neighboring districts	0.230	-0.012	0.007	5,075
	(0.421)	(0.019)	(0.019)	
Residence in an urban area	0.179	-0.001	0.016	5,075
	(0.383)	(0.019)	(0.024)	
In Nairobi	0.118	-0.016	0.001	5,075
	(0.323)	(0.016)	(0.014)	
In Mombasa	0.024	0.016**	0.011	5,075
	(0.152)	(0.007)	(0.011)	
Residence outside of Kenya	0.042	0.011	-0.003	5,075
	(0.202)	(0.011)	(0.016)	
Distance from original primary school to 2007-09 residential location greater than 500 km	0.031	0.018*	0.020	5,052
	(0.174)	(0.010)	(0.013)	

Notes: Each row is from a separate OLS regression analogous to equation 4. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview**,** a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator.

[a] This sample includes surveyed individuals with residential location information. Outcomes are indicators for location of residence at the time of survey. [b] Districts neighboring Busia include Siaya, Busia (Uganda), and other districts in Kenya’s Western Province.

Table 6: Deworming impacts on labor earnings (2007-2009)

	Dependent variable: Ln(Total labor earnings, past month)			
	(1)	(2)	(3)	(4)
Deworming Treatment indicator	0.193**	0.187**	0.253***	0.277***
	(0.077)	(0.076)	(0.093)	(0.104)
Deworming Treatment pupils within 6 km (in ‘000s), demeaned			0.199	0.194
			(0.168)	(0.170)
Total pupils within 6 km (in ‘000s), demeaned			-0.098	-0.094
			(0.127)	(0.129)
Group 2 school indicator				-0.060
				(0.099)
Cost sharing school (in 2001) indicator	-0.104	-0.139	-0.159*	-0.154*
	(0.085)	(0.094)	(0.088)	(0.090)
Additional controls	No	Yes	Yes	Yes
R^2	0.064	0.176	0.182	0.183
Observations	710	710	710	710
Mean (s.d.) in the control group	7.86	7.86	7.86	7.86
	(0.88)	(0.88)	(0.88)	(0.88)

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 7: 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)	
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)	
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 equation 4, 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. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview**,** a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator.

Table 8: The impact of eliminating moderate-heavy worm infections on economic outcomes, two-sample instrumental variable (TSIV) estimates

Dependent variable	Control group variable mean (s.d.)	TSIV coefficient estimate (s.e.) on predicted years of moderate-heavy worm infection	Obs.
Self-reported health "very good", 2007-2009	0.673	-0.093^{***}	5070
	(0.469)	(0.030)	
Total years enrolled in school, 1998-2007	6.69	-0.229	5037
	(2.97)	(0.203)	
Number of meals eaten yesterday	2.16	-0.099^{*}	5083
	(0.64)	(0.047)	
Hours worked (for wages, self-employed, agriculture) in last week	15.2	-3.14^{**}	5084
	(21.9)	(1.24)	
Hours worked (for wages, self-employed, agriculture) in last week, if hours worked > 0	23.0	-3.23^{*}	3514
	(23.4)	(1.59)	
Hours worked (for wages or in-kind) in the last week, if hours worked > 0	42.2	-7.96^{**}	693
	(24.7)	(3.65)	
Ln(Total labor earnings, past month)	7.86	-0.380^{**}	710
	(0.88)	(0.133)	
Ln(Total labor earnings, most recent month worked)	7.88	-0.266^{**}	1175
	(0.91)	(0.108)	
Ln(Wage = Total labor earnings / hours, past month)	2.82	-0.175	625
	(0.96)	(0.154)	

Notes: Two-sample instrumental variable estimates. Standard errors are bootstrapped clustering by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview, a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator. The instrumental variables in the first-stage are the deworming treatment indicator, the number of deworming Treatment pupils within 6 km (in '000s) demeaned, and the cost-sharing indicator variable.

Table 9: Deworming impacts among the self-employed and in agriculture

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				
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 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)	
Panel B: Agricultural work, sales, hours and practices				
Total value (KSh) of crop sales past year (if farm household)	576	-81	-460**	3,758
	(2458)	(148)	(206)	
Uses “improved” agricultural practice (if farming household)	0.310	0.032	0.005	3,766
	(0.462)	(0.026)	(0.024)	

Notes: Each row is from a separate OLS regression analogous to equation 4. “Agricultural work” includes both farming and pastoral activities. The average of “typical monthly” and last week recall is used for household consumption. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview**,** a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator.

Table 10: Social returns to child deworming investments

Panel A: Benefits (per pupil in the full sample)	Total (including externalities)	Externalities alone	Excluding externalities
Assume only the wage earner subsample has gains:			
Total lifetime earnings (over 40 years, no time discount)	$3,145	$2,956	$188
Lifetime earnings from wage productivity gains (over 40 years, no time discount)	$686	$575	$112
Assume the entire sample has gains:			
Total lifetime earnings (over 40 years, no time discount)	$18,607	$17,493	$1,114
Lifetime earnings from wage productivity gains (over 40 years, no time discount)	$4,062	$3,401	$661
Panel B: Costs (per pupil in the full sample)			
Deworming pills and delivery (2.41 additional years in treatment schools)	$0.44	$0	$0.44
Deadweight loss of taxation (from raising revenue for deworming pills and delivery)	$0.09	$0	$0.09
Child opportunity cost of attending more school (as described in the text)	$37.22	$35.89	$1.33
Panel C: Internal rate of return (per annum)			
Assume only the wage earner subsample has gains:			
Total lifetime earnings – exclude child opportunity costs	82.7%	81.6%	41.3%
Total lifetime earnings – all costs in Panel B	42.6%	42.7%	32.6%
Lifetime earnings from wage productivity gains – exclude child opportunity costs	58.5%	56.0%	35.3%
Lifetime earnings from wage productivity gains – all costs in Panel B	22.6%	21.4%	26.7%
Assume the entire sample has gains:			
Total lifetime earnings – exclude child opportunity costs	117.1%	115.8%	65.7%
Total lifetime earnings – all costs in Panel B	73.8%	74.0%	56.8%
Lifetime earnings from wage productivity gains – exclude child opportunity costs	87.2%	84.1%	57.9%
Lifetime earnings from wage productivity gains – all costs in Panel B	46.6%	44.9%	49.0%

Notes: Calculated in the KLPS sample assuming alternately that (i) only those in the wage earner subsample have earnings gains, or (ii) the entire sample experiences the gains observed in the wage earner subsample. The details of the construction of benefits and costs are in the text. The cost of deworming (and the associated deadweight loss) is per pupil in the full sample, not just the treatment group. The analogous costs per individual in the treatment group are $0.65 for deworming pills and delivery and $0.13 for the deadweight loss of taxation.

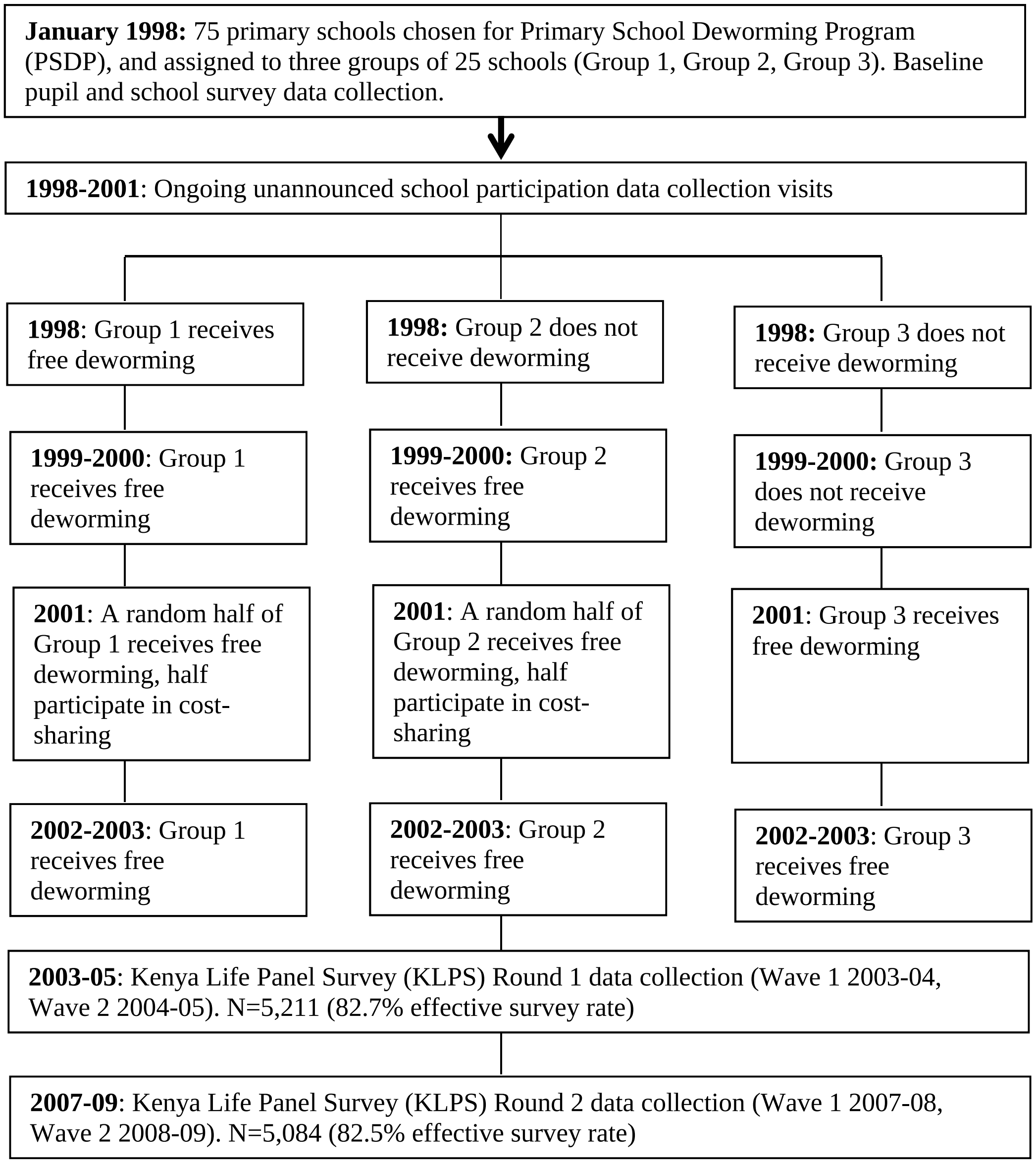


Figure 1: Project Timeline of the Primary School Deworming Program (PSDP) and the Kenya Life Panel Survey (KLPS)

Figure 2: The distribution of hours worked in the last week, deworming treatment versus control (if working 10 to 80 hours)
Panel A (top-left): Full sample; Panel B (top-right): Wage earner subsample;
Panel C (bottom-left): Self-employed subsample; Panel D (bottom-right): Agricultural work subsample

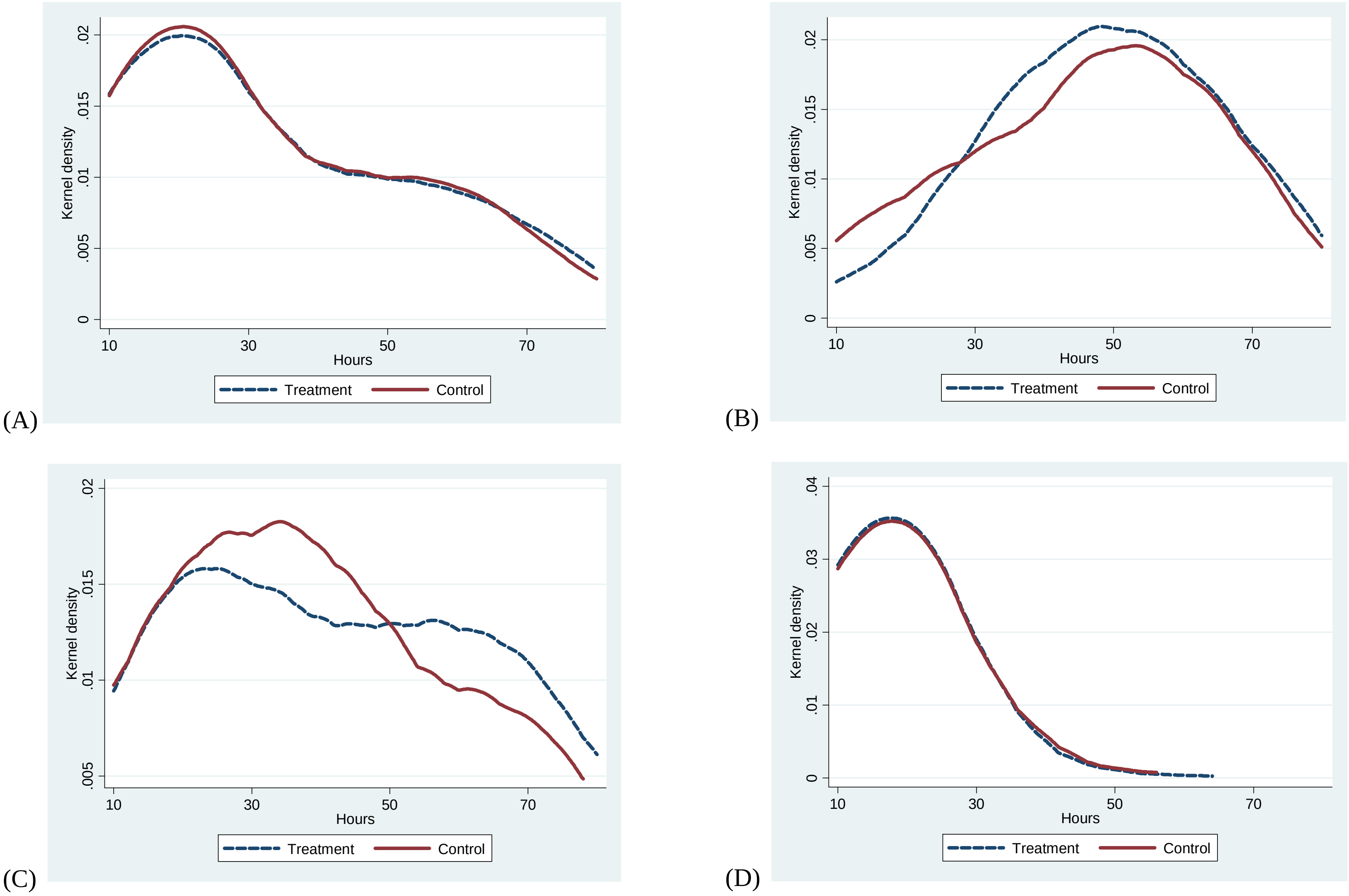
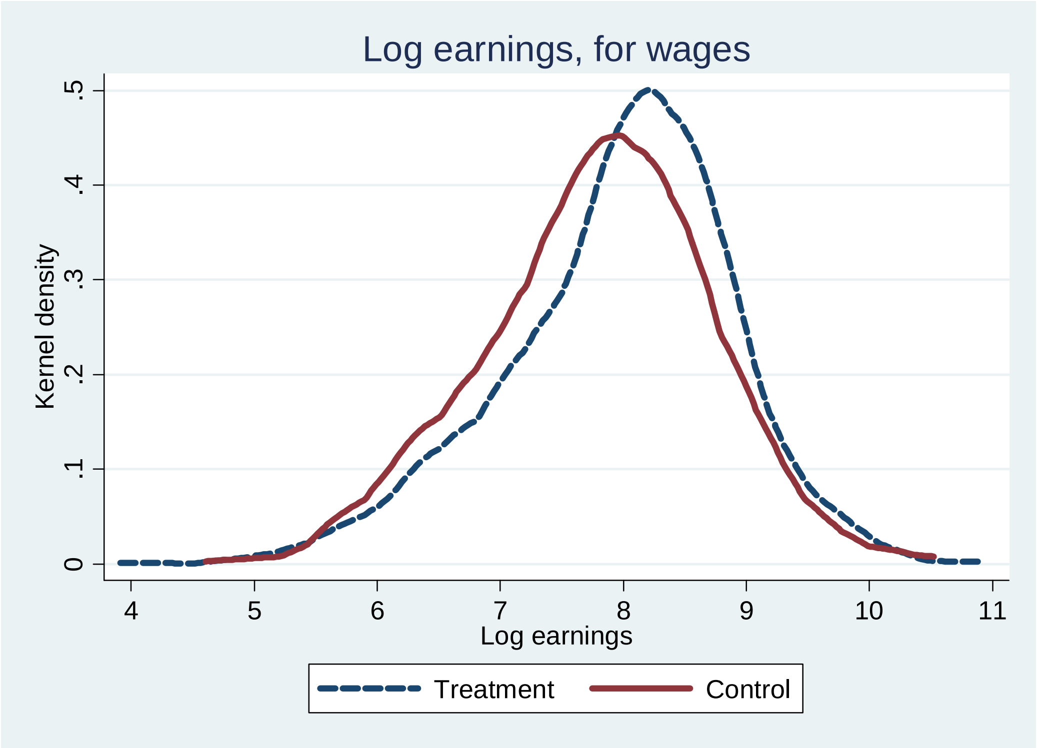


Figure 3: The distribution of log labor earnings in the last month, deworming treatment versus control (among those with positive labor earnings)



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.

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

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[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.

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[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
Age (1998)	13.2	13.2	13.0	0.204	0.202
	(1.8)	(1.9)	(1.7)	(0.391)	
Grade (1998)	4.87	4.86	4.91	-0.054	0.445
	(1.61)	(1.63)	(1.57)	(0.141)	
Female	0.233	0.209	0.280	-0.071	--
	(0.423)	(0.407)	(0.450)	(0.045)	
School average test score (1996)	-0.010	-0.027	0.024	-0.050	0.273
	(0.408)	(0.415)	(0.391)	(0.106)	
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)	
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
Age (1998)	12.4	12.4	12.5	12.7	-0.37***
	(2.2)	(2.2)	(2.2)	(2.1)	(0.09)
Grade (1998)	4.26	4.24	4.24	4.32	-0.105
	(1.69)	(1.68)	(1.70)	(1.70)	(0.063)
Female	0.486	0.461	0.495	0.535	-0.072***
	(0.500)	(0.499)	(0.501)	(0.499)	(0.016)
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 Table A3: 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

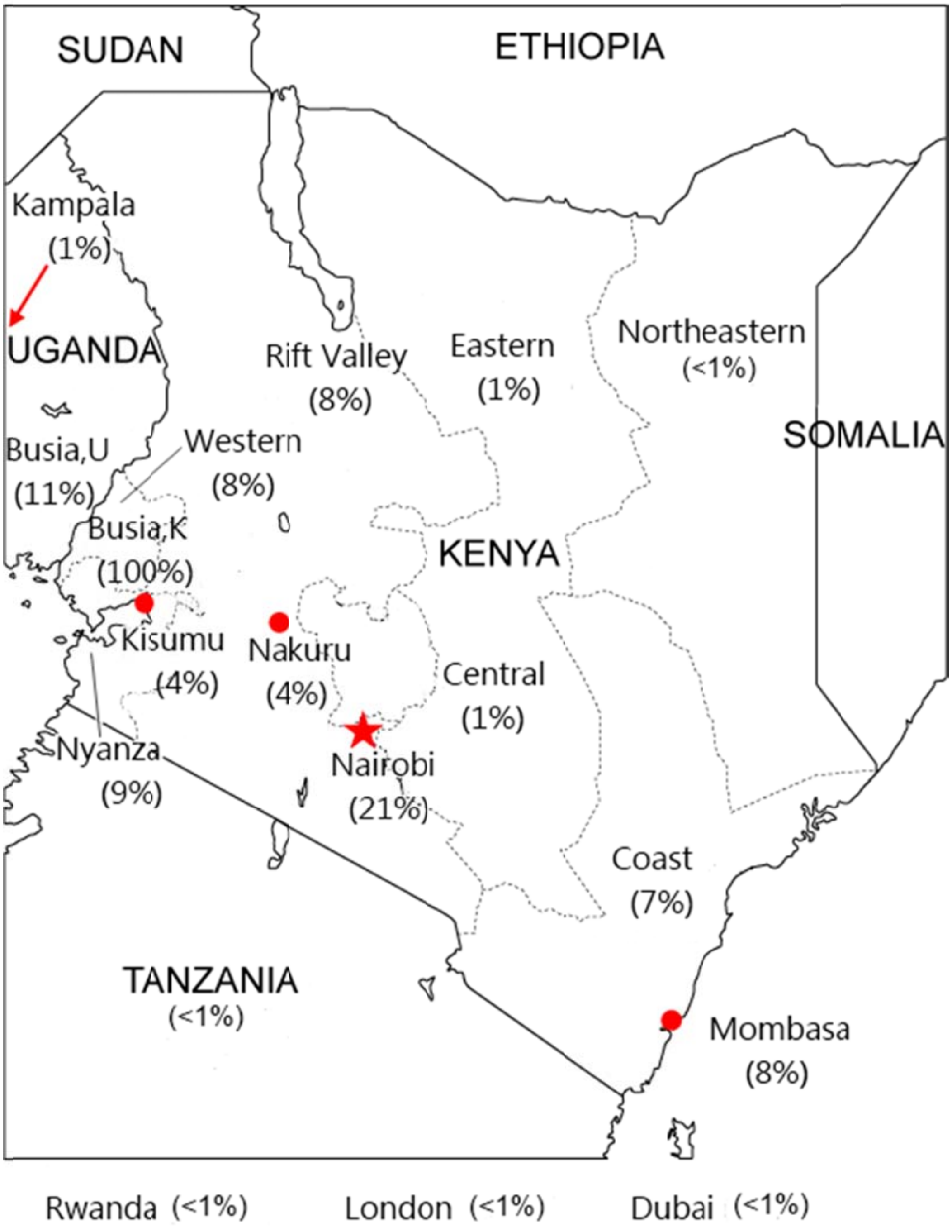
Notes: The sample used in Panel A includes all individuals who were surveyed in KLPS2. The sample used in Panel B includes a subset of these individuals who additionally have school participation data from at least one of the years between 1998 and 2001. All regressions include controls for baseline 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.

Supplementary Appendix Table A4: Deworming impacts on labor market outcomes among subgroups

	Dependent variable:								
	Ln(Total labor earnings, past month)			Indicator for worked for wages or in-kind in last month			Hours worked (for wages, self-employed, agriculture) last week		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Deworming Treatment indicator	0.219**	0.297*	0.255***	0.002	-0.018	-0.016	3.40**	1.53	1.79*
	(0.103)	(0.152)	(0.092)	(0.024)	(0.019)	(0.018)	(1.40)	(1.10)	(0.96)
Female	-0.473***			-0.128***			-3.43**		
	(0.140)			(0.022)			(1.68)		
Female * Treatment	0.121			-0.035			-3.41*		
	(0.195)			(0.027)			(1.98)		
Grades 5-7 in 1998		0.497***			0.105***			7.46***	
		(0.164)			(0.023)			(1.71)	
Grades 5-7 * Treatment		-0.069			0.004			0.43	
		(0.186)			(0.028)			(2.02)	
Moderate-heavy worm infection rate at the zonal level (1998), demeaned			-0.048			-0.035*			-0.70
			(0.084)			(0.018)			(0.85)
Moderate-heavy infection rate * Treatment			0.071			0.028*			0.81
			(0.078)			(0.015)			(0.77)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.183	0.170	0.183	0.093	0.085	0.094	0.065	0.057	0.064
Observations	710	710	710	5081	5081	5081	5084	5084	5084
Mean (s.d.) in the control group	7.86	7.86	7.86	0.166	0.166	0.166	15.2	15.2	15.2
	(0.88)	(0.88)	(0.88)	(0.372)	(0.372)	(0.372)	(21.9)	(21.9)	(21.9)

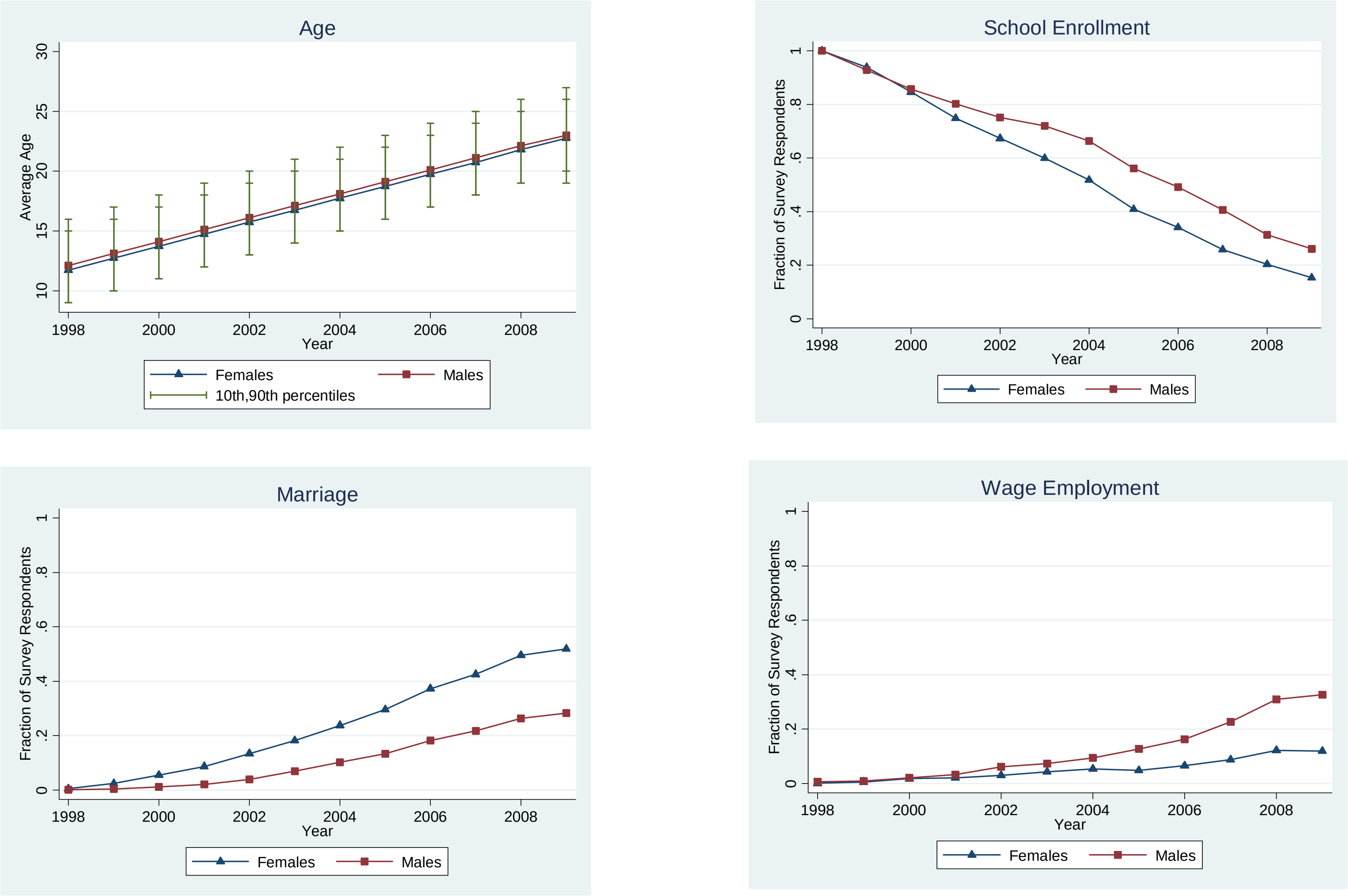
Notes: The sample used in columns (1)-(3) includes all individuals surveyed in the KLPS2 who report positive labor earnings at the time of survey and include data for the relevant dependent variable. The sample used in columns (4)-(6) includes all surveyed individuals with non-missing information on 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.

Supplementary Appendix Figure A1: Migration residential location map

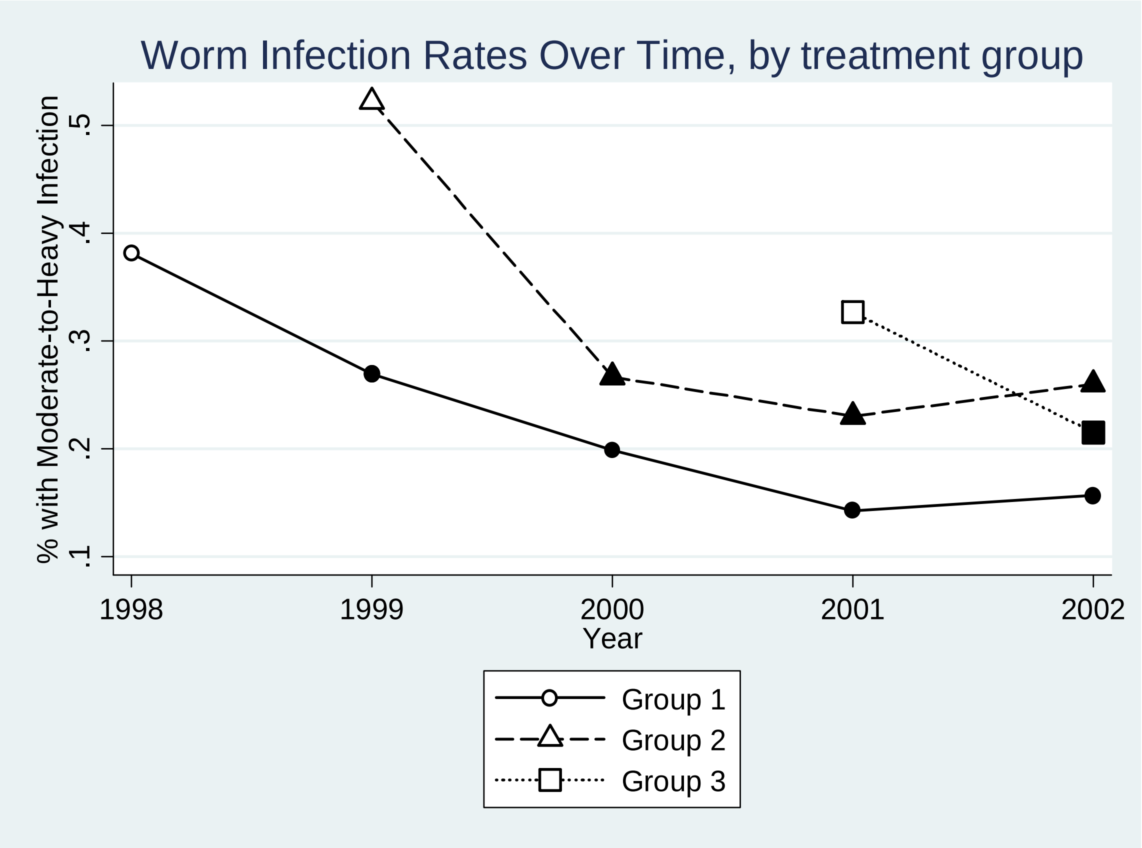


Notes: Percentages sum to greater than one, since they capture residential location (for at least four consecutive months) at any point during 1998-2009.

Supplementary Appendix Figure A2: Age, School Enrollment, Marriage and Employment Patterns over 1998-2009



Supplementary Appendix Figure A3: Moderate-heavy worm infection rates over time by PSDP treatment group



Notes: Hollow symbols (circles, triangles, squares) denote pre-deworming observations (for the group), and filled symbols denote post-deworming. Group 1 and Group 2 schools are jointly considered "treatment" in most of the analysis in the paper. Note that half of the Group 1 and Group 2 schools took part in deworming cost-sharing in 2001, likely accounting for some of the slight rise in infection rates observed in those groups between 2001 and 2002.