

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

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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, hours worked increase by 12%, and work days lost to illness fall by a third in the treatment group. Treatment individuals report eating an average of 0.1 additional meals per day. Point estimates suggest substantial 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 21 to 29% 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. The results indicate that school based deworming is a very attractive public investment.

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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 rate of 83% among a representative subset of individuals enrolled in these schools. 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 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, years enrolled in school increased by approximately 0.3 years, and some test scores rose in the treatment group. 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 unbiased 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. Point estimates suggest substantial externalities among those living within 6 km of treatment schools.

Among the subsample with wage employment, we find that earnings are 21 to 29% higher in the deworming treatment group. These labor market gains are accompanied by marked shifts in employment sector for the treatment group, with more than a doubling of well-paid manufacturing jobs (especially among males) and declines in both casual labor and domestic services employment. Changes in the subsector of employment account for nearly all of the earnings gains in deworming treatment group in a Oaxaca-style decomposition. This pattern indicates that health investments not only boost productivity and work capacity in existing activities, but, by leading individuals to shift into more lucrative economic activities (like manufacturing employment), may also contribute to the structural transformation of the economy a whole. Understanding how to promote this transition has long been a central theme within development economics (see Lewis 1954, among many others), and our results provide a piece of suggestive evidence that health investments may speed this transition.

Measuring labor productivity is more challenging for the majority of our subjects who were either self-employed or working in subsistence agriculture, rather than working for wages, although even in these groups there is some evidence of positive impacts. The estimated impacts on the small business performance of the self-employed, namely measures of profits and employees hired, are also positive and relatively large.

alone justify fully subsidizing school-based deworming.

Our findings contribute to several strands of existing work. The most closely related studies are by Bleakley (2007a, 2007b, 2010), who examines the impact of a large-scale deworming campaign in the U.S. South during the early 20th century on schooling and adult earnings, by comparing heavily infected versus lightly infected regions over time in a difference-in-difference design. He finds that deworming raised adult income by roughly 17%, and, extrapolating these findings to the even higher worm infection rates found in tropical Africa, estimates that deworming in Africa could lead to income gains of 24%, similar to our estimated earnings gains. Taken together, these findings lend credence to the view that treating intestinal worm infections can substantially increase labor productivity.¹ As Bleakley (2010) notes, the fact that deworming reduces morbidity but has negligible effects on mortality means it is particularly likely to boost per capita living standards.

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

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.

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.

The rest of the paper is organized as follows. Section 2 presents a simple model of health, educational investments and income. Section 3 contains background on the school deworming project and the follow-up survey. Section 4 lays out the estimation strategy and describes the impacts of deworming on health, education, and labor market outcomes. 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

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 childhood malaria prophylaxis, and early childhood under nutrition (Alderman *et al.*, 2003; Mendez and Adair, 1999; Glewee *et al.*, 2001), among many others. Though these studies are generally non-experimental (Jukes *et al.*, 2006 is an exception), taken together they provide considerable evidence that adult cognitive performance may be affected by nutrition in the womb and early childhood. Related work on the long-run benefits of child health and nutrition investments in the U.S. include Currie and Thomas (1995), Currie, Garces and Thomas (2002), and Case and Paxson (2010). Other noteworthy micro-empirical contributions on nutrition, health and productivity include Schultz (2005), Alderman (2007), Thomas *et al.* (2008), and Pitt, Rosenzweig and Hassan (2011), and recent contributions in macroeconomics on health and economic growth include Acemoglu and Johnson (2007), Ashraf, Lester and Weil (2009), and Aghion, Howitt and Murin (2010).

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:

$$(eqn. 1) \quad \frac{de^*}{dh} = - \frac{b_{eh} - c_{eh}}{b_{ee} - c_{ee}}$$

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):

$$(eqn. 2) \quad \frac{dy^*}{dh} = \frac{\partial y}{\partial h} \Big|_{e^*} + \frac{\partial y}{\partial e} \Big|_{e^*} \times \frac{de^*}{dh}$$

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.⁴ The seminal model of health capital developed in Grossman (1972) argues that the fundamental difference between health capital and other forms of human capital, such as those created through education, is precisely the fact that 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 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.

⁴ The relevant expression is $\frac{du^*}{dh} = \frac{\partial u}{\partial h}\bigg|_{L^*} + \frac{\partial u}{\partial L}\bigg|_{L^*} \times \frac{dL^*}{dh}$,

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

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).⁵ The same balance is also evident among the subsample of respondents currently working for wages (see Supplementary Appendix Table A1).

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

⁵ Miguel and Kremer (2004) present a fuller set of baseline covariates for the treatment and control groups.

exogenous variation in deworming treatment that we can exploit. In 2002 and 2003, all sample schools received free treatment.

Children in Group 1 and 2 schools thus were assigned to receive 2.41 more years of deworming than Group 3 children on average (Table 1), and these early beneficiaries are what we call the deworming treatment group below. We focus on a single treatment indicator rather than separating out effects for Group 1 versus Group 2 schools since this simplifies the analysis, and because we find few statistically significant differences between Group 1 and 2, 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.⁶ We focus on intention-to-treat (ITT) estimates, as opposed to actual individual deworming treatments, in the analysis below. This is natural as compliance rates are high. To illustrate, 81.2% of grades 2-7 pupils scheduled to receive deworming treatment in 1998 actually received at least some treatment. Absence from school on the day of drug administration was the leading reported cause of non-compliance. The ITT approach is also attractive since previous research showed that untreated individuals within treatment

⁶ Following World Health Organization recommendations (WHO 1992), schools with geohelminth 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.

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.⁷ 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)
$$ETR = RTR + (1 - RTR) * ITR$$

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

Table 2, Panel A provides a summary of tracking rates in KLPS-2. Over 86% of respondents were located by the field team, with 82.5% surveyed while 3% were either deceased, refused to participate, or were found but were unable to be surveyed. These are very high tracking rates for any age group over a decade, and especially for a highly mobile group of adolescents and young adults, and they are on par with some of the best-known panel survey efforts in less developed countries,

⁷ After 12 months of tracking, 64% of the Wave 1 sample (2,404 pupils) had been successfully surveyed, refused, or had died. Among the remaining 1,341 respondents, for budgetary reasons a representative one quarter were “intensively” tracked. As expected, individuals found during the intensive phase were 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.

such as the Indonesia Family Life Survey (Thomas *et al.* 2001, 2010), and several recent African panel surveys.⁸ Reassuringly, tracking rates are nearly identical in the treatment and control groups.

We also have information on where surveyed respondents were living (Table 2, Panel B); the locations of residence (for at least four consecutive months at any point during 1998-2009) are presented in the map in Appendix Figure A1. There is considerable migration out of Busia District, at nearly 30%, which once again is balanced between the treatment and control groups. Since the approximately 14% of individuals we did not find, and thus did not obtain residential information for, are plausibly even more likely to have moved out of the region, these figures almost certainly understate true out-migration rates. Nearly 8% of individuals had moved to neighboring districts, including just across the border into the Ugandan districts of Busia and Bugiri, while 22% of those with location information were living further afield, with most in Kenya's major cities of Nairobi, Mombasa or Kisumu. While there are some significant differences in the migration rates to Nairobi versus Mombasa across the treatment and control groups, they are relatively minor in magnitude.

We focus on the KLPS-2 data, rather than KLPS-1, in this paper since it was collected at a more relevant time point for us to assess adult life outcomes: the majority of sample respondents are adults by 2007-09 (with median age at 22 years as opposed to 18 in KLPS-1), have completed their schooling, many have married, and a growing share are engaging in wage employment or self-employment, as shown graphically in Appendix Figure A2. While the most common economic occupation is farming, as expected in rural Kenya, 16% worked for wages in the last month and 24% at some point since 2007, while 11% were currently self-employed outside of farming (Table 2, Panel C). The rates of wage work and self-employment are nearly identical across the deworming treatment and control groups, as discussed further below. This pattern simplifies the interpretation of some impacts estimated below, although they are somewhat surprising given the deworming impacts

⁸ 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.

we estimate on other labor market dimensions, including the shifts across employment sectors among wage earners. The issue of selection into the wage earning subsample is discussed further below.

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

This section lays out the estimation strategy and describes 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 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), $Y_{ij,2007-09}$, for individual i from school j , as measured in the 2007-09 KLPS-2 survey:

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

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 $e_{ij,2007-09}$, which is clustered at the school level.⁹ The $X_{ij,0}$ controls include school geographic and demographic

⁹ 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

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 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 more structural estimate of the impact of eliminating intestinal worm infections per se. 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)

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).

in a specification similar to equation 4 above.¹⁰ We present these first stage results in Table 3 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 impact of different interventions that affect worm loads (e.g., free treatment, cross-school spillovers, and cost-sharing) is proportional to the reduction in moderate-heavy infection. This is restrictive 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 appear likely to overstate the effects of eliminating a worm infection for another reason. As Miguel and Kremer (2004) discuss, since worm 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 much greater on average over the interval from 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.

¹⁰ Since the parasitological exams were collected very 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.

4.2 Impacts on health and nutrition

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 3, 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).¹¹ 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). The growing evidence that deworming improves immunological resistance to other infections, such as malaria (i.e., Kirwan *et al.* 2010), also implies that deworming might generate broader health benefits. We are able to assess the claim about malaria with the 1999 survey data, and find that self-reported malaria in the last week fell in the treatment group by 1.9 percentage points (s.e. 1.7), with an externality effect that is similar in magnitude. Although not statistically significant, this is a large reduction of nearly 10% given the self-reported malaria rate of 21.8 percentage points in the control group, providing weak suggestive evidence that deworming might have led to broader childhood health benefits in the treatment group.

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 3, panel B). Many studies have found that self-reported health reliably predicts actual morbidity and mortality even when other known health risk factors are accounted for (Idler and Benyamini 1997, Haddock *et al.* 2006, Brook *et al.* 1984). Note that it is somewhat difficult to interpret this impact causally since it may partially reflect health gains driven by the higher adult earnings detailed below, in addition to

¹¹ The time pattern of moderate-heavy worm infections across deworming treatment groups 1, 2 and 3 are presented graphically in Appendix Figure A3.

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.

In terms of other health outcomes, there is no evidence that deworming improved self-reported happiness or wellbeing or reduced major health shocks. 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). Total health expenditures by the respondent in the last month are significantly higher in the treatment group (91.1 Shillings, s.e. 30.0). One possible interpretation is 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. However, it is also possible that this reflects higher overall income levels or different health needs.

4.3 Impacts on education

We examine school enrollment and attendance using two different data sources in Table 4. 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, Table 4, Panel A).

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 4, Panel B), 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.

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

4.4 Deworming Impacts on Living Standards and Labor Market Outcomes

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. Our first measure, the number of meals consumed by the respondent yesterday, is narrower than total consumption but has the advantage that we collected it for the entire sample. Deworming treatment

individuals consume 0.096 more meals (s.e. 0.028, significant at 99% confidence, Table 5, Panel A) than the control group, and the externality impact is also large and positive (0.080, s.e. 0.023, 99% confidence). This suggests that deworming led to higher living standards in the full sample.¹²

Turning to labor market outcomes, 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 5, Panel B) on a control group mean of 15.3 hours, a 12% increase in the full sample that is significant at 90% confidence. The increase in hours worked is even more pronounced among the 66.2% of the sample that worked at all in the last week, at 2.40 hours (s.e. 1.16), on a base of 23.0 hours in the control group. Note that 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. Hours worked for wages or in-kind in particular increases substantially in the deworming treatment group by 5.2 hours (significant at 90% confidence), an increase of 12% on a base of 42.2 hours worked on average in the control group. There is also a large, positive and significant coefficient estimate on the term capturing local deworming treatment externalities, at 6.6 (s.e. 2.9). Some of these gains appears to be the direct result of improved health boosting individual work capacity among wage earners: days lost to poor health in the last month falls by a third, or 0.499 of a day (s.e. 0.235) in the treatment group. 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.

¹² A consumption expenditure module was also 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.

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, both of which have considerable mass near zero. In the wage-earning subsample (panel B), though, a noticeably larger share of treatment individuals were working approximately full-time (roughly 40 hours per week) with fewer working part-time.

The distribution of wage earnings is also shifted sharply to the right in deworming treatment schools (Figure 3), another piece of evidence that deworming affected labor market outcomes.¹³ In the regression analysis, we find that deworming treatment leads to higher earnings in: log transformations of earnings (Table 6, columns 1-4) and linear specifications (columns 5-8); 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, but the difference between Groups 1 and 2 (that together comprise the treatment group) is not 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).

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 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

¹³ 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.

roughly $(917/1000) \times (19.9 \text{ log points}) = 18.2 \text{ log points}$, or 20 percent. We also include an indicator for inclusion in the randomly chosen group of 2001 cost-sharing schools in all specifications; recall that cost-sharing was associated with much lower deworming take-up in 2001. Consistent with this drop, the point estimate on the cost-sharing indicator in the regression shown in Table 6, column 3 is negative and marginally significant at -15.9 log points (s.e., 8.8). This provides further evidence that deworming treatment is associated with higher earnings.

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 columns 3 and 6 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 (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).

4.5 Selection into Wage Earning

The degree of selection into the wage earner subsample is a key issue in assessing the validity of the earnings results. For example, estimates could be biased downward if deworming led some individuals with relatively low labor productivity to enter the wage earner sample. While there is no single ideal solution, 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.

Confirming the result in Table 2, we again find no evidence that deworming treatment individuals are more likely to be working for wages or in-kind in the last month (Table 7, 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 (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 $(0.2) \times (0.028) = 0.0056$ 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, and the same is true for other percentiles above the 90th (not shown).

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 of deworming on log wage earnings of 0.285 (s.e., 0.108, Table 7, Panel C), and similar impacts on the larger subsample that had earnings since 2007 (not shown).

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 Impacts on employment sector

The increased earnings in the deworming treatment group can largely be accounted for by pronounced shifts in the sector of employment, out of relatively low-skilled and low wage sectors into better paid sectors. We present the share of control group individuals working in each of the major employment sectors in the first column of Table 8, 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 two columns present average earnings and hours worked in this sector in the control group.

The most striking impacts are a large increase in manufacturing work for deworming treatment individuals, with a point estimate of 0.072 (s.e. 0.024, Table 8), signifying a tripling of manufacturing employment overall. The gains among males are particularly pronounced at 0.090 (s.e. 0.030). The two most common types of manufacturing jobs in our sample are in food processing and textiles, with establishments ranging in size from small local corn flour mills up to large blanket factories. On the flip side, casual labor employment falls significantly (-0.038, s.e. 0.018), as does domestic service work for females (-0.174, s.e. 0.110), although this latter effect is only marginally significant. Local deworming spillover effects have a consistent sign in all of these cases, and are significant for domestic employment among females (-0.435, s.e. 0.180). Not surprisingly given these shifts, a somewhat larger proportion of treatment group wage earners live in urban areas.

Manufacturing jobs tend to be quite highly paid, with average real monthly earnings of 5,311 Shillings (roughly US\$68), compared to casual labor (2,246 Shillings) and domestic services (3,047 Shillings). Manufacturing jobs are also characterized by somewhat longer work weeks than average

at 53 hours per week. A decomposition along the lines of Oaxaca (1973) indicates that over 90% of the increase in labor earnings for the treatment group, and nearly a third of the increase in hours worked, can be explained by the sectoral shifts documented in Table 8. 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 sectoral shifts.

4.7 Impacts on self-employment and agricultural outcomes

As with wage earning, there is no evidence of differential selection into self-employment or own agricultural work among deworming treatment individuals (Table 9, Panels A and B), simplifying the interpretation of the estimated impacts in these subsamples. Unfortunately, reliable measures of productivity are much harder to generate among the self-employed and those working on their own farms relative to wage work, making it 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, 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.

4.8 Instrumental Variable Estimates

We next go beyond intention to treat estimates and generate instrumental variable estimates of the impact of years of moderate-heavy worm infections on later outcomes. The first stage results are presented in Table 3 (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 results are broadly similar to the ITT estimates in terms of statistical significance levels, although magnitudes and interpretation differ (Appendix Table A5). 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) and earnings in the most recent month worked by 26.6 log points (s.e. 10.8). As mentioned above, in our view these estimates are likely to overstate the true impacts of eliminating a moderate-heavy worm infection for one year since the worm infection measures are taken with a considerable lag after treatment and thus understate the true number of infection eliminated due to rapid reinfection.

5. 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 approximately 21 to 29%, 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. 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 the external validity of both findings.

The main implication of this paper is that childhood health investments like school-based deworming can substantially boost adult earnings. It goes without saying that deworming alone, and its associated increase in earnings, cannot make more than a small dent in the large gap in living standards between poor African countries like Kenya and the world's rich countries. Yet that obvious point does not make deworming any less attractive as a public policy option given its extraordinarily

high social rates of return, and the fact that boosting income by one quarter would have major welfare impacts for households living near subsistence.

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

	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)	Kolmogorov- Smirnov p-value
Age (1998)	11.9 (2.6)	11.9 (2.6)	12.0 (2.6)	-0.04 (0.11)	0.258
Grade (1998)	4.23 (1.68)	4.22 (1.70)	4.25 (1.66)	-0.03 (0.05)	0.450
Female	0.470	0.469	0.473	-0.004 (0.019)	--
Assignment to the deworming treatment group	0.678	1	0	--	--
Years of assigned deworming treatment, 1998-2003	3.31 (1.82)	4.09 (1.52)	1.68 (1.23)	2.41 ^{***} (0.08)	--
Primary school located in Budalangi division	0.370	0.364	0.381	-0.017 (0.137)	--
Population of primary school	476 (214)	494 (237)	436 (146)	58 (54)	0.307
School average test score (1996)	0.029 (0.427)	0.024 (0.436)	0.038 (0.406)	-0.013 (0.109)	0.310
Total treatment (Group 1, 2) primary school students within 6 km	3,180 (917)	3,085 (845)	3,381 (1,022)	-296 (260)	0.206
Total primary school students within 6 km	4,709 (1,337)	4,698 (1,220)	4,732 (1,555)	-34 (389)	0.119

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

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

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

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

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

Table 3: Impacts on health, wellbeing and nutritional outcomes

Dependent variable	Comparison group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming treatment indicator	Coefficient estimate (s.e.) on deworming Treatment school pupils within 6 km (in '000s), demeaned
Panel A: Health outcomes during 1999-2001			
Moderate-heavy worm infection (1999, 2001 parasitological surveys)	0.321 (0.467)	-0.245*** (0.030)	-0.075*** (0.026)
Hemoglobin (Hb) level (1999, 2001 parasitological survey samples)	126.1 (14.7)	1.03 (0.81)	0.91 (0.96)
Falls sick often (self-reported), 1999	0.154 (0.361)	-0.037** (0.015)	0.001 (0.014)
Malaria in the last week (self-reported), 1999	0.218 (0.413)	-0.019 (0.017)	-0.018 (0.018)
Panel B: Health, wellbeing and nutritional outcomes, KLPS-2 (2007-09)			
Self-reported health “very good”	0.673 (0.469)	0.041** (0.018)	0.028 (0.022)
Self-reported currently “very happy”	0.673 (0.469)	0.020 (0.018)	0.028 (0.023)
Index of wellbeing (0 to 1)	0.831 (0.290)	0.018 (0.012)	-0.013 (0.012)
Body mass index (BMI = Weight in kg / (height in m) ²)	27.2 (1.3)	0.024 (0.044)	0.064 (0.053)
Height (cm)	167.3 (8.0)	-0.12 (0.26)	-0.39 (0.33)
Respondent health expenditures (medicine, in/out-patient) in past month (KSh)	119.2 (389.9)	91.1*** (30.0)	40.7 (55.9)

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. Self-reported currently “very happy” takes on a value of one if the answer to the question “Taking everything together, would you say you are somewhat happy, very happy or not happy?” is “very happy”, and is zero otherwise. The underlying index of well being takes on a value of 0 to 4 where 4 implies answering no to all of the following four questions: “In the past week, have you felt tense, nervous or worried?” “In the past week have you generally not enjoyed your daily activities?” “In the past week have you felt more unhappy than usual?” “In the past week have you found it difficult to make decisions?” Indicator of no major health problem since 1998 takes on a value of one if they answer no to the following question “Have you experienced any major health problems that seriously affected your life or work, since 1998?”.

Table 4: Impacts on schooling and test score outcomes

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

Notes: Each row is from a separate OLS regression analogous to equation 4 using the full KLPS-2 sample. 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 5: 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 (0.64)	0.096 ^{***} (0.028)	0.080 ^{***} (0.023)	5,083
Panel B: Labor supply				
Hours worked (for wages, self-employed, agriculture) in last week	15.2 (21.9)	1.76 [*] (0.97)	1.54 (1.16)	5,084
Hours worked (for wages, self-employed, agriculture) in last week, among those with hours worked > 0	23.0 (23.4)	2.40 ^{**} (1.16)	2.75 ^{**} (1.36)	3,514
Hours worked (for wages) in the last week, among those with hours worked > 0	42.2 (24.7)	5.19 [*] (2.74)	6.60 ^{**} (2.93)	693
Days of work missed due to poor health (among those working for wages), past month (negative binomial)	1.46 (2.99)	-0.499 ^{**} (0.235)	-0.337 (0.305)	718
Hours worked (as self-employed) in last week, among those with hours worked > 0	33.9 (25.7)	8.9 ^{***} (3.0)	8.0 ^{***} (3.0)	583
Hours worked (in agriculture) in last week, among those with hours worked > 0	9.5 (9.1)	0.48 (0.53)	-0.75 (0.48)	2,829
Indicator for hours worked > 0 (for wages, self-employed, agriculture) in last week	0.662 (0.473)	0.010 (0.022)	-0.007 (0.025)	5,084

Notes: Each row is from a separate OLS regression analogous to equation 4, except the negative binomial. The household consumption expenditure per capita results trim the top 2% of households. 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 6: Deworming impacts on labor earnings (2007-2009)

	Dependent variable:							
	Ln(Total labor earnings, past month)				Total labor earnings, past month (in Kenya Shillings)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deworming Treatment indicator	0.193** (0.077)	0.187** (0.076)	0.253*** (0.093)	0.277*** (0.104)	611** (285)	627** (306)	780* (417)	906* (504)
Deworming Treatment pupils within 6 km (in '000s), demeaned			0.199 (0.168)	0.194 (0.170)			451 (740)	424 (740)
Total pupils within 6 km (in '000s), demeaned			-0.098 (0.127)	-0.094 (0.129)			-201 (575)	-179 (571)
Group 2 school indicator				-0.060 (0.099)				-327 (449)
Cost sharing school (in 2001) indicator	-0.104 (0.085)	-0.139 (0.094)	-0.159* (0.088)	-0.154* (0.090)	-390 (370)	-540 (425)	-584 (410)	-557 (409)
Additional controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
R ²	0.064	0.176	0.182	0.183	0.060	0.125	0.126	0.127
Observations	710	710	710	710	710	710	710	710
Mean (s.d.) in the control group	7.86 (0.88)	7.86 (0.88)	7.86 (0.88)	7.86 (0.88)	3,739 (3,744)	3,739 (3,744)	3,739 (3,744)	3,739 (3,744)

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

Table 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 (0.88)	0.253 ^{***} (0.093)	0.199 (0.168)	710
Ln(Total labor earnings, past month) – top 1% trimmed	7.83 (0.85)	0.269 ^{***} (0.092)	0.237 (0.161)	698
Ln(Total labor earnings, past month) – with all gender-age fixed effects	7.86 (0.88)	0.270 ^{***} (0.093)	0.197 (0.159)	710
Ln(Wage = Total labor earnings / hours, past month)	2.82 (0.96)	0.165 (0.117)	0.012 (0.160)	625
Indicator for worked for wages (or in-kind) in last month	0.166 (0.372)	-0.015 (0.018)	-0.002 (0.020)	5,081
Panel B: Wage earner since 2007 subsample				
Ln(Total labor earnings, most recent month worked)	7.88 (0.91)	0.211 ^{***} (0.072)	0.170 (0.116)	1,175
Indicator for worked for wages (or in-kind) since 2007	0.244 (0.430)	0.000 (0.021)	0.040 (0.024)	5,081
Panel C: Full sample				
Ln(Total labor earnings, past month) – Heckman selection correction	7.86 (0.88)	0.285 ^{***} (0.108)	0.148 (0.170)	5,082
Total labor earnings, past month, earnings=0 for non- earners	619 (2,060)	27 (81)	-17 (97)	5,084
Total labor earnings, past month – 95 th percentile (quantile regression), earnings=0 for non-earners	619 (2,060)	290 ^{**} (117)	123 (140)	5,084

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: Deworming impacts on employment sector and occupation

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

Notes: The sample used here includes all individuals surveyed in the KLPS2 who report working for pay (with earnings greater than zero) at the time of the survey. Each row is from a separate OLS regression analogous to 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.

Table 9: Deworming impacts on other economic outcomes

Dependent variable	Control group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming Treatment indicator	Coefficient estimate (s.e.) on deworming Treatment pupils within 6 km (in '000s), demeaned	Obs.
Panel A: Self-employed profits, hours and employees				
Mean effect size (three profits measures, and total employees hired)	0.000 (1.000)	0.175** (0.089)	0.014 (0.097)	555
Total self-employed profits (self-reported) past month (among those >0)	1,766 (2,619)	343 (306)	-151 (320)	585
Total self-employed profits (constructed) past month (among those >0)	1,535 (6,524)	1,211 (1,091)	2,088 (1,886)	595
Total self-employed profits (self-reported) past year (among those >0)	12,193 (17,346)	1,952 (2,286)	-1,753 (2,590)	566
Total employees hired (excluding self), among the self-employed	0.188 (0.624)	0.446 (0.361)	0.044 (0.492)	633
Indicator for self-employed earnings in last month	0.100 (0.300)	0.015 (0.012)	0.004 (0.011)	5,083
Panel B: Agricultural work, sales, hours and practices				
Total value (KSh) of crop sales past year (if farm household)	576 (2458)	-81 (148)	-460** (206)	3,758
Uses “improved” agricultural practice (if farming household)	0.310 (0.462)	0.032 (0.026)	0.005 (0.024)	3,766
Indicator for respondent did agricultural work in last week	0.531 (0.499)	-0.010 (0.025)	0.005 (0.031)	5,080

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.

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

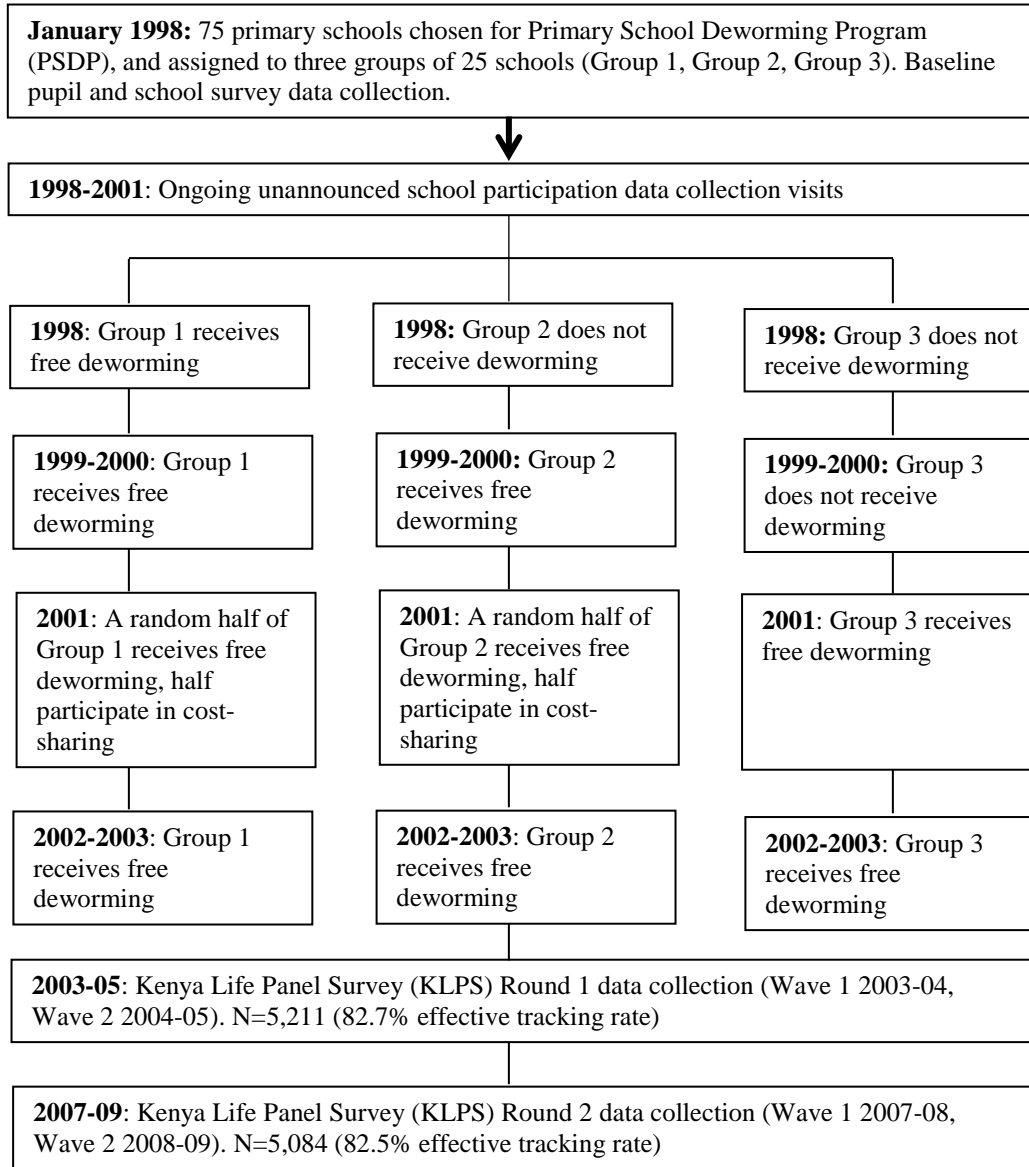
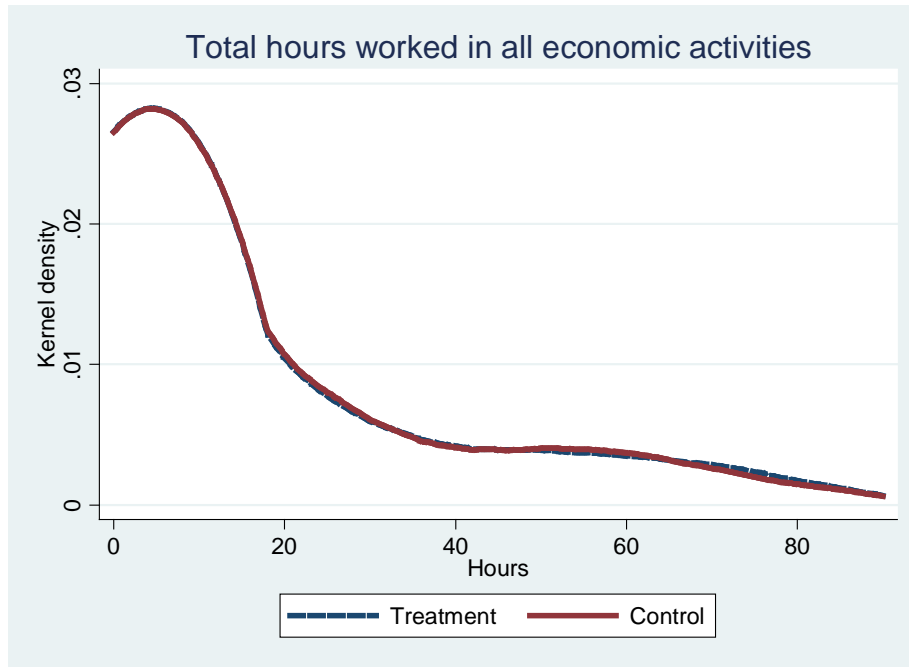
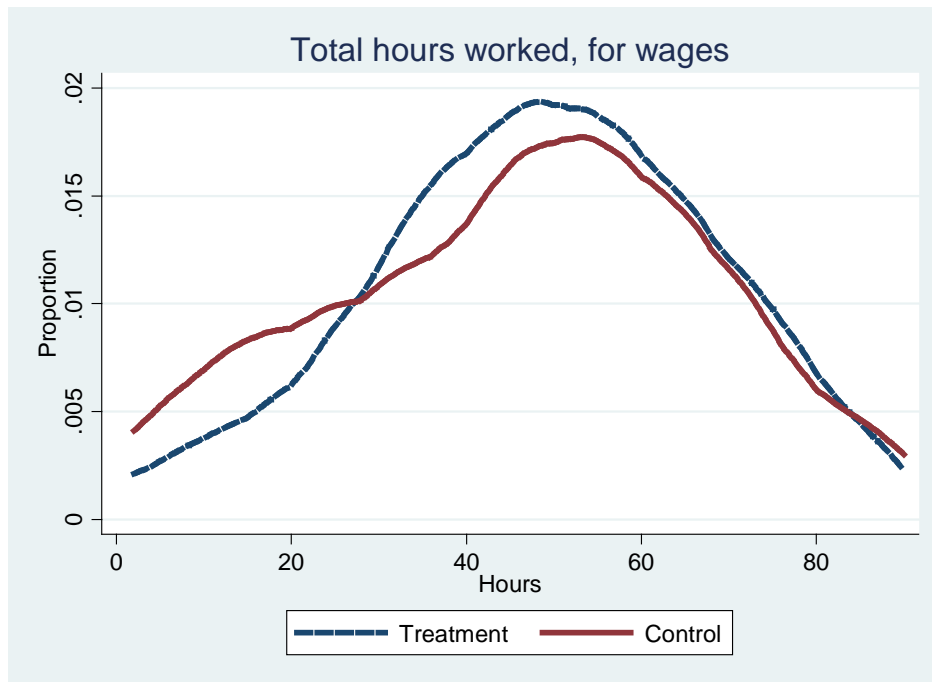


Figure 2:

Panel A: The distribution of hours worked in the last week (among those working for wages, self-employment or in agriculture), deworming treatment versus control

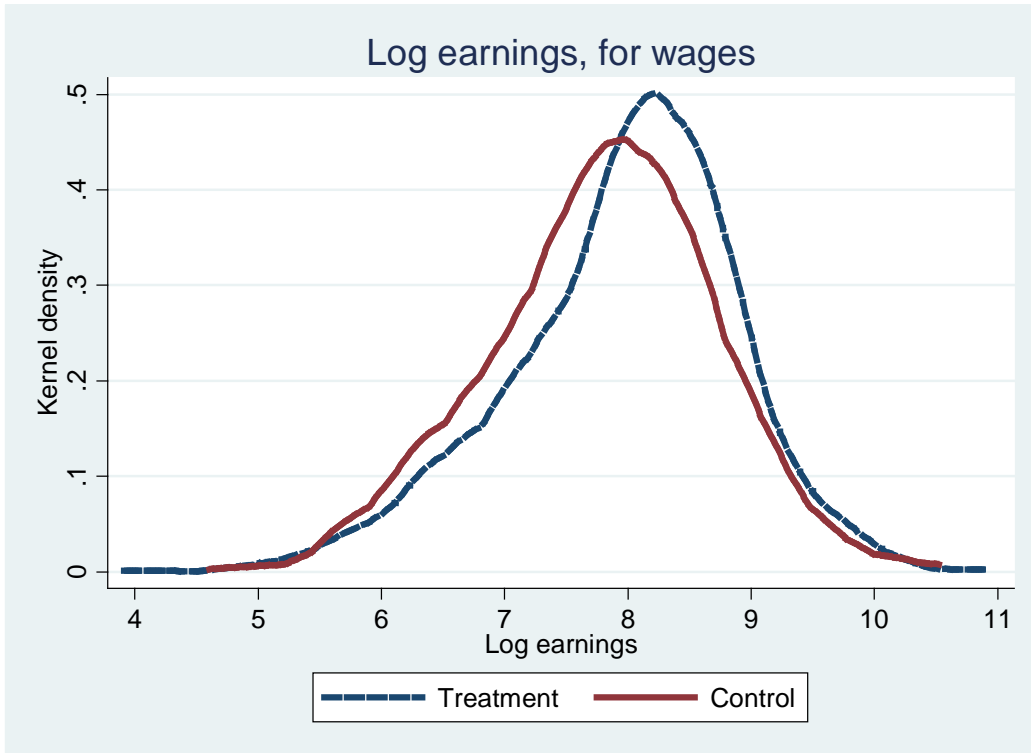


Panel B: The distribution of hours worked in the last week (among those working for wages), deworming treatment versus control



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

Figure 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)¹⁶, 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

¹⁶ 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.¹⁷ 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 4. Then moving onto the next zone, the first school in that stratum was assigned to Group 1, the next school to Group 2, 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.

¹⁷ 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 (1.8)	13.2 (1.9)	13.0 (1.7)	0.204 (0.391)	0.202
Grade (1998)	4.87 (1.61)	4.86 (1.63)	4.91 (1.57)	-0.054 (0.141)	0.445
Female	0.233 (0.423)	0.209 (0.407)	0.280 (0.450)	-0.071 (0.045)	--
Primary school located in Budalangi division	0.412 (0.493)	0.430 (0.496)	0.378 (0.486)	0.052 (0.144)	--
Population of primary school	477 (218)	504 (246)	425 (136)	78 (56)	0.342
School average test score (1996)	-0.010 (0.408)	-0.027 (0.415)	0.024 (0.391)	-0.050 (0.106)	0.273
Total treatment (Group 1, 2) primary school students within 6 km	3206 (908)	3115 (802)	3383 (1064)	-267 (283)	0.172
Total primary school students within 6 km	4731 (1332)	4731 (1173)	4730 (1598)	1.72 (420)	0.342

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 (2.2)	12.4 (2.2)	12.5 (2.2)	12.7 (2.1)	-0.37*** (0.09)
Grade (1998)	4.26 (1.69)	4.24 (1.68)	4.24 (1.70)	4.32 (1.70)	-0.105 (0.063)
Female	0.486 (0.500)	0.461 (0.499)	0.495 (0.501)	0.535 (0.499)	-0.072*** (0.016)
Assignment to the deworming treatment group	0.675 (0.468)	0.681 (0.466)	0.665 (0.473)	0.664 (0.472)	0.006 (0.020)
Group 1 school	0.357 (0.479)	0.355 (0.479)	0.354 (0.479)	0.362 (0.481)	-0.015 (0.025)
Group 2 school	0.318 (0.466)	0.326 (0.469)	0.311 (0.463)	0.302 (0.459)	0.021 (0.021)
Years of assigned deworming treatment during 1998-2003	3.29 (1.83)	3.32 (1.82)	3.25 (1.83)	3.22 (1.85)	0.069 (0.090)
Primary school located in Budalangi division	0.380 (0.486)	0.361 (0.480)	0.389 (0.488)	0.420 (0.494)	-0.067*** (0.023)
Population of primary school	484 (221)	480 (223)	465 (178)	496 (222)	-20** (8)
School average test score (1996)	0.043 (0.439)	0.035 (0.434)	0.023 (0.416)	0.066 (0.453)	-0.026 (0.021)
Total treatment (Group 1 and 2) primary school students within 6 km	3171 (910)	3182 (915)	3174 (918)	3149 (900)	30 (36)
Total primary school students within 6 km	4678 (1340)	4713 (1342)	4691 (1335)	4602 (1334)	93 (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 (in '000s), demeaned	N/A	0.011	0.014	0.024	0.026	0.015	0.008	0.016	0.034	-0.011	0.138
		(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 (in '000s), demeaned	0.019	-0.008	-0.019	0.009							0.044
	(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** (0.103)	0.297* (0.152)	0.255*** (0.092)	0.002 (0.024)	-0.018 (0.019)	-0.016 (0.018)	3.40** (1.40)	1.53 (1.10)	1.79* (0.96)
Female	-0.473*** (0.140)			-0.128*** (0.022)			-3.43** (1.68)		
Female * Treatment	0.121 (0.195)			-0.035 (0.027)			-3.41* (1.98)		
Grades 5-7 in 1998		0.497*** (0.164)			0.105*** (0.023)			7.46*** (1.71)	
Grades 5-7 * Treatment		-0.069 (0.186)			0.004 (0.028)			0.43 (2.02)	
Moderate-heavy worm infection rate at the zonal level (1998), demeaned			-0.048 (0.084)			-0.035* (0.018)			-0.70 (0.85)
Moderate-heavy infection rate * Treatment			0.071 (0.078)			0.028* (0.015)			0.81 (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 (0.88)	7.86 (0.88)	7.86 (0.88)	0.166 (0.372)	0.166 (0.372)	0.166 (0.372)	15.2 (21.9)	15.2 (21.9)	15.2 (21.9)

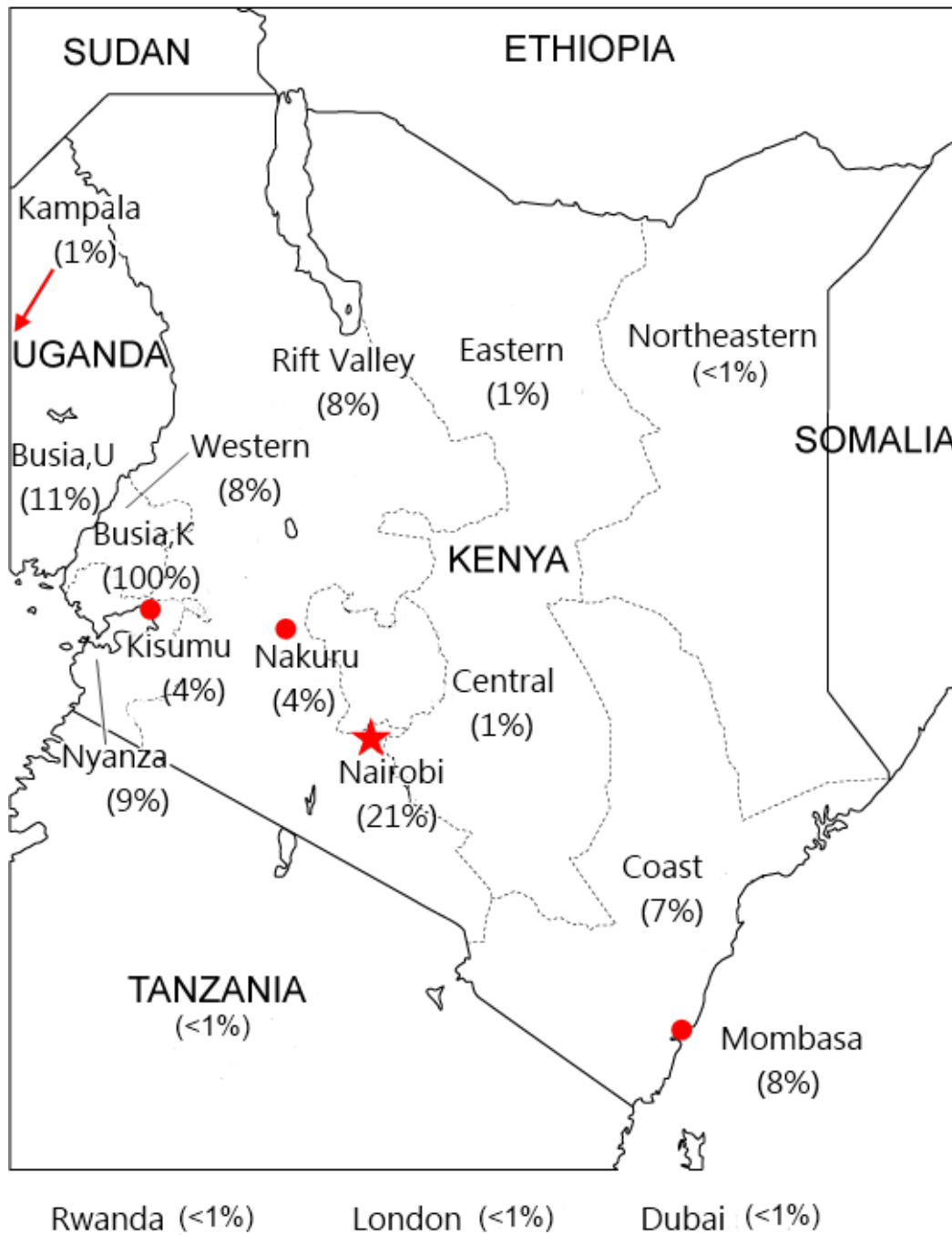
Notes: The sample used in columns (1)-(3) includes all individuals surveyed in the KLPS2 who report positive labor earnings at the time of survey and include data for the relevant dependent variable. The sample used in columns (4)-(6) includes all surveyed individuals with non-missing information on 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 Table A5: The impact of eliminating moderate-heavy worm infections on economic outcomes, two-sample instrumental variable estimates

Dependent variable	Control group variable mean (s.d.)	IV-2SLS coefficient estimate (s.e.) on predicted years of moderate-heavy worm infection	Obs.
Self-reported health “very good”, 2007-2009	0.673 (0.469)	-0.093 ^{***} (0.030)	5070
Total years enrolled in school, 1998-2007	6.69 (2.97)	-0.229 (0.203)	5037
Number of meals eaten yesterday	2.16 (0.64)	-0.099 [*] (0.047)	5083
Hours worked (for wages, self-employed, agriculture) in last week	15.2 (21.9)	-3.14 ^{**} (1.24)	5084
Hours worked (for wages, self-employed, agriculture) in last week, among those with hours worked > 0	23.0 (23.4)	-3.23 [*] (1.59)	3514
Hours worked (for wages or in-kind) in the last week	42.2 (24.7)	-7.96 ^{**} (3.65)	693
Ln(Total labor earnings, past month)	7.86 (0.88)	-0.380 ^{**} (0.133)	710
Ln(Total labor earnings, most recent month worked)	7.88 (0.91)	-0.266 ^{**} (0.108)	1175
Ln(Wage = Total labor earnings / hours, past month)	2.82 (0.96)	-0.175 (0.154)	625

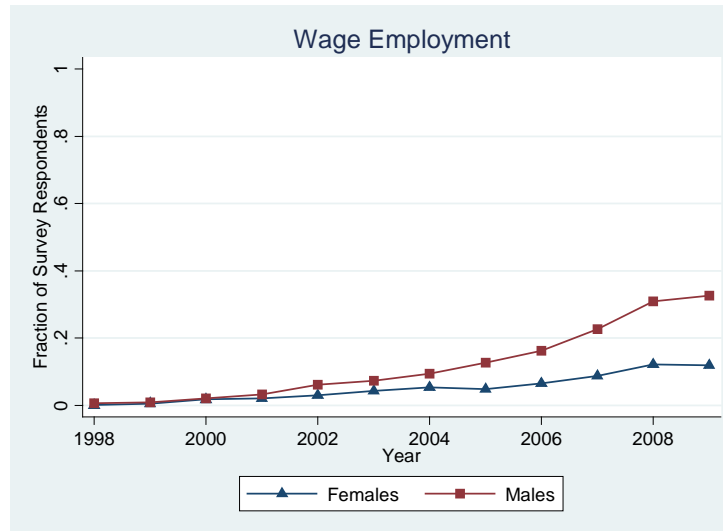
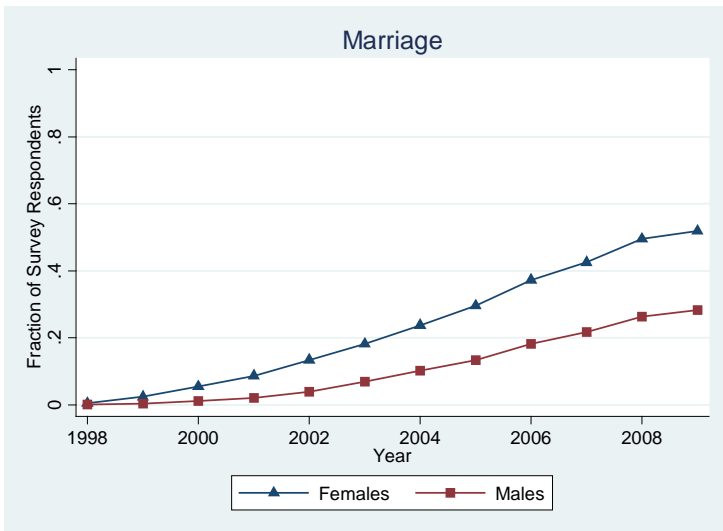
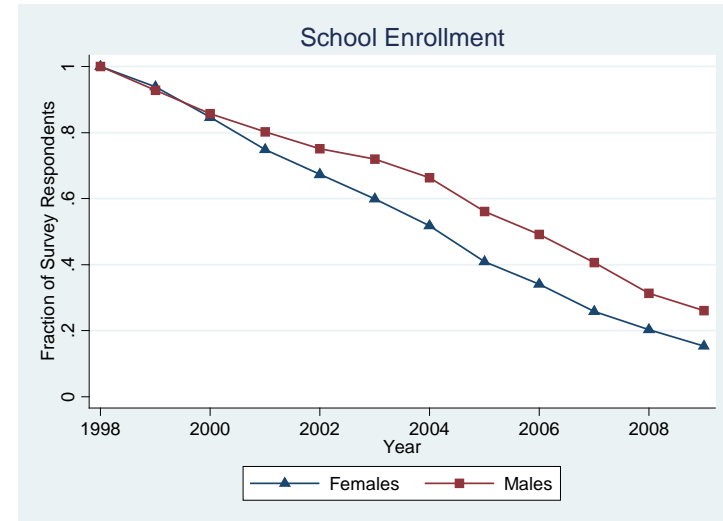
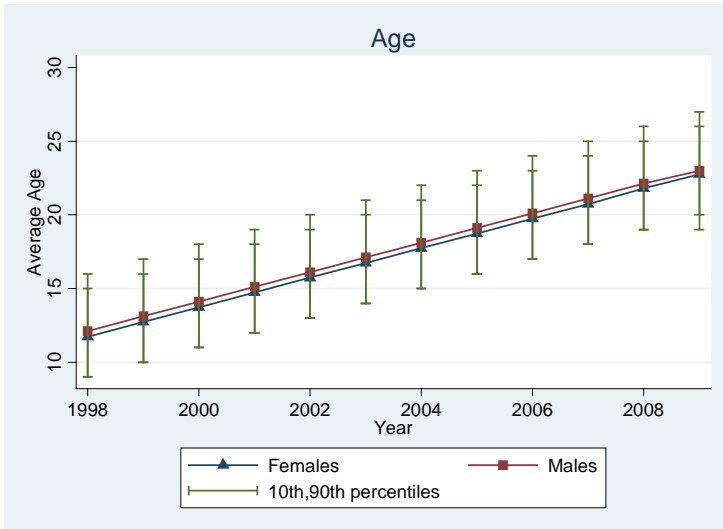
Notes: Two-sample instrumental variable estimates. Standard errors are bootstrapped and clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview, 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. 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.

Supplementary Appendix Figure A1: Migration residential location map

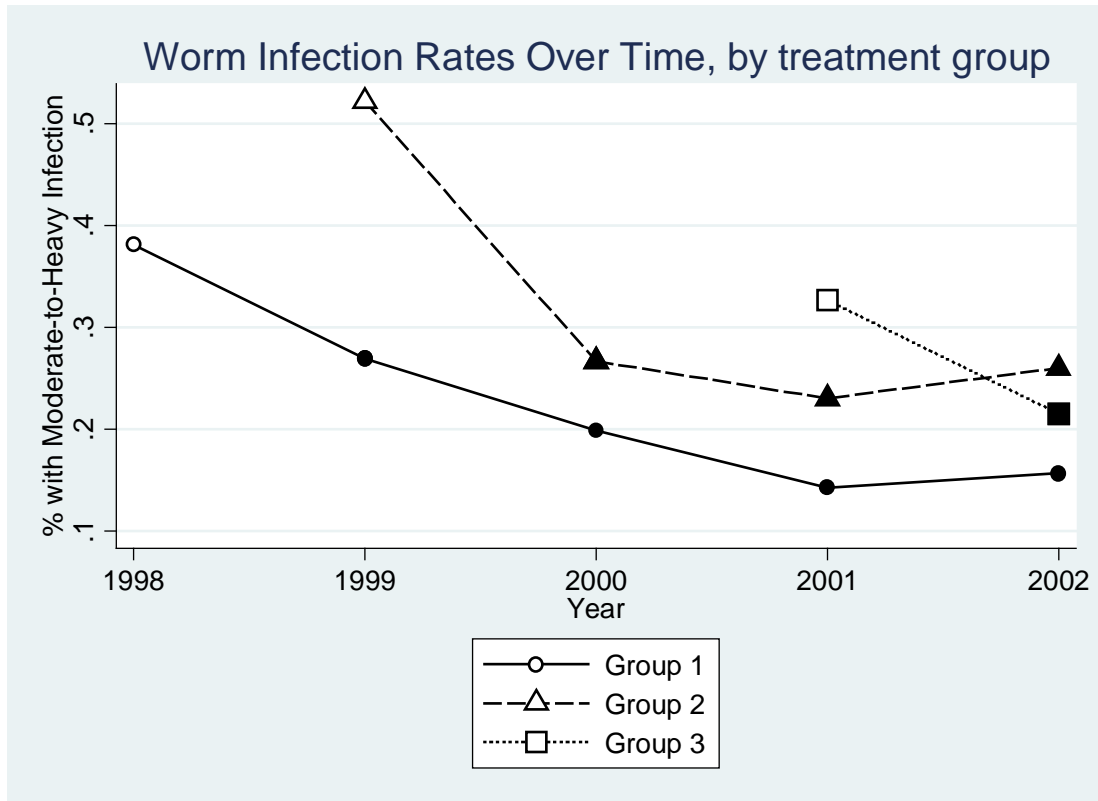


Notes: Percentages sum to greater than one, since they capture residential location (for at least four consecutive months) at any point during 1998-2009.

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. 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.