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

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First version: October 2010
This version: September 2011

We use data from a survey of young Kenyan adults who participated in a deworming program as children to calibrate a version of the Grossman (1972) model, in which investments in health increase future endowments of healthy time. Mean hours worked increase by 17% in the treatment group among those not still in school, or 3.1 more hours each week on a base of 18.5. Treatment respondents report eating an average of 0.1 additional meals per day on a base of 2.2. There is evidence of externalities from deworming in both work hours and meals eaten. Gains are concentrated outside of traditional agriculture among small business owners and those working for wages. Wage earners in the treatment group earned over 20% more, with manufacturing employment tripling. These results suggest health improvements may increase labor supply and facilitate structural transformation. A calibration of the model combining data on the impacts of deworming and the price responsiveness of deworming take-up suggests that fully subsidizing deworming yields greater welfare than partial subsidies or laissez-faire.

[*] Acknowledgements: Chris Blattman, Hana Brown, Lorenzo Casaburi, Lisa Chen, Garret Christensen, Lauren Falcao, Francois Gerard, Eva Arceo Gomez, Jonas Hjort, Maryam Janani, Anne Karing, Andrew Fischer Lees, Jamie McCasland, Owen Ozier, Martin Rotemberg, Changcheng Song, Sebastian Stumpner, Paul Wang, and Ethan Yeh provided excellent research assistance on the KLPS project. We thank Michael Anderson, Jere Behrman, David Card, Alain de Janvry, Erica Field, Fred Finan, Michael Greenstone, Adriana Lleras-Muney, Steve Luby, Isaac Mbiti, Mark Rosenzweig, T. Paul Schultz, Jim Smith, John Strauss, Alix Zwane and seminar participants at U.C. Berkeley, USC, Harvard, the J-PAL Africa Conference, the Pacific Conference on Development Economics, UCSF, the Gates Foundation WASH Convening in Berkeley, Yale, Hamilton College, RAND, CGD, the World Bank, Maseno University, and the NBER Labor Studies group for helpful suggestions. We gratefully acknowledge our NGO collaborators (International Child Support and Innovations for Poverty Action Kenya), and funding from NIH grants R01-TW05612 and R01-HD044475, NSF grants SES-0418110 and SES-0962614, the World Bank, the Social Science Research Council, and the Berkeley Population Center. All errors remain our own.

1. Introduction

We use data on the impact of child deworming on adult outcomes to calibrate a model of health investment along the lines of Grossman (1972), in which current health investments expand future endowments of healthy time. Miguel and Kremer (2004) found that children who were dewormed spend substantially more time in schooling. We follow participants a decade later, when most were 19 to 26 years old, and find that deworming improves self-reported health and increases mean hours worked each week by 17% from a base of 18.5 hours. Living standards improve as well, with treatment respondents eating one-tenth of a meal more per day. Both effects are found not only in the treatment group, but among neighbors, consistent with substantial positive externalities from reduced disease transmission. A calibration of the model combining these results with estimates of the responsiveness of deworming drug take-up to price from Kremer and Miguel (2007) suggest that full subsidies for deworming generate greater social welfare than either zero or partial subsidies over a wide range of plausible estimates of the deadweight loss of taxation. The analysis is based on a new longitudinal data set with an effective tracking survey rate of 84% over roughly ten years.

Decomposing the impact suggests the availability of more healthy hours has differential effects across sectors. Among those engaged only in agriculture, there is a small increase in hours worked, consistent with theories in which the marginal product of labor in traditional agriculture is small (Lewis 1954). There is some evidence of small increases in the use of improved agricultural practices (e.g., fertilizer) and a shift to cash crops. In contrast, among those operating small businesses or working for wages, average work hours increase by five hours in the treatment group, on a base of 45 hours. Earnings increase among wage workers increase by more than 20%, and although estimates are imprecise, point estimates suggest higher profits for owners of small non-agricultural businesses. Work days lost to illness fall by a third among wage earners. Treatment group members are more than twice as likely to work in manufacturing, and less likely to do casual labor or work in domestic service, consistent with the hypotheses that the ability to do regular, full-time work allows people to get better jobs. Manufacturing jobs are among the most demanding in our dataset, with long average work weeks. In a Oaxaca-style decomposition, these shifts in employment occupation account for nearly all of the earnings gains in the treatment group and much of the increase in hours worked.

The increase in work hours we document may shed light on an understudied issue in development economics, namely, the determinants of labor supply. While there is considerable discussion about how work hours in developed countries differ with tax rates or labor market institutions (Prescott 2004, Costa 2000), differences in labor hours associated with economic development across space and time have been less studied, despite the fact that they are often larger than differences across wealthy countries.

Many historians see a move to a work life governed by long, regular hours and factory discipline as an important part of the industrial revolution (Clark 1994). While factory workers in less developed countries put in long work days,[1] work hours are often low in some rural low-income contexts. For example, in Sahelian Burkina Faso, Fafchamps (1993) finds that people only work an average of two to three hours per day on their farms. One potential explanation for low work hours is that the marginal productivity of additional labor in agriculture is low (Lewis 1954). Fafchamps (1993) argues that the low levels of labor supply he observed among peasant farmers are due to low marginal products of labor in the traditional rain-fed agricultural sector, with farmers in rainy areas working nearly twice as many hours as those in drier areas. Others have advanced cultural theories. Colonial observers advanced racial or ethnic theories of Africans' "laziness", love of leisure and lack of ambition (see Abudu 1986 for a discussion of

[1] http://www.nytimes.com/2008/01/05/business/worldbusiness/05sweatshop.html

colonial accounts in West Africa). A growing body of work in labor economics emphasizes cultural (though not racial) differences across groups as key drivers of labor supply decisions (Fernandez and Fogli, 2009). Finally, some have advanced efficiency wage stories in which low incomes limit investments in nutrition and health and this in turn leads to lower labor supply (Dasgupta and Ray 1986).

Our finding that the respondents who received health investments as children work significantly more hours as adults echoes existing evidence on the link between disease and work absenteeism in other African settings (Schultz and Tansel 1997), and is consistent with other findings that moderate increases in morbidity affect labor supply (e.g, Ichino and Moretti, 2009).

The results also contribute to the debate on government subsidies for prevention of infectious disease. While some child public health investments, such as immunization, are routinely provided for free by governments, others, such as water treatment, and deworming are not. There has been a lively debate over subsidies, with evidence accumulating that many people who will utilize these measures when they are free will not use them when they must pay (e.g., Kremer and Miguel, 2007, Kremer and Holla 2009, Cohen and Dupas 2010, Ashraf, Berry and Shapiro 2010, Dupas 2011, Kremer, Snyder and Williams 2011). However, to understand whether public investments are worthwhile, it is also important to know both the direct and externality impact of these investments.

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.[2] Yet assessing the long-

[2] TheINCAP experiment in Guatemala (Hodinott *et al.* 2008, Maluccio *et al.* 2009, Behrman *et al.* 2009) provided nutritional supplementation to two villages while two others served as a control, and finds gains in male wages of one third, improved cognitive skills among both men and women, and positive intergenerational effects on the nutrition of beneficiaries' children. Beyond the small sample size of four villages, a limitation of the INCAP studies

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. Many existing studies track production within a firm, examining the impact of contemporaneous health on plantation workers' productivity, for example (Fox et al. 2004). Our evidence suggests this approach may miss important gains, in particular by missing out entirely on how health investments affect shifts across employment occupations and sectors.

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-age children can have important effects. These gains do not appear to be mediated mainly by improved cognitive ability but rather by increased hours, both in work and in school, with its potential impact on learning and non-cognitive outcomes.

Our results also contribute to the debate over the flexibility of labor markets in developing countries. Many have argued that less developed country labor markets are inflexible, consisting of a formal sector with institutionally determined wages in which jobs are rationed (and allocated through a mix of personal connections and credentialism), as well as a large informal sector in which people queue for formal sector jobs (Harris and Todaro 1970). In this model, an increase in individual human capital or labor supply would not necessarily translate into greater manufacturing employment. Our finding that an investment in human capital (without a corresponding increase in years of completed education) leads to a tripling of manufacturing employment suggests far greater labor market flexibility than is commonly imagined. The finding of much larger gains in work hours

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is their 40% attrition rate over the 35 years of follow-up surveys. Other studies have studied long-run economic impacts of child health, including effects of war-induced famine in Zimbabwe (Alderman *et al.,* 2006a) and economic shocks driven by rainfall variation in Indonesia (Maccini and Yang, 2009). Other noteworthy micro-empirical contributions on nutrition, health and productivity include Glewwe *et al.* (2001), Alderman *et al.* (2003), Schultz (2005), Jukes et al. (2006), Alderman (2007), Thomas *et al.* (2008), and Pitt, Rosenzweig and Hassan (2011) Related U.S. work includes Currie et al. (2002), Currie (2009), Smith (2009), and Case and Paxson (2010).

in wage employment than in traditional agricultural work is consistent with the hypothesis that land and labor markets in agriculture are highly imperfect, but that resources are allocated more efficiently in manufacturing and other non-agricultural work.

The rest of the paper is organized as follows. Section 2 presents a simple model of health as human capital investment related to Grossman (1972). Section 3 discusses the Kenyan context, the deworming project, and the survey. Section 4 lays out the estimation strategy and describes the impacts of deworming on health, education, and labor market outcomes. Section 5 uses the data on price responsiveness and deworming impacts to calibrate the model, and finds that full subsidies for deworming yield greater welfare than partial or no subsidies. The final section concludes, discussing external validity and implications for research and policy.

2. A model of health investment with spillovers (preliminary)

In section 2.1 we first describe a framework related to Grossman's (1972) model of health capital and discuss the relationship between health investments, endowment of healthy time, and work hours, as well as optimal health investment subsidies in the presence of externalities. In 2.2 we extend the model to consider economic sectors characterized by land and labor market imperfections, discuss the work hours response to health investments, and characterize patterns of mobility across sectors.

2.1 Health investment, work hours and deworming subsidies

In the classic Grossman (1972) model, better health status increases "the total amount of time [one] can spend producing money earnings and commodities" (p. 224). We consider a variant of this model in which health investments may lead to increased time endowment of healthy time not just for the individual but also for neighbors through an epidemiological externality, and discuss how optimal subsidies depend on the externality and direct benefit of the health investment, the responsiveness of health behavior to price, and the deadweight loss of taxation. We will then calibrate the model using the empirical estimates in the rest of the paper

Suppose there are *N* individuals in an area, and in each period *t* people can spend their time working (l_t) for income *Y(l)*, or in leisure. Income can be spent on a consumption good, with the amount of consumption denoted c_t. In the initial period (denoted period 0) people can also purchase deworming medicine. There is no saving or borrowing. Deworming involves paying a price p for a competitively-provided drug and incurring a one-time disutility $d_i \sim f$. Abusing notation, let $F(d)$ be a function representing the fraction of individuals with disutility less than or equal to d. Deworming increases an individual's health time endowment by *x* in future periods. It also creates a positive externality for everyone nearby, increasing their time by γ. Each individual's endowment of healthy time in period *t* is $E_t = 1 + (N-1)\gamma\phi - x + D_i x$, where ϕ is the fraction of the rest of the population who have taken deworming medicine, 1 is the amount of time if everyone else was untreated, and D_i is an indicator variable for having previously dewormed. People are infinitely lived and maximize the discounted sum of utility using discount rate δ.

Each individual *i* has Cobb-Douglas preferences over consumption and leisure at time *t*,

(1) $$U_t(c,l) = Y(l_t)^{\alpha}(E_t - l_t)^{1-\alpha} - D_{it}d_i$$

where E_t is their stock of healthy time and D_{it} is an indicator for individual *i* having taken the medicine in period *t*, and therefore maximize the value function:

(2) $$V_t(c,l) = \sum_{j=t}^{\infty} \delta^{j-t}\left(Y(l_j)^{\alpha}(E_t - l_j)^{1-\alpha} - D_{ij}d_i\right).$$

There are two potential sources of employment. One is the non-agricultural sector, where people earn a fixed wage *w*. The other is the agricultural sector, where due to imperfect land and labor markets, people are constrained to work on their fixed plot of land $\overline{K}_i$. Agricultural production is Cobb-Douglas with production parameter β, $y = A\overline{K}_i^{\beta}L^{1-\beta}$, and people keep their production. A simple way to describe how people choose industries is to assume that everyone has the same endowments and access to jobs, but have heterogeneous disutilities of moving from agriculture to get a wage job, $m_i \sim g$ (with *G(m)* representing the fraction of people with disutility less than or equal to m), independent of the disutility from deworming.

To start, assume that $\overline{K}_i$ and m_i are sufficiently low that there is full employment in the non-agricultural sector. Once we have set up the basic model of deworming choices, we will add in further frictions below.

Proposition 1: If there are competitive labor markets, so $Y(l_t) = \mathrm{wl_t}$, then the fraction of time spent working is α regardless of the wage and the time endowment in every period after the initial period.

Supplementary appendix A contains all proofs. Note that this implies that if we observe deworming increases work time by z, we can infer that deworming increases the endowment of healthy time by $\frac{1}{\alpha}z$.

Proposition 2: If there are competitive labor markets $Y(l_t) = \mathrm{wl_t}$, then the proportion of the population that deworms at a given price of deworming medicine $\widehat{P}$ is F$\left(\left(\frac{\delta}{1-\delta}x - \frac{\widehat{P}}{w}\right) * (\mathrm{w}\alpha)^{\alpha}(1-\alpha)^{1-\alpha}\right)$.

This comes from the fact that the utility gain from an increase in time of x in a given period is equivalent to the utility gain from a cash transfer of x*w, and is equal to $x(\mathrm{w}\alpha)^{\alpha}(1-\alpha)^{1-\alpha}$. As a result, a cash transfer of $\frac{wd_i}{(w\alpha)^{\alpha}((1-\alpha))^{1-\alpha}}$ increases agent *i*'s utility by exactly d_i. Let $\tilde{d}_i$. equal the cash transfer which therefore corresponds to the value of d_i.

It follows that take-up is increasing in benefits *x* and decreasing in costs *p*. A government seeking to increase take-up therefore can do so by subsidizing the cost of the medicine. Below we consider policy for a government that seeks to maximize social welfare (the sum of utility of all *N* members of the population) but only has access to distortionary taxation (or faces constraints leading to wasteful expenditures). Let *DWL* denote the loss associated with raising and spending one dollar of revenue. Furthermore, let $\overline{s_i}$ denote the disutility of deworming for someone who is indifferent to treatment at a price of $p - s_i$. That is to say, $\overline{s_i} = \left(\left(\frac{\delta}{1-\delta}x - \frac{p-s_i}{w}\right) * (\mathrm{w}\alpha)^{\alpha}(1-\alpha)^{1-\alpha}\right)$.

Proposition 3: Consider two different levels of subsidies, s_1 and s_2 where $s_2 > s_1$. If the government faces a deadweight loss (DWL), it prefers subsidy s_2 to s_1 if:

(3) $$\int_{md_i=\overline{s_1}}^{\overline{s_2}} \left(\frac{\delta}{1-\delta}(x + \gamma(N-1))w\right) dF(d_i) - (s_2 - s_1)DWL \int_{d_i=0}^{\overline{s_1}} dF(d_i) - \int_{m_i=\overline{s_1}}^{\overline{s_2}} [s_2 * DWL + p + \tilde{d}_i] dF(d_i) \geq 0.$$

In the absence of deadweight loss, the government can price deworming drugs to align private benefits with social benefits.

Corollary 1: If the government faces no deadweight loss from taxation, subsidizing the price of medicine by $\frac{\delta}{1-\delta} w\gamma(\mathrm{N}-1)$ maximizes welfare.

2.2 Responses in agriculture versus other sectors

People's deworming decisions, and therefore the cost-benefit analysis done by a benevolent government, change if there are additional externalities (such as those experienced by younger children in treatment areas, as in Ozier 2010), or if people are not optimally making deworming decisions, potentially because of incomplete markets, incomplete information, or behavioral frictions (such as inefficiently low take-up at positive prices, as in Ariely and Shampan'er 2006). Another source of concern is industry choices, since over half of the sample works in agriculture, where wages are not fixed or paid by the hour. Given a fixed plot of land in the production function described above, the marginal benefit of labor is declining in hours.

Proposition 4: In the absence of land and labor markets, agriculturalists will work a constant fraction of their time $\alpha\frac{1-\beta}{1-\alpha\beta} \leq \alpha$, regardless of their total stock of time or land.

This implies that hours in agriculture do not depend on the fixed stock of land. Furthermore, it implies that extra time is worth relatively less to those in agriculture due to the decline in returns to agricultural labor. Agriculturalists with an initial utility of *u* who receive *x* extra units of time have a new utility of $u\left(1+\frac{x}{E_t}\right)^{1-\alpha\beta}$, whereas if their wage were flat (as in the non-agricultural sector in our model) the new utility would be $u\left(1+\frac{x}{E_t}\right)$, which is larger. However, if one were to impute the change in total time endowment by taking the change in work hours and multiplying by $\frac{1}{\alpha}$, as one would in the non-agricultural sector, one would understate the benefits from extra time in agriculture.

Corollary 2. Suppose agent i works z hours more after an intervention, before which her utility was u. By assuming perfect labor markets, one would then calculate their new utility as $u\left(1+\frac{z}{\alpha E_t}\right)$, *which is below the true new utility of* $u\left(1+\frac{z}{\alpha E_t}\frac{1-\alpha\beta}{1-\beta}\right)^{1-\alpha\beta}$ if $\alpha < .5$.

Therefore, people will join the wage-earning sector if

$$(4) \arg\max_{T_t}\{\sum_{j=t}^{\infty} \delta^{j-t} (\alpha E_t)^{\alpha}(E_t - l_t)^{1-\alpha}\} - m_i >$$

$$\arg\max_{T_t}\{\sum_{j=t}^{\infty} \delta^{j-t} \left(A\bar{K}_i^{\ \beta} \left(\alpha \frac{1-\beta}{1-\alpha\beta} E_t\right)^{1-\beta}\right)^{\alpha} \left(E_t \left(1 - \alpha \frac{1-\beta}{1-\alpha\beta}\right)\right)^{1-\alpha}\}.$$

Proposition 5: Increasing deworming subsidies (weakly) increases participation in non-agricultural work.

If there were more than two sectors, and we were able to rank the sectors in terms of utility gains from extra time endowment, then we would expect that increased time would cause people to move out of the lower ranked sectors into the higher ranked sectors. For instance, if domestic workers benefited less from marginal amounts of extra time than those in manufacturing, we would expect movement from domestic work to manufacturing.[3]

3. Background

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

3.1 The context

The health problem we examine - intestinal worm infections — is among the world's most widespread, with roughly one in four people infected with hookworm, whipworm, roundworm, or schistosomiasis (Bundy 1994, de Silva *et al*. 2003). Although light worm infections are often asymptomatic, more intense infections can lead to lethargy, anemia and growth stunting. Treating worm infections (once to twice per year) can improve child appetite, growth and physical fitness (Stephenson *et al.* 1993), and reduce anemia (Guyatt *et al.* 2001, Stoltzfus *et al.* 1997). It also can

[3] Another consequence of the model is that if it were possible to know individuals' disutilities from moving out of agriculture, we would be able to predict not only who was most likely to switch sectors, but also who was more likely to take deworming medicine at a given price. We leave this for future research.

strengthen children's immunological response to other infections, potentially producing broader health benefits, such as reduced infection prevalence with *Plasmodium*, the malaria parasite (Kirwan *et al.* 2010). 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).

Previous work in our Kenyan sample shows that deworming treatment led to large medium-run gains in school attendance and health outcomes. Due to worms' infectious nature, sizeable externality benefits also accrued to the untreated within treatment communities and to those living near treatment schools (Miguel and Kremer 2004), as well as to younger children in the treatment communities and especially the younger siblings of the treated (Ozier 2010). Ozier (2010) shows that children who were 0-3 years old when the deworming program was launched and lived in the catchment area of a treatment school themselves show large cognitive gains ten years later, with average test score gains for those who were less than one year old when their communities received mass deworming treatment of 0.4 standard deviation units, equivalent to 0.5-0.8 of a year of school learning in his sample.

Bleakley (2007, 2010), examines the impact of a large-scale deworming campaign in the U.S. South during the early 20th century, 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 for wage workers.[4]

[4] 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

We study the impact of a school-based deworming program in a rural area of Kenya that is somewhat poorer than the Kenyan average.[5] Survey respondents attended originally rural schools and are now young adults mainly in their early 20s, with roughly one quarter still enrolled in school at the time of the survey. .Agriculture in Busia is rain-fed with two cropping seasons per year, and there are few draft animals. Unlike other parts of Kenya, where many farmers have turned to horticulture, growing vegetables for local markets, or flowers, coffee or tea for international markets, there is little intensification of production with only one percent of respondents (in the control group) growing cash crops, as discussed below.

The Lewis (1954) model assumption that young adults working in traditional family agriculture receive a share of output rather than their marginal product is plausible in this context. Markets for agricultural land and labor exist in this area but are not highly developed. Young adults have the option of staying on their parents farms or leaving home, to seek paid work, to start businesses, or, if female, to marry. Sons typically inherit land from their parents, with many receiving inter-vivos land transfers. Daughters co-locate with their husbands at marriage (Government of Kenya 1986). If adult children are entitled to share food if living at home, but not otherwise, then whether moving away creates a positive or negative externality for their family depends on how their marginal productivity at home compares to what they consume.

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gains of 15-27% on Costa Rican plantations after deworming. Weisbrod et al (1973) document small correlations between worm infections and labor productivity and test scores in St. Lucia. Bundy *et al*. (2009) argue that many studies understate deworming's benefits since they fail to consider externalities by using designs that randomize within schools; focus almost exclusively on biomedical criteria and ignore cognitive, education and income gains; and do not address sample 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.

[5] The 2005 Kenya Integrated Household Budget Survey found 62% of Busia households fall below the poverty line compared to 41% nationally, and 75% of Busia adults were literate versus 80% nationally. Given that Kenyan per capita income is somewhat above the sub-Saharan African average (excluding South Africa), the fact that Busia is slightly poorer than the Kenyan average probably makes the district more representative of rural Africa as a whole.

In Kenya unemployment and underemployment of young men is widely seen as leading to political or criminal violence, and reducing it is considered a major public priority. A number of public policies are currently oriented towards that end, such as the Kazi Kwa Vijana (Work for Youth) program, which provides low-paid public works employment opportunities.

3.2 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 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.

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 B 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).[6] The same balance is also evident among the subsample of respondents not still enrolled in school, and among those currently working for wages (see supplementary appendix tables A1, A2).

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 control schools, and in 1999 and 2000, Group 1 and 2 schools were treatment and Group 3 schools were control, and so on. The NGO typically requires cost sharing, and in 2001, a randomly chosen half of the Group 1 and 2 schools took part in a program in which parents had to pay a small positive price to purchase the drugs, while the other half of Group 1 and 2 schools received free treatment (as did all Group 3 schools). Kremer and Miguel (2007) show that cost-sharing led to a sharp drop in deworming treatment, by 60 percentage points, introducing further exogenous variation in deworming treatment that we can exploit. In 2002 and 2003, all sample schools received free treatment.

Children in Group 1 and 2 schools thus were assigned to receive 2.41 more years of deworming than Group 3 children on average (Table 1, Panel A), and these early beneficiaries are what we call the deworming treatment group below. We focus on a single treatment indicator rather than separating out effects for Group 1 versus Group 2 schools since this simplifies the analysis. We also 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

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

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.[7] 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 respondents within treatment communities experienced significant health and education gains (Miguel and Kremer 2004), complicating estimation of treatment effects on the treated. Miguel and Kremer (2004) show that deworming treatment improved self-reported health and reduced school absenteeism by one quarter during 1998-1999. Large externality benefits of treatment also accrued to individuals attending other schools within 6 kilometers of program treatment schools. There were no significant academic test score or cognitive test score gains during 1998-2000.

3.3 Kenya Life Panel Survey (KLPS)

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

The Kenyan Life Panel Survey (KLPS-2) was collected during 2007-2009, and tracked a representative sample of approximately 7,500 respondents who had been enrolled in primary school grades 2-7 in the 75 PSDP schools at baseline in 1998.[8]

Survey enumerators traveled throughout Kenya and Uganda to interview those who had moved out of local areas.[9] A random subsample containing approximately one-quarter of target respondents not found locally 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:

(5) $$ETR = RTR + (1 - RTR)*ITR$$

This is analogous to the approach in the Moving To Opportunity project (Kling *et al.* 2007, Orr *et al.* 2003).

Overall, the RTR in KLPS-2 is 65.0% and the ITR is 62.1%, which implies that 86% of respondents were effectively 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 (Table 1, Panel B). The effective survey rate among those still alive is 84%. These are high tracking rates for any age group over a decade, and especially for a highly mobile group of adolescents and young adults, and they are on par with some of the best-known panel survey efforts in less developed countries, such as

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[8] A midterm round (KLPS-1) was conducted in 2003-05. We focus on the KLPS-2, rather than KLPS-1, since it was collected at a more relevant time point for us to assess adult life outcomes: the majority of respondents are adults by 2007-09 (with median age at 22 years versus 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 in Appendix Figure A2.

[9] See supplementary appendix table A3. Baird, Hamory and Miguel (2008) further discusses the tracking approach.

the Indonesia Family Life Survey (Thomas *et al*. 2001, 2010), and several recent African surveys.[10] Reassuringly, survey tracking rates are nearly identical in the treatment and control groups (Panel B).

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

This section presents the estimation strategy and 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.

We first present estimates in which we separately estimate the impact of being in a treatment school (which received either two or three more years of deworming) and the externality impact of being within 6 km of such a school. Since we have insufficient statistical power to distinguish between the impact of being assigned to a Group 1 versus Group 2 school in the baseline "unconstrained" analysis, we group both together as the treatment group. We then report estimates in which we constrain both the treatment school and externality impacts to be linear in the intensity of deworming treatment, constraining impacts to be proportional to reductions in worm loads found in the data used in Miguel and Kremer (2004). This latter instrumental variables (IV) approach uses all of the available exogenous variation in deworming, including the variation associated with

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

assignment to Group 1 versus Group 2 schools, and to cost sharing (along with the associated variation in externalities).

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

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

The labor market outcome is a function of the assigned deworming program treatment status of the individual's primary school (T_j), and thus this is an intention to treat (ITT) estimator; a vector $X_{ij,0}$ of baseline individual and school controls; the number of treatment school pupils (N_j^T) and the total number of primary school pupils within 6 km of the school (N_j); and a disturbance term $e_{ij,2007\text{-}09}$, which is clustered at the school level.[11] The $X_{ij,0}$ controls include school geographic and demographic characteristics used in the "list randomization", the student gender and grade characteristics used for stratification in drawing the KLPS sample, the pre-program average school test score to capture school academic quality, the 2001 cost-sharing school indicator, as well as controls for the month and wave of the interview.

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

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

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

In the "restricted" IV analysis, among 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 school group indicators, cost-sharing indicator, and the associated externality variables (and other standard controls) in a specification similar to the estimating equation above.[12] We present these first stage results in Table 2 below. This generates the predicted number of years with moderate-heavy worm infections between 1998-2001 at the individual-level, which serves as the endogenous variable in the IV specifications. We then use a two-sample IV approach with bootstrapped standard errors (Angrist and Pischke 2008) to generate the estimated impact of eliminating a moderate-heavy worm infection for one year.

The restricted specification imposes the condition that the direct labor market impacts of different interventions that affected worm loads (e.g., two years of assigned treatment, three years of treatment, cost-sharing, and associated externalities) are proportional to treatment intensity as defined as the reduction in worm loads induced by the treatment. We do not interpret the resulting estimates literally as the impact of eliminating a moderate-heavy worm infection, since there may also be effects of reducing worm load that do not lead to crossing the threshold of moderate-heavy infection, and since effects may also be due to complementarities in school attendance between children, for

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

example. The availability of multiple instruments allows us to conduct over-identification tests of the assumption that labor gains are proportional to reductions in worm infections.

4.2 Impacts on health and education

We first document that deworming led to large reductions in moderate to heavy worm infections (defined as in Miguel and Kremer 2004) during the course of the original deworming intervention, using the parasitological stool sample data from 1999 and 2001 (Table 2, Panel A). As in the earlier study, there are large direct impacts of being assigned to a treatment school (-0.245, s.e., 0.030) as well as externality benefits for those living within 6 kilometers of treatment schools (-0.075, s.e., 0.026).[13] There is weak evidence of improved hemoglobin status (1.03, s.e. 0.81). In a 1999 survey conducted among a representative subsample of pupils, there is also a significant reduction in self-reported "falling sick often", by 3.7 percentage points (s.e. 1.5).

Adult health also improved as a result of deworming: respondent self-reported health (on a normalized 0 to 1 scale) rose by 0.041 (s.e. 0.018, significant at 95% confidence, Table 2, panel B). Many studies have found that self-reported health reliably predicts actual morbidity and mortality even when other known health risk factors are accounted for (Idler and Benyamini 1997, Haddock *et al.* 2006, Brook *et al.* 1984). Note that it is somewhat difficult to interpret this impact causally since it may partially reflect health gains driven by the higher adult earnings detailed below, in addition to the direct health benefits of earlier deworming. Yet the fact that there were similar positive and statistically significant impacts on self-reported health in earlier periods, namely, in the 1999 survey before most were working, suggests that at least part of the effect is directly due to deworming.

Deworming did not lead to detectable height gains, even when we restrict attention to younger individuals (those in grades 2-4 in 1998, regression not shown). The height result is reassuring since the deworming beneficiaries were already of primary school age when the program

[13] The time pattern of moderate-heavy worm infections across treatment groups is presented in Appendix Figure A3.

started, and thus beyond the age at which we would expect nutritional and health improvements to translate into permanent anthropometric gains.

We examine school enrollment and attendance using two different data sources in Table 2, Panel C. We first report school participation, namely, being found present in school by survey enumerators on the day of an unannounced school attendance check. This is our most objective measure of actual time spent at school, and was a main outcome measure in Miguel and Kremer (2004), but two important limitations are that it was only collected during 1998-2001, and only at primary schools in the study area; the falling sample size between 1998 to 2001 (shown in appendix Table A4) is mainly driven by students graduating from primary school. Total school participation gains are 0.129 of a year of schooling (s.e. 0.064, significant at 95% confidence).

Another outcome variable is school enrollment as reported by the respondent in the KLPS-2 survey, which equals one if the individual was enrolled for at least part of a given year. These show consistently positive effects from 1999 to 2007 both on the deworming treatment indicator and the externalities term, and the total increase in school enrollment in treatment relative to control schools over the period is 0.279 years (s.e. 0.147, significant at 90% confidence). The treatment effect estimates are largest during 1999-2003 before tailing off during 2004-07 (appendix table A3). By the 2007-09 survey there are no differences in school enrollment, and the treatment and control respondents still in school have comparable observables (appendix table A1). 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 respondents attended school more and were enrolled for more years on average, but do not attain significantly more grades in part because repetition rises 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 impacts on human capital and skills. While the impact of deworming on primary school academic test score performance in 1999 is positive but not statistically significant (Table 2, Panel D), there is some evidence that the passing rate did improve on the key primary school graduation exam, the Kenya Certificate of Primary Education (point estimate 0.046, s.e. 0.031), and that English vocabulary knowledge (collected in 2007-09) is higher in the treatment group (impact of 0.076 standard deviations in a normalized distribution, s.e., 0.055). The mean effect size of the 1999 test score, the indicator for passing the primary school leaving exam, and the English vocabulary score in 2007-09 taken together yields a normalized point estimate of 0.112 that is significant at 90% confidence (s.e. 0.067), providing suggestive evidence of moderate human capital gains. As expected, there is no effect on the Raven's Matrices cognitive exam, which is designed to capture general intelligence rather than acquired skills. While many would argue that nutritional gains in the first few years of life could in fact generate improved cognitive functioning as captured in a Raven's exam – as Ozier (2010) indeed does find among younger siblings of these deworming beneficiaries – it was seemingly already "too late" for such gains among the primary school age children in our study.

4.3 Deworming Impacts on Labor Supply

When assessing labor supply impacts, we focus on those respondents who are not still enrolled in school as the relevant population. As noted above, nearly identical proportions of respondents in the treatment and control groups are no longer enrolled in school, at roughly 75%, and we cannot reject that observable characteristics are the same across groups.

Hours worked increase substantially in the deworming treatment group. Mean hours worked increased by 3.10 hours (s.e. 1.21, Table 3, Panel A) on a control group mean of 18.5 hours, a 17% increase that is significant at 95% confidence. Much of this increase is driven by an increase in full-time work of at least 35 hours per week, which rises by 5.1 percentage points (s.e. 2.3, 95% confidence) on a base of 21.5 in the control group. In contrast, there is no significant change in the proportion in the treatment group working at all (greater than zero hours in the past week), which is roughly three quarters of those not still in school, a considerable degree of "non-activity" for a young adult population (although some might be engaged in home production or child-rearing that is not classified as work here).

We next focus on those who worked at all in the last week, by employment sector. 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, and by employment sector in panels B-D. There are some visible shifts in the treatment group distribution to the right overall that appear to be driven almost entirely by those not employed in agriculture (either self-employed or wage work). In both the self-employed subsample (panel C) and the wage-earning subsample (panel D), more treatment respondents work approximately full-time (roughly 40 hours per week), with fewer working part-time. The concentration of work hour gains in non-agricultural employment is confirmed in the regression analysis.

Hours in agriculture increase by 1.1 hours (s.e. 0.66, significant at 90%) in the treatment group on a base of 9.8 hours in the control group. This modest increase is consistent with the idea that the marginal product of labor in traditional agriculture is quite low (Lewis 1954). The small number of average hours worked per week in agriculture is also noteworthy, echoing earlier studies (e.g., Fafchamps 1993), as we confirm that hours worked in agriculture remain low even in peak planting and harvest months (not shown). Among those working outside of agriculture, the deworming treatment group worked 5.0 more hours (significant at 95% confidence), an increase of 11% on a base of 44.6 hours. There are even larger increases in hours worked in self-employment in the last week, at 6.7 hours (s.e. 3.0) on a base of 38.2 hours, or 18%. This magnitude is similar to the change from moving from a country with the average number of work hours in the OECD (New Zealand) to the OECD country with the highest weekly work hours, South Korea (OECD 2010). There are similarly large hours gains among wage earners, at 4.53 hours (s.e. 2.67) on a base of 47.3 hours per week.

Some of these gains may be the result of improved health boosting individual work capacity among wage earners: while impacts in the full sample of labor market participants are negative but not significant at traditional levels (point estimate -0.062, s.e., 0.165), among wage earners the number of days lost to poor health in the last month falls by more than a third, or 0.526 of a day per month (s.e. 0.250), in the treatment group, accounting for one fifth of the increase in total hours.

Point estimates suggest a similar pattern for neighboring treatment schools, although only some of the coefficients are statistically significant. Point estimates are positive but not significant for all those not still enrolled in school, with a t-statistic above one (1.71, s.e. 1.44). Among those working positive hours, there are statistically significant spillovers on total hours (3.51 hours, s.e. 1.58), hours worked outside of agriculture, hours worked in self-employment, and for wages. The externality effects are large in 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, is associated with an increase of three work hours per week.

An interesting methodological question is the extent to which the results we present here would differ had the survey data collection not included efforts to trace respondents living outside the original study district. While individual found in the “intensive” tracking phases do differ significantly on mean observable characteristics (see supplementary appendix Table A3), we cannot reject that treatment effects are unchanged among the subsample of respondents who resided in Busia district at the time of the 2007-09 follow-up survey (not shown).

4.4 Impacts on employment sector, occupation, and migration

Treatment does not lead to significant shifts between employment in agriculture on the one hand, and non-agricultural small business and wage employment on the other. However, within these sectors, treatment leads respondents to shift from food crops to cash crops and from less remunerative occupations where part-time work is common to better-paid, full-time, jobs in fields such as manufacturing.

The rates of agricultural, non-agricultural self-employment and wage earning work are nearly identical across the deworming treatment and control groups (Table 4, Panel A). The most common employment sector is farming (53.6% in the control group), as expected in rural Kenya, 21.0% of respondents worked for wages in the last month (and 24.4% at some point since 2007), while 13.3% were currently self-employed outside of farming.

Among those who work primarily on their own farm, there is evidence that deworming led to a shift towards cash crops (e.g., cotton, sugar, and tobacco) and away from traditional staple crops: the effect is 2.0 percentage points (s.e. 0.8) on a very low base of 1.0 percent in the control group, for a tripling of the proportion in the control group.

Treatment also leads to pronounced shifts in the occupation of employment among wage earners, out of relatively low-skilled and low wage sectors into better paid sectors (Table 4, Panel B). Deworming treatment respondents are three times more likely to work in manufacturing from a low base of 0.031 (coefficient 0.067, s.e. 0.025). The gains among males are particularly pronounced at 0.082 (s.e. 0.033). Survey responses indicate that the two most common types of manufacturing jobs in our sample are in food processing and textiles, with establishments ranging in size from small local corn flour mills up to large blanket factories in Nairobi. On the flip side, casual labor employment falls significantly (-0.041, s.e. 0.019), as does domestic service work for females (-0.190, s.e. 0.113). Local deworming spillover effects have a consistent sign in all of these cases, and are significant for domestic employment among females (-0.445, s.e. 0.154). Not surprisingly given these shifts in occupation of employment, a somewhat larger proportion of treatment group wage earners live in urban areas (not shown).

Manufacturing jobs tend to be quite highly paid, with average real monthly earnings of 5,311 Shillings (roughly US$68), compared to casual labor (2,246 Shillings) and domestic services (3,047 Shillings). Manufacturing jobs are also characterized by somewhat longer work weeks than average at 53 hours per week, in contrast to 43 hours for all wage earning jobs. Workers in manufacturing jobs also tend to have relatively few work days missed due to poor health, at just 1.1 days (in the control group), compared to 1.4 days among all wage earning jobs. One explanation for this pattern that ties into our earlier labor supply findings is that health investments improve individuals' capacity to carry out physically demanding jobs, characterized by long work weeks and little tolerance of absenteeism, and thus allow them to access higher paid jobs such as those in manufacturing. Casual laborers typically do not have to commit to work a certain number of days in a week in advance, so the significant reduction in casual work is also consistent with the hypothesis that deworming helps people obtain jobs that require regular attendance.

Just as deworming treatment does not appear to affect broad sources of income (i.e., agriculture versus non-agriculture), but does lead to shifts within each sector, treatment does not affect overall migration rates but there is some evidence that it leads respondents to migrate further from their homes. As illustrated in Table 5 (and the map in Appendix Figure A1), roughly 30%, of respondents resided outside of Busia District in 2007-09, with rates roughly balanced between the treatment and control groups.[14] However, treatment group respondents are somewhat more likely to live at least 500 km away from Busia, primarily due to greater likelihood of moving to Mombasa, Kenya's main port, with an increase of 1.7 percentage points (s.e. 1.0) on a base of 3.1 percent in the control group. This tendency for treatment group respondents to live farther away from the home district may capture greater effort exerted in the job search process. While the point estimates are not significant at traditional confidence levels, there is suggestive evidence that treatment individuals are somewhat more likely to move to a city for a job or to look for work (5.3 percentage points, s.e. 5.6).

4.5 Impacts on production and living standards

Just as we decompose the increase in overall hours into changes in hours in agriculture, non-agricultural self-employment, and hours working for wages, it is useful to separately estimate treatment impacts on output and productivity in each sector.

The impact on wage earners is perhaps easiest to measure. Here point estimates of the increase in earnings are larger than those of the increase in hours, consistent with the hypotheses that certain jobs require higher numbers of work hour, worked on a regular schedule, and that these jobs are better paying. It is also consistent with the idea that people adjust their work effort along intensive as well as extensive margins, as we find some evidence for wage gains, as well.

Treatment shifts the distribution of wage earnings sharply to the right (Figure 3).[15] In the regression analysis, we find that deworming treatment leads to higher log earnings (Table 6), with a gain of 23.0 log points (s.e. 7.3), with results unchanged if we do not use the log transformation (results not shown); with and without regression controls (columns 1 and 2); and when cross-school

[14] 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.

[15] Here and below we present real earnings measures that account for the higher prices found in the urban areas of Nairobi and Mombasa. We collected our own comparable price surveys in both rural western Kenya and in urban Nairobi during the administration of the KLPS-2 surveys, and base the urban price deflator on these data. Results are unchanged without this price adjustment, however.

externalities are accounted for. In our preferred specification with the full set of regression controls (column 3), the impact is 30.1 log points (standard error 9.1, 99% confidence). The earnings result is robust to several alternative specifications. It changes little in response to trimming the top 1% of earners, so the result is not driven by outliers; to including a full set of gender-age fixed effects; 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).

A decomposition along the lines of Oaxaca (1973) indicates that over 90% of the increase in labor earnings for the treatment group (Table 6), and nearly a third of the increase in hours worked (Table 3), can be explained by the occupational shifts documented in Table 4. While there are standard errors around these estimates and thus the exact figures should be taken with a grain of salt, they indicate that the bulk of the earnings gains can be accounted for by such shifts.

There is suggestive evidence for deworming externalities on earnings. While the coefficient estimate on the local density of treatment pupils (in thousands) is not significant at traditional confidence levels (22.8 log points, s.e. 16.3, in Table 6, column 3), it reassuringly has the same sign as the main deworming treatment effect, and a substantial magnitude: an increase of one standard deviation in the local density of treatment school pupils would boost labor earnings by roughly (917/1000)*(22.8 log points) = 20.9 log points.

Additional sources of exogenous variation in worm treatment intensity tell a similar story. Respondents in Group 2 school (who were assigned to one less year of deworming treatment) and those in schools randomly chosen for the 2001 cost-sharing program (which had much lower deworming take-up in 2001) have lower earnings in 2007-09, although only the latter effect is statistically significant (Table 6, column 4).

As shown in Table 7, using our preferred specification with the full set of regression controls (equivalent to equation 6 and as in column 3 in Table 6), log wages computed as earnings per hour worked rise 20.3 log points (s.e. 11.1) in the deworming treatment group, and the effect is significant at 90% confidence.

Positive wage earnings impacts are similar in the larger group of respondents, 24.4% of the full sample, who have worked for wages at any point since 2007, where we use their most recent monthly earnings if they are not currently working for wages. The mean impact on log earnings is 0.211 (s.e. 0.072), and there is once again suggestive evidence of positive externality effects (0.170, s.e. 0.116, Table 7, Panel B).

We find no significant evidence that deworming impacts on hours worked or wage earnings differ by gender (Appendix Table A5), or individual age at baseline.

Theoretically, the sign of the interaction with the local level of serious worm infections at baseline is ambiguous, and the effect of the program at higher levels of initial disease prevalence need not be monotonic. This is because areas with higher prevalence will typically have conditions more conducive to transmission of the disease. Re-infection is thus likely to occur more quickly in these areas and hence that the impact of treatment could potentially be smaller in these areas than in areas where it takes longer for re-infection to occur.

Empirically, we find that gains in hours worked are significantly larger in areas with higher initial infection rates (Appendix Table A5, column 3), at 2.1 hours (s.e. 0.9 hours). We use the zonal-level baseline 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, and thus this interaction effect is likely to understate true effects. Wage earnings are no larger in areas with higher initial infection rates (column 6).[16]

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[16] Deworming does not seem to affect the likelihood that people become wage earners or the process by which observable characteristics influence the likelihood of becoming a wage earner. In Table 4, we found no evidence that deworming treatment respondents are more likely to be working for wages or in-kind payments in the last month (Panel A, estimate -0.006, s.e. 0.022). There is similarly no differential selection into the subsample who have worked for wages, at any point since 2007 by treatment group (Table 7, Panel B). We further confirm that we cannot reject that the observable characteristics of wage earners, including academic performance measures, are the same in

Point estimates of the percentage increases in profits among owners of non-agricultural businesses are similar to the percentage increases in earnings among wage earners, but are estimated with less precision, partly because fewer people work in the sector and partly because the underlying variance of reported profits is higher than that of reported wages (presumably due to a combination of stochastic variation and measurement error). The estimated deworming treatment effect on the profits of the self-employed (as directly reported in the survey) is positive at 409 Shillings (s.e. 313, Table 7, Panel C), although this 23% gain is not significant at traditional confidence levels. There are large, positive but not statistically significant impacts on a monthly profit measure based directly on revenues and expenses reported in the survey (553 Shillings, s.e. 940), reported profits in the last year (2,515 Shillings, s.e. 2,332), as well as on the total number of employees hired (0.641 additional employees on a base of 0.189, s.e. 0.374, significant at 90%). 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 0.200 (s.e., 0.093), where the magnitude is interpretable as 0.200 standard deviations of the normalized control group distribution.

We next construct a measure of total monthly non-agricultural earnings by summing wage earnings and self-employed profits, and among all of those not still in school, we estimate a treatment effect of 240 Shillings (s.e. 135, significant at 90%, Table 7 Panel D) on a base of 974 in the control group, for a 24.6% increase. The majority of this sample has zero non-agricultural earnings, making this a particularly stringent test.

Measuring the on-farm productivity of an individual worker in the context of a farm where multiple household members (and sometimes hired labor) all contribute to different facets of the

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the treatment and control groups (Appendix Table A2). These factors all point towards an interpretation of the difference in labor earnings between the deworming treatment and control groups primarily reflecting causal treatment, rather than a selection effect. Further evidence is provided by a Heckman (1979) approach explicitly modeling the process of selection into wage earning. We use a marital status indicator and marital status interacted with gender as variables that predict selection into earning but are excluded from the earnings regression; marital status is strongly positively (negatively) correlated with any wage earning among males (females), results not shown. Keeping in mind the standard caveats to selection correction models, this approach yields an almost unchanged estimated impact on log wage earnings (not shown).

production process is difficult, and thus we do not have a measure of individual production in agriculture analogous to the wages or profits of those working in other sectors. We also lack sufficiently detailed information on farming choices to compute a reliable yield measure, and thus rely on proxies for productivity. There is no indication that deworming led to higher crop sales in the past year (Table 7, Panel E). 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 (as discussed below). We do find evidence of increased adoption of "improved" agricultural practices including fertilizer, hybrid seeds, or irrigation, with an increase of 4.7 percentage points (s.e. 2.7) on a base of 29.5 percent, suggesting somewhat greater agricultural productivity. While these results should be taken with a grain of salt as we cannot easily measure individual on-farm productivity, they suggest there were modest improvements in agricultural productivity, consistent with the finding of small increases in hours worked in agriculture (Table 3).

Consumption is commonly used to assess living standards in rural areas of less developed countries, where most households engage in subsistence agriculture rather than wage work. We do not have data from a consumption module[17], but did collect data on the number of meals consumed. Deworming treatment respondents consume 0.096 more meals per day (s.e. 0.028, significant at 99% confidence, Table 8) than the control group, and the externality impact is also large and positive (0.080, s.e. 0.023, 99% confidence). Among those not still enrolled in school, the gains are nearly identical, at 0.103 additional meals (s.e. 0.029) and an externality gain of 0.101 (s.e. 0.032).

There are statistically significant improvements in meals eaten in all employment sectors, and mirroring the hours worked and productivity results, the gains are largest outside of agriculture. Deworming led to an increase of 0.205 meals eaten (s.e. 0.059) among wage earners and the non-

[17] A fuller consumption expenditure module was collected as a pilot for roughly 5% of the KLPS-2 sample during 2007-09, for a total of 254 complete surveys. The estimated treatment effect for total consumption is near zero and not statistically significant (-$14, s.e. $66), but the confidence interval is large and includes substantial gains, since average consumption levels are $580.

agricultural self-employed, with large externality effects of 0.180 (s.e. 0.067), with a smaller, though still statistically significant gain of 0.076 meals (s.e. 0.035) among those engaged in household agriculture. This suggests that the labor market gains documented for respondents in the non-agricultural sector translate into higher living standards, as well.

4.6 Restricted Estimates

In the previous sections, we estimated both the direct and externality effects of 2 to 3 years of extra deworming treatment, allowing each parameter to vary freely. Here we estimate a model in which we restrict the impact of three years treatment, two years of treatment, and cost-sharing to be linear in treatment intensity (defined as the fraction of people treated times the number of years of treatment), and the externality impacts from each of these programs to be proportional to both treatment intensity and the reduction in worm infection levels documented in Miguel and Kremer (2004), and as found using some additional parasitological data. The linearity assumption is restrictive, but it allows us to simultaneously use all sources of exogenous variation in treatment to estimate impacts. As discussed above, we use a two-sample instrumental variable (TSIV) approach, where the endogenous variable is the predicted number of years with moderate-heavy infections between 1998 and 2001.

Using this method, there are significant negative impacts of moderate-heavy infection on hours worked (-4.91 hours per week, s.e. 1.92, Table 9), log labor earnings (-47.8 log points, s.e. 16.8), and meals eaten (-1.28, s.e. 0.055), and all of these estimates are statistically significant at over 95% confidence. As mentioned above, we are reluctant to interpret these estimates literally as structural estimates of the impact of eliminating a moderate-heavy worm infection for one year since there may also be effects of reducing worm load that do not lead to crossing the threshold of moderate-heavy infection, and since effects may also be due to complementarities in school attendance between children.

The availability of multiple instruments allows us to carry out over-identification tests, in this case, implemented in GMM (among those with 2001 parasitological data) to generate the Hansen J-statistic. The p-values on this test are 0.183, 0.912, and 0.623 for the three dependent variables, indicating that we cannot reject the assumption that deworming impacts are proportional to health gains as measured by reductions in moderate-heavy worm infections.

5. Socially Optimal Subsidies for Deworming

We first calibrate the Grossman (1972) style model to derive the optimal level of deworming subsidies in section 5.1, before carrying out a more traditional analysis of the social rate of return to deworming as a human capital investment in section 5.2. In both cases, we conclude that large subsidies for deworming are justified.

5.1 Calibrating the health investment model

It is possible to calibrate the model with data on impact of deworming from the restricted estimator and data from Kremer and Miguel (2007) on price responsiveness. A lower bound on the utility associated with any given level of subsidy is given by assuming that all those who dewormed at that price level had sufficiently high disutility from deworming, m_i, that they were just indifferent between deworming and not deworming. In this case, the utility gain from a higher subsidy consists only of the increase in take-up times the externality benefit of increased take-up. For concreteness, in what follows we assume that $\alpha = \frac{1}{3}$. [18]

From Table 7, we calculate a lower bound on hourly earnings in non-agricultural work (either wage work or self-employment) as \$0.19 per hour. From Table 3, we estimate that if an extra person dewormed, those within 6 km of them (and who are not currently in school) are currently

[18] Working 2,000 hours per year corresponds to $\alpha = 0.34$, assuming people are endowed with 16 hours of healthy working time per day.

working an extra 0.00171 hours a week. Since people only work a third of their total healthy time, it implies a daily increase of 0.000733 hours of healthy time a day. This is worth 0.000733 x US$0.19 x (365 days) = US$0.05 in money-metric utility terms in the first year. We assume that earnings first rise and then gradually fall over the life cycle in an inverted-U shaped manner, as documented by Knight, Sabot, and Hovey (1992) for Kenyan labor markets, with earnings increasing proportionally in the deworming treatment group. With an annual discount rate of 10%, and labor force participation for 40 years, we estimate that the NPV of the externality benefit for each person affected is US$0.22 (starting discounting 10 years before labor earnings start, since deworming was done in 1998 and we start observing labor earnings a decade later). From Table 1, each person who is dewormed generates externality benefits to an average of 4,709 others (who attended school within 6 km), so the total externality utility benefit per person who actually deworms is US$0.22 *4,709= US$1,028.

We use current estimates of per pupil mass treatment costs (provided by the NGO Deworm The World) of US$0.59 per year. This cost incorporates the time of personnel needed to administer drugs through a mass school-based program, and accounts for the fraction of our sample that requires treatment with the more expensive drug for schistosomiasis (praziquantel). The total direct deworming cost then is the 2.41 years of average deworming in the treatment group times US$0.59, or US$1.42.

Taken together, these figures allow us to estimate the expected costs and benefits of targeting one additional person for deworming at various subsidy levels, and the level of deadweight loss that would make that level of subsidy less socially beneficial than no government intervention (or a lower subsidy level). Table 10 lays out the results. The first column presents the price for deworming drugs paid by individuals under the three subsidy regimes: no subsidy, partial subsidy (the 2001 cost-sharing program), and full subsidies for deworming. Drawing on Kremer and Miguel (2007), the second column presents the take-up levels that resulted from these prices, with sharply declining take-up at higher prices, and the next column presents the average subsidy price per targeted student, taking into account that actual spending is low in the case of partial subsidies since take-up is so low. The fourth column presents the total utility benefits generated by deworming externalities (in money-metric terms) under each of the three subsidy regimes, as described above.

The final column presents the deadweight loss that would be needed to have the costs per capita outweigh the benefits, and it is immediate that they are massive, with DWL in the thousands of percent in all cases. In particular, the full subsidy case generates higher aggregate utility than either the partial or no subsidy cases under a wide range of DWL rates (under roughly 37,000%), which is far above the rates that are considered realistic.

5.2 Deworming as a human capital investment

An alternative approach is to calculate the internal rate of return (IRR) on deworming investments to assess its relative future benefits and costs, including for those who took deworming themselves. This approach complements the Grossman-style (1972) model calibration presented above.

On the benefits side, we consider the earnings gains estimated over 40 years of an individual's work life, making the same assumptions on lifecycle earnings in Kenya. We make several assumptions that imply that our rate of return estimates are lower bounds on the true returns to deworming. An important assumption in some calculations is that only the subset of current wage earners (21% of those not still in school) will experience improved living standards as a result of deworming. Disregarding living standards gains experienced by non-wage earners is conservative, given that the number of meals eaten rose in the full sample and that small business performance measures improved among the self-employed. There may also be broader community-wide benefits to deworming among those not of school age, for example, among the younger siblings of the treated (Ozier 2010). We conservatively also ignore these gains.

As outlined above, cost for 2.41 years of treatment is approximately $1.42. Multiplying by the average compliance rate (46% in treatment schools, accounting for cost-sharing years and the fact that some of the sample ages out of primary school each year) gives an average cost of or $0.65 per treatment pupil and $0.44 per pupil in the full sample (Table 10, Panel B). Auriol and Walters (2009) suggest that deadweight loss is around 20%, so we estimate an average total cost per student in the treatment schools of $0.78.

Under these assumptions, the average gain in total lifetime earnings (undiscounted) from deworming treatment per pupil in the PSDP sample is $3,145 (Table 10, Panel B). The externality benefits to deworming treatment – including both the cross-school externalities presented above, and estimated within-school spillovers – are the lion's share of the gains (at $2,956 per sample pupil, or 94% of total benefits), and thus substantially boost the rates of return reported below. Miguel and Kremer (2004) estimate that the reduction in moderate-heavy worm infections experienced by treatment school respondents who did not receive deworming drugs themselves were 78% as large as those experienced by treatment school respondents who did receive the drugs (see Table 6, panel B in that paper). We apply this estimate of the magnitude of within-school externalities here, such that 78% of the gain experienced by treated individuals in treatment schools is attributed to externalities. Adding in the future wage benefits to younger untreated cohorts would further increase raises the share of future earnings benefits that can be attributed to externalities. The overwhelming share of benefits generated by externalities is a plausible explanation for the relatively low private demand for deworming drugs in rural Kenya, as found by Kremer and Miguel (2007).[19]

An estimated IRR of 82.7% is obtained by considering the increase in total earnings, and treating time spent in school as having no net benefits or costs beyond the impact on earnings. The interpretation is that a social planner with an annual discount rate or cost of capital of less than 82.7%

[19] The second potential component of costs is the opportunity cost of time spent in school rather than doing something else, presumably working. We focus on the case where all of the additional days spent in school were due to an increase in non-sick time, as in the Grossman (1972) framework. An alternative assumption is that all days of additional schooling came at the expense of days worked. This would be an upper bound on the actual opportunity cost of time, if school participation instead increased at least in part because children were simply sick less often. The internal rate of return figures remain large even under this more conservative assumption (see supplementary appendix table A6 for the details).

would choose to invest in deworming as a human capital investment. Considering the externality benefits alone, the IRR remains very high at 81.6%. For reference, at the time of writing nominal commercial interest rates in Kenya are 10-12% per annum, the rate on long-term sovereign debt is 11% and inflation is 3% (according to the Central Bank of Kenya).[20] Deworming appears to be an attractive investment given the real cost of capital in Kenya.[21]

A more conservative approach focuses entirely on the earnings benefits due to wage productivity gains, and ignores the extensive margin of greater hours worked. This assumption is opposite in spirit to the Grossman (1972) framework (which focuses on utility gains due to greater time endowment), but provides another bound on the returns to deworming. In this case, the IRR remains attractive at 58.5% (and 56.0% if considering only externalities, Table 10 Panel B).

We have so far focused on wage earners because their productivity gains are more accurately measured than those working in self-employment or agriculture. If we abandon the assumption that earnings and wage gains were only experienced by those with wage earnings, and assume that the full sample experienced analogous living standard gains, the social internal rate of return would be much larger: a 117.1% per annum return (see supplementary appendix table A6).

Another policy concern is that real-world programs in Kenya or other countries with high worm infection levels would not be implemented as cost effectively as the program we study, leading to higher costs per child treated, for instance, if a certain share of the funds was misused or stolen. However, we find that the level of such leakage would have to be extremely large to drive the annualized IRR down even to a still respectable 10%. In particular, for the case where we consider lifetime earnings gains for the full sample, over 99% of funds would have to be misused to drive the IRR down to 10% per annum (from 117.1%). The immediate implication is that even mismanaged real-world school deworming programs are still likely to generate very high social rates of return.

[20] This figure was obtained at: http://www.centralbank.go.ke/ (accessed November 1, 2010).

6. Conclusion

We exploit an unusually useful setting for estimating the impact of child health gains on adult earnings and other life outcomes. The Kenya Primary School Deworming Program was experimentally phased-in across 75 rural schools between 1998 and 2001 in a region with high rates of intestinal worm infections, one of the world's most widespread diseases. As a result, the treatment group exogenously received an average of two to three more years of deworming treatment than the control group. A representative subset of the sample was followed up for roughly a decade through 2007-09 in the Kenya Life Panel Survey, with high survey tracking rates, and the labor market outcomes of the treatment and control groups are compared to assess impacts.

There were large increases in average hours worked (by 12%), and a reduction in work days lost to sickness as a result of deworming. Among those working for wages, average adult earnings rise by over 20%, and these gains are accompanied by sharp shifts in employment towards high-paying manufacturing sector jobs (especially for males) and away from casual labor and domestic services employment (for females). These findings complement Bleakley's work on historical deworming programs in the U.S. South in the early 20th century, and the correspondence between the two sets of results – using distinct research designs and data – increases confidence in both findings.

The 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. We cannot decompose how much of our labor market impacts are working through health versus education without imposing strong assumptions, but both channels may play a role. It is particularly difficult to pin down the effect of schooling since the extra years of education may have non-cognitive impacts, accustoming people to a schedule in which they have to turn up regularly to work, that are not captured in test scores.

Kenya's labor market laws are considered quite rigid so a natural supposition would be that a program which affects human capital but has little signaling component (because it does not change completed years of education significantly) would have minimal impact on labor market outcomes. This would be the case if people were queuing, unemployed, waiting for formal sector jobs, as in the Harris and Todaro (1970) model. Our finding of considerable labor market impacts (outside of the agricultural sector) suggests that Kenyan labor markets are more flexible than is often believed.

The social returns to child deworming treatment appear high using an approach based on calibration the Grossman (1972) model, or an alternative social planner approach, where the latter generates an annualized social internal rate of return of 82.7%. The estimates suggest that the externality benefits alone justify fully subsidizing school-based deworming. 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, and KLPS (2007-09) survey attrition patterns

	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)	Kolmogorov-Smirnov p-value
Panel A: Baseline summary statistics					
Age (1998)	11.9	11.9	12.0	-0.04	0.258
	(2.6)	(2.6)	(2.6)	(0.11)	
Grade (1998)	4.23	4.22	4.25	-0.03	0.450
	(1.68)	(1.70)	(1.66)	(0.05)	
Female	0.470	0.469	0.473	-0.004	--
				(0.019)	
School average test score (1996)	0.029	0.024	0.038	-0.013	0.310
	(0.427)	(0.436)	(0.406)	(0.109)	
Primary school located in Budalangi division	0.370	0.364	0.381	-0.017	--
				(0.137)	
Population of primary school	476	494	436	58	0.307
	(214)	(237)	(146)	(54)	
Total treatment (Group 1, 2) primary school students within 6 km	3,180	3,085	3,381	-296	0.206
	(917)	(845)	(1,022)	(260)	
Total primary school students within 6 km	4,709	4,698	4,732	-34	0.119
	(1,337)	(1,220)	(1,555)	(389)	
Years of assigned deworming treatment, 1998-2003	3.31	4.09	1.68	2.41***	--
	(1.82)	(1.52)	(1.23)	(0.08)	
Panel B: Sample attrition, KLPS					
Found[a]	0.862	0.860	0.867	-0.007	--
				(0.017)	
Surveyed	0.825	0.824	0.827	-0.003	--
				(0.018)	
Not surveyed, dead	0.017	0.018	0.014	0.004	--
				(0.004)	
Not surveyed, refused	0.015	0.014	0.017	-0.003	--
				(0.005)	

Notes: The data in Panel A are from the PSDP, and includes all individuals surveyed in the KLPS2. There are 5,084 observations for all variables, except for Age (1998) where there are 5,072 observations due to missing survey data. All variables in Panel A are 1998 values unless otherwise noted. Years of assigned deworming treatment is calculated using the treatment group of the respondent's school and their grade, but is not adjusted for the treatment ineligibility of females over age 13 or assignment to cost-sharing in 2001. Those respondents who "age out" of primary school are no longer considered assigned to deworming treatment. The average school test score is from the 1996 Busia District mock exam, and has been converted to units of normalized individual standard deviations. The sample used in Panel B includes all individuals surveyed, found deceased, refused participation, found but unable to survey, and not found but sought in intensive tracking during KLPS2, a total of 5,569 respondents (3,686 treatment and 1,883 control). All observations are weighted to maintain initial population proportions. The "Treatment – Control" differences are derived from a linear regression of the outcome on a constant and the treatment indicator, but results are similar if we include further controls (for survey wave, 1998 administrative zone of residence, cost sharing school indicator, and baseline 1998 population of the individual's primary school). Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. The Kolmogorov-Smirnov p-values are only presented for the non-binary variables, where it is informative.

[a] The proportion "Found" is the combination of pupils surveyed, found deceased, refused and found but unable to survey. [b] Districts neighboring Busia include Siaya, Busia (Uganda), and other districts in Kenya's Western Province.

Table 2: Impacts on health, nutrition and education outcomes

Dependent variable	Control group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming treatment indicator	Coefficient estimate (s.e.) on deworming Treatment school pupils within 6 km (in '000s), demeaned
Panel A: Health outcomes during 1999-2001			
Moderate-heavy worm infection (1999, 2001 parasitological surveys)	0.321	-0.245***	-0.075***
	(0.467)	(0.030)	(0.026)
Hemoglobin (Hb) level (1999, 2001 parasitological survey samples)	126.1	1.03	0.91
	(14.7)	(0.81)	(0.96)
Falls sick often (self-reported), 1999	0.154	-0.037**	0.001
	(0.361)	(0.015)	(0.014)
Panel B: Health and nutrition outcomes, KLPS (2007-09)			
Self-reported health "very good"	0.673	0.041**	0.028
	(0.469)	(0.018)	(0.022)
Height (cm)	167.3	-0.12	-0.39
	(8.0)	(0.26)	(0.33)
Panel C: School participation, enrollment and attainment			
Total primary school participation, 1998-2001	2.51	0.127***	-0.115*
	(1.12)	(0.064)	(0.060)
Total years enrolled in school, 1998-2007	6.69	0.279*	0.138
	(2.97)	(0.147)	(0.149)
Grades of schooling attained	8.72	0.153	0.070
	(2.21)	(0.143)	(0.146)
Indicator for repetition of at least one grade (1998-2007)	0.672	0.060***	0.010
	(0.470)	(0.017)	(0.023)
Enrolled in school in year of 2007-09 survey	0.252	0.003	-0.045*
	(0.434)	(0.022)	(0.026)
Panel D: Test scores			
Mean effect size (1999 test, passed primary school exam, 2007-09 English test)	0.000	0.112	0.068
	(1.000)	(0.067)*	(0.058)
Academic test score (normalized across all subjects), 1999	0.026	0.059	0.158
	(1.000)	(0.090)	(0.101)
Passed primary school leaving exam during 1998-2007	0.505	0.048	0.032
	(0.500)	(0.031)	(0.029)
English vocabulary test score (normalized), 2007-09	0.000	0.076	0.067
	(1.000)	(0.055)	(0.053)
Raven's Matrices cognitive test score (normalized), 2007-09	0.000	-0.011	0.055
	(1.00)	(0.048)	(0.042)

Notes: The sample size in Panel A is 2,720 for worm infection, 1,765 for Hb, and 3,861 for health self-reports. Representative subsets of pupils in all schools were surveyed for these 1999 and 2001 pupil surveys. The sample in Panels B, C and D includes all individuals surveyed in KLPS-2. Each row is from a separate OLS regression analogous to equation 4. All observations are weighted to maintain initial population proportions. Standard errors are clustered by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview, a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator. Self-perceived health "very good" takes on a value of one if the answer to the question "Would you describe your general health as somewhat good, very good, or not good?" is "very good", and zero otherwise.

Table 3: Deworming impacts on 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: Labor Supply				
Hours worked (for wages, self-employed, agriculture) in last week	18.5	3.10**	1.71	3,873
	(23.8)	(1.21)	(1.44)	
Indicator for hours worked > 35 (for wages, self-employed, agriculture) in last week	0.215	0.051**	0.037	3,873
	(0.411)	(0.023)	(0.027)	
Indicator for hours worked > 0 (for wages, self-employed, agriculture) in last week	0.704	0.023	-0.027	3,873
	(0.457)	(0.024)	(0.030)	
Among those with positive hours worked:				
Hours worked (for wages, self-employed, agriculture) in last week	26.3	3.23**	3.51**	2,853
	(24.5)	(1.44)	(1.58)	
Hours worked (in agriculture) in last week	9.8	1.10*	-0.77	2,187
	(9.1)	(0.66)	(0.62)	
Hours worked (for wages, self-employed) in last week	44.6	5.03**	7.40***	1,120
	(23.0)	(2.19)	(2.39)	
Hours worked (as self-employed) in last week	38.2	6.7**	7.7***	528
	(24.0)	(3.0)	(2.9)	
Hours worked (for wages) in the last week	47.3	4.53*	5.06**	605
	(21.3)	(2.67)	(3.11)	
Panel B: Health related absenteeism (negative binomial results)				
Work days missed due to poor health, past month	1.53	-0.062	0.002	2,853
	(3.19)	(0.165)	(0.211)	
Work days missed due to poor health (in agriculture), past month	1.51	0.182	0.093	1,360
	(2.92)	(0.165)	(0.211)	
Work days missed due to poor health (if work for wages, self-employed), past month	1.43	-0.336	-0.338	1,097
	(3.23)	(0.222)	(0.238)	
Work days missed due to poor health (if self-employed), past month	1.63	-0.221	-0.188	504
	(4.08)	(0.302)	(0.351)	
Work days missed due to poor health (if work for wages), past month	1.34	-0.526**	-0.197	608
	(2.62)	(0.250)	(0.297)	

Notes: Each row in Panel A is from a separate OLS regression, and each row in Panel B is a negative binominal specification. All observations are weighted to maintain initial population proportions. The sample is restricted to respondents who were not enrolled in school in the year of the survey. In the "work days missed due to poor health" regressions, the sample is further restricted to respondents who worked at least 10 hours in the last week. 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 4: Deworming impacts on employment sector and occupation

	Control group proportion	Coefficient estimate (s.e.) on deworming treatment indicator	Coefficient estimate (s.e.) on deworming treatment pupils within 6 km (in '000s), demeaned	Mean (s.d.) hours per week worked in sector, control group	Mean (s.d.) days of work lost to poor health in last month, control group	Mean (s.d.) earnings in sector, past month (Kenya Shillings), control
Panel A: Employment Sector[a]						
Agriculture	0.536	-0.013	-0.006	10	1.6	--
		(0.031)	(0.038)	(9)	(2.9)	
Agriculture–cash crop (cotton, tobacco, sugar)	0.013	0.017*	-0.002	7	1.4	--
		(0.009)	(0.008)	(9)	(3.6)	
Self-Employment	0.133	0.023	0.006	34	1.8	--
		(0.016)	(0.016)	(26)	(4.4)	
Wage Employment	0.210	-0.006	-0.002	43	1.4	3,572
		(0.022)	(0.025)	(25)	(2.9)	(3,586)
Panel B: Occupation in Wage Employment						
Agriculture and fishing	0.221	-0.045	-0.159**	34	2.1	2,886
		(0.062)	(0.082)	(25)	(4.0)	(1,801)
Retail	0.146	-0.015	0.025	43	1.1	2,231
		(0.039)	(0.043)	(27)	(2.1)	(1,716)
Trade contractors	0.096	-0.009	0.056	26	0.9	3,191
		(0.030)	(0.045)	(22)	(2.5)	(2,183)
Manufacturing	0.031	0.067***	0.043	53	1.1	5,311
		(0.025)	(0.032)	(24)	(1.8)	(3,373)
Manufacturing – males only	0.032	0.082**	0.034	49	1.0	6,277
		(0.033)	(0.034)	(20)	(1.9)	(3,469)
Casual/Construction laborer	0.031	-0.041**	-0.022	51	0.4	2,246
		(0.019)	(0.018)	(31)	(1.0)	(1,576)
Wholesale trade	0.028	0.014	0.017	44	0.7	4,727
		(0.029)	(0.034)	(14)	(1.9)	(3,953)
Services (all)	0.414	0.040	0.040	50	1.4	4,345
		(0.057)	(0.077)	(22)	(2.7)	(4,837)
Domestic	0.122	-0.015	-0.028	61	1.5	3,047
		(0.033)	(0.039)	(17)	(2.5)	(1,754)
Domestic – females only	0.346	-0.190*	-0.445***	65	1.6	2,795
		(0.113)	(0.154)	(17)	(2.6)	(888)
Restaurants, cafes, etc.	0.064	-0.032	0.023	53	1.2	4,194
		(0.025)	(0.034)	(21)	(2.5)	(3,567)

Notes: The sample used in Panel A includes all individuals surveyed in the KLPS2 who were not enrolled in school in the year of the survey. The sample used in Panel B additionally restricts the sample to those respondents 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.

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

Table 5: Deworming impacts on migration

Dependent variable	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.) hours per week worked, control group	Mean (s.d.) days of work lost to poor health in last month, control group	Mean (s.d.) earnings, past month (Kenya Shillings), control
Panel A: Full sample						
Residence in Busia district	0.740	0.14	-0.019	4.5	1.5	328
	(0.439)	(0.022)	(0.025)	(15)	(3.1)	(1,264)
Residence in a city	0.179	-0.001	0.016	14	1.3	1,199
	(0.383)	(0.019)	(0.024)	(26)	(3.0)	(2,461)
Residence > 500 km from 1998 primary school	0.031	0.017*	0.018	15	1.9	1,201
	(0.174)	(0.010)	(0.013)	(27)	(4.6)	(2,482)
Residence outside of Kenya	0.042	0.011	-0.003	12	1.6	770
	(0.202)	(0.011)	(0.016)	(23)	(2.6)	(1,617)
Migrated to city for a new job or to look for work	0.319	0.053	0.045	33	1.1	2,715
	(0.467)	(0.056)	(0.072)	(31)	(2.0)	(3,092)
Panel B: Individuals not in school						
Residence in Busia district	0.668	0.012	0.000	6.2	1.6	428
	(0.471)	(0.026)	(0.030)	(17)	(3.3)	(1,294)
Residence in a city	0.201	0.010	0.019	16	1.4	1,384
	(0.401)	(0.023)	(0.026)	(28)	(3.2)	(2,584)
Residence > 500 km from 1998 primary school	0.036	0.018*	0.018	17	2.0	1,373
	(0.187)	(0.011)	(0.015)	(28)	(4.7)	(2,610)
Residence outside of Kenya	0.048	0.003	-0.014	13	1.8	828
	(0.214)	(0.013)	(0.019)	(24)	(2.8)	(1,583)
Migrated to city for a new job or to look for work	0.377	0.052	0.045	33	1.1	2,672
	(0.486)	(0.059)	(0.076)	(32)	(2.0)	(3,069)

Notes: The sample used in Panel A includes all individuals surveyed in the KLPS2 with residential location information. Panel C restricts only to those not enrolled in school in the year of survey. Each row is from a separate OLS regression. Outcomes are indicators for location of residence at the time of survey. 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)			
	(1)	(2)	(3)	(4)
Deworming Treatment indicator	0.230**	0.224**	0.301***	0.332***
	(0.073)	(0.073)	(0.091)	(0.102)
Deworming Treatment pupils within 6 km (in ‘000s), demeaned			0.228	0.223
			(0.163)	(0.166)
Total pupils within 6 km (in ‘000s), demeaned			-0.119	-0.115
			(0.124)	(0.125)
Group 2 school indicator				-0.080
				(0.095)
Cost sharing school (in 2001) indicator	-0.134	-0.170*	-0.194**	-0.188**
	(0.083)	(0.093)	(0.084)	(0.085)
Additional controls	No	Yes	Yes	Yes
R^2	0.071	0.178	0.186	0.187
Observations	687	687	687	687
Mean (s.d.) in the control group	7.84	7.84	7.84	7.84
	(0.85)	(0.85)	(0.85)	(0.85)

Notes: The sample used here includes all individuals surveyed in the KLPS2 who report positive labor earnings at the time of survey and were not enrolled in school in the year of the 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, and self-employment (non-agriculture)

Dependent variable	Control group variable mean (s.d.)	Coefficient estimate (s.e.) on deworming Treatment indicator	Coefficient estimate (s.e.) on deworming Treatment pupils within 6 km (in '000s), demeaned	Obs.
Panel A: Wage earner subsample				
Ln(Total labor earnings, past month)	7.84	0.301***	0.228	687
	(0.84)	(0.091)	(0.163)	
Ln(Wage = Total labor earnings / hours, past month)	2.76	0.203*	0.027	605
	(0.94)	(0.111)	(0.155)	
Panel B: Wage earner since 2007 subsample				
Ln(Total labor earnings, most recent month worked)	7.88	0.211***	0.170	1,175
	(0.91)	(0.072)	(0.116)	
Indicator for worked for wages (or in-kind) since 2007	0.244	0.000	0.040	5,081
	(0.430)	(0.021)	(0.024)	
Panel C: Self-employed (non-agriculture)				
Mean effect size (total employees hired, three profits measures)	0.000	0.200**	0.060	536
	(1.000)	(0.093)	(0.101)	
Total employees hired (excluding self), among the self-employed	0.189	0.641*	0.623	616
	(0.625)	(0.374)	(0.530)	
Total self-employed profits (self-reported) past month (among those >0)	1,771	409	-53	570
	(2,621)	(313)	(361)	
Total self-employed profits (constructed) past month (among those >0)	1,539	553	-156	580
	(6,534)	(940)	(957)	
Total self-employed profits (self-reported) past year (among those >0)	12,230	2,515	-511	552
	(17,356)	(2,332)	(2,605)	
Panel D: Full sample earnings (wages and self-employment)				
Total earnings (wages, self-employed profits), past month (=0 for non-earners)	974	240*	40	3,873
	(2,388)	(135)	(185)	
Panel E: Agriculture				
Total value (KSh) of crop sales past year (if farm household)	578	126	-168	2,732
	(2534)	(198)	(264)	
Uses "improved" agricultural practice (fertilizer, seed, irrigation)	0.295	0.047*	0.035	2,738
	(0.456)	(0.027)	(0.028)	

Notes: The sample used in Panel C includes all individuals who were surveyed in KLPS2 who were not enrolled in school in the year of the survey. Panel A additional restricts to those who report positive earnings at the time of survey. Panel B does not restrict on school enrollment during the year of survey, and instead restricts on reporting positive earnings between 2007 and the year of the survey. "Agricultural work" in Panel E includes both farming and pastoral activities. Each row is from a separate OLS regression. 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 meals eaten

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.
Number of meals eaten yesterday	2.16	0.096***	0.080***	5,083
	(0.64)	(0.028)	(0.023)	
Number of meals eaten yesterday, among those not in school	2.16	0.103***	0.101***	3,872
	(0.64)	(0.029)	(0.032)	
Number of meals eaten yesterday, among those in agriculture	2.13	0.076**	0.120***	2,186
	(0.63)	(0.035)	(0.035)	
Number of meals eaten yesterday, among wage earners and self-employed	2.15	0.205***	0.180***	1,263
	(0.65)	(0.059)	(0.067)	
Number of meals eaten yesterday, among wage earners	2.15	0.224***	0.173*	695
	(0.65)	(0.072)	(0.101)	
Number of meals eaten yesterday, among self-employed	2.13	0.149*	0.193*	584
	(0.69)	(0.084)	(0.079)	

Notes: The sample used here is all individuals who were surveyed in KLPS2. Rows 3-6 further restrict to those who were not enrolled in school in the year of 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: The restricted specification: coefficient estimate on moderate-heavy worm infections (two-sample instrumental variable TSIV estimates) and over-identification tests

Dependent variable	Control group variable mean (s.d.)	TSIV coefficient estimate (s.e.) on predicted years of moderate-heavy worm infection	Obs.	Over-identification test (Hansen J-statistic), p-value
Hours worked (for wages, self-employed, agriculture) in last week	18.5	-4.91**	3,873	0.183
	(23.8)	(1.92)		
Ln(Total labor earnings, past month)	7.84	-0.478**	687	0.912
	(0.85)	(0.168)		
Number of meals eaten yesterday	2.16	-0.128**	3,872	0.623
	(0.64)	(0.055)		

Notes: Two-sample instrumental variable estimates. Standard errors are bootstrapped clustering by school. Significant at 90% (*), 95% (**), 99% (***) confidence. All regressions include controls for baseline 1998 primary school population, geographic zone of the school, survey wave and month of interview, a female indicator variable, baseline 1998 school grade fixed effects, the average school test score on the 1996 Busia District mock exams, total primary school pupils within 6 km, and the cost-sharing school indicator. The instrumental variables in the first-stage are the deworming treatment indicator, the number of deworming Treatment pupils within 6 km (in '000s) demeaned, and the cost-sharing indicator variable. The Hansen J-statistic is computed using a GMM approach for the subset of respondents with 2001 parasitological data.

Table 10: The returns to deworming as a health investment

Panel A: Health investment model calibration results Deworming subsidy level	Deworming price paid by individuals (per year)	Deworming take-up rate (Kremer and Miguel 2007)	Cost (USD) per targeted student (cost x take-up x 2.41 years)	Net externality benefit, in money-metric utility (USD)	DWL below which subsidy preferable to: no subsidy [partial subsidy]
No subsidy	$1.33	5%	0	$51.42	-- [--]
Partial subsidy	$0.27	19%	$0.53	$195.40	> 27,000% [--]
Full subsidy	$0.00	75%	$2.57	$771.33	> 28,000% [>37,000%]
Panel B: Deworming as a human capital investment	Total benefits (per pupil), USD	Externality benefits only (per pupil)	Cost of deworming and DWL (per pupil), USD	Internal rate of return (per annum), total benefits	Internal rate of return (per annum), externality only
Total lifetime earnings (over 40 years) among wage earners	$3,145	$2,956	$0.78	82.7%	81.6%
Wage productivity gains only (over 40 years), wage earners	$686	$575	$0.78	58.5%	56.0%

Notes: The take-up levels and deworming subsidies and prices are taken from Kremer and Miguel (2007).

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

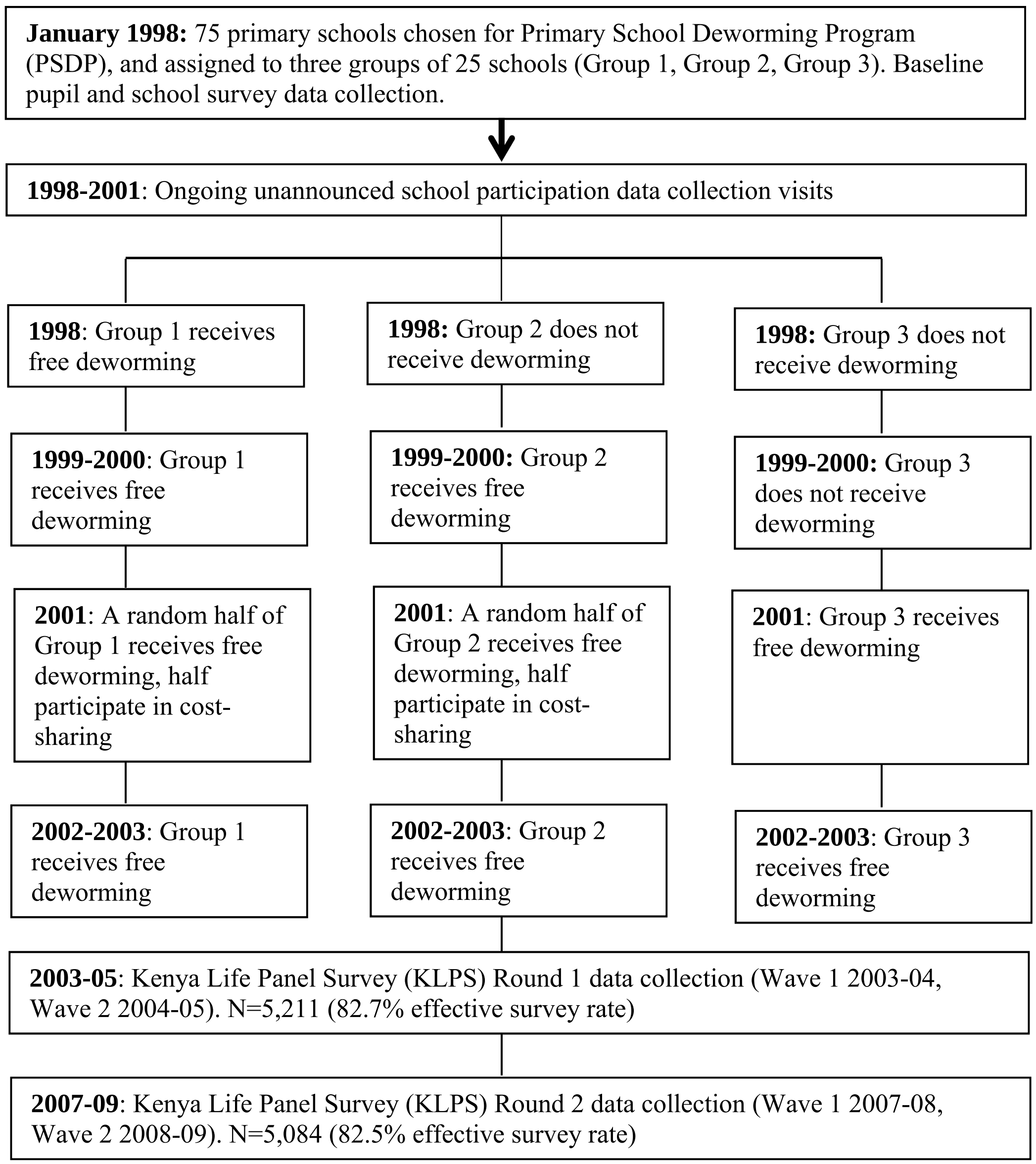


Figure 2: The distribution of hours worked in the last week, deworming treatment versus control (if working 10 to 80 hours)
Panel A (top-left): Full sample; Panel B (top-right): Agricultural work subsample;
Panel C (bottom-left): Self-employed subsample; Panel D (bottom-right): Wage earner subsample.

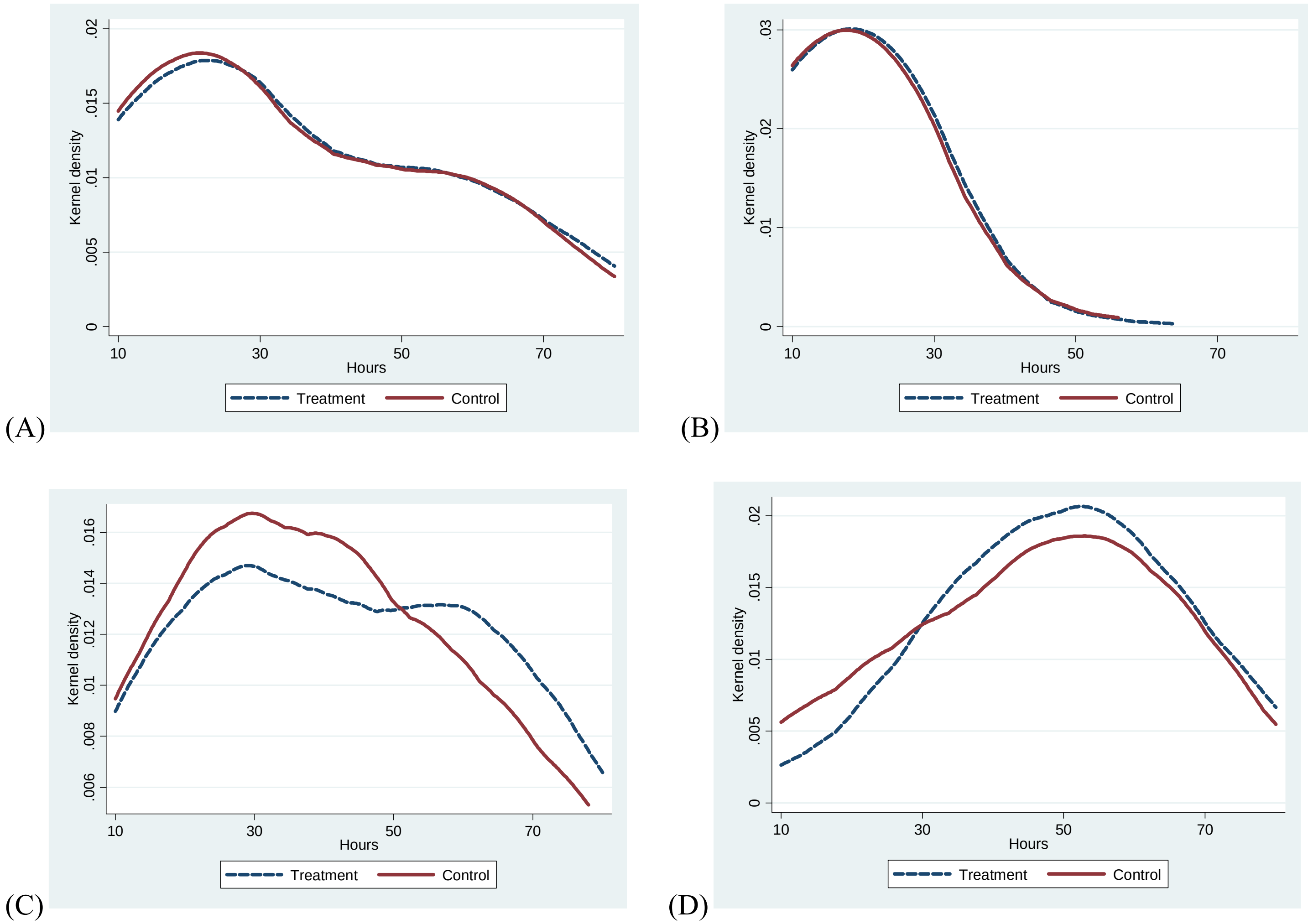
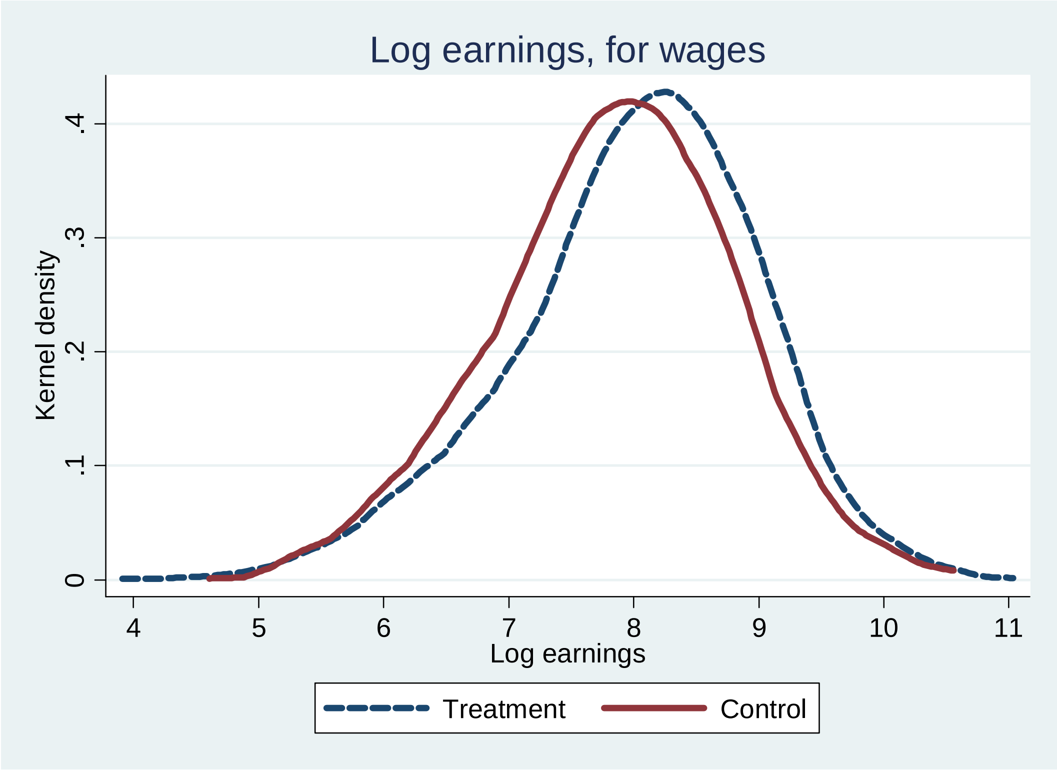


Figure 3: The distribution of log labor earnings in the last month, deworming treatment versus control (among those with positive labor earnings)



Notes: The sample used here includes all individuals who were surveyed in KLPS-2, were not enrolled in school at the time of survey and reported working for wages or in-kind in the last month. All observations are weighted to maintain initial population proportions.

Supplementary Appendix A: Theory appendix (not intended for publication)

Proposition 1: If there are competitive labor markets, so $Y(l_t) = \mathrm{w}l_t$, then the fraction of time spent working is α regardless of the wage and the time endowment in every period after the initial period.

Proof: Since there are no savings, the labor decision is separable each period. In a given period, agent maximize $U_t = {c_t}^{\alpha}(E_t - l_t)^{1-\alpha}$ such that $c_t = wl_t$. The FOC implies that $\mathrm{l_t}(1-\alpha) = \alpha(E_t - l_t)$, so $\mathrm{l_t} = \alpha E_t$. □

Proposition 2: If there are competitive labor markets so $Y(l_t) = \mathrm{w}l_t$, then the proportion of the population who deworms at a given price of deworming medicine $\widehat{P}$ is F$\left(\left(\frac{\delta}{1-\delta}x - \frac{\widehat{P}}{w}\right) * (\mathrm{w}\alpha)^{\alpha}(1-\alpha)^{1-\alpha}\right)$.

Proof: Conditional on buying deworming medicine, agents choose l in the initial period, denoted period zero, to maximize $\mathrm{U_o} = (\mathrm{wl_o} - p)^{\alpha}(E_0 - l_0)^{1-\alpha}$. The FOC simplifies to $(\mathrm{wl_0} - p)(1-\alpha) = \alpha w(E_0 - l_o)$, implying $\mathrm{l_t} = \alpha E_t + \frac{p-\alpha p}{w}$. This implies that utility in the initial period conditional on taking deworming medicine is
$(w\alpha E_o - \alpha p)^{\alpha}((1-\alpha)E_o - \frac{p-\alpha p}{w})^{1-\alpha} - d_i = (1 - \frac{p}{\mathrm{E_o}W})(\mathrm{w\alpha E_o})^{\alpha}((1-\alpha)E_o)^{1-\alpha} - d_i$, whereas the utility in the initial period conditional on not deworming is $(\mathrm{w\alpha E_o})^{\alpha}((1-\alpha)E_o)^{1-\alpha}$.Therefore, the utility decrease in the initial period is $p * \frac{(\mathrm{w\alpha})^{\alpha}((1-\alpha))^{1-\alpha}}{W} - d_i$.

After the initial period, deworming increases healthy time by x per period. Let $\underline{E_t}$ be the time endowment if someone has not taken deworming medicine. Utility conditional on deworming in each period after the initial period is therefore $\left(\mathrm{w\alpha}(\underline{E_t} + x)\right)^{\alpha}((1-\alpha)(\underline{E_t} + x))^{1-\alpha}$

$$= (1 + \frac{x}{\underline{E_t}})\left(\mathrm{w\alpha}\underline{E_t}\right)^{\alpha}(1-\alpha)\underline{E_t})^{1-\alpha}.$$

As a result, the per-period increase in utility from deworming is equal to $x * (\mathrm{w\alpha})^{\alpha}((1-\alpha))^{1-\alpha}$.

It follows that individual i will deworm if the discounted value of increased time in future periods exceeds the price of the medicine and the utility costs of deworming, or $\frac{\delta}{1-\delta}x * (w\alpha)^{\alpha}\left((1-\alpha))^{1-\alpha} - \widehat{P} * \frac{(w\alpha)^{\alpha}((1-\alpha))^{1-\alpha}}{w}\right) \geq d_i$. Aggregating over all individuals completes the proof. □

Proposition 3: Consider two different levels of subsidies, s_1 and s_2 where $s_2 > s_1$. If the government faces a deadweight loss (DWL), it prefers subsidy s_2 to s_1 if:

$$\int_{md_i=\overline{s_1}}^{\overline{s_2}} \left(\frac{\delta}{1-\delta}(x + \gamma(N-1))w\right) dF(d_i) - (s_2 - s_1)DWL \int_{d_i=0}^{\overline{s_1}} dF(d_i) - \int_{m_i=\overline{s_1}}^{\overline{s_2}} [s_2 * DWL + p + \widetilde{d}_i] dF(d_i) \geq 0 \quad (3).$$

Proof: For each person who is induced to deworm, they personally benefit by $\left(\frac{\delta}{1-\delta}xw\right)$ from increased time and society benefits by $\left(\frac{\delta}{1-\delta}\gamma w(N-1)\right)$ from the externality benefits of increased time. However, they also incur personal costs of $\tilde{d}_i$ from the disutility of medicine and p from the price of the medicine. The middle integral captures the deadweight loss from having to further subsidize people who would have dewormed with the lower subsidy. Aggregating over all of the individuals who are induced by the higher subsidy represents the monetary-equivalent impact of increased deworming. □

Corollary 1: If the government faces no deadweight loss from taxation, it weakly prefers to subsidize the price of medicine by $\frac{\delta}{1-\delta}w\gamma(N-1)$.

Proof: If $DWL = 0$, we can rewrite the government optimization problem as maximizing $\int_{d_i=\overline{s_1}}^{\overline{s_2}}\left(\frac{\delta}{1-\delta}\left(x+\gamma(N-1)\right)w-p-\tilde{d}_i\right)dF(d_i)$, since all of the other terms are 0. The solution is such that, for the marginal user $\left(\frac{\delta}{1-\delta}x-\frac{p-\frac{\delta}{1-\delta}w\gamma(N-1)}{w}\right)-\tilde{d}_i=0$, since for lower subsidies the integrand is (weakly) positive, and for higher subsidies it is (weakly) negative. This implies that the social planner solution is to subsidize deworming by the amount of its externality. □

Corollary 3: If a given increase in subsidy levels does not increase take up, the lower subsidy yields greater social welfare.
If a given increase does not increase take-up, then all it induces are extra costs, and therefore is worse for welfare. This is weakly true even if DWL = 0. □

Proposition 4: In the absence of land and labor markets, agriculturalists will work a constant fraction of their time $\alpha\frac{1-\beta}{1-\alpha\beta}\leq\alpha$, regardless of their total stock of time or land.

Proof: For an amount of labor L, the agriculturalist gets to consume $A\overline{K}^{\beta}L^{1-\beta}$, and so wants to maximize utility $\left(A\overline{K}_i^{\beta}L^{1-\beta}\right)^{\alpha}(E_t-L)^{1-\alpha}$. The FOC implies that $\alpha(1-\beta)(E_t-L)=(1-\alpha)L$, so $L=\alpha\frac{1-\beta}{1-\alpha\beta}E_t$, which less than what the wage earner worked unless $\beta=0$ (in which case returns in agriculture would also be linear). □

Corollary 2. Suppose agent i works z hours more after an intervention, before which their utility was u. By assuming perfect labor markets, one would then calculate their new utility as $u\left(1+\frac{z}{\alpha E_t}\right)$*, which is below the true new utility of* $u\left(1+\frac{z}{\alpha E_t}\frac{1-\alpha\beta}{1-\beta}\right)^{1-\alpha\beta}$ if $\alpha<.5$.

Proof: For $\beta=0$ or $z=0$, $u\left(1+\frac{z}{\alpha E_t}\right)=\left(1+\frac{z}{\alpha E_t}\frac{1-\alpha\beta}{1-\beta}\right)^{1-\alpha\beta}$. For $\beta>0$, we do the proof by looking at the increase in calculated utility as work hours go up. If we assume perfect labor markets, the derivative of calculated utility with respect to increases in work hours is $\frac{u}{\alpha E_t}$. However, since people are adjusting work hours less than assumed, the true marginal increase in utility is actually $\frac{u}{\alpha E_t}\frac{(1-\alpha\beta)^2}{1-\beta} * \left[\left(1 + \frac{z}{\alpha E_t}\frac{1-\alpha\beta}{1-\beta}\right)^{-\alpha\beta}\right]$. The term in brackets is strictly greater than 1 for $\beta > 0$, and the coefficient outside the brackets is greater than one as long if $2\alpha - 1 < \alpha^2\beta$, which implies that the true utility multiplier is greater than the imputed one for any positive change of hours if land is relevant for agriculture. □

Proposition 5: Increasing deworming subsidies weakly increases participation in non-agricultural work.

Proof [incomplete]: This follows from the fact that, since wage earners are unconstrained, they benefit relatively more from extra time than do those in agriculture, and so subsidies would never induce people to switch away from non-agricultural work. Anyone who before the increase was indifferent to switching sectors would now switch away from agriculture, since the gains to deworming from staying are lower. □

Supplementary Appendix B: 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)[22], 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 not find statistically significant average project impacts on a wide range of educational outcomes.[23] The schools that benefited from these previous programs were found in all eight geographic zones; the distribution of the 27 schools across the eight zones is: Agenga/Nanguba (5 schools), Bunyala Central (1), Bunyala North (4), Bunyala South (2), Bwiri (4), Funyula (5), Namboboto (1), Nambuku (5). The results in the current paper are robust to including controls for inclusion in these other NGO programs (results not shown).

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

Deaton (2010) raises concerns about the list randomization approach, in the case where the first school listed in the first randomization "triplet" is different than other schools (in our case, it has lower than average school enrolment); the same concerns would apply to several other well-known recent field experiments in development economics, most notably Chattopadhyay and Duflo's 2004 paper "Women as policymakers: Evidence from a randomized policy experiment in India" in *Econometrica*. However, this is not a major threat to our empirical approach. Following Bruhn and McKenzie (2009) we include all variables used in the randomization procedure (such as baseline

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[22] 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).

[23] 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.

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.

Supplementary Appendix Table A1: Baseline (1998) summary statistics and PSDP randomization checks, subsample not enrolled in school

	All mean (s.d.)	Treatment mean (s.d.)	Control mean (s.d.)	Treatment – Control (s.e.)	Kolmogorov-Smirnov p-value
Age (1998)	12.7	12.6	12.7	-0.114	0.319
	(2.4)	(2.4)	(2.4)	(0.118)	
Grade (1998)	4.56	4.54	4.61	-0.072	0.207
	(1.64)	(1.66)	(1.59)	(0.063)	
Female	0.500	0.496	0.508	-0.012	--
	(0.500)	(0.500)	(0.500)	(0.022)	
School average test score (1996)	0.013	0.009	0.020	-0.011	0.266
	(0.417)	(0.425)	(0.400)	(0.105)	
Primary school located in Budalangi division	0.386	0.375	0.408	-0.033	--
	(0.487)	(0.484)	(0.492)	(0.139)	
Population of primary school	477	498	433	65	0.370
	(218)	(241)	(148)	(55)	
Total treatment (Group 1, 2) primary school students within 6 km	3156	3071	3335	-264	0.193
	(923)	(845)	(1064)	(271)	
Total primary school students within 6 km	4663	4661	4667	-6.83	0.243
	(1352)	(1235)	(1571)	(400)	

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 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)	13.3	13.1	13.5	-0.358	0.285
	(2.4)	(2.4)	(2.5)	(0.281)	
Grade (1998)	4.86	4.84	4.90	-0.057	0.477
	(1.61)	(1.63)	(1.57)	(0.142)	
Female	0.235	0.208	0.289	-0.081*	--
	(0.424)	(0.406)	(0.454)	(0.046)	
School average test score (1996)	-0.014	-0.030	0.020	-0.049	0.301
	(0.411)	(0.415)	(0.400)	(0.109)	
Primary school located in Budalangi division	0.412	0.434	0.397	0.037	--
	(0.494)	(0.496)	(0.490)	(0.146)	
Population of primary school	480	506	427	78	0.348
	(220)	(247)	(137)	(57)	
Total treatment (Group 1, 2) primary school students within 6 km	3196	3111	3369	-258	0.168
	(906)	(801)	(1069)	(290)	
Total primary school students within 6 km	4718	4728	4699	29	0.204
	(1331)	(1176)	(1602)	(430)	

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 A3: Baseline (1998) summary statistics and attrition checks

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

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

Supplementary Appendix Table A4: Impacts on school enrollment and participation

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

Notes: The sample used in Panel A includes all individuals who were surveyed in KLPS2. The sample used in Panel B includes a subset of these respondents 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 respondents 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 A5: Deworming impacts on labor market outcomes among subgroups

	Dependent variable:								
	Hours worked (for wages, self-employed, agriculture) last week			Ln(Total labor earnings, past month)			Number of meals eaten yesterday		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Deworming Treatment indicator	4.55**	2.48*	3.16***	0.274***	0.260*	0.302***	0.160***	0.060	0.100***
	(1.87)	(1.49)	(1.10)	(0.106)	(0.140)	(0.092)	(0.046)	(0.042)	(0.028)
Female	-7.10***	-9.04***	-9.05***	-0.482***	-0.433***	-0.427***	0.168***	0.092***	0.093***
	(1.99)	(1.06)	(1.06)	(0.157)	(0.103)	(0.100)	(0.058)	(0.029)	(0.029)
Female * Treatment	-2.83	--	--	0.093	--	--	-0.112*	--	--
	(2.28)			(0.213)			(0.066)		
Grades 5-7 in 1998	--	4.00*	--	--	0.372***	--	--	-0.046	--
		(2.06)			(0.138)			(0.043)	
Grades 5-7 * Treatment	--	1.14	--	--	0.057	--	--	0.085	--
		(2.38)			(0.168)			(0.055)	
Moderate-heavy worm infection rate at the zonal level (1998), demeaned	--	--	-1.40	--	--	-0.029	--	--	-0.026
			(1.02)			(0.085)			(0.024)
Moderate-heavy infection rate * Treatment	--	--	2.09**	--	--	0.051	--	--	0.013
			(0.86)			(0.079)			(0.025)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.064	0.062	0.065	0.186	0.174	0.186	0.038	0.037	0.036
Observations	3,873	3,873	3,873	687	687	687	3,872	3,872	3,872
Mean (s.d.) in the control group	18.5	18.5	18.5	7.8	7.8	7.8	2.16	2.16	2.16
	(23.8)	(23.8)	(23.8)	(0.85)	(0.85)	(0.85)	(0.64)	(0.64)	(0.64)

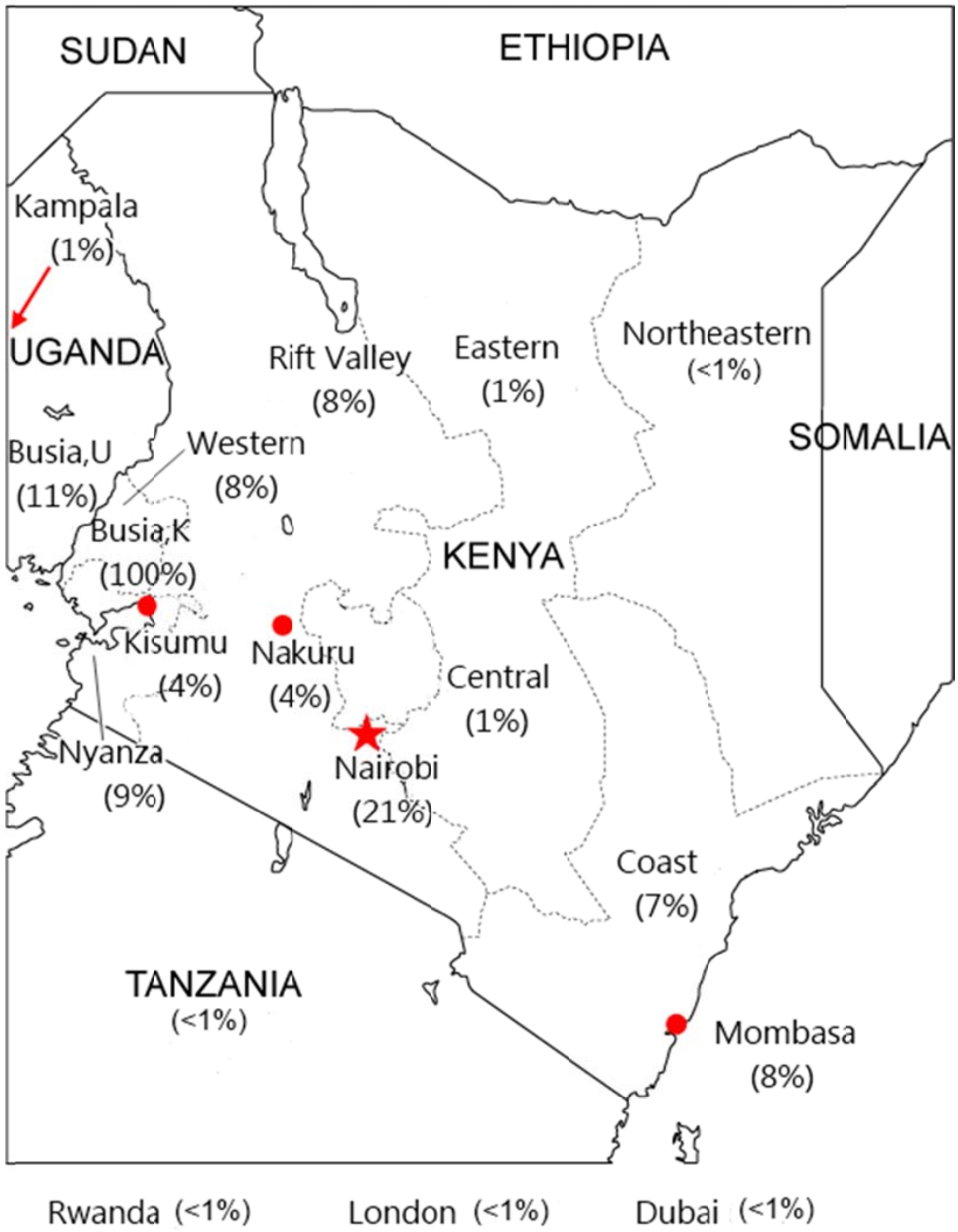
Notes: The sample used in columns (1)-(3) and (7)-(9) include all individuals surveyed in the KLPS2 with data for the relevant dependent variable who were not enrolled in school at the time of survey. The sample used in columns (4)-(6) additionally restricts to those who report positive labor earnings at the time of survey. 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 A6: Social returns to child deworming as a human capital investment

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

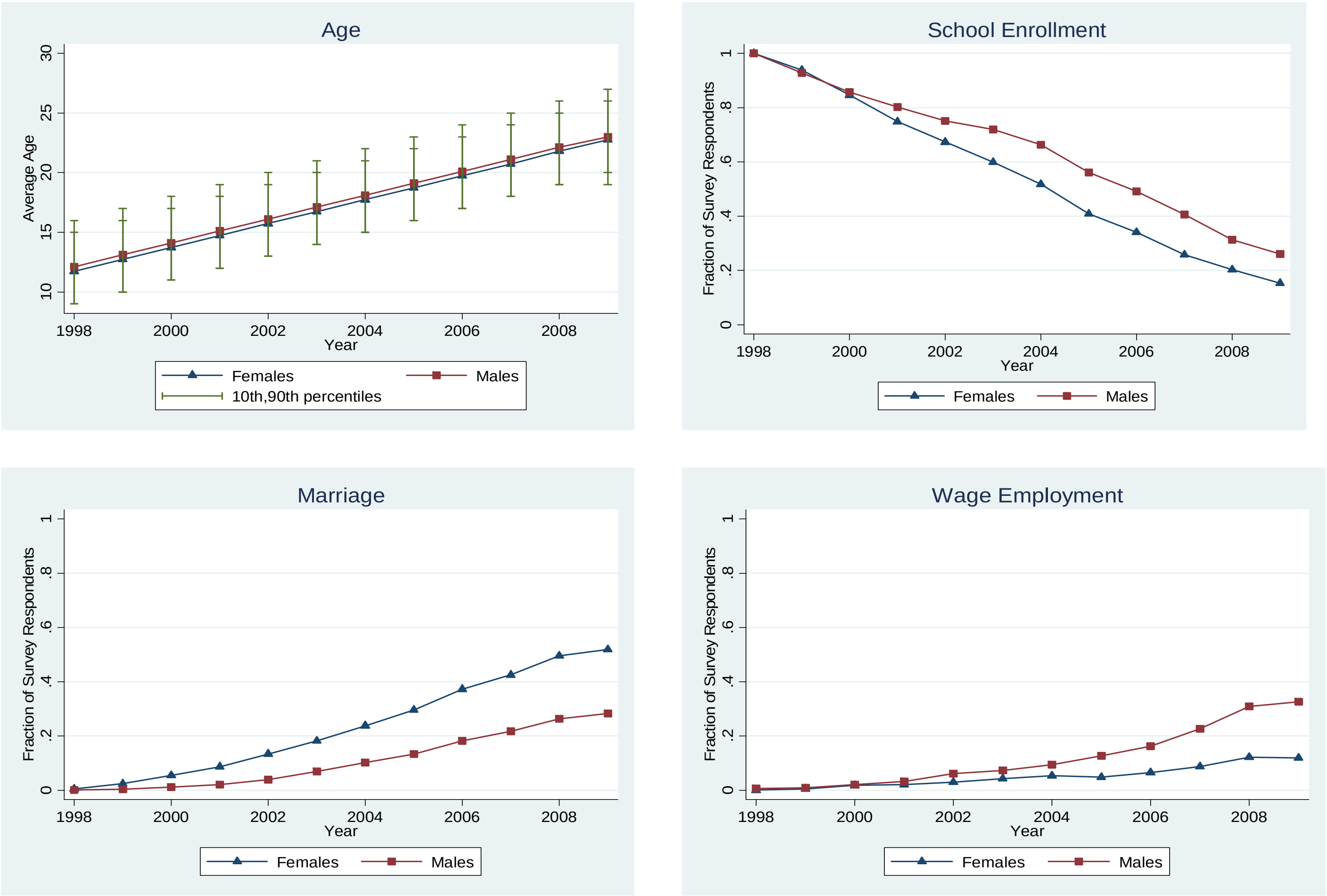
Notes: Calculated in the KLPS sample assuming alternately that (i) only those in the wage earner subsample have earnings gains, or (ii) the entire sample experiences the gains observed in the wage earner subsample. The details of the construction of benefits and costs are in the text. The cost of deworming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

Supplementary Appendix Figure A1: Migration residential location map

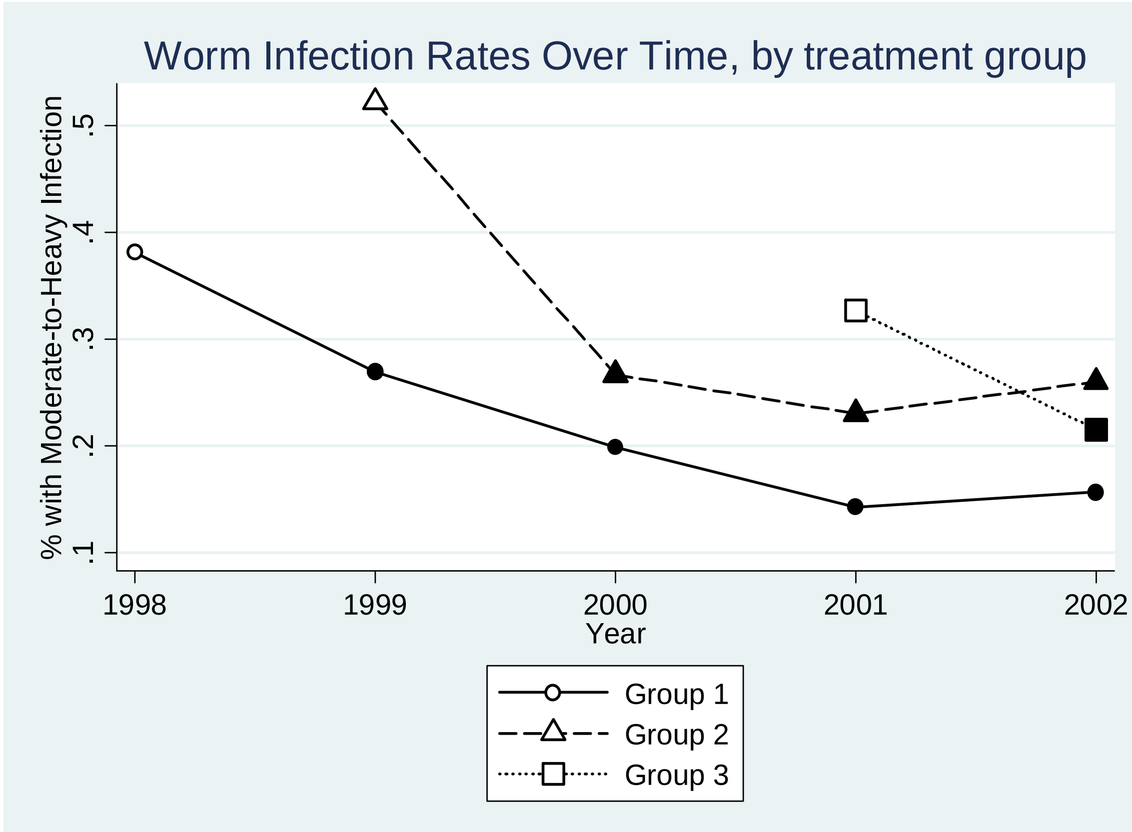


Notes: Percentages sum to greater than one, since they capture residential location (for at least four consecutive months) at any point during 1998-2009.

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



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



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