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WORMS: IDENTIFYING IMPACTS ON EDUCATION AND HEALTH IN THE PRESENCE OF TREATMENT EXTERNALITIES

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Intestinal helminths—including hookworm, roundworm, whipworm, and schistosomiasis—infect more than one-quarter of the world's population. Studies in which medical treatment is randomized at the individual level potentially doubly underestimate the benefits of treatment, missing externality benefits to the comparison group from reduced disease transmission, and therefore also underestimating benefits for the treatment group. We evaluate a Kenyan project in which school-based mass treatment with deworming drugs was randomly phased into schools, rather than to individuals, allowing estimation of overall program effects. The program reduced school absenteeism in treatment schools by one-quarter, and was far cheaper than alternative ways of boosting school participation. Deworming substantially improved health and school participation among untreated children in both treatment schools and neighboring schools, and these externalities are large enough to justify fully subsidizing treatment. Yet we do not find evidence that deworming improved academic test scores.

I suppose they want to see what external effects deworming has in form of school att and test scores

KEYWORDS: Health, education, Africa, externalities, randomized evaluation, worms.

1. INTRODUCTION

HOOKWORM, ROUNDWORM, WHIPWORM, and schistosomiasis infect one in four people worldwide. They are particularly prevalent among school-age children in developing countries. We examine the impact of a program in which seventy-five rural Kenyan primary schools were phased into deworming treatment in a randomized order. We find that the program reduced school absenteeism by at least one-quarter, with particularly large participation gains among the youngest children, making deworming a highly effective way to boost school participation among young children. We then identify cross-school externalities—the impact of deworming for pupils in schools located near treatment schools—using exogenous variation in the local density of treatment school pupils generated by the school-level randomization, and find that deworming reduces worm burdens and increases school participation among

exp many worms in children

worms contagious, spillover in schools and across schools

schools were randomly assigned, but not pupils

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children in neighboring primary schools. There is also some evidence of within-school treatment externalities, although given that randomization took place across schools, rather than across pupils within schools, we cannot use experimental identification to decompose the overall effect on treatment schools into a direct effect and a within-school externality effect, and must rely on necessarily more tentative nonexperimental methods.

Including the externality benefits, the cost per additional year of school participation is only \$3.50, making deworming considerably more cost-effective than alternative methods of increasing school participation, such as school subsidies (see Kremer (2003)). Moreover, internalizing these externalities would likely require not only fully subsidizing deworming, but actually paying people to receive treatment.

worm cheapest to boost school att

We do not find any evidence that deworming increased academic test scores. However, the school participation gains we estimate are not large enough to generate statistically significant test score gains given the observed cross-sectional relationship between school attendance and test scores.

deworm no effect on test scores

There is a large literature documenting positive correlations between health and economic outcomes. Our results suggest a causal link running from health to education.[2] The finding that treatment externalities are large also suggests a potentially important role for subsidies for treatment, especially given that nearly half of Africa's disease burden is due to infectious and parasitic disease (WHO (1999)).

much lit: corr betw health and econ. here: causal health to econ

Our approach can be distinguished from that in several recent studies in which treatment is typically randomized at the individual level and its educational impact is estimated by comparing cognitive ability among those treatment and comparison pupils who attend a later testing session. Dickson et al. (2000) review these studies and conclude that they do not provide convincing evidence for educational benefits of deworming. However, these studies fail to account for potential externalities for the comparison group from reduced disease transmission. Moreover, if externalities benefit the comparison group, outcome differences between the treatment and comparison groups will understate the benefits of treatment on the treated. This identification problem is closely related to the well-known issue of contamination of experimental job programs in active labor markets, where programs have externality effects on program nonparticipants (typically by worsening their outcomes, as discussed in Heckman, LaLonde, and Smith (1999)).

collective treatment shows external effects. ind. treatment not (Q!)

[2] Refer to Strauss and Thomas (1998) for a survey of the literature on health and income. While nonexperimental studies have found that poor early childhood nutrition is associated with delayed primary school enrollment and reduced academic achievement in Ghana (Glewwe and Jacoby (1995)) and the Philippines (Glewwe, Jacoby, and King (2001)), and several prospective studies suggest iron supplementation improves academic outcomes of anemic children (Nokes, van den Bosch, and Bundy (1998)), Behrman's (1996) review argues that given the limited experimental evidence and the difficulty of inferring causality from correlations in nonexperimental data, aside from anemia, the existing literature on child health and education is inconclusive.

<- Q2! answer teil

We use two approaches to deal with the problem of identification in the presence of local externalities. First, because randomization took place at the level of schools, we are able to estimate the overall effect of deworming on a school even if there are treatment externalities among pupils within the school. Second, we identify cross-school externalities—the impact of deworming for pupils in schools located near treatment schools—using exogenous variation in the local density of treatment school pupils generated by the school-level randomization. As discussed above, we find large deworming treatment externalities both on health and education, and our analysis suggests that failure to account for these externalities would lead to substantially underestimating the impacts of deworming.

they use school randomziatio and statistics to find local externalities

The paper is organized as follows. Section 2 reviews the existing literature on helminths and education. Section 3 describes the project we evaluate in rural Kenya and presents the baseline educational and medical characteristics. Section 4 describes the estimation strategy. Sections 5, 6, and 7 discuss the program's effect on health, school participation, and test scores, respectively. Section 8 examines the cost-effectiveness of deworming relative to other ways of improving health and school participation and argues the estimated externalities justify fully subsidizing deworming. The final section summarizes and discusses implications of the results.

structure of paper

2. INTESTINAL HELMINTH (WORM) INFECTIONS

Hookworm and roundworm each infect approximately 1.3 billion people around the world, while whipworm affects 900 million and 200 million are infected with schistosomiasis (Bundy (1994)).While most have light infections, which may be asymptomatic, a minority have heavy infections, which can lead to iron-deficiency anemia, protein-energy malnutrition, abdominal pain, and listlessness. [3] Schistosomiasis can also have more severe consequences, for instance, causing enlargement of the liver and spleen.

wroms are bad

Low-cost single-dose oral therapies can kill the worms, reducing hookworm, roundworm, and schistosomiasis infections by 99 percent, although single-dose treatments are only moderately effective against severe whipworm infections (Butterworth et al. (1991), Nokes et al. (1992), Bennett and Guyatt (2000)). Reinfection is rapid, however, with worm burden often returning to eighty percent or more of its original level within a year (Anderson and May (1991)), and hence geohelminth drugs must be taken every six months and schistosomiasis drugs must be taken annually. The World Health Organization has endorsed mass school-based deworming programs in areas with high helminth infections, since this eliminates the need for costly individual parasitological screening (Warren et al. (1993), WHO (1987)), bringing cost down to as little

worms therapry...

[3]Refer to Adams et al. (1994), Corbett et al. (1992), Hotez and Pritchard (1995), and Pollitt (1990).

as 49 cents per person per year in Africa (PCD (1999)). Known drug side effects are minor, and include stomach ache, diarrhea, dizziness, and vomiting in some cases (WHO (1992)). However, due to concern about the possibility that the drugs could cause birth defects (WHO (1992), Cowden and Hotez (2000)), standard practice in mass deworming programs has been to not treat girls of reproductive age (Bundy and Guyatt (1996)).[4]

.. is cheap

Medical treatment could potentially interfere with disease transmission, creating positive externalities. School-aged children likely account for the bulk of helminth transmission (Butterworth et al. (1991)). Muchiri, Ouma, and King (1996) find that school children account for 85 to 90 percent of all heavy schistosomiasis infections in nine eastern Kenyan villages. Moreover, conditional on infection levels, children are most likely to spread worm infections because they are less likely to use latrines and more generally have poor hygiene practices (Ouma (1987), Butterworth et al. (1991)).[5]

Q! mostly children spread, and treatment stops spread

Treatment externalities for schistosomiasis are likely to take place across larger areas than is typical for geohelminth externalities due to the differing modes of disease transmission. Geohelminth eggs are deposited in the local environment when children defecate in the "bush" surrounding their home or school, while the schistosomiasis parasite is spread through contact with infected fresh water. Children in the area are often infected with schistosomiasis by bathing or fishing in Lake Victoria, and children who live some distance from each other may bathe or fish at the same points on the lake. Moreover, the water-borne schistosome may be carried considerable distances by stream and lake currents, and the snails that serve as its intermediate hosts are themselves mobile.

different spread ways

In the absence of frequent reinfection, individual worm burdens are likely to fall rapidly given the relatively short typical life spans of intestinal worms: twelve months for roundworm and whipworm, two years for hookworm, and three years for schistosomiasis (Bundy and Cooper (1989), Anderson and May (1991)), so that if the age of worms within a human host is uniformly distributed, worm burden may halve in six to eighteen months depending on the worm. There is existing only limited empirical evidence on deworming treatment externalities, but that which exists suggests that school-based deworming may create substantial externalities.[6] However, these studies rely on pre-post

1-3 years one worm infection

[4]With a lengthening track record of safe use, this practice is now changing.

[5]Animal-human transmission is not a serious concern in this area for hookworm, whipworm, and schistosomiasis (Cambridge University Schistosomiasis Research Group (2000), Corwin (2000)), and is unlikely to be a major concern for roundworm. A roundworm species that predominantly infects pigs (*Ascaris suum*) may also sometimes infect humans, but is unlikely to be a major problem in this area since fewer than 15 percent of households keep pigs at home.

[6]Adult worm burden fell by nearly fifty percent after fifteen months on the island of Montserrat in communities where children were mass treated for worms (Bundy et al. (1990)). We examine four other related studies—two of which do not explicitly discuss externalities, but whose published results allow us to compute them—and find reductions of up to fifty percent in infec-

comparisons in the same villages to estimate externalities for untreated individuals. This leaves them without a plausible comparison group, which is particularly problematic since infection rates vary widely seasonally and from year to year due to rainfall variation and other factors (Kloos et al. (1997)). The randomized phase-in across schools of the deworming intervention that we examine allows us to capture the overall effect of deworming even in the presence of externalities across individuals within schools. School-level randomization also naturally generates local variation in the density of treatment that we use to estimate spillovers across schools. Our sample of 75 schools is also much larger than existing studies, which were typically conducted in five or fewer villages.

The educational impact of deworming is considered a key issue in assessing whether the poorest countries should accord priority to deworming (Dickson et al. (2000)). It has been hypothesized that intense worm infections reduce educational achievement (Bundy (1994), Del Rosso, Miller, and Marek (1996), Drake et al. (1999), Stoltzfus et al. (1997)), either by inducing anemia, which is known to affect educational outcomes (Nokes, van den Bosch, and Bundy (1998)), or through other channels, including protein-energy malnutrition. However, in an influential Cochrane review published in the *British Medical Journal*, Dickson et al. (2000) claim that "the evidence of benefit for mass [deworming] treatment of children related to positive effects on [physical] growth and cognitive performance is not convincing. In light of these data, we would be unwilling to recommend that countries or regions invest in programmes that routinely treat children with anthelmintic drugs."

literature disproving worm treat effect on education

Yet the existing randomized evaluations on worms and education on which Dickson et al. (2000) base their conclusions suffer from several shortcomings. First, existing studies randomize the provision of deworming treatment *within* schools to treatment and placebo groups, and then examine the impact of deworming on cognitive outcomes. Their within-school randomization designs prevent existing studies from credibly estimating externality benefits. Moreover, the difference in educational outcomes between the treatment and placebo groups understates the actual impact of deworming on the treatment group if placebo group pupils also experience health gains due to local treatment externalities. In fact, re-examination of these recent randomized studies suggests that untreated placebo pupils often experienced substantial worm load reductions, as would be consistent with the hypothesis of within-school externalities.[7]

my authors disproce that bc not school level randomization

tion intensity among untreated individuals in communities where school children received mass deworming (Butterworth et al. (1991), Holland et al. (1996), Muchiri, Ouma, and King (1996), Thein-Hlaing, Than-Saw, and Myat-Lay-Kyin (1991)).

[7]In Simeon, Grantham-McGregor, Callender, and Wong (1995), all pupils started with heavy whipworm infections (over 1200 eggs per gram, epg). Thirty-two weeks into the study, heavy infections fell 95 percent in the treatment group and 43 percent among the placebo group, and treatment and placebo pupils showed an identical gain of 0.3 in body mass index (low body mass index is associated with acute nutritional deficiencies). Simeon, Grantham-McGregor, and Wong

A second shortcoming of existing randomized studies is that although they report the impact of deworming on tests of cognitive performance (such as tests of recall), they typically do not examine other outcomes of interest to policymakers, including school attendance, enrollment, academic test scores, or grade promotion. Only two studies examine effects on attendance and both should be interpreted with caution since the data were drawn from attendance registers, which are notoriously inaccurate in many developing countries. Treating growth-stunted Jamaican children with heavy whipworm infections increased school attendance by 9.9 percentage points, reducing absenteeism by one-third (Simeon, Grantham-McGregor, Callender, and Wong (1995)).Thirty-five percent of pupils were missing attendance data. Watkins, Cruz, and Pollitt (1996a, 1996b) find no effect of treatment of roundworm and whipworm on primary school attendance. However, periods of extended school absence are dropped, leading to high rates of recorded attendance (90 percent). If treated pupils were healthier and had fewer inactive periods, this creates attrition bias and will thus understate the true impact of deworming on school attendance. However, nonexperimental studies suggest that worms do affect school participation. [8]

... other studies omit other vars apart from cognigitve performance like attendance

To the extent that deworming increases school participation, as we suggest, other existing studies may also suffer serious attrition bias. For example, Nokes et al. (1992) report test score data for 89 percent of students in their treatment group but only 59 percent in their comparison group.

(1995), which was conducted among a subsample of the study population in Simeon, Grantham-McGregor, Callender, and Wong (1995), find that median whipworm load fell from 2523 epg for the treatment pupils pre-treatment, to 0 epg after 32 weeks, while among placebo pupils median whipworm load fell from 2946 to 1724 epg, a drop of roughly one-third among placebo pupils. In Nokes et al. (1992), average hookworm infection intensity fell by fifty percent among the placebo pupils (although there was no change in roundworm or whipworm infection for placebo pupils). Since the samples in these studies were selected based on high worm load, the fall in worm load among placebo pupils could potentially be due to mean reversion as well as to externalities. However, Watkins, Cruz, and Pollitt (1996a) did not select their sample based on worm load, and find that mean roundworm epg fell roughly 25 percent among placebo pupils after twenty-four weeks of treatment with albendazole.

[8]Geissler et al. (2000) interviewed school children from a nearby region of western Kenya, and argue that worms may caused school absence in five percent of all interviews (and account for nearly half of all absences). Bleakley (2002) finds that areas in the U.S. South with higher hookworm infection levels prior to the 1910–1920 Rockefeller Sanitary Commission deworming campaign experienced greater increases in school attendance after the intervention, and estimates that each case of hookworm reduced the number of children attending school by 0.23 (which is similar to our estimates presented below). Although it is difficult to fully rule out omitted variable bias using a nonexperimental approach, an important strength of Bleakley (2002) is that the Rockefeller campaign was introduced throughout a large geographic area, and thus the estimates are not subject to the biases faced by medical studies that randomize treatment at the individual level. (Brinkley (1994) argues that the Rockefeller campaign also dramatically increased agricultural productivity.)

3. THE PRIMARY SCHOOL DEWORMING PROJECT IN BUSIA, KENYA

We evaluate the Primary School Deworming Project (PSDP), which was carried out by a Dutch nonprofit organization, Internationaal Christelijk Steunfonds Africa (ICS), in cooperation with the Busia District Ministry of Health office. The project took place in southern Busia, a poor and densely-settled farming region in western Kenya, in an area with the highest helminth infection rates in Busia district. The 75 project schools consist of nearly all rural primary schools in this area, and had a total enrolment of over 30,000 pupils between ages six to eighteen.

75 schools
3ok kids

In January 1998, the seventy-five PSDP schools were randomly divided into three groups of twenty-five schools each: the schools were first stratified by administrative subunit (zone) and by their involvement in other nongovernmental assistance programs, and were then listed alphabetically and every third school was assigned to a given project group.[9] Due to ICS's administrative and financial constraints, the health intervention was phased in over several years. Group 1 schools received free deworming treatment in both 1998 and 1999, Group 2 schools in 1999, while Group 3 schools began receiving treatment in 2001. Thus in 1998, Group 1 schools were treatment schools, while Group 2 and Group 3 schools were comparison schools, and in 1999, Group 1 and Group 2 schools were treatment schools and Group 3 schools were comparison schools.

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25 deworm in 98+99
25 deworm in 99
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deworm = treatment

3.1. *Baseline Characteristics*

ICS field staff administered pupil and school questionnaires in early 1998 and again in early 1999. Prior to treatment, the groups were similar on most demographic, nutritional, and socioeconomic characteristics, but despite randomized assignment—which produces groups with similar characteristics in expectation—Group 1 pupils appear to be worse off than Group 2 and 3 pupils along some dimensions, potentially creating a bias against finding significant program effects (Table I). There are no statistically significant differences across Group 1, 2, and 3 schools in enrolment, distance to Lake Victoria, school sanitation facilities, pupils' weight-for-age,[10] asset ownership, self-reported malaria, or the local density of other primary school pupils located within three kilometers or three to six kilometers. Helminth infection rates in the surrounding geographic zone are also nearly identical across the three groups. School attendance rates did not differ significantly in early 1998 before the first round of medical treatment, although this baseline attendance

datenerhebung bevor treat 98,99

significant diffs between groups in some vars

[9]Twenty-seven of the seventy-five project schools were also involved in other NGO projects, which consisted of financial assistance for textbook purchase and classroom construction, and teacher performance incentives. Appendix Table AI presents a detailed project timeline.

[10]Unfortunately, due to problems with field data collection, we do not have usable baseline height data.

TABLE I

1998 AVERAGE PUPIL AND SCHOOL CHARACTERISTICS, PRE-TREATMENT[a]

	Group 1 (25 schools)	Group 2 (25 schools)	Group 3 (25 schools)	Group 1 – Group 3	Group 2 – Group 3
Panel A: Pre-school to Grade 8					
Male	0.53	0.51	0.52	0.01	−0.01
				(0.02)	(0.02)
Proportion girls <13 years, and all boys	0.89	0.89	0.88	0.00	0.01
				(0.01)	(0.01)
Grade progression (= Grade − (Age − 6))	−2.1	−1.9	−2.1	−0.0	0.1
				(0.1)	(0.1)
Year of birth	1986.2	1986.5	1985.8	0.4**	0.8***
				(0.2)	(0.2)
Panel B: Grades 3 to 8					
Attendance recorded in school registers (during the four weeks prior to the pupil survey)	0.973	0.963	0.969	0.003	−0.006
				(0.004)	(0.004)
Access to latrine at home	0.82	0.81	0.82	0.00	−0.01
				(0.03)	(0.03)
Have livestock (cows, goats, pigs, sheep) at home	0.66	0.67	0.66	−0.00	0.01
				(0.03)	(0.03)
Weight-for-age Z-score (low scores denote undernutrition)	−1.39	−1.40	−1.44	0.05	0.04
				(0.05)	(0.05)
Blood in stool (self-reported)	0.26	0.22	0.19	0.07**	0.03
				(0.03)	(0.03)
Sick often (self-reported)	0.10	0.10	0.08	0.02**	0.02**
				(0.01)	(0.01)
Malaria/fever in past week (self-reported)	0.37	0.38	0.40	−0.03	−0.02
				(0.03)	(0.03)
Clean (observed by field workers)	0.60	0.66	0.67	−0.07**	−0.01
				(0.03)	(0.03)
Panel C: School characteristics					
District exam score 1996, grades 5–8[b]	−0.10	0.09	0.01	−0.11	0.08
				(0.12)	(0.12)
Distance to Lake Victoria	10.0	9.9	9.5	0.6	0.5
				(1.9)	(1.9)
Pupil population	392.7	403.8	375.9	16.8	27.9
				(57.6)	(57.6)
School latrines per pupil	0.007	0.006	0.007	0.001	−0.000
				(0.001)	(0.001)
Proportion moderate-heavy infections in zone	0.37	0.37	0.36	0.01	0.01
				(0.03)	(0.03)
Group 1 pupils within 3 km[c]	461.1	408.3	344.5	116.6	63.8
				(120.3)	(120.3)
Group 1 pupils within 3–6 km	844.5	652.0	869.7	−25.1	−217.6
				(140.9)	(140.9)

TABLE I
(CONTINUED)

	Group 1 (25 schools)	Group 2 (25 schools)	Group 3 (25 schools)	Group 1 – Group 3	Group 2 – Group 3
Total primary school pupils within 3 km	1229.1	1364.3	1151.9	77.2 (205.5)	212.4 (205.5)
Total primary school pupils within 3–6 km	2370.7	2324.2	2401.7	−31.1 (209.5)	−77.6 (209.5)

[a] School averages weighted by pupil population. Standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Data from the 1998 ICS Pupil Namelist, 1998 Pupil Questionnaire and 1998 School Questionnaire.

[b] 1996 District exam scores have been normalized to be in units of individual level standard deviations, and so are comparable in units to the 1998 and 1999 ICS test scores (under the assumption that the decomposition of test score variance within and between schools was the same in 1996, 1998, and 1999).

[c] This includes girls less than 13 years old, and all boys (those eligible for deworming in treatment schools).

information comes from school registers, which are not considered reliable in Kenya.

To the extent that there were significant differences between treatment and comparison schools, treatment schools were initially somewhat worse off. Group 1 pupils had significantly more self-reported blood in stool (a symptom of schistosomiasis infection), reported being sick more often than Group 3 pupils, and were not as clean as Group 2 and Group 3 pupils (as observed by NGO field workers). They also had substantially lower average scores on 1996 Kenyan primary school examinations than Group 2 and 3 schools, although the difference is not significant at traditional confidence levels.

treatment schools at beginning worse health

In January and February 1998, prior to treatment, a random sample of ninety grade three to eight pupils (fifteen per grade) in each of the 25 Group 1 schools were selected to participate in a parasitological survey conducted by the Kenya Ministry of Health, Division of Vector Borne Diseases.[11] Ninety-two percent of surveyed pupils had at least one helminth infection and thirty-seven percent had at least one moderate-to-heavy helminth infection (Table II),[12] although these figures understate actual infection prevalence to the extent that the most heavily infected children were more likely to be absent from school on the day of the survey. Worm infection rates are relatively high in this region by international standards, but many other African settings have similar

stichprobe beginn 98: 92% aller kids worm

[11] Following the previous literature, infection intensity is proxied for worm eggs per gram (epg) in stool (Medley and Anderson (1985)). Each child in the parasitological sample was given a plastic container and asked to provide a stool sample; samples were examined in duplicate within twenty-four hours using the Kato-Katz method. Group 2 and Group 3 schools were not included in the 1998 parasitological survey since it was not considered ethical to collect detailed health information from pupils who were not scheduled to receive medical treatment in that year.

[12] Following Brooker, Miguel, et al. (2000), thresholds for moderate infection are 250 epg for *Schistosomiasis. mansoni* and 5,000 epg for Roundworm, the WHO standards, and 750 epg for Hookworm and 400 epg for Whipworm, both somewhat lower than the WHO standard.

TABLE II

JANUARY 1998 HELMINTH INFECTIONS, PRE-TREATMENT, GROUP 1 SCHOOLS[a]

	Prevalence of infection	Prevalence of moderate-heavy infection	Average infection intensity, in eggs per gram (s.e.)
Hookworm	0.77	0.15	426 (1055)
Roundworm	0.42	0.16	2337 (5156)
Schistosomiasis, all schools	0.22	0.07	91 (413)
Schistosomiasis, schools <5 km from Lake Victoria	0.80	0.39	487 (879)
Whipworm	0.55	0.10	161 (470)
At least one infection	0.92	0.37	–
Born since 1985	0.92	0.40	–
Born before 1985	0.91	0.34	–
Female	0.91	0.34	–
Male	0.93	0.38	–
At least two infections	0.31	0.10	–
At least three infections	0.28	0.01	–

[a]These are averages of individual-level data, as presented in Brooker, Miguel, et al. (2000); correcting for the oversampling of the (numerically smaller) upper grades does not substantially change the results. Standard errors in parentheses. Sample size: 1894 pupils. Fifteen pupils per standard in grades 3 to 8 for Group 1 schools were randomly sampled. The bottom two rows of the column "Prevalence of moderate-heavy infection" should be interpreted as the proportion with at least two or at least three moderate-to-heavy helminth infections, respectively.

The data were collected in January to March 1998 by the Kenya Ministry of Health, Division of Vector Borne Diseases (DVBD). The moderate infection thresholds for the various intestinal helminths are: 250 epg for *S. mansoni*, and 5,000 epg for Roundworm, both the WHO standard, and 750 epg for Hookworm and 400 epg for Whipworm, both somewhat lower than the WHO standard. Refer to Brooker, Miguel, et al. (2000) for a discussion of this parasitological survey and the infection cut-offs. All cases of schistosomiasis are *S. mansoni*.

infection profiles (Brooker, Rowlands, et al. (2000)). Moderate-to-heavy worm infections are more likely among younger pupils and among boys. Pupils who attend schools near Lake Victoria also have substantially higher rates of schistosomiasis. Latrine ownership is negatively correlated with moderate-to-heavy infection (results not shown).

3.2. *The Intervention*

Following World Health Organization recommendations (WHO (1992)), schools with geohelminth prevalence over 50 percent were mass treated with albendazole every six months, and schools with schistosomiasis prevalence over 30 percent were mass treated with praziquantel annually.[13] All treatment

[13]The medical protocol was designed in collaboration with the Partnership for Child Development, and was approved by the Ethics Committee of the Kenya Ministry of Health and Busia

schools met the geohelminth cut-off in both 1998 and 1999. Six of twenty-five treatment schools met the schistosomiasis cut-off in 1998 and sixteen of fifty treatment schools met the cut-off in 1999.[14] Medical treatment was delivered to the schools by Kenya Ministry of Health public health nurses and ICS public health officers. Following standard practice (Bundy and Guyatt (1996)), 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 (WHO (1992)).[15]

all or most treament schools got treament form ministry

In addition, treatment schools received worm prevention education through regular public health lectures, wall charts, and the training of teachers in each treatment school on worm prevention. Health education stressed the importance of hand washing to avoid ingesting roundworm and whipworm larvae, wearing shoes to avoid hookworm infection, and not swimming in infected fresh water to avoid schistosomiasis.

prevention lecture in al (tretement?) schools

ICS obtained community consent in all treatment schools in 1998. A series of community and parent meetings were held in treatment schools, at which the project was described and parents who did not want their child to participate in the project were asked to inform the school headmaster. Under the recommendation of the Kenya Ministry of Health, beginning in January 1999 ICS required signed parental consent for all children to receive medical treatment; consent typically took the form of parents signing their name in a notebook kept at school by the headmaster. This is not a trivial requirement for many households: travelling to school to sign the book may be time-consuming, and some parents may be reluctant to meet the headmaster when behind on school fees, a common problem in these schools.

98: treament consent fast immer

99: weniger cnnsent weil aufweniger

District Medical Officer of Health. The 30 percent threshold for mass praziquantel treatment is less than the WHO standard of 50 percent, although in practice few schools had schistosomiasis prevalence between 30 to 50 percent. Pupils in the parasitological subsample who were found to be infected with schistosomiasis, but attended schools that did not qualify for mass treatment with praziquantel, were individually treated. However, there were few such pupils: the proportion of moderate-to-heavy schistosomiasis among the thirty-four schools that fell below the 30 percent threshold in 1999 was just 0.02.

[14] In 1998, pupils received 600 mg albendazole doses during each round of treatment, following the protocol of an earlier Government of Kenya Ministry of Health deworming project in Kwale District; in 1999, pupils were treated with 400 mg albendazole (WHO (1992)). Praziquantel was provided at approximately 40 mg/kg (WHO (1992)) in both 1998 and 1999. The NGO used generic drugs in 1998, and SmithKline Beecham's Zentel (albendazole) and Bayer's Biltricide (praziquantel) in 1999.

[15] Pregnancy test reagent strips are not practical during mass treatment (Bundy and Guyatt (1996)). Personal interviews (i.e., asking girls when they had their most recent menstrual period) may not be effective in determining pregnancy in this setting because pregnant girls might fear that the information would not be held in confidence; pregnant girls are often expelled from Kenyan primary schools (although this is not official government policy).

3.3. *Assigned and Actual Deworming Treatment*

Seventy-eight percent of those pupils assigned to receive treatment (i.e., girls under thirteen years old and all boys in the treatment schools) received at least some medical treatment through the program in 1998 (Table III).[16] Since approximately 80 percent of the students enrolled prior to the start of the pro-

TABLE III

PROPORTION OF PUPILS RECEIVING DEWORMING TREATMENT IN PSDP[a]

	Group 1		Group 2		Group 3	
	Girls <13 years, and all boys	Girls ≥ 13 years	Girls <13 years, and all boys	Girls ≥ 13 years	Girls <13 years, and all boys	Girls ≥ 13 years
	Treatment		*Comparison*		*Comparison*	
Any medical treatment in 1998 (For grades 1–8 in early 1998)	0.78	0.19	0	0	0	0
Round 1 (March–April 1998), Albendazole	0.69	0.11	0	0	0	0
Round 1 (March–April 1998), Praziquantel[b]	0.64	0.34	0	0	0	0
Round 2 (Oct.–Nov. 1998), Albendazole	0.56	0.07	0	0	0	0
	Treatment		*Treatment*		*Comparison*	
Any medical treatment in 1999 (For grades 1–7 in early 1998)	0.59	0.07	0.55	0.10	0.01	0
Round 1 (March–June 1999), Albendazole	0.44	0.06	0.35	0.06	0.01	0
Round 1 (March–June 1999), Praziquantel[b]	0.47	0.06	0.38	0.06	0.01	0
Round 2 (Oct.–Nov. 1999), Albendazole	0.53	0.06	0.51	0.08	0.01	0
Any medical treatment in 1999 (For grades 1–7 in early 1998), among pupils enrolled in 1999	0.73	0.10	0.71	0.13	0.02	0
Round 1 (March–June 1999), Albendazole	0.55	0.08	0.46	0.08	0.01	0
Round 1 (March–June 1999), Praziquantel[b]	0.53	0.07	0.45	0.07	0.01	0
Round 2 (Oct.–Nov. 1999), Albendazole	0.65	0.09	0.66	0.11	0.01	0

[a]Data for grades 1–8. Since month of birth information is missing for most pupils, precise assignment of treatment eligibility status for girls born during the "threshold" year is often impossible; all girls who turn 13 during a given year are counted as 12 year olds (eligible for deworming treatment) throughout for consistency.

[b]Praziquantel figures in Table III refer only to children in schools meeting the schistosomiasis treament threshold (30 percent prevalence) in that year.

[16]In what follows, "treatment" schools refer to all twenty-five Group 1 schools in 1998, and all fifty Group 1 and Group 2 schools in 1999.

gram were present in school on a typical day in 1998, absence from school on the day of drug administration was a major cause of drug noncompliance. Nineteen percent of girls thirteen years of age or older also received medical treatment in 1998. This was partly because of confusion in the field about pupil age, and partly because in the early stages of the program several of the Kenya Ministry of Health nurses administered drugs to some older girls, judging the benefits of treatment to outweigh the risks. This was particularly common in schools near the lake where schistosomiasis was more of a problem.

1998: 80% von treament kids haben auch treatm bekommen

A somewhat lower proportion of pupils in school took the medicine in 1999. Among girls younger than thirteen and boys who were enrolled in school for at least part of the 1999 school year, the overall treatment rate was approximately 72 percent (73 percent in Group 1 and 71 percent in Group 2 schools), suggesting that the process of selection into treatment was fairly similar in the two years despite the change in consent rules. Of course, measured relative to the baseline population of students enrolled in early 1998, a smaller percentage of students were still in school in 1999 and hence, treatment rates in this baseline sample were considerably lower in 1999 than in 1998: among girls under thirteen years of age and all boys in treatment schools from the baseline sample, approximately 57 percent received medical treatment at some point in 1999, while only nine percent of the girls thirteen years of age and older received treatment.[17]

99: 70% treatment der kids

Only five percent of comparison school pupils received medical treatment for worms independently of the program during the previous year, according to the 1999 pupil questionnaire.[18] An anthropological study examining worm treatment practices in a neighboring district in Kenya (Geissler et al. (2000)), finds that children self-treat the symptoms of helminth infections with local herbs, but found no case in which a child or parent purchased deworming

5% treament in comparison kids

[17]The difference between the 72 percent and 57 percent figures is due to Group 2 pupils who dropped out of school (or who could not be matched in the data cross years, despite the efforts of the NGO field staff) between years 1 and 2 of the project. Below, we compare infection outcomes for pupils who participated in the 1999 parasitological survey, all of whom were enrolled in school in 1999. Thus the parasitological survey sample consists of pupils enrolled in school in both 1998 and 1999 for both the treatment and comparison schools. To the extent that the deworming program itself affected enrolment outcomes—1999 school enrolment is approximately four percentage points higher in the treatment schools than the comparison schools—the pupils enrolled in the treatment versus comparison schools in 1999 will have different characteristics. However, since drop-out rates were lower in the treatment schools, this is likely to lead to a bias toward zero in the within-school health externality estimates, in which case our estimates serve as lower bounds on true within-school effects.

ab hier nur gelesen... ... ...

[18]A survey to assess the availability of deworming drugs in this area, conducted during May to July 1999, found no local shops surveyed carried either WHO-recommended broad-spectrum treatments for geohelminths (albendazole and mebendazole) or schistosomiasis (praziquantel) in stock on the day of the survey, though a minority carried cheaper but less effective drugs (levamisole hydrochloride and piperazine). Some clinics and pharmacies carried broad-spectrum drugs, but these were priced far out of range for most of the population.

TABLE IV

PROPORTION OF PUPIL TRANSFERS ACROSS SCHOOLS

	1998 transfer to a			1999 transfer to a		
School in early 1998 (pre-treatment)	Group 1 school	Group 2 school	Group 3 school	Group 1 school	Group 2 school	Group 3 school
Group 1	0.005	0.007	0.007	0.032	0.026	0.027
Group 2	0.006	0.007	0.008	0.026	0.033	0.027
Group 3	0.010	0.010	0.006	0.022	0.036	0.022
Total transfers	0.021	0.024	0.021	0.080	0.095	0.076

drugs. To the extent that children in Busia also self-treat helminth symptoms with herbs, in this study we measure the net benefit of deworming drugs above and beyond the impact of herbs and of any individually purchased medicines.

Although pupils assigned to comparison schools could also potentially have transferred to treatment schools to receive deworming medical treatment through the program, there is no evidence of large asymmetric flows of pupils into treatment schools, which could bias the results (Table IV). Among sample pupils, approximately two percent transferred into a different school in 1998, with nearly equal proportions transferring into Groups 1, 2, and 3 schools, and approximately eight percent of pupils had transferred into a different school by the end of 1999, again with similar proportions transferring to all three groups (the transfer rates from early 1998 through the end of 1999 are substantially higher than rates through the end of 1998 because most transfers occur between school years). As we discuss in Section 4, we also use a standard intention-to-treat (ITT) estimation strategy, in which pupils are assigned the treatment status of the school in which they were initially enrolled in early 1998 even if they later switched schools, to address potential transfer bias.

fast keine schulwechsel

3.4. *Health Outcome Differences Between Group 1 and Group 2 Schools*

Before proceeding to formal estimation in Section 4, we present simple differences in health outcomes between treatment and comparison schools, although as we discuss below, these differences understate overall treatment effects if there are deworming treatment externalities across schools. The Kenyan Ministry of Health conducted a parasitological survey of grade three to eight pupils in Group 1 and Group 2 schools in January and February 1999, one year after the first round of treatment but before Group 2 schools had been treated. Overall, 27 percent of pupils in Group 1 (1998 treatment) schools had a moderate-to-heavy helminth infection in early 1999 compared to 52 percent in Group 2 (1998 comparison) schools, and this difference is significantly different than zero at 99 percent confidence (Table V). The prevalences of moderate-to-heavy hookworm, roundworm, schistosomiasis, and whipworm infections were all lower in Group 1 (1998 treatment) schools than in Group 2

TABLE V

JANUARY TO MARCH 1999, HEALTH AND HEALTH BEHAVIOR DIFFERENCES BETWEEN GROUP 1 (1998 TREATMENT) AND GROUP 2 (1998 COMPARISON) SCHOOLS[a]

	Group 1	Group 2	Group 1 – Group 2
Panel A: Helminth Infection Rates			
Any moderate-heavy infection, January–March 1998	0.38	–	–
Any moderate-heavy infection, 1999	0.27	0.52	−0.25***
			(0.06)
Hookworm moderate-heavy infection, 1999	0.06	0.22	−0.16***
			(0.03)
Roundworm moderate-heavy infection, 1999	0.09	0.24	−0.15***
			(0.04)
Schistosomiasis moderate-heavy infection, 1999	0.08	0.18	−0.10*
			(0.06)
Whipworm moderate-heavy infection, 1999	0.13	0.17	−0.04
			(0.05)
Panel B: Other Nutritional and Health Outcomes			
Sick in past week (self-reported), 1999	0.41	0.45	−0.04**
			(0.02)
Sick often (self-reported), 1999	0.12	0.15	−0.03**
			(0.01)
Height-for-age Z-score, 1999	−1.13	−1.22	0.09*
(low scores denote undernutrition)			(0.05)
Weight-for-age Z-score, 1999	−1.25	−1.25	−0.00
(low scores denote undernutrition)			(0.04)
Hemoglobin concentration (g/L), 1999	124.8	123.2	1.6
			(1.4)
Proportion anemic (Hb < 100g/L), 1999	0.02	0.04	−0.02**
			(0.01)
Panel C: Worm Prevention Behaviors			
Clean (observed by field worker), 1999	0.59	0.60	−0.01
			(0.02)
Wears shoes (observed by field worker), 1999	0.24	0.26	−0.02
			(0.03)
Days contact with fresh water in past week	2.4	2.2	0.2
(self-reported), 1999			(0.3)

[a]These are averages of individual-level data for grade 3–8 pupils; disturbance terms are clustered within schools. Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence.

Obs. for parasitological results: 2328 (862 Group 1, 1467 Group 2); Obs. for hemoglobin results: 778 (292 Group 1, 486 Group 2); Obs. for 1999 Pupil Questionnaire health outcomes: 9,102 (3562 Group 1, 5540 Group 2 and Group 3).

Following Brooker, Miguel, et al. (2000), moderate-to-heavy infection thresholds for the various intestinal helminths are: 250 epg for *S. mansoni*, and 5,000 epg for Roundworm, both the WHO standard, and 750 epg for Hookworm and 400 epg for Whipworm, both somewhat lower than the WHO standard. Kenya Ministry of Health officials collected the parasitological data from January to March 1998 in Group 1 schools, and from January to March 1999 in Group 1 and Group 2 schools. A random subset of the original 1998 Group 1 parasitological sample was resurveyed in 1999. Hb data were collected by Kenya Ministry of Health officials and ICS field officers using the portable Hemocue machine. The self-reported health outcomes were collected for all three groups of schools as part of Pupil Questionnaire administration.

(1998 comparison) schools. The program was somewhat less effective against whipworm, perhaps as a result of the lower efficacy of single-dose albendazole treatments for whipworm infections, as discussed above.[19]

Note that it is likely that substantial reinfection had occurred during the three to twelve months between 1998 deworming treatment and the 1999 parasitological surveys, so differences in worm burden between treatment and comparison schools were likely to have been even greater shortly after treatment. In addition, to the extent that pupils prone to worm infections are more likely to be present in school on the day of the parasitological survey in the Group 1 schools than the Group 2 schools due to deworming health gains, these average differences between Group 1 and Group 2 schools are likely to further understate true deworming treatment effects.

Group 1 pupils also reported better health outcomes after the first year of deworming treatment: four percent fewer Group 1 pupils reported being sick in the past week, and three percent fewer pupils reported being sick often (these differences are significantly different than zero at 95 percent confidence). Group 1 pupils also had significantly better height-for-age—a measure of nutritional status—by early 1999, though weight-for-age was no greater on average.[20]

Although Group 1 pupils had higher hemoglobin concentrations than Group 2 pupils in early 1999, the difference is not statistically different than zero. Recall that anemia is the most frequently hypothesized link between worm infections and cognitive performance (Stoltzfus et al. (1997)). Severe anemia is relatively rare in Busia: fewer then 4 percent of pupils in Group 2 schools (comparison schools in 1998) fell below the Kenya Ministry of Health anemia threshold of 100 g/L in early 1999 before deworming treatment. This is low relative to many other areas in Africa, of which many have substantial helminth problems: a recent survey of studies of anemia among school children in less developed countries (Hall and Partnership for Child Development (2000)) indicates that there is considerably less anemia in Busia than in samples from Ghana, Malawi, Mali, Mozambique, and Tanzania.[21]

[19]The rise in overall moderate-to-heavy helminth infections between 1998 and 1999 (refer to Table II) is likely to be due to the extraordinary flooding in 1998 associated with the El Niño weather system, which increased exposure to infected fresh water (note the especially large increases in moderate-to-heavy schistosomiasis infections), created moist conditions favorable for geohelminth larvae, and led to the overflow of latrines, incidentally also creating a major outbreak of fecal-borne cholera.

[20]Although it is somewhat surprising to find height-for-age gains but not weight-for-age gains, since the latter are typically associated with short-run nutritional improvements, it is worth noting that Thein-Hlaing, Thane-Toe, Than-Saw, Myat-Lay-Kyin, and Myint-Lwin's (1991) study in Myanmar finds large height gains among treated children within six months of treatment for roundworm while weight gains were only observed after twenty-four months, and Cooper et al. (1990) present a similar finding for whipworm, so the result is not unprecedented.

[21]One possible explanation for low levels of anemia in this area is geophagy (soil eating): Geissler et al. (1998) report that 73 percent of a random sample of children aged 10–18 in a

Health education had a minimal impact on behavior, so to the extent the program improved health, it almost certainly did so through the effect of anthelmintics rather than through health education. There are no significant differences across treatment and comparison school pupils in early 1999 in three worm prevention behaviors: observed pupil cleanliness,[22] the proportion of pupils wearing shoes, or self-reported exposure to fresh water (Table V).

4. ESTIMATION STRATEGY

4.1. *Econometric Specifications*

Randomization of deworming treatment across schools allows estimation of the overall effect of the program by comparing treatment and comparison schools, even in the presence of within-school externalities.[23] However, externalities may take place not only within, but also across schools, especially since most people in this area live on their farms rather than being concentrated in villages, and neighbors (and even siblings) often attend different schools since there is typically more than one primary school within walking distance. Miguel and Gugerty (2002) find that nearly one-quarter of all households in this area have a child enrolled in a primary school which is not the nearest one to their home. We estimate cross-school externalities by taking advantage of variation in the local density of treatment schools induced by randomization. Although randomization across schools makes it possible to experimentally identify both the overall program effect and cross-school externalities, we must rely on nonexperimental methods to decompose the effect on treated schools into a direct effect and within-school externality effect.

We first estimate program impacts in treatment schools, as well as cross-school treatment externalities:[24]

$$(1) \qquad Y_{ijt} = a + \beta_1 \cdot T_{1it} + \beta_2 \cdot T_{2it} + X'_{ijt}\delta + \sum_d (\gamma_d \cdot N^T_{dit}) + \sum_d (\phi_d \cdot N_{dit}) + u_i + e_{ijt}.$$

neighboring region of Western Kenya reported eating soil daily. Given the average amount of soil children were observed eating daily, and the measured mean iron content of soil in this area, Geissler et al. conclude that soil provides an average of 4.7 mg iron per day—over one-third of the recommended daily iron intake for children. Unfortunately, geophagy could also increase exposure to geohelminth larvae, promoting reinfection.

[22]This also holds controlling for initial 1998 levels of cleanliness, or using a difference-in-differences specification.

[23]Manski (2000) suggests using experimental methods to identify peer effects. Other recent papers that use group-level randomization of treatment to estimate peer effects include Duflo and Saez (2002) and Miguel and Kremer (2002). Katz, Kling, and Liebman (2001), Kremer and Levy (2001), and Sacerdote (2001) use random variation in peer group composition to estimate peer effects.

[24]For simplicity, we present the linear form, but we use probit estimation below for discrete dependent variables.

Y_{ijt} is the individual health or education outcome, where i refers to the school, j to the student, and $t \in \{1, 2\}$ to the year of the program; T_{1it} and T_{2it} are indicator variables for school assignment to the first and second year of deworming treatment, respectively; and X_{ijt} are school and pupil characteristics. N_{dit} is the total number of pupils in primary schools at distance d from school i in year t, and N^T_{dit} is the number of these pupils in schools randomly assigned to deworming treatment. For example, in Sections 5 and 6, $d = 03$ denotes schools that are located within three kilometers of school i, and $d = 36$ denotes schools that are located between three to six kilometers away.[25] Individual disturbance terms are assumed to be independent across schools, but are allowed to be correlated for observations within the same school, where the school effect is captured in the u_i term.

Since local population density may affect disease transmission, and since children who live or attend school near treatment schools could have lower environmental exposure to helminths, which would lead to less reinfection and lower worm burdens, worm burden may depend on both the total number of primary school pupils (N_{dit}) and the number of those pupils in schools randomly assigned to deworming treatment (N^T_{dit}) within a certain distance from school i in year t of the program.[26] Given the total number of children attending primary school within a certain distance from the school, the number of these attending schools assigned to treatment is exogenous and random. Since any independent effect of local school density is captured in the N_{dit} terms, the γ_d coefficients measure the deworming treatment externalities across schools. In this framework $\beta_1 + \sum_d(\gamma_d \overline{N}^T_{dit})$ is the average effect of the first year of deworming treatment on overall infection prevalence in treatment schools, where $\overline{N}^T_{dit}$ is the average number of treatment school pupils located at distance d from the school, and $\beta_2 + \sum_d(\gamma_d \overline{N}^T_{dit})$ is the analogous effect for the second year of deworming. β_1 and β_2 capture both direct effects of deworming treatment on the treated, as well as any externalities on untreated pupils within the treatment schools.[27]

[25]Under spatial externality models in which a reduction in worm prevalence at one school affects neighboring schools and this in turn affects their neighbors, some externalities would spill over beyond six kilometers. To the extent that there are externalities beyond six kilometers from the treatment schools, equation (1) yields a lower bound on treatment effects, but we think any such spillovers are likely to be relatively minor in this setting.

[26]Since cross-school externalities depend on the number of pupils eligible for treatment rather than the total number of pupils, we use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population (N_{dit} and N^T_{dit}) for all schools in the remainder of the paper. Measurement error in GPS locations—due to U.S. government downgrading of GPS accuracy until May 2000—leads to attenuation bias, making it more difficult to find treatment externalities.

[27]Unfortunately, we do not have data on the location of pupils' homes, and hence cannot examine if pupils living near treatment schools actually obtain greater externality benefits.

The assigned deworming treatment group is not significantly associated with the density of other local treatment school pupils within three kilometers or within three to six kilometers (Table I); in other words, approximately as many treated pupils are located near Group 1 schools as near Group 2 or 3 schools. The 1998 and 1999 deworming compliance rates are also not significantly associated with the local density of treatment school pupils conditional on the total local density (Appendix Table AII).

Cross-school deworming externalities are likely to increase with the proportion of the local population that receives deworming treatment. Although the school-level randomization induced a range of variation in local treatment densities in our sample, with only 49 schools we cannot estimate how marginal externalities vary with local treatment levels.[28] Yet since large-scale deworming programs in most poor countries would likely use community consent for treatment, rather than individual parental consent—as in the first year of the program we examine—we estimate the likely extent of treatment externalities under conditions of interest to public health policymakers.

Including school and pupil variables X_{ijt} controls for those pre-treatment differences across schools that were present despite randomization, increasing statistical precision. These controls include the average school score on the 1996 Kenya government district exams for grades 5 to 8;[29] the prevalence of moderate-to-heavy helminth infections in the pupil's grade and geographic zone (the pre-treatment average); indicators for school involvement in other nongovernmental organization assistance projects; time controls (indicator variables for each six-month period capture the downward trend in school participation due to dropouts); and grade cohort indicator variables.

4.2. *Estimating Within-School Externalities*

Because randomization was conducted at the level of schools, rather than individuals within schools, it is possible to both estimate the overall treatment effect on treated schools and to conduct a cost-benefit analysis using equation (1). However, it is not possible to experimentally decompose the effect for treatment schools into a direct effect on treated pupils and an externality effect on untreated pupils within treatment schools. It is not valid to use assignment to a treatment school as an instrumental variable for actual medical treatment

[28] Quadratic terms of local treatment densities are not significantly related to the rate of any moderate-to-heavy helminth infection (results not shown), and thus we opt to focus on the linear specification, as in equation (1).

[29] Average school scores from 1996—two years before the first year of the project—were employed since the district exam was not offered in 1997 due to a national teacher strike. Average school exam scores are used because individual exam results are incomplete for 1996. However, the 1996 scores are corrected to be in units of individual level standard deviations, and are thus comparable to the 1998 and 1999 test scores under the assumption that the decomposition of test score variance within and between schools was the same in 1996, 1998, and 1999.

in the presence of such externalities (Angrist, Imbens, and Rubin (1996)) since the exclusion restriction fails to hold: assignment to a treatment school affects pupil health through externalities, rather than only through the likelihood of receiving medical treatment.

In thinking about nonexperimental approaches to such a decomposition, it is worth bearing in mind that there is no evidence that sicker pupils were more likely to obtain deworming treatment; in fact if anything, the evidence seems more consistent with the hypothesis that pupils with higher worm load were somewhat less likely to obtain treatment, either because they were less likely to be in school on the day of treatment or because their households were less willing and able to invest in health. As Panels A and B in Table VI indicate, among girls under 13 and all boys, the children who would remain untreated were slightly more likely to be moderately to heavily infected prior to the intervention than those who ultimately obtained treatment, both for Group 1 schools (in 1998) and Group 2 schools (in 1999). Among girls at least 13 years of age, there is little difference in 1998 infection rates (prior to treatment) between Group 1 pupils who later obtained treatment and those who did not, while the Group 2 pupils who later obtained treatment were substantially less likely to have been moderately to heavily infected in early 1999 than their counterparts who later went untreated.

As suggested above, a major cause of missing treatment is school absenteeism: a 2001 parent survey indicates that most noncompliance from absenteeism is due to pupil illness, and we show in Section 6 that pupils with worms miss school more often. Poorer pupils may also have lower compliance if parents who have not paid school fees are reluctant to visit the headmaster to provide consent.

We assume in what follows that children obtain treatment if the net gain from treatment is more than a cut-off cost. Formally, $D_{1ij} = 1(S(X_{ijt}, e_{ijt}) + \varepsilon_{ijt} > C_t)$, where D_{1ij} takes on a value of one if individual j in school i received treatment in the first year that her school was eligible for treatment (1998 for Group 1, 1999 for Group 2), and zero otherwise; here, $1(\cdot)$ is the indicator function, C_t is the total cost to the household of obtaining treatment in year t (which varies between the two years due to the changing consent requirements), and ε_{ijt} is an unobserved random variable that could depend on the distance of the pupil's home from school, or whether the pupil was sick on the treatment day, for example.

Given that there was no randomization of treatment within schools, Group 1 pupils who did not receive treatment in 1998 are compared to Group 2 pupils who did not receive treatment in 1999, the year that Group 2 schools were incorporated into treatment, to at least partially deal with potential bias due to selection into medical treatment. For the health outcomes, we compare these two groups as of January to February 1999, when Group 1 schools had already been treated (in 1998) but Group 2 schools had not, while for school participation we compare Groups 1 and 2 during the first year of treatment.

G1 no-treat vs G2 no-treat: in 99:

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TABLE VI

DEWORMING HEALTH EXTERNALITIES WITHIN SCHOOLS, JANUARY TO MARCH 1999[a]

	Group 1, Treated in 1998	Group 1, Untreated in 1998	Group 2, Treated in 1999	Group 2, Untreated in 1999	(Group 1, Treated 1998) – (Group 2, Treated 1999)	(Group 1, Untreated 1998) – (Group 2, Untreated 1999)
Panel A: Selection into Treatment						
Any moderate-heavy infection, 1998	0.39	0.44	–	–	–	–
Proportion of 1998 parasitological sample tracked to 1999 sample[b]	0.36	0.36	–	–	–	–
Access to latrine at home, 1998	0.84	0.80	0.81	0.86	0.03	−0.06
					(0.04)	(0.05)
Grade progression (= Grade − (Age − 6)), 1998	−2.0	−1.8	−1.8	−1.8	−0.2**	−0.0
					(0.1)	(0.2)
Weight-for-age (Z-score), 1998 (low scores denote undernutrition)	−1.58	−1.52	−1.57	−1.46	−0.01	−0.06
					(0.06)	(0.11)
Malaria/fever in past week (self-reported), 1998	0.37	0.41	0.40	0.39	−0.03	−0.01
					(0.04)	(0.06)
Clean (observed by field worker), 1998	0.53	0.59	0.60	0.66	−0.07	−0.07
					(0.05)	(0.10)
Panel B: Health Outcomes						
Girls <13 years, and all boys						
Any moderate-heavy infection, 1999	0.24	0.34	0.51	0.55	−0.27***	−0.21**
					(0.06)	(0.10)
Hookworm moderate-heavy infection, 1999	0.04	0.11	0.22	0.20	−0.19***	−0.09*
					(0.03)	(0.05)
Roundworm moderate-heavy infection, 1999	0.08	0.12	0.22	0.30	−0.14***	−0.18**
					(0.04)	(0.07)
Schistosomiasis moderate-heavy infection, 1999	0.09	0.08	0.20	0.13	−0.11*	−0.05
					(0.06)	(0.06)
Whipworm moderate-heavy infection, 1999	0.12	0.16	0.16	0.20	−0.04	−0.05
					(0.16)	(0.09)
Girls ≥13 years						
Any moderate-heavy infection, 1998	0.31	0.28	–	–	–	–
Any moderate-heavy infection, 1999	0.27	0.43	0.32	0.54	−0.05	−0.10
					(0.17)	(0.09)
Panel C: School Participation						
School participation rate, May 1998 to March 1999[c]	0.872	0.764	0.808	0.684	0.064**	0.080**
					(0.032)	(0.039)

[a] These are averages of individual-level data for grade 3–8 pupils in the parasitological survey subsample; disturbance terms are clustered within schools. Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The data are described in the footnote to Table V. Obs. for the 1999 parasitological survey: 670 Group 1 treated 1998, 77 Group 1 untreated 1998, 873 Group 2 treated 1999, 352 Group 2 untreated 1999.

[b] We attempted to track a random sample of half of the original 1998 parasitological sample. Because some pupils were absent, had dropped out, or had graduated, we were only able to resurvey 72 percent of this subsample.

[c] School averages weighted by pupil population. The participation rate is computed among pupils enrolled in the school at the start of 1998. Pupils present in school during an unannounced NGO visit are considered participants. Pupils had 3.8 participation observations per year on average. Participation rates are for grades 1 to 7; grade 8 pupils are excluded since many graduated after the 1998 school year, in which case their 1999 treatment status is irrelevant. Pre-school pupils are excluded since they typically have missing compliance data. All 1998 pupil characteristics in Panel A are for grades 3 to 7, since younger pupils were not administered the Pupil Questionnaire.

As we discussed above, the parental consent rules changed between 1998 and 1999, leading to a reduction in the fraction of pupils receiving treatment within treatment schools. Thus, restricting the sample to Group 1 and Group 2 schools (and holding the X_{ijt} terms constant for the moment, for clarity):

$$
\begin{aligned}
(2)\qquad & E(Y_{ij1}|T_{1i1}=1, X_{ij1}, D_{1ij}=0) - E(Y_{ij1}|T_{1i1}=0, X_{ij1}, D_{1ij}=0) \\
&= \beta_1 + \sum_d \gamma_d \cdot \big[E(N^T_{di1}|T_{1i1}=1, D_{1ij}=0) \\
&\qquad\qquad - E(N^T_{di1}|T_{1i1}=0, D_{1ij}=0)\big] \\
&\quad + \sum_d \gamma_d \cdot [E(N_{di1}|T_{1i1}=1, D_{1ij}=0) - E(N_{di1}|T_{1i1}=0, D_{1ij}=0)] \\
&\quad + [E(e_{ij1}|T_{1i1}=1, X_{ij1}, D_{1ij}=0) - E(e_{ij1}|T_{1i1}=0, X_{ij1}, D_{1ij}=0)],
\end{aligned}
$$

where T_{1i1} is the treatment assignment of the *school* in 1998 ($t=1$), and this takes on a value of one for Group 1 and zero for Group 2 schools. The first term on the right-hand side of the equation (β_1) is the within-school externality effect. The second and third terms are effects due to differing local densities of primary schools between treatment and comparison schools; these are approximately zero (as we show in Table I) and in any case we are able to control for these densities in the estimation. The key final term, which can be rewritten as

$$
\begin{aligned}
& E(e_{ij1}|T_{1i1}=1, X_{ij1}, C_1 - S(X_{ij1}, e_{ij1}) > \varepsilon_{ij1}) \\
&\quad - E(e_{ij1}|T_{1i1}=0, X_{ij1}, C_2 - S(X_{ij2}, e_{ij2}) > \varepsilon_{ij2}),
\end{aligned}
$$

captures any unobserved differences between untreated pupils in the Group 1 and Group 2 schools. If $C_1 = C_2$, then by randomization this term equals zero and (2) can be used to estimate β_1. However, it is likely that $C_2 > C_1$ due to imposition of the signed parental consent requirement in 1999. In our sample, infected people are no more likely to be treated—and in fact seem somewhat *less* likely to be treated—and this is robust to conditioning on the full set of X_{ijt} variables described above (results not shown).[30] If S is in fact nondecreasing in e_{ijt} (which can be thought of as unobserved characteristics associated with good health outcomes in this specification), then $C_2 > C_1$ implies that the final term will be zero or negative, so the left-hand side of the equation will if anything underestimate the within-school externality, β_1.[31] In other words, due to changes in the process of selection into treatment, some Group 2 pupils who would have been treated had they been in Group 1 were in fact not treated in 1999, and this implies that average unobservables e_{ijt} will be at least as great among the untreated in Group 2 as among the untreated in Group 1 (and also

[30] Pooling 1998 data for Group 1 pupils and 1999 data for Group 2 pupils, the estimated marginal effect of a moderate-to-heavy infection on drug take-up is −0.008, and this effect is not significantly different than zero.

[31] This claim also relies on the assumption that individual e_{ijt} terms are autocorrelated across the two years.

that average e_{ijt} will also be at least as great among the treated Group 2 as among the treated Group 1).

The change in overall infection rates between the first two years of the program (captured in X_{ijt} in the above model) may also have affected individual deworming treatment decisions. Infection rates changed across years both due to sizeable cross-school treatment externalities associated with the program, which acted to reduce infection levels, as well as to natural intertemporal variation (e.g., the 1998 flooding) which led to higher rates of moderate-to-heavy infection. This second effect appears to have dominated, leading to higher overall infection rates in 1999 relative to 1998 (Tables II and V), and complicating efforts to sign the direction of the bias in the within-school externality estimates. However, the fact that fewer people obtained treatment in year 2 than year 1 suggests that overall, given the changed consent requirements, the process of selection into treatment became more stringent, so that it is plausible that e_{ijt} is at least as great among the Group 2 pupils who were untreated in their first year of eligibility as among Group 1 pupils who were untreated in their first year of eligibility. Q4:?

Turning to the data suggests that Group 1 pupils untreated in 1998 and Group 2 pupils untreated in 1999 are in fact similar, and that any bias is likely to be small. First, as noted earlier, moderate-to-heavily infected pupils are no more likely to seek treatment than their less infected fellow pupils. Second, there are no statistically significant differences between the Group 1 pupils untreated in 1998 and the Group 2 pupils untreated in 1999 in five baseline characteristics likely to be associated with child health—latrine ownership, grade progression, weight-for-age, self-reported health status, and cleanliness—and point estimates suggest that the Group 1 untreated pupils are actually somewhat less healthy, less clean, and less likely to have access to a latrine than their counterparts in Group 2 (Table VI, Panel A).[32] These results are consistent with the hypothesis that e_{ijt} in part reflects differences among households in ability and willingness to take action to improve their children's health, and that those pupils with high values of e_{ijt} were somewhat more likely to obtain treatment.[33,34]

A further piece of evidence comes from comparing the initial moderate-heavy infection rates (in early 1998) of Group 1 pupils treated in 1998 *and*

[32]The analogous comparison with the larger sample used in the school participation estimation (in Table IX) also suggests that Group 1 pupils untreated in 1998 and the Group 2 pupils untreated in 1999 are similar along these characteristics (results not shown).

[33]In other words, as the cost of treatment increased between years 1 and 2, the individuals who still opted to receive treatment in year 2—those with higher ε_{ijt}, conditional on observables—had higher values of e_{ijt} than the individuals who were not treated in year 2 but would have been treated given the year 1 cost. Thus e_{ijt} and ε_{ijt} must be positively correlated among these individuals at the margin of receiving treatment.

[34]We have also calculated Manski bounds on within-school externalities in the presence of selection into treatment, but these are largely uninformative given the change in take-up between 1998 and 1999 (results not shown).

treated in 1999, to those treated in 1998 but *not* treated in 1999; this is not a perfect comparison, since Group 1 pupils were in their second year of treatment in 1999, while Group 2 pupils were experiencing their first year of treatment in 1999, but it still provides useful information on how changing the costs of treatment affects take-up. We find that the initial 1998 infection rates of the Group 1 pupils treated in 1999 and those untreated in 1999 differ by less than one percentage point (results not shown), providing further evidence that the change in consent rules between 1998 and 1999 did not substantially change the health status of those who chose to receive treatment through the program.

If the expectation of e_{ij1} is the same for the Group 1 pupils who missed their first year of treatment in 1998, and the Group 2 pupils who missed treatment in 1999, then we can estimate both within-school and cross-school treatment externalities in 1998 using equation (3):

$$Y_{ijt} = a + \beta_1 \cdot T_{1it} + b_1 \cdot D_{1ij} + b_2 \cdot (T_{1it} * D_{1ij}) + X'_{ijt}\delta + \sum_d (\gamma_d \cdot N^T_{dit}) + \sum_d (\phi_d \cdot N_{dit}) + u_i + e_{ijt}. \tag{3}$$

Here, β_1 is the within-school externality effect on the untreated, and $(\beta_1 + b_2)$ is the sum of the within-school externality effect plus the additional direct effect of treatment on the treated. If the final term in equation (2) is negative, as we suggest above, this specification underestimates within-school externalities and overstates the impact on the treated within treatment schools; of course, the estimation of overall program effects based on equation (1) is independent of the decomposition into effects on the treated and untreated within treatment schools. The total externality effect for the untreated in treatment schools is the sum of the within-school externality term and the cross-school externality in equation (3). In certain specifications we interact the local pupil density terms with the treatment school indicator to estimate potentially differential cross-school externalities in treatment and comparison schools.

4.3. *Initial Evidence on Within-School Deworming Externalities*

Before presenting results using this unified estimation framework in Sections 5, 6, and 7, we preview the within-school externality results by comparing the January–March 1999 infection levels of the Group 1 pupils who did not receive treatment in 1998 and the Group 2 pupils who did not receive treatment in 1999 (the year that Group 2 schools were incorporated into the treatment group). Among girls under thirteen years of age and all boys—those children who were supposed to receive medical treatment through the project—rates of moderate-to-heavy infections were 21 percentage points lower among Group 1 pupils who did not receive medical treatment in 1998 (34 percent) than among Group 2 pupils who did not receive treatment in 1999 (55 percent), and this difference is significant at 95 percent confidence (Table VI). These differences are negative and statistically significant for hookworm and roundworm, and negative but insignificant for schistosomiasis and whipworm; since the overall difference in whipworm infection between Group 1 and 2 schools was minimal, and there is evidence that single-dose albendazole treatments are sometimes ineffective against whipworm, it is not surprising that evidence of within-school externalities is weaker for whipworm. By way of contrast, Group 1 pupils who were treated in 1998 had a 24 percent chance of moderate-to-heavy infection in January to February 1999, while Group 2 pupils who would obtain treatment later in 1999 had a 51 percent chance of infection, for a difference of 27 percentage points. Thus at the time infection status was measured in early 1999, the difference in the prevalence of moderate-to-heavy infections among the untreated was approximately three-quarters the difference in prevalence for the treated (21 versus 27 percentage points).

The relatively large ratio of externality benefits to benefits for the treated is plausible given the timing of 1998 treatment and the 1999 parasitological survey. Following treatment of part of a population at steady-state worm infection intensity, the treated group will be reinfected over time and their worm load will asymptote to its original level. As discussed in Section 2, other studies have found that prevalence of hookworm, roundworm, and schistosomiasis falls by over 99 percent immediately after treatment, but that reinfection occurs rapidly. On the other hand, worm load among the untreated will gradually fall after the treatment group is dewormed, since the rate of infection transmission declines. Eventually, however, worm load among the untreated will rise again, asymptoting to its original steady-state level as the treated population becomes reinfected. The ratio of worm load among the treated to that among the untreated then approaches one over time. Since we collect data on worm infections some time after treatment—the January–March 1999 parasitological survey was carried out nearly one year after the first round of medical treatment and three to five months since the second round of treatment—and worm loads among the treated are substantial by this point, it seems reasonable to think that reinfection subsequent to the date of treatment accounts for much of observed worm load, and that the average difference in prevalence between treatment and comparison schools over the course of the year was likely to have been considerably greater than the difference observed in early 1999.

Two additional sources of evidence are consistent with positive within-school deworming treatment externalities. First, although girls aged 13 years and older were largely excluded from deworming treatment, moderate-to-heavy infection rates among older girls in Group 1 schools were ten percentage points lower than among similar girls in Group 2 schools, though this difference is not significantly different than zero (Table VI, Panel B).[35]

[35]It is not surprising that the magnitude of within-school externalities is somewhat smaller for older girls than for the population as a whole since these girls have lower rates of moderate to heavy infection (Table II), and are also twice as likely to wear shoes (results not shown), limiting reinfection. As a robustness check, we also estimate equation (3) using an instrumental variables

Second, a parasitological survey of 557 children entering preschool who had not yet had any opportunity to receive medical treatment through the program found that in early 2001, before Group 3 schools had begun receiving deworming treatment, children entering preschool in Group 1 and 2 schools had 7.1 percentage points fewer moderate-to-heavy hookworm infections than those entering Group 3 schools, an effect that is significantly different than zero at 90 percent confidence (results not shown). Given that only 18.8 percent of the Group 3 preschool children suffered from moderate-to-heavy hookworm infections, this constitutes a forty percent reduction in the proportion of such infections. The effects for the other worms were not statistically significant, which is not surprising for whipworm, since the direct treatment effects were small, or for schistosomiasis—for which externalities likely are less localized, and may not be as relevant for young children who are likely to stay near home, rather than going fishing in Lake Victoria—but is somewhat unexpected for roundworm (note, however, that Nokes et al. (1992) also find externalities for hookworm but not other geohelminths).

5. DEWORMING TREATMENT EFFECTS ON HEALTH AND NUTRITION

Formal estimation confirms that children in deworming treatment schools experienced a range of health benefits, and provides evidence that these benefits spilled over both to nontreated pupils in the treatment schools and to pupils in neighboring schools. Consistent with the differing modes of disease transmission, geohelminth externalities were primarily within schools, while schistosomiasis externalities were primarily across schools.

Estimation of equation (1) indicates that the proportion of pupils with moderate to heavy infection is 25 percentage points lower in Group 1 schools than Group 2 schools in early 1999 and this effect is statistically significant at 99 percent confidence (Table VII, regression 1). We next estimate equation (3), which decomposes the effect of the program on treated schools into an effect on treated pupils and a within-school externality effect. The within-school externality effect, given by the coefficient estimate on the Group 1 indicator variable, is a 12 percentage point reduction in the proportion of moderate-to-heavy infections, while the additional direct effect of deworming treatment is approximately 14 percentage points, and both of these coefficient estimates are significantly different than zero (Table VII, regression 2). Children who attend primary schools located near Group 1 schools had lower rates of moderate-to-heavy helminth infection in early 1999: controlling for the total number of

approach, instrumenting for actual deworming treatment with an indicator variable taking on a value of one for girls under 13 years of age and for all boys interacted with the school treatment assignment indicator. This yields a negative, but statistically insignificant, effect of treatment of schoolmates on infection among older girls (Appendix Table AIV). We cannot reject the hypothesis that the IV estimates of the within-school externality are the same as the probit estimates presented below.

TABLE VII

DEWORMING HEALTH EXTERNALITIES WITHIN AND ACROSS SCHOOLS, JANUARY TO MARCH 1999[a]

	Any moderate-heavy helminth infection, 1999			Moderate-heavy schistosomiasis infection, 1999			Moderate-heavy geohelminth infection, 1999		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Indicator for Group 1 (1998 Treatment) School	−0.25***	−0.12*	−0.09	−0.03	−0.02	−0.07	−0.20***	−0.11**	−0.03
	(0.05)	(0.07)	(0.11)	(0.03)	(0.04)	(0.06)	(0.04)	(0.05)	(0.09)
Group 1 pupils within 3 km (per 1000 pupils)	−0.26***	−0.26***	−0.11	−0.12***	−0.12***	−0.11**	−0.12*	−0.12*	−0.01
	(0.09)	(0.09)	(0.13)	(0.04)	(0.04)	(0.05)	(0.06)	(0.07)	(0.07)
Group 1 pupils within 3–6 km (per 1000 pupils)	−0.14**	−0.13**	−0.07	−0.18***	−0.18***	−0.27***	0.04	0.04	0.16
	(0.06)	(0.06)	(0.14)	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)	(0.10)
Total pupils within 3 km (per 1000 pupils)	0.11***	0.11***	0.10**	0.11***	0.11***	0.13***	0.03	0.04	0.02
	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Total pupils within 3–6 km (per 1000 pupils)	0.13**	0.13**	0.12*	0.12***	0.12***	0.16***	0.04	0.04	0.01
	(0.06)	(0.06)	(0.07)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
Received first year of deworming treatment, when offered (1998 for Group 1, 1999 for Group 2)		−0.06*			0.03**			−0.04**	
		(0.03)			(0.02)			(0.02)	
(Group 1 Indicator) ∗ Received treatment, when offered		−0.14*			−0.02			−0.10***	
		(0.07)			(0.04)			(0.04)	
(Group 1 Indicator) ∗ Group 1 pupils within 3 km (per 1000 pupils)			−0.25*			−0.04			−0.18**
			(0.14)			(0.07)			(0.08)
(Group 1 Indicator) ∗ Group 1 pupils within 3–6 km (per 1000 pupils)			−0.09			0.11			−0.15
			(0.13)			(0.07)			(0.10)
Grade indicators, school assistance controls, district exam score control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	2328	2328	2328	2328	2328	2328	2328	2328	2328
Mean of dependent variable	0.41	0.41	0.41	0.16	0.16	0.16	0.32	0.32	0.32

[a]Grade 3–8 pupils. Probit estimation, robust standard errors in parentheses. Disturbance terms are clustered within schools. Observations are weighted by total school population. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The 1999 parasitological survey data are for Group 1 and Group 2 schools. The pupil population data is from the 1998 School Questionnaire. The geohelminths are hookworm, roundworm, and whipworm. We use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population for all schools.

(age and sex eligible) children attending any primary school within three kilometers, the presence of each additional thousand (age and sex eligible) pupils attending Group 1 schools located within three kilometers of a school is associated with 26 percentage points fewer moderate-to-heavy infections, and this coefficient estimate is significantly different than zero at 99 percent confidence. Each additional thousand pupils attending a Group 1 school located between three to six kilometers away is associated with 14 percentage points fewer moderate-to-heavy infections, which is smaller than the effect of pupils within three kilometers, as expected, and is significantly different than zero at 95 percent confidence (Table VII, regression 1).[36] Due to the relatively small size of the study area, we are unable to precisely estimate the impact of additional treatment school pupils farther than six kilometers away from a school, and thus cannot rule out the possibility that there were externalities at distances beyond six kilometers and possibly for the study area as a whole, in which case the estimates presented in Table VII (and discussed below) would be lower bounds on actual externality benefits.[37,38]

[36]We experimented with alternative measures of infection status. One such measure normalizes the egg count for each type of infection by dividing each egg count by the moderate-heavy infection threshold for that helminth, and then summing up the normalized egg counts across all four infections (hookworm, roundworm, schistosomiasis, and whipworm) to arrive at an overall infection "score." The results using this measure are similar to those using the moderate-to-heavy infection indicator, although the estimated reduction in worm prevalence due to within-school externalities becomes statistically insignificant (results available upon request).

[37]The use of the intention-to-treat estimation method could potentially create spurious findings of cross-school deworming externalities, since students initially in comparison schools who transfer into treatment schools in time to receive treatment are still classified as comparison pupils. However, we do not think this is a serious problem in practice since our results are nearly identical when we classify students not by their original school, but by the school they actually attended at the time of the parasitological survey (results available upon request). The relevant transfer rate between March 1998 and November 1998 is simply too small to account for the externalities we detect: only 1.6 percent of students in Groups 2 and 3 transferred into Group 1 schools during 1998, and only 1.4 percent of students in Group 1 transferred to Groups 2 or 3 (Table IV). Given that some of the Group 2 and 3 children presumably transferred too late in the school year to benefit from treatment, and that some early transfers did not receive treatment, fewer than 1 percent of comparison pupils were treated (Table III).

[38]These results are largely robust to including the proportion of Group 1 pupils in the surrounding area as the explanatory variable, rather than the total number of Group 1 pupils in the surrounding area (see regressions 3 and 7 in Appendix Table AIII). The use of spatially correlated disturbance terms does not lead to substantial changes in standard errors and confidence levels (see regressions 2 and 6 in Appendix Table AIII). The school participation results in Table IX are also robust to the use of spatially correlated disturbance terms (results not shown). We examined the extent of spatial correlation across schools using Conley (1999) and Chen and Conley's (2001) semi-parametric framework, and as expected, find a positive and declining relationship between the correlation in infection rates and distance between schools, although the spatial correlation is relatively small once we condition on school-level characteristics. The cross-school externality results are also robust to controlling for initial 1998 infection levels among the sample of Group 1 pupils with both 1998 and 1999 parasitological data (see regressions 4 and 8

We estimate that moderate-to-heavy helminth infections among children in this area were 23 percentage points (standard error 7 percentage points) lower on average in early 1999 as a result of health spillovers across schools—over forty percent of overall moderate-to-heavy infection rates in Group 2 schools. To see this, note that the average spillover gain is the average number of Group 1 pupils located within three kilometers divided by 1000 ($\overline{N}_{03}^{T}$) times the average effect of an additional 1000 Group 1 pupils located within three kilometers on infection rates (γ_{03}), plus the analogous spillover effect due to schools located between three to six kilometers away from the school (refer to equation (1)). Based on the externality estimates in Table VII, regression 1, this implies the estimated average cross-school externality reduction in moderate-to-heavy helminth infections is $[\gamma_{03} * \overline{N}_{03,1}^{T} + \gamma_{36} * \overline{N}_{36,1}^{T}] = [0.26 * 454 + 0.14 * 802]/1000 = 0.23$.

Note that deworming drugs kill worms already in the body, but the drugs do not remain in the body and do not provide immunity against future re-infection, so it is plausible that the benefit from having fewer sources of re-infection is reasonably orthogonal to current infection status. However, own treatment and local treatment intensity need not simply have an additive effect on moderate-to-heavy infections: the interaction effect will be negative if cross-school externalities alone do not typically reduce infection levels below the moderate-to-heavy infection threshold for comparison school pupils as of the date of the parasitological survey, but the interaction of own treatment and externalities often does reduce infection below the threshold for treatment school pupils.[39] We find that the average cross-school externality reduction in moderate-to-heavy infections for comparison school (Group 2) pupils is 9 percentage points, while the effect for treatment school (Group 1) pupils is considerably larger, at nearly 29 percentage points (Table VII, regression 3). As discussed below, this difference is primarily due to geohelminths externalities, since externalities for the more serious schistosomiasis infections are similar for treatment and comparison schools.

The existence of cross-school health externalities implies that the difference in average outcomes between treatment and comparison schools—a "naïve" treatment effect estimator—understates the actual effects of mass deworming treatment on the treated. If externalities disappear completely after six kilometers, the true reduction in moderate-to-heavy infection rates among pupils in Group 1 schools is the sum of the average cross-school externality for comparison school pupils (9 percentage points) and the effect of being in a treatment school in early 1999 presented in Table VII, regression 1 (25 percentage

in Appendix Table AIII). We can only control for initial 1998 infection levels in the subsample of Group 1 schools, since these data were not collected for the other schools.

[39]More generally, the distribution of individual worm infection relative to the threshold level is also important for gauging the likely interaction effect between own treatment and the local treatment intensity.

points), for a total of 35 percentage points (the standard error is 9 percentage points, taking into account the covariance structure across coefficient estimates from Table VII, regression 3). The cross-school externality is thus over one-quarter as large as the total effect on the treated. The estimated number of moderate-to-heavy helminth infections eliminated through the program is thus $(0.35) * (9{,}817$ pupils in Group 1 schools$) + (0.09) * (19{,}493$ Pupils in Group 2 and 3 schools$) = 5190$ infections.

This is nearly one infection eliminated per treated child in Group 1 schools. Even this figure underestimates the actual total treatment effect of the program by excluding any benefits to schools more than six kilometers from treatment schools, and benefits for school-age children not enrolled in school, other community members not of school age—such as the pre-primary children discussed above—and people who live in villages bordering the study area, whom we did not survey.

As discussed in Section 2, externalities are likely to operate over larger distances for schistosomiasis than for geohelminths. In fact, the cross-school externality effects are mainly driven by reductions in moderate-to-heavy schistosomiasis infections (Table VII, regression 4), while cross-school geohelminth externalities are negative and marginally significant within three kilometers but not significantly different than zero from three to six kilometers (regression 7). The within-school effect is driven by geohelminth infections (coefficient estimate -0.10, standard error 0.04, regression 8), while the within-school schistosomiasis externalities are negative but insignificant (regression 5).

Finally, the coefficient estimates on interaction terms between treatment group and local treatment intensity are not statistically significantly different than zero for moderate-to-heavy schistosomiasis infections (Table VII, regression 6), but the interaction between treatment group and local treatment intensity from zero to three kilometers is negative and significant for moderate-to-heavy geohelminth infections (regression 9). In other words, pupils in comparison and treatment schools benefit similarly from proximity to treatment schools in terms of reduced schistosomiasis infection, but treatment school pupils experience larger cross-school geohelminth externalities than comparison pupils.[40]

6. DEWORMING TREATMENT EFFECTS ON SCHOOL PARTICIPATION

This section argues that deworming increased school participation in treatment schools by at least seven percentage points, a one-quarter reduction in to-

[40]For schistosomiasis, one explanation for this results is that cross-school externalities are sufficiently large to reduce infection levels below the moderate-to-heavy threshold for many pupils in both treated and comparison schools, and as a result coefficient estimates on the interaction terms are not significant.

tal school absenteeism.[41] Deworming may have improved school participation by allowing previously weak and listless children to attend school regularly or by improving children's ability to concentrate, which may have made attending school increasingly worthwhile relative to other activities, such as agricultural labor, staying at home, or fishing.

As with the health impacts, deworming creates externalities in school participation both within and across schools; after accounting for externalities we estimate that overall school participation in this area likely increased by at least 0.14 years of schooling per pupil actually treated through the program. This effect is larger than would be expected from nonexperimental estimates of the correlation between worm burden and school participation, as we discuss below.

Our sample consists of all pupils enrolled in school or listed in the school register during the first term in 1998.[42] Since many pupils attend school erratically, and the distinction between an absent pupil and a dropout is often not clear from school records, it is difficult to distinguish between dropping out and long-term absenteeism; moreover, measuring pupil attendance conditional on not dropping out is unattractive since dropping out is endogenous. We therefore focus on a comprehensive measure of school participation: a pupil is considered a participant if she or he is present in school on a given day, and a nonparticipant if she or he is not in school on that day. Since school attendance records are often poorly kept, school participation was measured during unannounced school visits by NGO field workers. Schools received an average of 3.8 school participation check visits per year in 1998 and 1999. Note that since the days of medical treatment were pre-announced, and the school

[41] School participation in the area is irregular, and the large effect we estimate is consistent with the hypothesis that many children are at the margin of whether or not to attend school given the cost of school fees and uniforms, low school quality, and perceived declining returns to education (Mensch and Lloyd (1997)). Further evidence that many children are at the margin of whether to attend school is provided by a program in the same region that paid for required school uniforms, increasing school participation by 15 percent (Kremer, Moulin, and Namunyu (2002)).

[42] Since many pupils who were recorded as dropouts in early 1998 re-enrolled in school at some point during the 1998 or 1999 school years, we include them in the sample. However, many initial dropouts were not assigned a grade by the NGO field staff, complicating the analysis of participation rates by grade. Such pupils are assigned their own grade indicator variable in Table IX. Some pupils have missing year of birth information due to absence from school on days of questionnaire or exam administration, and certain assumptions need to be made regarding the treatment assignment status of girls with missing age information (since older girls were supposed to be excluded from treatment). Girls in treatment schools in preschool and grades 1, 2, and 3 are assumed to be eligible for treatment, while those in grades 7 and 8 are assumed not to be, since all but a small fraction of girls in these grades meet the respective age eligibility criterion. We do not know if girls with missing ages in grades 4, 5, and 6 were younger than 13 and hence were supposed to receive treatment, and therefore we drop them from the sample, eliminating 99 girls from the sample of approximately 30,000 children. An additional 119 pupils are dropped from the sample due to both missing age and sex information.

participation figures do not include attendance on these days, effects on attendance are not due to children coming to school in the hope of receiving medicine.

6.1. *School Participation Differences across Treatment and Comparison Schools*

Before proceeding to formal estimation using equations (1) and (3), we first present differences in school participation across the project groups and through time. Since these do not take cross-school externalities into account, they potentially underestimate overall treatment effects. Among girls younger than thirteen years old and all boys, the difference in school participation for the five post-treatment participation observations in the first year after medical treatment is 9.3 percentage points, and this is significantly different than zero at 99 percent confidence (Table VIII). The difference is larger among boys and young girls than among the older girls (5.7 percentage points), which is consistent with the fact that a far smaller proportion of older girls actually received medical treatment (Table III).

The differences in 1999 school participation for boys and younger girls are also large and significantly different than zero at 90 percent confidence for both Group 1 (1998 and 1999 treatment schools) and Group 2 (1999 treatment schools), at 5.0 and 5.5 percentage points, respectively. Average school participation rates fall during the second year of the study as children from the original sample—and especially those in the older grades—left school through graduation or dropping-out.

One possible explanation for the smaller impact of the program on school participation in 1999 is the lower proportion of pupils taking deworming drugs compared to 1998 (Table III), which should reduce both treatment effects on the treated and externality effects. The larger participation differences between treatment and comparison schools in 1998 may also have been due to the widespread El Niño flooding in this region in early 1998, which substantially increased worm loads between early 1998 and early 1999 (to see this, compare Tables II and V). Finally, the difference may be due in part to chance: we cannot reject the hypothesis that gaps between treatment and comparison schools in 1998 and 1999 are the same.

The time pattern of school participation differences is consistent with a causal effect of deworming on school participation. Figure 1 presents school participation rates from May 1998 to November 1999 for girls under thirteen and for all boys. Diamonds represent the differences in average school participation between Group 1 and Group 3 schools, and squares represent the difference between Group 2 and Group 3 schools. School participation rates for Group 1 schools are consistently higher than rates in Group 3 schools in both 1998 and 1999, and the gap stands at nearly ten percentage points by November 1999. Group 2 schools have lower school participation than Group 3 schools in 1998 when both groups were comparison schools, but begin to show

TABLE VIII

SCHOOL PARTICIPATION, SCHOOL-LEVEL DATA[a]

	Group 1 (25 schools)	Group 2 (25 schools)	Group 3 (25 schools)		
Panel A:					
First year post-treatment (May 1998 to March 1999)	*1st Year Treatment*	*Comparison*	*Comparison*	*Group 1 − (Groups 2 & 3)*	*Group 2 − Group 3*
Girls <13 years, and all boys	0.841	0.731	0.767	0.093***	−0.037
				(0.031)	(0.036)
Girls ≥13 years	0.864	0.803	0.811	0.057**	−0.008
				(0.029)	(0.034)
Preschool, Grade 1, Grade 2 in early 1998	0.795	0.688	0.703	0.100***	−0.018
				(0.037)	(0.043)
Grade 3, Grade 4, Grade 5 in early 1998	0.880	0.789	0.831	0.070***	−0.043
				(0.024)	(0.029)
Grade 6, Grade 7, Grade 8 in early 1998	0.934	0.858	0.892	0.059***	−0.034
				(0.021)	(0.026)
Recorded as "dropped out" in early 1998	0.064	0.050	0.030	0.022	0.020
				(0.018)	(0.017)
Females[b]	0.855	0.771	0.789	0.076***	−0.018
				(0.027)	(0.032)
Males	0.844	0.736	0.780	0.088***	−0.044
				(0.031)	(0.037)
Panel B:					
Second year post-treatment (March to November 1999)	*2nd Year Treatment*	*1st Year Treatment*	*Comparison*	*Group 1 − Group 3*	*Group 2 − Group 3*
Girls <13 years, and all boys	0.713	0.717	0.663	0.050*	0.055*
				(0.028)	(0.028)
Girls ≥14 years[c]	0.627	0.649	0.588	0.039	0.061*
				(0.035)	(0.035)
Preschool, Grade 1, Grade 2 in early 1998	0.692	0.726	0.641	0.051	0.085**
				(0.034)	(0.034)
Grade 3, Grade 4, Grade 5 in early 1998	0.750	0.774	0.725	0.025	0.049**
				(0.023)	(0.023)
Grade 6, Grade 7, Grade 8 in early 1998	0.770	0.777	0.751	0.020	0.026
				(0.027)	(0.028)
Recorded as "dropped out" in early 1998	0.176	0.129	0.056	0.120*	0.073
				(0.063)	(0.053)
Females[b]	0.716	0.746	0.648	0.067**	0.098***
				(0.027)	(0.027)
Males	0.698	0.695	0.655	0.043	0.041
				(0.028)	(0.029)

[a]The results are school averages weighted by pupil population. Standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The participation rate is computed among all pupils enrolled in the school at the start of 1998. Pupils who are present in school on the day of an unannounced NGO visit are considered participants. Pupils had 3.8 participation observations per year on average. The figures for the "Preschool–Grade 2"; "Grade 3–5"; "Grade 6–8"; and "Dropout" rows are for girls <13 years, and all boys.

[b]396 pupils in the sample are missing information on gender. For this reason, the average of the female and male participation rates does not equal the overall average.

[c]Examining girls ≥14 years old eliminates the cohort of girls in Group 1 schools (12 year olds in 1998) who were supposed to receive deworming treatment in 1998.

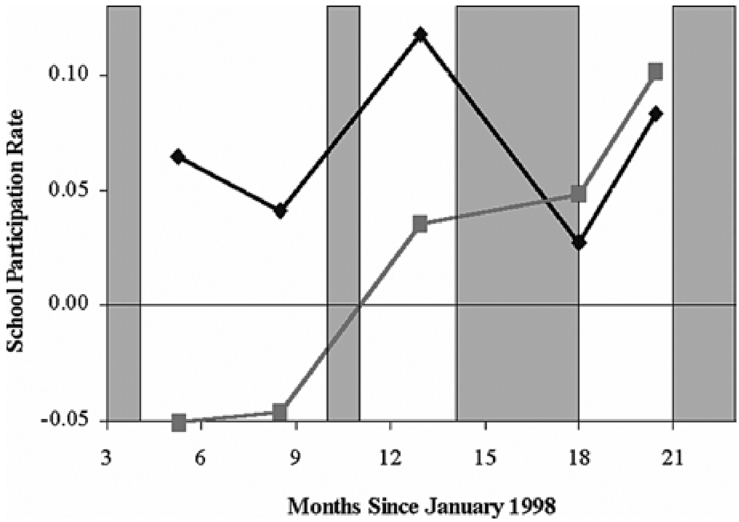

FIGURE 1.—School participation rate May 1998 to November 1999 for girls under 13 years old and for all boys (difference between Group 1 and Group 3 (diamonds), and difference between Group 2 and Group 3 (squares)).[a]

[a]The shaded regions are periods in which medical treatment was being provided (in March–April and November 1998 to Group 1 schools, and March–June and October–November 1999 to Group 1 and Group 2 schools).

participation gains in early 1999. Participation in Group 2 schools is substantially greater than in Group 3 schools by mid-1999 when the first round of 1999 treatment was concluded. These gains resulted primarily from a greater proportion of pupils with participation above 80 percent, although there were also substantially fewer dropouts (results not shown).

The school participation gains are particularly large among the youngest pupils: in 1998 the average difference in participation between treatment and comparison groups for preschool through grade 2 was 10.0 percentage points (significantly different than zero at 99 percent confidence), while for pupils in grades 6 to 8 it was 5.9 percentage points, and in 1999 the comparable gains for Group 2 pupils were 8.5 percentage points and 2.6 percentage points, respectively. The larger impact of treatment in lower grades may partially result from higher rates of moderate-to-heavy infection among younger pupils (Table II). It is also possible that school participation is more elastic with respect to health for younger pupils; many Kenyan children drop out before reaching the upper primary grades, so older children who remain in school may be the most academically serious and determined to attend school despite illness.

Untreated pupils in Group 1 (1998 treatment) had higher school participation than their counterparts in Group 2 schools who were later untreated during 1999, consistent with deworming externalities on school participation. Among girls under thirteen years old and all boys, May 1998 to March

1999 school participation was 8.0 percentage points greater among untreated Group 1 pupils, which is significantly different than zero at 95 percent confidence (Table VI, Panel C). Group 1 pupils who were treated in 1998 had 6.4 percentage points higher May 1998 to March 1999 school participation than Group 2 pupils who were treated in 1999.[43]

The large participation gains among older girls—who were not supposed to be treated through the program—in 1998 and 1999 also suggest that school participation externality benefits were substantial (Table VIII). Although the 1998 gains among older girls could have been driven in part by nontrivial rates of medical treatment, there were also large participation gains among older girls in Group 2 schools in 1999 despite the fact that only ten percent of them received medical treatment (Table III). An alternative, nonhealth explanation for the participation gains among older girls is that the improved school participation of younger siblings allowed them to attend school more regularly, as we discuss below.

6.2. *Estimating Overall School Participation Impacts*

School participation externality estimates across schools using individual-level data are presented in Table IX. The dependent variable is average individual school participation in either the first year (May 1998 to March 1999) or the second year (April 1999 to November 1999) of the project. Regressions 1 and 2 present "naïve" treatment effects that ignore the possibility of externalities. The average school participation gain for treatment schools relative to comparison schools across both years of the project is 5.1 percentage points, and this is significantly different than zero at 99 percent confidence (regression 1). Point estimates are 6.2 percentage points for the first year of treatment and 4.0 percentage points for the second year, with significance levels of 99 percent and 90 percent, respectively (regression 2), although confidence intervals are wide enough that we cannot reject the hypothesis that the effect is the same in both years. The magnitude of the effects remains nearly unchanged when pupils initially recorded as dropouts in early 1998 are excluded from the sample (results not shown).

The ratio of externalities to direct effects is likely to be smaller for measured school participation than for measured worm load, since the ratio of externalities to direct effects is very low immediately after treatment but then asymptotes to one. As we discussed in Section 4, worm load is measured between three months to a year after deworming treatment, while school participation

[43]It may seem odd that the point estimate of the absolute increase in school participation is greater for the untreated, but it is worth noting that the proportional decline in school nonparticipation was one-third for the treated while the decline among the untreated was one-fourth, and that we cannot reject the hypothesis that the difference for treated pupils is somewhat larger than for untreated pupils.

TABLE IX

SCHOOL PARTICIPATION, DIRECT EFFECTS AND EXTERNALITIES[a]
DEPENDENT VARIABLE: AVERAGE INDIVIDUAL SCHOOL PARTICIPATION, BY YEAR

	OLS (1)	OLS (2)	OLS (3)	OLS (4) May 98–March 99	OLS (5) May 98–March 99	OLS (6) May 98–March 99	IV-2SLS (7) May 98–March 99
Moderate-heavy infection, early 1999						−0.028*** (0.010)	−0.203* (0.094)
Treatment school (T)	0.051*** (0.022)						
First year as treatment school (T1)		0.062*** (0.015)	0.060*** (0.015)	0.062* (0.022)	0.056*** (0.020)		
Second year as treatment school (T2)		0.040* (0.021)	0.034* (0.021)				
Treatment school pupils within 3 km (per 1000 pupils)			0.044** (0.022)		0.023 (0.036)		
Treatment school pupils within 3–6 km (per 1000 pupils)			−0.014 (0.015)		−0.041 (0.027)		
Total pupils within 3 km (per 1000 pupils)			−0.033** (0.013)		−0.035* (0.019)	0.018 (0.021)	0.021 (0.019)
Total pupils within 3–6 km (per 1000 pupils)			−0.010 (0.012)		0.022 (0.027)	−0.010 (0.012)	−0.021 (0.015)
Indicator received first year of deworming treatment, when offered (1998 for Group 1, 1999 for Group 2)					0.100*** (0.014)		
(First year as treatment school Indicator) ∗ (Received treatment, when offered)					−0.012 (0.020)		
1996 district exam score, school average	0.063*** (0.021)	0.071*** (0.020)	0.063*** (0.020)	0.058 (0.032)	0.091** (0.038)	0.021 (0.026)	0.003 (0.023)

is measured continuously beginning immediately following treatment, including the period when the ratio of externalities to direct effects is likely to be low.[44]

[44]The cross-school externalities for school participation may also be weaker than worm infection externalities because only schistosomiasis has robust health externalities across schools, and moderate to heavy schistosomiasis infection is rarer than geohelminth infection (only seven percent of Group 1 pupils had moderate to heavy schistosomiasis infections prior to treatment, while

TABLE IX

(CONTINUED)

	OLS (1)	OLS (2)	OLS (3)	OLS (4) May 98–March 99	OLS (5) May 98–March 99	OLS (6) May 98–March 99	IV-2SLS (7) May 98–March 99
Grade indicators, school assistance controls, and time controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.23	0.23	0.24	0.33	0.36	0.28	–
Root MSE	0.273	0.272	0.272	0.223	0.219	0.150	0.073
Number of observations	56487	56487	56487	18264	18264	2327	49 (schools)
Mean of dependent variable	0.747	0.747	0.747	0.784	0.784	0.884	0.884

[a]The dependent variable is average individual school participation in each year of the program (Year 1 is May 1998 to March 1999, and Year 2 is May 1999 to November 1999); disturbance terms are clustered within schools. Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Additional explanatory variables include an indicator variable for girls <13 years and all boys, and the rate of moderate-heavy infections in geographic zone, by grade (zonal infection rates among grade 3 and 4 pupils are used for pupils in grades 4 and below and for pupils initially recorded as drop-outs as there is no parasitological data for pupils below grade 3; zonal infection rates among grade 5 and 6 pupils are used for pupils in grades 5 and 6, and similarly for grades 7 and 8). Participation is computed among all pupils enrolled at the start of the 1998 school year. Pupils present during an unannounced NGO school visit are considered participants. Pupils had approximately 3.8 attendance observations per year. Regressions 6 and 7 include pupils with parasitological information from early 1999, restricting the sample to a random subset of Group 1 and Group 2 pupils. The number of treatment school pupils from May 1998 to March 1999 is the number of Group 1 pupils, and the number of treatment school pupils after March 1999 is the number of Group 1 and Group 2 pupils.

The instrumental variables in regression 7 are the Group 1 (treatment) indicator variable, treatment school pupils within 3 km, treatment school pupils within 3–6 km, and the remaining explanatory variables. We use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population for all schools.

We estimate equation (1) in regression 3 and find that each additional thousand (potentially age and sex eligible) pupils attending treatment schools within three kilometers leads to an increase of 4.4 percentage points in average school participation (significant at 95 percent confidence). The effect of treatment pupils located between three to six kilometers is negative, but not significantly different than zero. Given the number of Group 1 pupils and Group 2 pupils within three kilometers, and between three to six kilometers, of the average primary school, the results of regression 3 imply that school

over thirty percent had some moderate to heavy geohelminth infection (Table II)). The coefficient estimates on the interactions between treatment indicators and distance to Lake Victoria—which is highly correlated with the prevalence of schistosomiasis in this area (Table II)—are not significantly different than zero, indicating that school participation treatment effects among those infected with both schistosomiasis and geohelminths are not considerably larger than the effects for children with geohelminth infections alone, and supporting the view that school participation effects work mainly through geohelminths.

participation was approximately 2.0 percentage points (standard error 1.3 percentage points) higher on average throughout this area in 1998 and 1999 due to deworming externalities, which is marginally statistically significant.[45] Regression 3 also implies that the total effect of deworming on school participation in treatment schools was 7.5 percentage points (standard error 2.7 percentage points) over 1998 and 1999.

To estimate the overall school participation gain due to the program, recall that the program increased school participation by about 2.0 percentage points on average among pupils in comparison schools, while children in treatment schools had about 7.5 percentage points higher participation. For every two treated children in a treatment school, there was almost exactly one untreated child on average in 1998 and 1999, and for each child in a treatment school there was one comparison school child for 1998 and 1999 (since one-third of schools were treated in 1998 and two-thirds in 1999). Hence treating one child led to an estimated lower bound increase in school participation of $(1 * 0.075) + (0.5 * 0.075) + (1.5 * 0.020) = 0.14$ school years (standard error 0.05).

To estimate within school externalities using equation (3) we can only use data from the first year of treatment, and so for comparison purposes, regression 4 presents the basic specification for the first year of data, and estimates a 6.2 percentage point school participation gain. Within-school participation externality benefits were positive and statistically significant at 99 percent confidence (5.6 percentage points) for untreated pupils in the treatment schools in the first year of the program (regression 5), and there is no significant difference in school participation rates between treated and untreated pupils in these schools (which is consistent with the externality results from Table VI, Panel C, reported above). In this restricted 1998 sample, the estimated cross-school externality effects are statistically insignificant.[46]

6.3. *Comparing Experimental and Nonexperimental Estimates*

Pupils who were moderately or heavily infected in early 1999 had 2.8 percentage points lower school participation over the period May 1998 to March

[45] Unlike infection rates, coefficient estimates on the interactions between school treatment indicators and local treatment school pupil densities are not significantly different than zero for school participation (results not shown), so we do not consider differential externality benefits for the three project treatment groups in the calculation of overall program impacts. There are at least two reasons why the cross-school externality relationships differ. First, if school participation varies continuously with infection levels, the threshold effects found for moderate to heavy infections might not apply. Second, school attendance is measured continuously over the study period, while infection levels are measured only once, up to one year after initial treatment.

[46] We obtain qualitatively similar results using the instrumental variables approach discussed in Section 5, which compares outcomes for older girls (who were largely excluded from deworming treatment) across the treatment and comparison schools to estimate the within-school externality. The IV results for within school externalities for school participation are insignificant, but we also cannot reject the hypothesis that the IV estimates are the same as the OLS results in Table IX (refer to Appendix Table AIV, regression 4).

1999 (Regression 6, Table IX). This nonexperimental estimate is restricted to the subsample of 2327 pupils in grades three to eight for whom there is 1999 parasitological data, and we thus lack information on the preschool, grade 1, and grade 2 pupils that exhibit the largest experimental treatment effect estimates. In contrast, an instrumental variable specification—which imposes the condition that all school participation gains work through changes in measured worm infection status—suggests that each moderate to heavy worm infection leads to 20.3 percentage points lower school participation on average (regression 7). The instrumental variables in regression 7 are the Group 1 (treatment) indicator variable, treatment school pupils within 3 km, and treatment school pupils within 3–6 km.

There are at least three reasons why the IV estimates of the impact of moderate-heavy infection on school participation are substantially larger than OLS estimates. First, since we measure infection up to a year after treatment, when many pupils will already have been reinfected with worms, the difference in infection levels between treated and untreated pupils was likely much greater on average over the interval from deworming treatment to the parasitological exam than it was at the time of the parasitological exam (given the documented efficacy of the drugs and high reinfection rates). As we discussed in Section 4, the parasitological exam data almost certainly understates the total number of moderate to heavy infections eliminated as a result of the program immediately after treatment. If 99 percent of pupils with moderate-to-heavy infections were in fact initially cleared of infection, the implied school participation gain for each pupil cleared of moderate to heavy infection (presented in regression 7) would be cut approximately in half.

Second, the exclusion restriction—that the program only affects pupils' school attendance by changing their health—may not hold, due to complementarities in school participation. For example, if the pre-schoolers, first-graders, and second-graders for whom we estimate the largest school participation effects stay home sick with worms in the comparison schools, their older sisters may also stay home to take care of them, and this may partly explain the relatively large treatment effects we find for older girls.[47] More generally, there may be complementarity in school attendance if children are more inclined to go to school if their classmates are also in school, so school participation gains in treatment schools may partially reflect increased school participation among children who were not infected with worms. Such effects would influence the impact of a large-scale deworming program on school participation and are captured in a prospective evaluation (like ours) in which treatment is randomized at the school level, but they would not be picked up in an individual-level regression of school participation on worm levels, or in a prospective study in which treatment is randomized at the individual level.

[47]Since we do not have data on family relationships, we cannot directly test this hypothesis in this setting.

A final reason why instrumental variable estimates of the deworming effect are larger than suggested by our nonexperimental estimates is attenuation bias due to error in measuring the severity of disease.[48]

7. DEWORMING TREATMENT EFFECTS ON TEST SCORES

Deworming could improve test scores both by increasing time spent in school and by improving learning while pupils are in school, but could also potentially reduce test scores through congestion or negative peer effects. We describe these various positive and negative mechanisms in Section 7.1, and then present the test score results in 7.2.

7.1. *Mechanisms Linking Deworming and Test Score Performance*

Deworming could potentially increase test scores by increasing the total amount of time spent in school, but this effect is likely to be weak given the observed impact of deworming on school participation and the cross-sectional relationship between school participation and test performance. In 1998 and 1999, ICS administered English, Mathematics, and Science-Agriculture exams to pupils in grades 3 to 8. Restricting attention to these grades reduces the sample size in Table X relative to Table IX. Exams were modelled on those given by the district office of the Ministry of Education, and prepared using the same procedure. The average score across all subjects is employed as the principal test score outcome measure for each set of tests, although the basic results are

[48]Measurement error in binary variables leads to bias toward zero in the OLS specification, provided errors are not too extreme (Aigner (1973), Kane et al. (1999)); the technical condition is that Pr(Type I Error) + Pr(Type II Error) < 1, which is reasonable in our case. Unfortunately, measurement error in binary variables can also lead to bias away from zero in IV estimates, which would lead us to somewhat overstate the effect of worm infection on attendance in Table IX, regression 7; the effect of a moderate-heavy worm infection on school participation is thus likely to lie between the OLS and IV coefficient estimates. Measurement error could take several forms: pure measurement error performing egg counts in the lab; time variation in worm burden, so that those who were moderately to heavily infected in early 1999 were not necessarily the same ones who were most heavily infected over the course of the school year; coarseness in our binary measure of worm burden; heterogeneity in the impact of different worm species on school participation; and interactions among worms that are not captured by our measure, so that some individuals who are classified as having multiple light worm infections in fact suffer substantial morbidity. Moreover, epidemiologists have argued that there is an imperfect relationship between worm egg counts—the standard measure of infection intensity—and actual worm infection burden (Medley and Anderson (1985)), further exacerbating error. Heterogeneous treatment effects may also interact with sample attrition to further exacerbate estimation biases because those pupils for whom high measured worm burdens are not associated with absenteeism are more likely to be in school on the day of the parasitological exam and hence to make it into our sample. Note, however, that this measurement error and resulting bias does not affect our main experimental estimates of program impacts presented above, but does help account for the difference between the experimental and nonexperimental estimates.

TABLE X

ACADEMIC EXAMINATIONS, INDIVIDUAL-LEVEL DATA[a]

	Dependent variable: ICS Exam Score (normalized by standard)		
	(1)	(2)	(3) Among those who filled in the 1998 pupil survey
Average school participation (during the year of the exam)	0.63***		
	(0.07)		
First year as treatment school (T1)		−0.032	−0.030
		(0.046)	(0.049)
Second year as treatment school (T2)		0.001	0.009
		(0.073)	(0.081)
1996 District exam score, school average	0.74***	0.71***	0.75***
	(0.07)	(0.07)	(0.07)
Grade indicators, school assistance controls, and local pupil density controls	Yes	Yes	Yes
R^2	0.14	0.13	0.15
Root MSE	0.919	0.923	0.916
Number of observations	24958	24958	19072
Mean of dependent variable	0.020	0.020	0.039

[a]Each data point is the individual-level exam result in a given year of the program (either 1998 or 1999); disturbance terms are clustered within schools. Linear regression, robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Regression 3 includes only pupils who completed the 1998 Pupil Questionnaire. Additional explanatory variables include an indicator variable for girls <13 years and all boys, and the rate of moderate-to-heavy infections in geographic zone, by grade (zonal infection rates among grade 3 and 4 pupils are used for pupils in grades 4 and below and for pupils initially recorded as dropouts as there is no parasitological data for pupils below grade 3; zonal infection rates among grade 5 and 6 pupils are used for pupils in grades 5 and 6, and similarly for grades 7 and 8). The local pupil density terms include treatment school pupils within 3 km (per 1000 pupils), total pupils within 3 km (per 1000 pupils), treatment school pupils within 3–6 km (per 1000 pupils), and total pupils within 3–6 km (per 1000 pupils). We use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population for all schools.

The ICS tests for 1998 and 1999 were similar in content, but differed in two important respects. First, the 1998 exam featured multiple-choice questions while the 1999 test featured short answers. Second, while each grade in 1998 was administered a different exam, in 1999 the same exam—featuring questions across a range of difficulty levels—was administered to all pupils in grades 3 to 8. Government district exams in English, Math, Science-Agriculture, Kiswahili, Geography-History, Home Science, and Arts-Crafts were also administered in both years. Treatment effect estimates are similar for both sets of exams (results not shown).

unchanged if subjects are examined separately (regressions not shown). For both 1998 and 1999, test scores were normalized to be mean zero and standard deviation one among comparison pupils initially enrolled in the same grade in early 1998.

A one percentage point increase in measured school participation is associated with a 0.63 standard deviation increase in test scores (Table X, regression 1). The coefficient estimate suffers from attenuation bias due to sampling error since the school participation measure for each individual is the average of only 3.8 participation observations per year, but it is straightforward to correct since the participation rate and the number of participation observations are known for each pupil.[49] The corrected coefficient estimate is 2.17, implying that a ten percentage point gain in attendance is associated with a 0.217 standard deviations higher score on the ICS exam. If deworming leads to test score gains solely through improvements in attendance, and average school participation in treatment schools exceeds that in comparison schools by approximately 5.1 percentage points as a result of deworming over 1998 and 1999 (Table IX), then the estimated "effect" of deworming on test scores in the absence of omitted variable bias would be $(0.051) * (2.17)$, or approximately 0.11 standard deviations.

However, the coefficient estimate on average school participation in this regression is likely to overstate the true impact of increased participation on test scores for two reasons. First, it reflects not only the causal impact of higher participation on test scores, but also unobserved pupil characteristics correlated with both test scores and school participation. Second, in a related point, the coefficient estimate on school participation is likely to reflect the impact of better attendance over the course of a child's entire school career, whereas this study only examines attendance gains over one or two years; 5.1 percentage points higher school participation for two years translates into fewer than twenty additional days of schooling, and this might plausibly have a limited effect on academic performance. For example, if omitted variable bias accounted for half of the observed correlation between test scores and school attendance, and if the remainder of the correlation reflects the effects of the past five years of schooling on academic performance, then one would expect that increasing attendance by 5.1 percentage points for two years would increase test scores by less than 0.02 standard deviations, a very small effect.

The second channel through which deworming could increase scores is by improving the efficiency of learning per unit of time spent in school. However, since severe anemia is rare in this area and there were only small differences in anemia between treatment and comparison schools (Table V), the most frequently hypothesized link between worm infections and cognitive performance may not have been operative during the study. Some evidence that the program did not increase the efficiency of learning is provided by a battery of cognitive exams—including picture search, Raven matrix, verbal fluency, digit span, Spanish learning, and a "dynamic" test using syllogisms—which were conducted in all three groups of schools during 2000. Deworming treatment effects are not significantly different than zero for any component of the cognitive exam (results available upon request).

[49]The true coefficient estimate on average annual attendance β is related to the coefficient estimate b by the standard attenuation bias formula: $\beta = b(\sigma_T^2/(\sigma_T^2 - \sigma_S^2))$, where the sampling variance of average annual participation is σ_S^2, and the total variance in average annual school participation is σ_T^2. We take into account that the number of participation observations differs across individuals in calculating the attenuation bias correction.

On the other hand, deworming could potentially have reduced test scores in treatment schools through congestion and peer effects. Classrooms were more crowded in treatment schools as previously ill children attended school more regularly, and the presence of these additional pupils in the classroom may have imposed negative learning externalities on other pupils.[50]

7.2. *Test Score Results*

The estimated differences in test scores between pupils in treatment and comparison schools are -0.032 standard deviations for the first year post-treatment and 0.001 standard deviations for the second year, neither of which is significantly different than zero (Table X, regression 2). The average cross-school deworming externality effect is statistically insignificant at -0.049 (standard error 0.052), and within-school externality effect estimates are also statistically insignificant (results not shown).

The results could potentially have been affected by differential attrition across treatment and comparison schools, if the additional treatment school pupils who participated in the exam after deworming were below-average performers. The fact that 85 percent of Group 1 pupils took the 1998 ICS exams, compared to 83 percent of Group 2 and Group 3 pupils, suggests that this is a possibility, although the attrition bias is likely to be small.[51] To address this issue, we restrict the sample to pupils who were administered the 1998 pupil questionnaire, eliminating over twenty percent of the sample and much of the potential exam participation bias since nearly identical proportions of these pupils took the ICS exam in treatment and comparison schools. Treatment effect estimates using this restricted sample are similar to those using the complete sample and remain insignificantly different than zero at traditional confidence levels (Table X, regression 3), suggesting that at least among this subsample, deworming did not substantially raise test scores.

It remains possible that benefits may have accrued disproportionately among the 15 percent of pupils who missed the ICS exam, especially if they suffered from the most intense helminth infections. However, we do not find a strong association between worm burden and the likelihood of missing the exam within the sample of students in the parasitological sample (results not shown).[52]

[50]Assuming that the relationship between class size and academic outcomes for Israeli schools in Angrist and Lavy (1999) holds in Kenya, deworming participation gains of the magnitude we found would lead to a drop of 0.02–0.05 standard deviations in average exam scores (calculations available from the authors upon request).

[51]Lee's (2002) bounds on the deworming treatment effect are near zero and statistically insignificant, both for this test and for the cognitive exams, given the relatively small difference in attrition between treatment and comparison schools (results available upon request).

[52]A subset of pupils who did not take the 1998 ICS exam (including dropouts) were followed up in 20 deworming schools and encouraged to sit for the exam, allowing us to impute test scores for dropouts. In total, 214 pupils were administered the follow-up exam in these schools. Among

A higher grade promotion rate would also have resulted if deworming increased learning among weak students who did not take ICS exams. Although promotion rates in treatment schools between 1998 and 1999 are in fact two percentage points higher than in comparison schools, this difference is not significantly different than zero (results not shown).

Given the observed cross-sectional relationship between participation and test scores, the absence of a strong time-in-school effect on test scores may not be surprising. However, the data do not support the hypothesis of a strong effect on the efficiency of learning per unit of time in school for the subsample who took the test. It is worth mentioning that several other primary school interventions in this region of Kenya—including textbook provision (Glewwe, Kremer, and Moulin (1999)) and school grant provision—have also had limited success in improving academic test scores. Note that there is an analogous result in the literature on health and labor productivity in less developed countries, namely, that although poor health typically reduces hours of labor supply, the existing empirical evidence on the impact of poor health on wage rates—a proxy for individual productivity—is largely inconclusive (Strauss and Thomas (1998)).

8. COST EFFECTIVENESS AND WELFARE ANALYSIS

We explore the controversy over whether mass school-based deworming treatment should be a public policy priority for the poorest countries using four different approaches. Under the *health cost effectiveness approach*, health projects are considered cost-effective up to some threshold cost per Disability-Adjusted Life Year (DALY) saved, perhaps $25 to $100 per DALY in the poorest countries. We also consider the *educational cost effectiveness* of promoting school participation through deworming rather than through alternative educational interventions. The *human capital investment approach* estimates the rate of return to deworming in future earnings. The *externality approach* attempts to identify the subsidy that would lead individuals to fully internalize treatment externalities.

The health externalities and school participation effects examined in this paper turn out to play an important role under a variety of approaches. For example, as discussed below, we find that under the health cost effectiveness approach, treatment of schistosomiasis is extremely cost effective, but that a naïve

grade 3–8 pupils with missing ICS exams, similar proportions were administered the follow-up exam in Group 1 (treatment) schools—34 percent—and Group 2 and 3 (comparison) schools—32 percent—suggesting that attrition bias is unlikely to be large. Missing 1998 ICS test score data was imputed in two steps. First, the normalized test scores of the follow-up pupils were regressed on a set of variables for grade, geographic zone, and school assistance group (assistance from other NGO projects) separately for Groups 1, 2, and 3 schools. Second, missing test score values for other pupils with missing tests are imputed as predicted values of this regression, again separately for Group 1, 2, and 3 schools. Treatment effect estimates remain insignificantly different than zero using this augmented sample (results not shown).

estimate ignoring externalities would severely underestimate its cost effectiveness. Treatment of geohelminths would not meet standard cost-effectiveness criteria in the poorest countries based on its health impact alone, but is extremely cost effective relative to other ways of increasing school participation that have also been examined using prospective evaluations in this part of Kenya. While estimates of the long-run labor market impact of deworming are of course speculative, our best estimate is that deworming is an excellent human capital investment given its impact on school participation, and that the externalities from deworming justify fully subsidizing treatment.

8.1. *Health Cost Effectiveness*

Annual government expenditure on health in Kenya was approximately five U.S. dollars per capita from 1990 to 1997 (World Bank (1999)), so mass deworming is only one of many health interventions competing for scarce public resources. For example, the vaccination rate against measles and DPT (diptheria, pertussis, and tetanus) among Kenyan infants of less than one year of age was just 32 percent in 1997 (World Bank (1999)), and these vaccinations are thought to be highly cost effective, at only 12 to 17 U.S. dollars per disability-adjusted life year (DALY) saved.

We use deworming program cost estimates from the Partnership for Child Development (PCD (1999)), which reports costs of 0.49 US dollars per pupil per year in a large-scale government intervention in Tanzania. These costs are probably more relevant for potential large scale programs than the PSDP costs, since the PSDP was not able to fully realize economies of scale in drug purchase and delivery, and since it is difficult to disentangle evaluation and delivery costs in the PSDP. [53]

According to the World Health Organization, schistosomiasis infections are associated with much greater disease burden per infected individual than geohelminths, on average.[54] Approximately 18 percent of those infected with helminths globally are thought to suffer morbidity as a result of their infection, and in our cost-effectiveness calculations we assume that the entire disease

[53]Excluding the costs most clearly linked to the evaluation yields a cost per pupil treated through the PSDP in 1999 of 1.46 US dollars, with nearly half of this cost in drug purchases. However, the PSDP used trained nurses, held meetings to explain consent procedures, individually recorded the names of all pupils taking medicine, and was headquartered in Busia town, several hours drive away from many project schools. These costs might have been unnecessary in a large-scale program that did not include an evaluation component.

[54]Given data on the burden of disease in WHO (2000), and the number of people infected worldwide, the implied average DALY burden per person infected is 0.0097 for schistosomiasis, 0.0013 for hookworm, 0.0005 for whipworm, and 0.0004 for roundworm.

burden is concentrated among individuals with moderate-to-heavy infections (Bundy et al. (2001)).[55]

In calculating the overall reduction in disease burden due to the program, we consider overall treatment effects (corrected for cross-school externalities) on the treated in treatment schools, externality effects (corrected for cross-school externalities) on the untreated in treatment schools, and externalities for untreated pupils in comparison schools, using results from specifications similar to regression 3 in Table VII, but including the within-school externality terms from Table VII, regression 2 (estimated separately for each type of worm infection). Given the randomized design, we assume that the Group 3 schools (which lack 1999 parasitological data) experienced the same externality benefits as Group 2 schools through early 1999, when neither group had received deworming treatment.

Summing these three components of the treatment effect, the total number of DALY's averted as a result of the program is 649, which translates into a cost of approximately $5 per DALY averted, using the costs of the PCD program in Tanzania. This estimate still ignores the health spillover benefits for other untreated children and adults in the treatment area, thus underestimating cost-effectiveness. Even if the PCD costs were underestimated by a factor of two, deworming would still be among the most cost-effective health interventions for less developed countries.

The externality benefits of treatment (both within and across schools) account for 76 percent of the DALY reduction. A naïve treatment effect estimate that failed to take externalities into account would underestimate program treatment effects, not only because externalities would be missed, but also because gains among the treatment group would be underestimated. Consequently, the naïve estimate would overestimate the cost per DALY averted by a factor of four, leading to the mistaken conclusion that deworming does not meet the strictest cost-effectiveness standards.

The health gains are overwhelmingly attributable to reductions in the prevalence of moderate-to-heavy schistosomiasis: 99 percent of the total DALY reduction is due to averted schistosomiasis. We can separately calculate the cost per DALY averted for the geohelminths; geohelminth infections lead to less morbidity according to the WHO, but are also much cheaper to treat than schistosomiasis. Assuming that drug delivery costs remain the same, but considering only albendazole drug costs in this exercise, the cost per geohelminth DALY averted would be $280, which implies that mass geohelminth treatment in areas without schistosomiasis would not meet strict cost effectiveness cri-

[55]Note that this implies that the burden of disease per infected individual in our sample is greater than the world average, which is appropriate, since levels of moderate-heavy infection are relatively high in this setting.

teria in the poorest countries based solely on health impacts.[56] As discussed below, however, it is likely to be justified on other grounds.

8.2. *Educational Cost Effectiveness*

Deworming was by far the most cost-effective method of improving school participation among a series of educational interventions implemented by ICS in this region of Kenya that were subject to randomized evaluations. ICS has implemented and evaluated textbook provision, grants to school committees, training for teachers, and incentives for teachers based on student test scores and dropout rates. Given that the deworming program increased school participation by approximately 0.14 years per treated child (see Section 6), a large scale program with the Tanzania PCD cost of 0.49 US dollars per child would cost approximately $0.49/0.14, or $3.50 US dollars per additional year of school participation, including both effects on the treated and externality benefits. Aside from deworming, the program that was most successful in increasing school participation was the ICS Child Sponsorship Program (CSP). This program had a number of components, but the key component was substantially reducing the cost of school attendance by paying for the uniforms that Kenyan children are required to wear to school. Even under optimistic assumptions, reducing the cost of schooling in this way costs approximately $99 per additional year of participation induced (refer to Kremer, Moulin, and Namunyu (2002)).[57,58]

8.3. *Deworming as Human Capital Investment*

Given that the PSDP increased school participation but not test scores, and that the empirical literature on effects of schooling examines years of schooling

[56]The cost per DALY for geohelmnith treatment would be lower if albendazole were delivered as part of an ongoing school-based project in areas where schistosomiasis is being treated, although schools would still have to be visited at least once more per year for an additional round of albendazole treatment.

[57]The assumptions about the cost of attracting children to school by reducing the cost of school are optimistic because we assume that CSP's impact on school participation was due entirely to reducing the cost of school. The program also provided textbooks and new classrooms; another evaluation in the same area found that provision of textbooks did not affect school participation. School participation improved immediately through CSP, while classrooms were only provided several years into the CSP program. In any case, if textbook or classroom costs are included in CSP, deworming appears even more cost effective.

[58]Even under the extreme assumptions that uniforms are a pure transfer to parents so the social cost of the CSP is simply the deadweight loss associated with raising tax revenue, and that households obtained no consumption benefits from the deworming program, the social cost of deworming per year of extra school participation is likely to be far lower than that of purchasing school uniforms.

completed rather than days of school participation, any calculation about its effects on human capital accumulation must necessarily be speculative. Nonetheless, a rough calculation suggests that the labor market benefits of deworming may far outweigh their costs. Knight and Sabot (1990) estimate returns to education in Kenya controlling for a wide range of variables including cognitive tests. They decompose the returns to education into a return to cognitive performance (on tests of literacy, numeracy, and reasoning) and a direct return to years of schooling and find that years of schooling alone accounts for approximately forty percent of the 17 percent rate of return to education.[59] If one interprets this as a human capital effect rather than a signalling effect, the return to an additional year of primary school would be approximately 7 percent.

Including externalities, the program increased school participation by 0.14 school years per pupil treated, as discussed in Section 6. Output per worker in Kenya is $570 (World Bank (1999)). To calculate the effect on the net present value of discounted wages, we assume that sixty percent of output per worker in Kenya is wages, and that wage gains from higher school participation are earned over forty years in the workforce and discounted at five percent per year. We assume no wage growth over time. Against this long-run wage increase, we set the opportunity cost of schooling, as children may work rather than attend school. However, children who are heavily infected with worms are unlikely to be particularly productive as workers and may not work at all. We assume that the average primary school child who misses school due to worms is half as productive as the average adult; this is likely to represent an upper bound on productivity of school-aged children in general, let alone sick children.[60] Under these assumptions, deworming increases the net present value of wages by over $30 per treated child at a cost of only $0.49.

Even if increased school participation led to negative congestion externalities by increasing class size, the benefits are large enough to pay for the additional teachers needed to offset the class size increases. To see this, note that the program increased school participation by 0.14 school years per pupil treated, and that with one teacher per thirty pupils, this would require an additional 0.0047 teachers. We estimate teacher compensation at $1942 per year (see Kremer, Moulin, and Namunyu (2002)), so this amounts to $9.06 per treated pupil. So a program that provided deworming and additional teachers

[59]Knight and Sabot (1990) performed this decomposition for returns to secondary education, but it serves as a useful approximation in the absence of a similar decomposition for primary education.

[60]Udry (1996) finds that children's agricultural labor productivity is much less than one-half that of adult agricultural labor productivity in another rural African setting (Burkina Faso). If one assumes that the children who miss school as a result of worms were only one-fifth as productive as adults, then the benefit-cost ratio for the program is still over ten even if the rate of return to an additional school year is only 1.5 percent (calculations not shown).

would generate at least \$30 in future wage benefits at a cost of approximately $\$9.06 + \$0.49 = \$9.55$.[61]

8.4. *Externalities and Optimal Deworming Subsidies*

The externality benefits of deworming in terms of future wages (as calculated in Section 8.3) alone appear to be far larger than the costs of deworming, suggesting a rationale for subsidies even under an orthodox externalities analysis. The total net externality gain (within and across schools) per child treated is then \$15.90 per child treated, over thirty times as large as the \$0.49 cost of deworming. This figure is likely to once again understate the true externality benefits, since it excludes the potentially substantial benefits experienced by school-age and younger children not enrolled in school, by adults in these communities, and individuals in areas bordering the study area. Even if increased school participation led to negative congestion externalities by increasing class size, the positive externalities (\$15.90) are more than fifty percent larger than the cost of additional teachers needed to offset class size increases plus drug costs (\$9.55), suggesting that a large government deworming subsidy is optimal.[62]

To summarize, treatment of schistosomiasis appears to be an extremely cost-effective health intervention under standard health cost effectiveness criteria for less developed countries, although this is less true for the treatment of geohelminths alone. Even in areas with geohelminths but little schistosomiasis, however, deworming is a cost-effective way to boost school participation relative to other educational interventions evaluated in the same area, such as directly reducing the cost of schooling through the provision of school uniforms. It also appears likely that deworming can be justified as a human capital investment. Finally, the externality benefits from deworming in the program we examine are likely sufficient to justify fully subsidizing treatment. Since externalities across schools are substantial, public subsidies should be determined at levels higher than local school committees, such as the district or provincial level.

Note that while we can conclude that there were substantial externalities from the deworming treatment provided through the PSDP, it is difficult to draw firm conclusions about optimal deworming subsidies in the absence of a fully-fledged behavioral and epidemiological model, since the marginal positive externalities from treatment depend on how many others are also being

[61] In future work, we hope to track the children in this study as they enter the labor market in order to estimate how child health gains from deworming affect adult income and other socioeconomic outcomes.

[62] Even under the assumption of a ten percent discount rate, and maintaining the conservative assumption that children are half as productive as adults, the externality benefit–cost ratio is approximately one.

treated. While positive externalities from PSDP were large, it is difficult to gauge how large treatment externalities would be at alternative coverage levels. In theory, depending on epidemiological parameters, some incomplete level of coverage could potentially be sufficient to eliminate the disease from the population, in which case there would be no point in raising subsidies above an amount that would generate this level of coverage. However, Miguel and Kremer (2002) find that use of deworming drugs is very low even at modest positive prices, so it seems likely that the externality benefits of deworming would be sufficient to warrant a zero price. Caution is needed in extrapolating these results to areas with different worm prevalence, since while the direct benefits of deworming may be proportional to worm burden, the externality benefits are likely to vary nonlinearly with worm burden. Clearly, additional research is needed to determine optimal deworming subsidies in this and other settings.

9. CONCLUSION

A school-based deworming program in Kenya led to a 7.5 percentage point average gain in primary school participation in treatment schools, reducing overall school absenteeism by at least one-quarter. Treatment created positive health and school participation externalities for untreated students. A rough calculation suggests that these spillovers alone are sufficient to justify not only fully subsidizing deworming treatment, but perhaps even paying people to receive treatment.

Our results have methodological implications for the literature on the educational effects of deworming, and for the design of randomized evaluations more generally. Existing estimates, from medical studies that randomize treatment within a school, doubly underestimate the effects of deworming programs. First, they entirely miss the external effects of deworming, and second, they underestimate the direct effects to the extent that the comparison group benefits from externalities, biasing existing treatment effect estimates toward zero. This problem can be addressed by randomizing at the level of larger units, such as schools rather than at the individual level. To the extent that spillovers take place within groups, group-level randomization allows identification of overall program impact on the group. Moreover, by the law of large numbers, group-level randomization creates more variation in local treatment densities than individual-level randomization, and this random variation can be used to estimate cross-group externalities. While group-level randomization can be used in other settings with externalities localized, either geographically or along some other dimension, such as the analysis of school vouchers or information transmission and technology diffusion, it cannot be used to estimate more global spillovers, such as those arising through general equilibrium price effects.

When local treatment externalities are expected, field experiments can be purposefully designed to estimate externalities by randomizing treatment at various levels.[63] A prospective research design for identifying externalities both within and across schools in rural Kenya would randomize treatment across pupils within schools, across schools within "clusters" of schools, and then among these clusters. Treatment rates could be varied across clusters to estimate externalities at various treatment levels. However, this multi-level design may not be practical in all contexts: for example, in our context it was not possible to randomize treatment within schools. Randomization at the level of clusters of schools also dramatically increases the sample size needed for adequate statistical power, raising project cost. The large improvement in school participation following deworming estimated in this study points to the important role that tropical diseases such as intestinal worms may play in reducing educational attainment in sub-Saharan Africa and provides microeconomic support for claims that Africa's high tropical disease burden is a causal factor contributing to its low income.[64] Our results also suggest that microeconomic and macroeconomic studies that estimate the impact of health on income conditional on educational attainment are likely to systematically underestimate its impact, since some of the overall health effect works through the education channel. To the extent that the treatment of other tropical infectious diseases also generates spillover benefits similar to deworming, the externality findings of the current study provide an additional rationale for a substantial public role in subsidizing medical treatment for infectious diseases in less developed countries. Miguel and Kremer (2002) examine the design of programs to promote deworming, why a large minority of children did not take the free deworming drugs, and the role of drug cost, social learning, and other behavioral factors in influencing take-up of deworming drugs.

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[63] See Duflo and Saez (2002).

[64] Of course, worms' impact on wages through education can only explain a small fraction of the enormous income gap between African and industrialized countries.

APPENDIX

APPENDIX TABLE AI

PRIMARY SCHOOL DEWORMING PROJECT (PSDP) TIMELINE, 1997–1999

Dates	Activity
1997	
October	Pilot Kenya Ministry of Health, Division of Vector Borne Disease (DVBD) parasitological survey. Pilot Pupil Questionnaire
1998	
January–March	Parent-teacher meetings in Group 1 schools
	Pupil Questionnaire administration in grades 3 to 8, and School Questionnaire administration in all schools
	DVBD parasitological survey for grades 3 to 8 in Group 1 schools
January–May	Heavy precipitation and widespread flooding associated with the El Niño weather system
March–April	First round of 1998 medical treatment (with albendazole, praziquantel) in Group 1 schools
October–November	ICS (NGO) examinations administered in grades 3 to 8 in all schools
November	Second round of 1998 medical treatment (with albendazole) in Group 1 schools
1999	
January–March	Parent-teacher meetings in Group 1 and Group 2 schools
	Pupil Questionnaire administration in grades 3 to 8, and School Questionnaire administration in all schools
	DVBD parasitological and hemoglobin surveys for grades 3 to 8 in Group 1 and Group 2 schools
March–June	First round of 1999 medical treatment (with albendazole, praziquantel) in Group 1 and Group 2 schools
May–July	Deworming drug availability survey of local shops, clinics, and pharmacies
October	ICS (NGO) examinations administered in grades 3 to 8 in all schools
October–November	Second round of 1999 medical treatment (with albendazole) in Group 1 and Group 2 schools

APPENDIX TABLE AII

LOCAL DENSITIES OF OTHER PRIMARY SCHOOLS AND DEWORMING COMPLIANCE RATES[a]

	Dependent variable:	
	1998 Compliance rate (any medical treatment) OLS (1)	1999 Compliance rate (any medical treatment) OLS (2)
Treatment school pupils within 3 km	−0.04	−0.08
(per 1000 pupils)	(0.06)	(0.09)
Treatment school pupils within 3–6 km	0.04	−0.01
(per 1000 pupils)	(0.07)	(0.05)
Total pupils within 3 km	0.05	0.05
(per 1000 pupils)	(0.05)	(0.08)
Total pupils within 3–6 km	−0.06	−0.02
(per 1000 pupils)	(0.06)	(0.05)
Grade indicators, school assistance controls, district exam score control	Yes	Yes
R^2	0.60	0.57
Root MSE	0.082	0.131
Number of observations	25	49
Mean of dependent variable	0.66	0.42

[a] Robust standard errors in parentheses. Observations are weighted by total school population. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The 1998 compliance data are for Group 1 schools, and the 1999 compliance data are for Group 1 and Group 2 schools. The pupil population data are from the 1998 School Questionnaire. We use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population for all schools. The number of treatment school pupils in 1998 is the number of Group 1 pupils, and the number of treatment school pupils in March 1999 is the number of Group 1 and Group 2 pupils.

APPENDIX TABLE AIII

DEWORMING HEALTH EXTERNALITIES—ROBUSTNESS CHECKS[a]

	Any moderate-heavy helminth infection, 1999				Moderate-heavy schistosomiasis infection, 1999			
	Probit	OLS, spatial s.e.	Probit	Probit (Group 1 only)	Probit	OLS, spatial s.e.	Probit	Probit (Group 1 only)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for Group 1 (1998 Treatment) School	−0.25***	−0.24***	−0.28***		−0.03	−0.08*	−0.04	
	(0.05)	(0.05)	(0.05)		(0.03)	(0.04)	(0.04)	
Group 1 pupils within 3 km	−0.26***	−0.17**		−0.30***	−0.12***	−0.13***		−0.06***
(per 1000 pupils)	(0.09)	(0.07)		(0.07)	(0.04)	(0.04)		(0.02)
Group 1 pupils within 3–6 km	−0.14**	−0.18***		−0.07	−0.18***	−0.20**		−0.05***
(per 1000 pupils)	(0.06)	(0.04)		(0.06)	(0.03)	(0.07)		(0.01)
Total pupils within 3 km	0.11***	0.09	0.07	0.04	0.11***	0.14***	0.10***	0.03***
(per 1000 pupils)	(0.04)	(0.06)	(0.05)	(0.04)	(0.02)	(0.03)	(0.03)	(0.01)
Total pupils within 3–6 km	0.13**	0.16***	0.08	0.03	0.12***	0.13**	0.08**	0.04
(per 1000 pupils)	(0.06)	(0.04)	(0.05)	(0.06)	(0.03)	(0.05)	(0.03)	(0.01)
(Group 1 pupils within 3 km)/			−0.29***				−0.13**	
(Total pupils within 3 km)			(0.11)				(0.07)	
(Group 1 pupils within 3–6 km)/			−0.12				−0.41***	
(Total pupils within 3–6 km)			(0.22)				(0.11)	
Any moderate-heavy helminth infection, 1998				0.25***				
				(0.03)				
Moderate-heavy schistosomiasis infection, 1998								0.22***
								(0.10)
Grade indicators, school assistance controls, district exam score control	Yes	No	Yes	Yes	Yes	No	Yes	Yes
R^2	–	0.57	–	–	–	0.48	–	–
Root MSE	–	0.177	–	–	–	0.168	–	–
Number of observations	2326 (pupils)	49 (schools)	2326 (pupils)	602 (pupils)	2326 (pupils)	49 (schools)	2326 (pupils)	603 (pupils)
Mean of dependent variable	0.41	0.41	0.41	0.25	0.16	0.16	0.16	0.08

[a]Grade 3–8 pupils. Robust standard errors in parentheses. Disturbance terms are clustered within schools for regressions 1, 3, 4, 5, and 7. Disturbance terms are allowed to be correlated across spaces using the method in Conley (1999) in regressions 2 and 6. Observations are weighted by total school population. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The 1999 parasitological survey data are for Group 1 and Group 2 schools. The pupil population data are from the 1998 School Questionnaire. We use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population for all schools.

APPENDIX TABLE AIV

IV ESTIMATES OF HEALTH AND SCHOOL PARTICIPATION EXTERNALITIES[a]

	Any moderate-heavy helminth infection, January–March 99		Average individual school participation, May 98–March 99	
	Probit (1)	IV-2SLS (2)	OLS (3)	IV-2SLS (4)
Indicator for Group 1 (1998 Treatment) School	−0.12*	−0.04	0.056***	0.024
	(0.07)	(0.10)	(0.020)	(0.028)
Group 1 pupils within 3 km (per 1000 pupils)	−0.26***	−0.22***	0.023	0.020
	(0.09)	(0.07)	(0.036)	(0.035)
Group 1 pupils within 3–6 km (per 1000 pupils)	−0.13**	−0.11**	−0.041	−0.041
	(0.06)	(0.05)	(0.027)	(0.026)
Total pupils within 3 km (per 1000 pupils)	0.11***	0.11***	−0.035*	−0.034*
	(0.04)	(0.04)	(0.019)	(0.019)
Total pupils within 3–6 km (per 1000 pupils)	0.13**	0.11**	0.022	0.021
	(0.06)	(0.05)	(0.027)	(0.027)
Indicator received first year of deworming treatment, when offered (1998 for Group 1, 1999 for Group 2)	−0.06*	−0.06	0.100***	0.013
	(0.03)	(0.05)	(0.014)	(0.030)
(First year as treatment school Indicator) ∗ (Received treatment, when offered)	−0.14*	−0.21*	−0.012	0.059
	(0.07)	(0.12)	(0.020)	(0.046)
Grade indicators, school assistance controls, district exam score control	Yes	Yes	Yes	Yes
Time controls	No	No	Yes	Yes
R^2	–	–	0.36	–
Root MSE	–	0.446	0.219	0.221
Number of observations	2326	2326	18264	18264
Mean of dependent variable	0.41	0.41	0.784	0.784

[a] Disturbance terms are clustered within schools. Robust standard errors in parentheses. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The two instrumental variables are an indicator for girls under age 13 and all boys (ELG), and (ELG) ∗ (Group 1 indicator). The coefficient on the Group 1 school indicator variable serves as an estimate of the within-school externality effect in 1998. This IV approach could overestimate the treatment effect if the treatment effect is heterogeneous, with sicker pupils benefiting most from treatment, and if among the girls over 13, the sickest girls are most likely to be treated in treatment schools. However, among the subsample of older girls, the compliance rate was not significantly related to infection status in 1998 (Table VI), and in 1999 under ten percent of older girls were treated (Table III). We find similar effects even when we exclude the schools near the lake where older girls were likely to be treated (results not shown). Note that the IV estimates of within-school participation externalities should be interpreted as local average treatment effects for the older girls. Since school participation treatment effects are largest for younger pupils, it is not surprising that the IV externality estimates among the older girls are smaller than the OLS estimates, which are for the entire population. We use the number of girls less than 13 years old and all boys (the pupils eligible for deworming in the treatment schools) as the school population for all schools.

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