Networks, Social Learning and Technology Adoption: The Case of Deworming Drugs in Kenya

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Abstract: We examine social learning using data on adoption of deworming medicine in Kenyan schools. These drugs kill worms already in the body; although people are soon reinfected, treatment breaks the cycle of transmission, generating positive externalities. We find that those randomly exposed to more information about deworming drugs through their social network were significantly *less* likely to take the drugs, and more likely to believe the drugs are "not effective". This finding is consistent with the hypothesis that individuals have overly optimistic priors about private drug benefits. The combination of strong social effects and extensive social networks among teenagers implies that a "child-to-child" public health approach focusing on teenagers will speed social learning.

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1. Introduction

Economic growth and development requires the adoption of new technologies. Some have argued that technology adoption is subject to a process of social learning, as individuals take into account the experiences of their social contacts when deciding whether to adopt. To the extent that other people learn from early adopters, there may be justification for public policies subsidizing early technology adopters, and for extension activities.

A large empirical literature beginning with Griliches (1957) examines the social determinants of technology adoption, and a number of recent papers examine the role of social learning in the spread of technologies in less developed countries, including Conley and Udry (2000), Foster and Rosenzweig (1995) and Munshi (2002).[2]

Establishing the existence of social effects in non-experimental studies is complicated by the possibility that omitted variables can lead to bias. People with similar characteristics, tastes, and beliefs may associate in the same social groups (Manski 1993) and correlations among outcomes within social networks may reflect these commonalities rather than social learning. The existing non-experimental literature attempts to address this issue through panel data sets that contain quite rich information on individual characteristics (Conley and Udry 2000); however, it remains difficult to rule out the possibility of omitted variable bias. This paper, as well as Duflo and Saez (2003), address this issue by randomizing assignment to treatment groups.

Showing that technology adoption is influenced by contact with earlier adopters is not sufficient to distinguish the hypotheses of learning and of imitation. For example, Kremer and Levy (2003) find that college students randomly assigned roommates who drink alcohol earn worse grades and are more likely to drink themselves, and Sacerdote (2001) finds similar peer effects in social activity, but these effects may stem from imitation or changing social norms rather than learning. We are able to address this issue

[2] Influential theoretical work on social learning includes Banerjee (1992), Bikhchandani et al (1992), and Ellison and Fudenberg (1995).

by examining a technology that did not spread, one that yielded private benefits that people apparently did not value highly.

Another key issue is understanding the channels through which they operate. In many contexts, researchers are forced to impose a particular definition of which peer group people learn from; for example, some researchers have compared people who are geographically close to each other (Burke et al 2003), or in the same work unit within an organization (Duflo and Saez 2003). However, information could potentially flow through a variety of channels, including kinship networks or community groups, and missing these channels could lead to the underestimation of actual information flows. Moreover, understanding the networks through which information flows is important both theoretically and for public policy. For instance, in the public health context we examine, some advocate "child-to-child" information campaigns, asserting that communication among children changes child behavior more effectively than adult-to-child communication (Hubley 1993, WHO 2000).

We examine social learning in the context of a program that encouraged the adoption of deworming drugs in Kenyan schools through subsidies and health education. Although the social benefits of deworming drugs are very large in terms of health and education, the private benefits are much smaller, due to pervasive treatment externalities (Miguel and Kremer 2003). These drugs kill worms already in the body, thus helping the individual who takes them, and generating positive externalities by breaking the transmission cycle. However, people are typically reinfected within months of treatment. Due to the financial and logistical constraints, the non-profit organization administering the deworming program phased it in over time. The "early treatment" and "late treatment" schools were randomly selected, generating random variation in the extent to which individuals' social contacts were exposed to the program. To understand the channels of information flow, we collected rich original survey data on social networks in which respondents could designate their own reference groups.

We find that children whose parents have (randomly) more social links to early treatment schools, controlling for their total number of social links, are themselves significantly less likely to take deworming drugs: for each additional social link a parent has to an early treatment school, her child is 3.1

percentage points less likely to take the drugs – evidence that people's self-defined reference group has a major impact on behavior. We find large differences between experimental and non-experimental social effect estimates, suggesting that omitted variable bias is positive and large.

A model of learning with over-optimistic prior beliefs is consistent with this main finding, and with other patterns in the data. Social learning effects are especially large in families with more schooling, and we find that these educated families start with particularly favorable beliefs about the technology, but then revise them downwards with more information – suggesting that the widely-noted tendency for the educated to be early adopters of technology could potentially reflect more optimistic initial beliefs, rather than faster learning per se. The evidence suggests that the social effects of adoption are due to transmission of information rather than simple imitation, since people are *less* likely to adopt the greater their exposure to schools where the program was previously introduced. Moreover, we find direct evidence that beliefs about the efficacy of the drugs are revised downwards in response to contact with those who are familiar with the medicine.

For those seeking to promote technological adoption it is important to have information on the channels through which information flows. We find that social effects are particularly strong in this context among teenagers – echoing findings from social psychology that teenagers are more susceptible to peer influence than either younger children or adults (Steinberg and Cauffman 1996, Gruber 2001). We also find that teenagers have more extensive social networks than younger children. These results suggest a potential role for child-to-child school health campaigns focusing on teenagers. We also find that even relatively "weak" social ties – for instance, those with whom the respondent speaks only infrequently – have a large impact on individual behavior.

The remainder of the paper is structured as follows. Section 2 discusses worm infections and the cultural understanding of health in the study area, and Section 3 describes the Primary School Deworming Project. Section 4 lays out a social learning model, Section 5 discusses the estimation of social effects and presents the main empirical results, and Section 6 presents patterns in the structure of social networks. The final section summarizes and discusses implications for the design of public health projects.

2. Intestinal Worm (Helminth) Infections

Over 1.3 billion people worldwide are infected with hookworm, 1.3 billion with roundworm, 900 million with whipworm, and 200 million with schistosomiasis (Bundy 1994). Most have light infections, which are often asymptomatic, but more severe worm infections can lead to iron deficiency anemia, protein energy malnutrition, stunting (a measure of chronic undernutrition), wasting (a measure of acute undernutrition), listlessness, and abdominal pain, and heavy schistosomiasis infections can have even more severe consequences.[3]

Helminths do not reproduce within the human host, so high worm burdens are the result of frequent re-infection. The geohelminths (hookworm, roundworm, and whipworm) are transmitted through ingestion of, or contact with, infected fecal matter, which can occur, for example, if children do not use a latrine and instead defecate in the fields near their home or school, areas where they also play.[4] Schistosomiasis is acquired through contact with infected freshwater; for example, in our Kenyan study area people often walk to nearby Lake Victoria to bathe and fish. Medical treatment for helminth infections creates externality benefits by reducing worm deposition in the community and thus limiting re-infection among other community members (Anderson and May 1991).

It is worth briefly discussing cultural understandings of health, and worms in particular, in our study area; our account draws heavily on the work of Geissler (1998a, 1998b, 2000). Medical anthropologists have long pointed out that people can simultaneously hold traditional and biomedical views of health, in a manner similar to religious syncretism. Geissler argues that this is the case for views about worms in western Kenya. In the traditional view, worms are an integral part of the human body and necessary for digestion, and many infection symptoms – including abdominal pain and malnutrition – are attributed to malevolent occult forces ("witchcraft") or breaking taboos (Government of Kenya 1986).

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

[4] Note that individuals are likely to have at least some knowledge of their infection status, since they can observe certain worms in their stool, and may also see them being expelled from their body after treatment.

Educated people are more likely to engage in the biomedical discourse and thus more likely to treat illnesses medically rather than use traditional remedies.

Geissler studies deworming take-up in a Kenyan district that borders our study area, with a nearly identical worm infection profile, and finds that, most people do not place much value on deworming treatment because worms are not seen as a pressing health problem – especially compared to malaria and HIV/AIDS.[5] As a result, there was essentially no deworming outside the school health program Geissler studies, and most children instead relied on herbal remedies to alleviate the abdominal discomfort caused by worms. It is perhaps surprising that more of a market for deworming drugs has not developed in western Kenya independently of school health projects, given how cheap the main drugs – albendzole and praziquantel – are to produce. While many medicines, such as aspirin and anti-malarials, are cheaply available in nearly all local shops, deworming drugs are found in few and only at high prices (where mark-ups are high presumably because the market is thin).[6]

Geissler (2000) also stresses that older children have considerable autonomy in making healthcare decisions. Children have extensive knowledge of both pharmaceuticals and traditional remedies, and often self-treat (Prince 2001). Nearly three-quarters of child illness events are not discussed at all with adult caregivers (Geissler 2000), which may explain the particularly strong influence of teen social contacts in our data, as discussed below.

3. The Primary School Deworming Project (PSDP) in Busia, Kenya

We study the Primary School Deworming Project (PSDP), a school health program carried out by a Dutch non-governmental organization (NGO), ICS Africa, in cooperation with the Kenyan Ministry of Health. The project took place in Busia district, a poor and densely-settled farming region in western Kenya, and

[5] Geissler studies an ethnically Luo area (Luos speak a Nilotic language). The majority of our sample are ethnically Luhya (a Bantu-speaking group) though Luos are four percent of our sample. However, traditional Luo views toward worms are closely related to views found among Bantu-speaking groups in other parts of Africa, including Mozambique (Green et al. 1994, Green 1997) and South Africa (Zondi and Kvalsig 1987).

[6] We conducted a survey in 1999 of all hospitals, health clinics, dispensaries, and pharmacies, as well as many local shops (*dukas*) in this area to assess the availability of deworming drugs. We found that none of the 64 local shops surveyed had either albendazole (or its close substitute, mebendazole) or praziquantel in stock, though a minority of shops carried less effective deworming drugs (levamisole hydrochloride and piperazine).

the 75 project schools include nearly all rural primary schools in this area, with over 30,000 enrolled pupils between the ages of six to eighteen, over 90 percent of whom suffer from intestinal worm infections. In January 1998, the PSDP schools were randomly divided into three groups (Groups 1, 2 and 3) of twenty-five schools each: the schools were first divided by administrative sub-unit (zone) and by involvement in other non-governmental assistance programs, and were then listed alphabetically and every third school assigned to a given project group.[7]

Due to administrative and financial constraints, the health intervention – which included both deworming medicine and health education on worm prevention behaviors – was phased in over several years. Group 1 schools received treatment in 1998-2001, and Group 2 schools in 1999-2001, while Group 3 began participating in 2001. This design implies that in 1998, Group 1 schools were treatment schools, while Group 2 and Group 3 schools were the comparison schools; and in 1999 and 2000, Group 1 and 2 schools were the treatment schools and Group 3 schools were comparison schools. Starting in 1999, signed individual parental consent was required for deworming, while in 1998 only "community consent" (meetings at which parents were informed of – and could opt out of – the program) had been required. At each school, the project started out with a community meeting of parents, teachers, and the school committee, which included a discussion of worm infections, the nature of medical deworming treatment, and worm prevention measures. All school communities agreed to participate in the project.

The project provided periodic treatment with deworming drugs in all schools where prevalence of the disease was sufficiently high. The geohelminths and schistosomiasis can be treated using the low-cost single-dose oral therapies of albendazole and praziquantel, respectively. The World Health Organization has endorsed mass school-based deworming in areas with prevalence over fifty percent, since mass treatment eliminates the need for costly individual screening (Warren et al. 1993, WHO 1987), and drugs delivered through a large-scale school program may cost as little as 0.49 USD per person per year in East Africa (PCD 1999).

[7] Appendix Table A1 presents a more detailed project timeline.

Side effects are minor and transient, rarely lasting more than one day, but may include stomach ache, diarrhea, dizziness, fever and even vomiting in some cases (WHO 1992). Side effects are more severe for children with heavier schistosomiasis infections, and in our data parents of children with more severe infections are in fact somewhat more likely to claim that the drugs had side effects (although this effect is not statistically significant).[8]

Non-participation in deworming treatment could have resulted from school absenteeism, or as a result of the implicit costs of participation. The observed take-up rate of roughly 70 percent is broadly consistent with the fact that on a typical day in schools in this area, about 30 percent of pupils are absent (Miguel and Kremer 2003).[9] Aside from the typically minor drug side effects, costs of participation include walking to school to provide written parental consent (for parents), any costs to remembering the day of deworming treatment at school, and any costs of visiting the headmaster for the many parents who are behind on school fee payments.

Mass treatment with deworming drugs generated substantial gains in health and school participation in these schools. Deworming reduced absenteeism in treatment schools by one quarter, and externalities from the treatment generated gains for neighboring schools as well. The reductions in serious worm infections for the untreated within treatment schools were about 70 to 80 percent as large as those for the treated, while reductions for pupils in neighboring schools were on the order of 30 percent as large as those for the treated. While the overall program impact is large, the gaps between the treated and untreated are not, due to these externalities (Miguel and Kremer 2003). Note also that there were no significant test score gains in treatment schools. Thus private benefits to treatment appears only moderate.

The NGO has a general policy of introducing community cost-recovery in all its rural development programs, to promote "sustainability" and to confer project "ownership" on the

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[8] Due to concern about the possibility the drugs could cause birth defects, standard practice in mass deworming programs at the time was to not treat girls of reproductive age, although with a lengthening track record of safe use, the WHO has recently called for deworming drugs to be administered to older girls as well as other children.

[9] The 2001 take-up data (collected by field officers) indicates that nearly 80 percent of non-treatment was due to pupil absence from school on the treatment day, with the remainder due to no parental consent; there were very few cases of child refusal to take the drugs (less than one percent).

beneficiaries. In this case, the NGO temporarily waived this policy initially, and then decided to phase it in gradually. The fifty Group 1 and Group 2 schools were stratified by treatment group and geographic location, and then twenty-five were randomly selected to pay user fees for medical treatment in early 2001, while the remaining twenty-five continued to receive free medical treatment; all Group 3 schools received free treatment in 2001. The average cost of deworming per child was slightly more than 0.30 U.S. dollars – still a heavily subsidized price, about one-fifth the cost of drug purchase and delivery through the program.[10] We find that this small cost led to a dramatic reduction in treatment rates: children in 75 percent of households in the free treatment schools received deworming drugs in 2001 (Table 1, Panel A), while the rate was only 19 percent in cost-sharing schools – providing evidence on the low value most households attach to deworming.[11]

In addition to medical deworming treatment, the project included intensive health education on worm prevention behaviors, mainly focusing on hand washing, wearing shoes, and avoiding infected fresh water. This health education component, provided by both teachers and adult NGO workers using culturally appropriate materials designed in Tanzania, failed to substantially change worm prevention behaviors (Kremer and Miguel 2003).

4. A Simple Model of Learning about a Health Technology

It is possible that once people gain experience with a new medical technology, they will learn about its effectiveness and then spread this information on to others. Below we consider a theoretical model of the spread of a technology through social learning, and then empirically examine the spread of deworming drug adoption and knowledge. We then examine how far we can go toward explaining take-up patterns in a rational framework with Bayesian learning (Section 5).

Imagine an individual *i* in school *j* deciding whether to adopt a new technology (or health practice), where an indicator variable for take-up is represented by $T_{ij} \in \{0, 1\}$. We represent the

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[10] Annual Kenyan per capita income is $340 (World Bank 1999), but incomes are thought to be lower in Busia.

[11] The data is described below. These results are discussed in greater detail in Kremer and Miguel (2003).

individual's expected overall private benefit of adoption by $\phi = E[U(T_{ij} = 1) - U(T_{ij} = 0)]$, which may include benefits in health, education, and other dimensions, where U is individual utility, the private benefit conditional on the treatment choices of other individuals.

At the moment the new technology is introduced, the individual has a prior belief on the expected private benefits, denoted ϕ_0, which may be greater or less than the actual expected benefit, ϕ. Priors about the impact of deworming may well have been quite variable across individuals, since people had little experience with mass deworming treatment before the program. ϕ_0 could be less than ϕ due to traditional beliefs about worms that emphasize their health benefits rather than their costs (Geissler 1998a). People could also have overly optimistic estimates of the private benefits of treatment for several reasons. NGO field officers may have over-emphasized its benefits during the community meetings held to publicize the program: although the scripts prepared for use by field officers did explain that people could become reinfected after taking the medicine, the NGO field officers see their jobs as promoting deworming medicine and prevention behaviors, and it is plausible that the enthusiasm of their description reflected the social rather than private, benefit of deworming. If people estimate the private benefit of treatment by simply comparing the health status of infected and uninfected people, they could similarly overestimate the effect of treatment, since treatment provides only short-run protection. Moreover, if people estimate their expected private benefits by comparing individuals in treatment and comparison schools, then to the extent that there are positive within-school treatment externalities, they will assign some of the externality value to private benefits, once again making priors on the private benefits of treatment overly optimistic.

We assume priors are based on n independent signals about treatment benefits, and individuals combine this prior information with additional information they receive from their social links in the early treatment schools. Individuals have N_{ij}^{E} early treatment links, and each such link yields one additional signal about private benefits, a noisy signal based on the link's own experiences as well as their observations of others. Individuals then compute the naïve sample average of private signals, from their prior and their social links, and the posterior belief on expected treatment benefits for an individual with N_{ij}^E early treatment links becomes:

$$E[U(T_{ij}=1)-U(T_{ij}=0)\mid N_{ij}^E]=\alpha(N_{ij}^E)\cdot\phi_0+(1-\alpha(N_{ij}^E))\cdot\phi \qquad (1)$$

where $\alpha(N_{ij}^E)=\dfrac{n}{n+N_{ij}^E}$, which goes to zero as individuals accumulate information through their social network, and as posterior beliefs approach the true expected benefit.[12]

This framework is the reduced-form of a model in which individuals use a simple updating rule, and compare observed outcomes across treated and untreated children in order to estimate the private treatment benefit in this setting – the variable of immediate interest to them – although implications are also likely to be similar to a more complex model in which people learn about the complicated underlying epidemiological model of treatment spillovers.[13]

The solution of the model is intuitive: when the prior is greater than the actual expected benefit ($\phi_0 > \phi$), those with more early treatment social links have lower posterior beliefs about expected benefits on average, and thus the likelihood of adoption declines in the number of early links. From Equation 1, the decline in the expected benefit of treatment with respect to early links will be convex, as in Figure 1, as the posterior asymptotically approaches the true expected benefit. Similarly, when the prior is less than the true expected benefit, the posterior asymptotically approaches the true expected benefit from below.

The prior can be a function of an individual characteristic X_{ij}, such that $\phi_{0,ij} = \phi_0(X_{ij})$, and we assume $\phi_0' > 0$ without loss of generality. For an illustration in the context of rural Kenya, formal schooling is considered an important predictor of favorable views to new health technologies (Akwara 1996, Kohler et al 2001), and more educated individuals are thus likely to have more "optimistic" priors

[12] For simplicity, we do not consider the case where individuals experiment today in order to gain more information about the technology for future rounds.

[13] Although individuals would be better able to react optimally to changes in public policy or to the disease environment in the theoretical framework where they learn about the underlying epidemiology, we do not think that such a model is realistic in this context, since the epidemiological model is complicated and few individuals in this area have even basic knowledge about worm infections: as we discuss below, the median resident is able to name just one of ten common worm infection symptoms, and fewer still can accurately describe infection transmission.

about technologies advocated by representatives of an NGO.[14] Note that this could reflect either the impact of education, or simply that those people who are more open to "modernity" are likely to obtain more education. When $\phi_0(X_{ij}) > \phi$ for all X_{ij}, individuals with more education generally have higher adoption rates, but additional early links lead to sharper drops in their adoption. Formally,

$$\frac{\partial^2 E[U(T_{ij}=1) - U(T_{ij}=0) \mid N_{ij}^E]}{\partial N_{ij}^E \partial X_{ij}} = -\phi_0{}'(X_{ij}) \cdot \frac{n}{(n+N_{ij}^E)^2} < 0 \tag{2}$$

Note that this cross-partial term is always negative, so as the prior becomes more optimistic, extra information will either cause a sharper drop (if $\phi_0(X_{ij}) > \phi$ for all X_{ij}) or a slower increase (if $\phi_0(X_{ij}) < \phi$) in the posterior belief about treatment benefits.

Treatment benefits may also be a function of an individual health characteristic, $W_{ij} = W(N_{ij}^E)$, such that $\phi_{ij} = \phi(W(N_{ij}^E))$, where health is allowed to be a function of early social links. For this study, W_{ij} should be thought of as the individual worm infection level, where those with higher infection levels typically have greater treatment benefits, $\phi' > 0$. Infection levels are also a function of social links due to epidemiological externalities: children whose families have close social interactions with households in early treatment schools may experience lower helminth re-infection rates and thus reductions in infection intensity, so in this setting $W' < 0$. The impact of early treatment links on the likelihood of adoption is presented in Equation 3 (where we make the algebraically convenient assumptions that $\phi_0' = \phi'$, and $\phi_0'' = \phi'' = 0$ at all infection levels, along the lines of a Taylor approximation):

$$\frac{\partial E[U(T_{ij}=1, W(N_{ij}^E)) - U(T_{ij}=0, W(N_{ij}^E)) \mid N_{ij}^E]}{\partial N_{ij}^E} \tag{3}$$

$$= \left\{ \left[\phi(W(N_{ij}^E)) - \phi_0(W(N_{ij}^E))\right] \cdot \frac{n}{(n+N_{ij}^E)^2} \right\} + \left\{ W'(N_{ij}^E) \cdot \phi' \right\}$$

[14] We thus depart from the standard assumption of common priors. For theoretical examinations of how different beliefs on political and health issues can persist, refer to Piketty (1995) and Das (2000), respectively.

The first right hand side term is the *information social effect*, and is negative when priors are overly optimistic, as above. The second term is the *infection social effect*, which should also be negative because having more early treatment links can lead to lower infection levels (due to epidemiological externalities) and this in turn reduces treatment benefits. We argue below that the infection social effect is small empirically, largely because child infection levels only weakly affect deworming take-up, and thus that the impact of early links on take-up is overwhelmingly driven by information rather than infection externalities.

It is worth elaborating further on the modeling of infection externalities. In the short-run treatment rids the individual of worms, but in the medium-run whether or not an individual is reinfected is determined by the local disease environment, not by her current treatment choice. The effect of other people's treatment choices on the magnitude of private treatment benefits is unclear *a priori*. As a benchmark case, if helminth re-infection rates are independent of current worm load, and if the health burden of infection is linear in worm load, then the private health benefits of treatment are independent of others' treatment decisions. If instead the health costs of infection are convex (concave) in worm load, then deworming treatment benefits will be greater (smaller) in an environment that is expected to have high exposure to worms in the future – and thus the private benefits of treatment will be *lower* (*higher*) if others are treated. In a previous study (Miguel and Kremer 2003) we estimate average overall deworming treatment spillovers and find that they are roughly linear in local treatment rates, but, due to data limitations, we have little power to detect higher order non-linear terms, to estimate an externality health benefits function of the sort described above. For simplicity, we thus do not explicitly model the expected treatment benefit as a function of others' choices here.

5. Empirical Results

5.1 Data and Measurement

The empirical work employs two new micro-datasets, the 2001 PSDP Parent Questionnaire and 2001 PSDP Pupil Questionnaire, which were administered by experienced enumerators under the supervision of NGO field managers, and allow us to separately examine the impact of parent and child social links. Parent Questionnaires were collected during household visits among a representative subsample of parents with children currently enrolled in Group 2 and Group 3 schools. A subsample of children (typically aged 10-17 years) actually in school on the day of survey administration were administered the Pupil Questionnaire. Survey refusal rates were very low for both surveys, as is typical for this region.

Parent Questionnaire respondents were asked for information on their closest social links: the five friends they speak with most frequently, the five relatives they speak with most frequently, additional social contacts whose children attend local primary schools, and individuals with whom they speak specifically about child health issues. These individuals are collectively referred to as the respondent's set of "social links". The survey also collected information on the deworming treatment status of social links' children and the effects of treatment on their health; how frequently the respondent speaks with each social link; which primary schools links' children attend; the global positioning system (GPS) location of the respondent's home; and the respondent's knowledge of worm infections and their attitudes toward deworming drugs. The Parent Questionnaire was administered in two rounds in 2001, with households randomly allocated between the rounds; the main difference between the rounds is that the Round 2 survey collected more detailed information on the impact of deworming on links' children.

The second survey, the 2001 Pupil Questionnaire, collected analogous information on social networks and attitudes toward deworming, but this time from schoolchildren themselves. Not all children were administered the 2001 Pupil Questionnaire, some because of limited financial resources for the research (which forced us to interview a representative subsample of children present in school on the day of survey administration, rather than all children), and others because of absence from school on that day.

Four different samples are used in the analysis. Sample I is the main parent sample, and contains 1678 parents surveyed in Rounds 1 or 2 with complete child treatment and parent social network data.[15]

[15] Eleven percent of surveyed households were dropped due to child ineligibility for deworming (i.e., the child was an older girl excluded from treatment), and a reasonably modest thirteen percent due to either missing parent network information, child treatment information, household characteristics, or difficulty matching observations across the 2001 surveys and earlier PSDP datasets.

Sample II contains the 886 parents surveyed in Round 2, and we use this sample to analyze the impact of the links' own deworming experiences on the respondent's choices. Sample III contains information for the 3164 children with complete 2001 Pupil Questionnaire data on social links and drug take-up, and who were eligible for treatment in 2001. Sample IV contains the 1239 pupils with complete Pupil Survey and Parent Survey information. Forty-six percent of the 1678 parents in Sample I and sixty-one percent of children in Sample III are dropped when we are restricted to this subsample with both complete parent and child data (note that there are multiple children in some households).

On average, parent respondents have 10.2 social links with children in primary school, of whom 4.4 attend the respondent's child's own school, 2.8 attend other project schools (Groups 1, 2 or 3), and 1.9 attend nearby "early treatment schools" (Groups 1 and 2 – refer to Table 1, Panels B and C). There is considerable variation across individuals in the number of early treatment links – the standard deviation is 2.0 – and approximately one-third of respondents have no social links to Group 1 or 2 schools at all, one-third have one or two links, and one-third have three or more links. Child social networks show similar patterns (Table 1, Panel D).

5.2 Estimating Social Effects

We test whether households with more social links to early treatment schools were more likely to take deworming drugs in 2001, conditional on the total number of social links they have to all project schools. The experimental design of the PSDP created exogenous random variation in the proportion of individuals whose children attend "early treatment" schools (Groups 1 and 2) versus "late treatment" schools (Group 3). To validate the identification strategy, we confirm that the deworming project randomization succeeded in creating "treatment" and "comparison" groups that are similar along a range of characteristics. The number of social links to early treatment schools, as well as the Group 2 indicator variable and the cost-sharing indicator, are generally not significantly associated with observable characteristics (Table 2), including parent years of education, community group membership (e.g., women's or farmers' groups), the total number of children in the household, and the distance from home to the primary school (although the number of early links is positively and significantly associated with iron roof ownership in one specification – Table 2, regression 4), or with household ethnic group or religious affiliation (results not shown). The number of child social links to early treatment schools is not significantly related to any of these characteristics in Sample IV, the sample of children with both complete parent and child data (results not shown).

The main parent analysis with parent social network data is conducted at the household level using probit estimation, and the outcome measure takes on a value of one if any child in the household received treatment with deworming drugs in 2001, and zero otherwise (though results are similar if the analysis is conducted using the child as the unit of observation – results not shown).[16] T_{ij} is the main dependent variable, the 2001 treatment indicator described above, where *i* is a household in school *j*. The idiosyncratic deworming benefit term, ε_{ij}, captures unobserved variation in parent beliefs about deworming or costs to obtaining treatment (for instance, whether the pupil was sick on the treatment day, which increases the cost of attending school). The individual treatment decision becomes T_{ij} = $1(\lambda N_{ij}^{E} + X_{ij}'\beta + \varepsilon_{ij} > 0)$, where N_{ij}^{E} is the number of parent social links to early treatment schools (not including the respondent's own school), and "early treatment schools" in 2001 are the Group 1 and 2 schools. We also examine the *proportion* of social links to early treatment schools as a robustness check. An analogous analysis is conducted for child social networks, and in some specifications we include both parent and child links as explanatory variables.

Among the explanatory variables, X_{ij}, we include total links to all program schools other than the respondent's own school, as well as the number of links to non-program schools (represented by the vector N_{ij}); given the experimental design of the original deworming program, the number of social links to early treatment schools is randomly assigned conditional on total links to other program schools. The cost-sharing indicator variable, C_j, takes on a value of one for schools participating in the cost-sharing

[16] Treatment choices across children in the same family are highly correlated, as expected, and hence the focus is on the household as the unit of observation when we focus on parent social networks.

project. Z_{ij} is a vector of additional household socioeconomic characteristics (parents' education and asset ownership), demographic characteristics (respondent fertility), and other controls (respondent membership in community groups and a Group 2 indicator) that may affect real or perceived deworming benefits and costs. Idiosyncratic disturbance terms are allowed to be correlated within each school as a result of common influences, such as headmaster efforts in promoting the program. Equation 4 presents the main probit specification:

$$\Pr(T_{ij} = 1) = \Phi\{\lambda_1 N_{ij}^E + N_{ij}'\lambda_2 + \gamma C_j + Z_{ij}'\theta + \varepsilon_{ij}\} \qquad (4)$$

We also include interaction terms between household characteristics and the number of links in some specifications to explore the possibility of heterogeneous treatment effects, for example, for individuals with different levels of education. We discuss additional econometric identification issues in Section 5.4.

5.3 Parent Social Learning Results

Each additional parent social link to an early treatment school is associated with 3.1 percentage points lower likelihood that the respondent's children received medical treatment in 2001, and this effect is significantly different than zero at over 95 percent confidence (Table 3, regression 1 – marginal probit coefficient estimates evaluated at mean values are presented). This suggests that the respondent's relatively small, self-defined social network has a major impact on health choices: having two additional early treatment social links (roughly a one standard deviation increase) reduces take-up by six percentage points, a reduction of ten percent of average take-up. Moreover, this result cannot simply be due to imitation, since the social effects are negative.

Figure 2 graphically presents the non-parametric social effect estimates – using the Fan local regression method – and indicates that the relationship between the number of early treatment links and take-up, conditional on the explanatory variables in regression 1, is negative and somewhat convex, consistent with our learning model. The term (Parent social links to early treatment schools)^2 is also statistically significantly different than zero at 95 percent confidence in some specifications (see Appendix Table A2). However, this quadratic term is not significant for child social links, or for interactions with household characteristics (results not shown). We thus principally focus on the linear measure of early treatment links in what follows.

None of the demographic or socioeconomic controls is significantly associated with 2001 take-up except for distance from home to school, which is negatively related to take-up; this finding makes sense since walking to school to provide written parental consent, and attending school for children, is more costly for geographically distant households.

Social effects are more negative for Group 3 schools (point estimate –0.041, Table 3, regression 2) than for Group 2 schools (point estimate –0.018), although the difference is not statistically significant. This result is consistent with the theoretical model: Group 2 parents have observed the impact of deworming treatment in their own household and community, and should therefore be less influenced than Group 3 parents by early links (i.e., in Equation 1, n is larger for Group 2 parents than Group 3 parents). Nonetheless, the persistent influence of early links on the behavior of Group 2 households after two years of treatment is noteworthy; one potential non-Bayesian explanation is that initial pieces of information about a new technology carry disproportionate weight in subsequent decision-making (Rabin and Schrag 1999).[17] The effect of early links is nearly identical for cost-sharing and non-cost-sharing schools (results not shown).

The results are robust to including the proportion of links with children in early treatment schools, rather than the number of such links (regression 3), and to controlling for the total number of parent social links non-parametrically using a set of indicator variables (results not shown).[18]

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[17] Note that a finding that casts some doubt on the "first impressions matter" explanation, however, is the fact that links to Group 1 schools (phased in during 1998) have nearly identical effects as links to Group 2 schools (phased in during 1999). Note that the persistent effect of early treatment links on take-up could potentially be reconciled with Bayesian learning if individuals believed that there were a school-year specific component to drug treatment effects, thus leading them to place additional weight on outcomes in other schools.

[18] The results are robust to a specification without socioeconomic controls (Appendix Table A2, regression 1), and to the inclusion of additional ethnic and religious controls and indicators for whether the respondent is a member of the dominant local ethnic and religious group (regression 2); none of the six ethnic group indicator variables is significantly related to take-up. The results are similar when the local density of early treatment school pupils (located within 3 km of the respondent's school), and the density of all local primary school pupils, are included as controls (regression 4), but the point estimate on early links falls by about one-third and becomes insignificant, possibly because the local density measures are in part also picking up the effect of interactions with individuals not

Several pieces of evidence suggest that learning takes place not only among those with strong social ties, but also among those with relatively weak ties, consistent with Granovetter (1973). When the framework is extended to include different types of parent social links – "close" friends (with whom the respondent speaks at least twice per week) versus "distant" friends, each additional "close" link to an early treatment school is associated with 0.030 lower probability of deworming treatment in 2001and the estimated effect of "distant" links is similar, although the standard error is larger, so it is not significant (Table 4, regression 1, estimate -0.033, standard error 0.033). We are similarly unable to reject the hypotheses that the social effect on take-up is the same for links to relatives versus friends, or for members of the respondent's own ethnic group versus other ethnic groups, conditional on being named a social link by the respondent (results not shown). One possible explanation for the ethnic results is the fact that over 90 percent of individuals in this area are members of closely related ethnic Luhya groups, and thus have strong linguistic and cultural ties to each other, even across most group boundaries.[19, 20]

Social effects are more strongly negative for respondents with more education (Table 4, regression 2), which is consistent with the prediction that those with the most optimistic priors experience the largest drops in take-up when they receive more information. Other studies – most notably Foster and Rosenzweig (1995) – find that educated individuals learn rapidly about new technologies, although in other cases this has led more educated individuals to be early adopters, unlike our case where they are the first to learn *not* to adopt.

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included in the roster of social links; still an F-test indicates that the early treatment social links and local density of early treatment pupils terms are jointly significant at 99 percent confidence.

[19] The largest Luhya subtribes in this area include the Samia (64 percent of children), Nyala (27 percent), Khayo (1 percent), and Marachi (1 percent); these are considered separate ethnic groups in the above empirical analysis, although it is debatable whether or not subtribes constitute separate "ethnic groups" or not. The two substantial non-Luhya groups in the sample are the Luo (4 percent) and the Teso (1 percent), both non-Bantu speaking groups. On average, 67 percent of named social links outside of their own school belong to the same ethnic group, a figure similar to the average proportion of the largest ethnic in a primary school (68 percent).

[20] Using another definition of strong and weak links yield similar results. While most links were provided in response to questions about the relatives and friends with whom the respondent speaks most frequently, others were provided in response to prompts about social contacts in particular local schools. There is not a statistically significant difference in the effects of these "unprompted" and "prompted" links on take-up (in fact, the prompted links are somewhat more influential – results not shown).

In the theoretical framework presented in Section 4, additional social links have a larger impact on more educated individuals because of their overly optimistic prior beliefs about the drugs, rather than due to greater receptiveness to new information *per se*. Although we are unable to decisively distinguish these two explanations empirically, the relationship between respondents' education and their stated belief that deworming drugs are "very effective" does provide some further suggestive evidence in support of the model in Section 4. Among Group 3 parents interviewed in Round 1, before deworming treatment was phased into their schools, there is a particularly strong link between education and positive views: each additional year of schooling is associated with an increase of 2.3 percentage points in the likelihood the respondent believes the drugs are "very effective". However, this falls to just 1.9 percentage points per additional year of schooling for those Group 3 parents interviewed in Round 2 (recall parents were randomly allocated between survey rounds), several months after deworming had been introduced into their schools. Finally, the education gradient falls to 1.8 among Group 2 parents in 2001, two years after their schools had begun receiving treatment – by which time views toward the drugs had partially converged across educational levels, as predicted by the model in Section 4.

The impact of early treatment social links on take-up is particularly negative for those parents with a teenage child (Table 4, regression 3) – the first piece of suggestive evidence that child networks may also be influential in determining treatment through the school-based deworming program. Additional evidence comes from the fact that an additional parent early treatment link is associated with a moderate drop of only -0.010 (standard error 0.013 – results not shown) in the provision of signed parental consent, the most direct means for parents to affect their children's treatment. We elaborate on the issue of parent versus child social effects in Section 5.7 below.

5.4 Further Econometric Identification Issues

We discuss some remaining econometric issues in this sub-section. Even though the randomizations were largely successful in creating comparable groups, social links to early treatment schools could also potentially affect adoption through the infection social effects described in Section 4.1. However, we argue that these infection effects are too small to explain even a small fraction of our overall estimated social effects. We find that having additional social links to early treatment schools is in fact associated with somewhat lower rates of moderate-heavy helminth infection, as expected (Table 2, regression 6), but the effect is relatively small and not statistically significant (coefficient estimate –0.012, standard error 0.017, relative to a mean moderate-heavy infection rate of 0.27). Note that the relatively weak relationship between early treatment social links and child infection status does not contradict the strong infection externality findings in Miguel and Kremer (2003): worm infections are not transmitted directly person to person, but rather through contaminated soil and water, and a child's named social links constitute only a small fraction of all people who defecate in fields near the child's home, school, market, and church, or who bathe at the same points on Lake Victoria.

These infection effects are driven entirely by early treatment links with whom the respondent has a "close" social relationship, speaking at least twice per week (Table 2, regression 7, coefficient estimate –0.018), while additional early links with whom the respondent speaks less frequently do not affect infection status (estimate 0.004). Thus "distant" social links appear to have a sizeable negative impact on adoption decisions (Table 4, regression 1), but have no effect on infection status – further bolstering the claim that information rather than infection externalities are driving the overall social effect.

In terms of the second step – from infection status to treatment decisions – we found that prior infection status is not significantly associated with drug treatment for either Group 1 in 1998 or Group 2 in 1999 (refer to Miguel and Kremer 2003), or for Groups 2 and 3 in 2001 (results not shown) and the point estimates suggest that moderate-to-heavy worm infection is actually somewhat *negatively* related to treatment rates.[21] Of course, the cross-sectional correlation between infection and treatment cannot be interpreted as causal due to omitted variable bias: children from unobservably low socio-economic status households may both have high infection rates and low take-up, for example. However, the treated and

[21] The 2001 worm infection results are for a subsample of only 575 children who were randomly sampled for stool collection, and were present in school on the day of the parasitological survey. Due to the relatively small sample size, we do not focus on the parasitological data in the main empirical analysis.

untreated look remarkably similar along many observable baseline socioeconomic and health characteristics (Miguel and Kremer 2003), weakening the case for strong selection effects into treatment.

Evidence on the effect of changes in infection status on drug take-up is provided by the 1999 cross-school infection externality estimates, which are identified using exogenous variation in the local density of early treatment schools: although we find large average reductions in moderate-heavy worm infection rates as a result of cross-school externalities (23 percentage points, Appendix Table A3), proximity to early treatment schools leads to an average reduction in drug take-up of only 2 percentage points – which has the expected sign but is near zero and statistically insignificant (standard error 3 percentage points, results not shown). Taken together, a variety of evidence thus indicates that child infection levels affect drug take-up only weakly, if at all.

Moreover, the relationship between infection and treatment would have to be implausibly large and positive for changes in health status to explain anything more than a tiny fraction of the overall social effect: for example, if eliminating a moderate-heavy infection reduced the likelihood of drug take-up by a massive fifty percentage points on average (rather than the two percentage points we estimate), health externalities could only account for a (0.5)*(0.012) = 0.006 reduction in take-up, less than one-fifth of the overall effect (Table 3, regression 1).

Pupil transfers among local primary schools during the course of the study are another potential identification concern. However this would likely work against our findings. For example, parents with more health-conscious social contacts – whose children may have been more likely to transfer to early treatment schools to receive deworming – may themselves also be more health-conscious and eager to have their own children receive treatment, biasing the estimated social effect upward. In any case, the rate of pupil transfers between treatment and comparison schools was low and nearly symmetric in both directions, suggesting that the transfer bias is likely to be small (Miguel and Kremer 2003).

A related identification issue concerns whether social networks measured in 2001 – three years after the program started – were themselves affected by the program. Any extent to which health-conscious individuals tended to become socially "closer" to individuals with children in early treatment schools would again lead to an upward bias, working against our findings. However, there is not evidence that this is likely: respondents were statistically no more likely to name early treatment links than links to other schools.[22]

5.5 Non-experimental Social Effect Estimates

Non-experimental social effect estimates are markedly different from the negative experimental estimates, suggesting that omitted variable bias in this context is large and positive. In a specification similar to many existing non-experimental studies, we examine the treatment rate of children in a pre-defined local social unit – here the primary school – as the key explanatory variable. We find that the local school treatment rate (excluding the respondent) is strongly positively correlated with take-up, with coefficient estimate 0.84 (standard error 0.11 – Table 5, regression 1). Take-up among children who are members of the respondent's own ethnic group in their school is more influential than take-up in other ethnic groups (p-value=0.09, regression not shown), as in Munshi and Myaux (2000) – although in our case we argue that this pattern is likely due to omitted variable bias, rather to actual social learning, as they claim. There is similarly a positive, though statistically insignificant (estimate 0.015, standard error 0.011) relationship between the number of treated links named in the survey (among those attending the respondent's school) and take-up (regression 2).

Social links' experiences with deworming may also affect the information individuals receive; in particular, we test whether take-up is higher when links had "good" experiences with deworming.[23] This non-experimental analysis may suffer from another form of omitted variable bias: individuals who themselves have more positive views toward treatment may also be more likely to report that their links had good effects, even if the links' experiences were not particularly good. There is suggestive evidence that having more links whose children had "good effects" is associated with higher take-up, while those

[22] The average number of links to early treatment schools is 1.92, while (Total number of links to project schools) * (Total # Group 1 and 2 pupils / Total # Group 1, 2 and Group 3 pupils) =1.91.

[23] One drawback of our dataset is that we only have information on social links' deworming choices provided by the respondent herself (unlike Conley and Udry 2000, we did not collect the full names of social links for privacy reasons and are thus unable to match the social links to the main database).

with more links who had "side effects" are less likely to be treated (regression 3) – the p-value on the hypothesis that the two estimates are equal is 0.22 – but this result is ultimately inconclusive.[24]

5.6 Parent Attitudes and Knowledge

Respondents with more early links are significantly more likely to claim that deworming drugs are "not effective" (Table 6, row 1). This is consistent with the hypothesis that some people thought deworming would provide long-term protection against worms, but learned otherwise from their contacts at early treatment schools. We do not find a significant impact of additional early links on the belief that deworming drugs are "very effective", although the point estimate is negative (row 2), nor that the drugs have "side effects" (row 3); this second result is evidence against the possibility that take-up was negatively affected by rumors about deworming drug side effects.

Although early treatment links do affect the belief that deworming drugs are "not effective", they do not affect beliefs that "worms and schistosomiasis are very bad for child health" (Table 6, row 4) – although of course, it is possible that parents simply say what they believe that the enumerator wants to hear regarding the health consequences of worms, as suggested by Geissler (1998a): 92 percent of the respondents claimed that helminth infections are "very bad" for child health. The number of early treatment links has no effect on parents' self-reported claim to "know about the ICS deworming program" (row 5), to "know about the effects of worms and schistosomiasis" (row 6), to know the deworming treatment status of their own child (not shown), nor on their objective knowledge of common worm infection symptoms (rows 7-10). Most respondents lack even basic knowledge about worm infections, and they were only able to name 1.8 symptoms on average. Thus not only did a substantial expenditure in

[24] The deworming experiences and choices of people in social links' communities may affect respondent take-up (Munshi 2000). For each early treatment school, we computed the average difference in school attendance between treated and untreated pupils in 1999, and used this to classify schools into "large treated minus untreated difference" schools (those above the median difference) and "small treated minus untreated difference" schools. The treated minus untreated difference measures the average observed private benefit to deworming in that school. However, we find no evidence that the experiences of children in links' schools affect take-up decisions: links to early treatment schools with low take-up do have a somewhat more negative effect on respondent treatment rates than links to schools with high take-up, but the difference between these estimates is not significant (regression not shown). Similarly, there is no statistically significant difference between the effect of links to early treatment

worm prevention health education through the original deworming program not affect recipients' behavior directly (as discussed in Kremer and Miguel 2003), but these messages also failed to spread to other community members. However, note that parent education is strongly positively correlated with nearly all of these knowledge measures (results not shown).

In contrast, the actual number of treated social links, and the number of social links with whom the respondent speaks about deworming, are both positively and significantly related to most deworming attitudes and knowledge outcomes – once again highlighting important differences between experimental and non-experimental estimates (Table 6). It appears that individuals with unobservably more interest in child health more frequently discuss worms with their social links, who are themselves more likely to have their own children receive treatment. In short, the positive observed correlation in outcomes within social networks in this context appears to be due to omitted variables rather than social effects..

5.7 Child Social Learning Results

We next turn to child social networks. As expected, the proportion of pupil links to early treatment schools is highly correlated with the proportion of parent links (correlation coefficient 0.5). We find that child early treatment links are also negatively and marginally significantly related to drug take-up in the full sample of 3164 children (point estimate -0.018, standard error 0.010 – Table 7, regression 1), and this relationship is similar for children in Group 2 and 3 schools (regression 2). The result is robust to the inclusion of the proportion of links in early treatment schools rather than the number (regression 3). Although there are no statistically significant effects of early links on any measure of child deworming attitudes, beliefs, or knowledge, additional early treatment links have a marginal positive impact on the belief that deworming drugs have side effects (estimate 0.006, standard error 0.005) and on the number of infection symptoms the child was able to name (estimate 0.010, standard error 0.007 – not shown).[25]

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schools where the difference in school participation between treated and untreated pupils was large (and thus the observed private treatment benefit large) versus those where this difference was small (results not shown).

[25] The attitudes and knowledge results are similar for teenagers and pre-teens (results not shown).

The child social effects links differ sharply by the age of the child: for teenagers (at least 13 years of age), each additional early treatment social link is associated with a drop in take-up of -0.028 percentage points (standard error 0.012 – Table 8, regression 1), while the effect for younger children is small and statistically insignificant (point estimate -0.006, standard error 0.014 – regression 3). Thus for teenagers the point estimate on additional early treatment social links is nearly as large as the point estimate on parent links (for instance, in Table 3, regression 1). There is no significant difference in the impact of additional links by respondent gender, or for children's relatives versus friends (not shown).

Estimated child social links (-0.022) are more than twice as influential as parent links (-0.009) in reducing take-up for teenagers (Table 8, regression 2), although we cannot reject the hypothesis that they are equal; unfortunately, including both child and parent links as explanatory variables greatly reduces the sample, limiting statistical precision. Still, this again suggests that teenagers play a central role in their own health care choices, a point made forcefully by Geissler (2000) in his study of a nearby district in Kenya and by advocates of "child-to-child" public health approaches (Hubley 1993), and raises the interesting possibility of non-monotonic peer effects by age, peaking for teenagers. The 2001 survey provides further suggestive evidence that health information is likely to flow primarily through child networks rather than parent networks in this setting: children claimed to know the deworming treatment status of 62 percent of their social links, twice as high as the proportion known by parents (32 percent).[26] Nine percent of parents did not even know the treatment status of their own children.

6. Implications of Social Network Structure for Program Design

The design of interventions should depend not only on the relative influence of different types of social links, but also on the structure of individual social networks. Although for the particular technology we examine, priors are often too optimistic and the technology creates positive externalities, so that faster social learning is not socially beneficial, in many other cases faster learning is likely to be beneficial and

[26] Note that teens and pre-teens had similar levels of knowledge about their links' treatment status (not shown).

it may be desirable to structure interventions to speed learning. For example, it may make sense to introduce new technologies first among people with more extensive social networks to facilitate spread.

Among children, we find that teenagers have significantly broader social networks – in terms of named social contacts outside of the child's own school – than younger children, with an average of 0.69 (standard error 0.18) additional external links (mean external child links is 3.8, Table 1 Panel D); this estimate is derived from an OLS regression and is conditional on other child characteristics (regression not shown). Thus targeting public health or other messages to teenagers is likely to lead to more rapid local information diffusion than focusing on younger children for at least two reasons: first, social effects are particularly strong among teenagers (as shown in Table 8), and second, due to their more extensive social contacts in other schools, teenagers are more likely to spread this new information to other areas. This project's unique combination of experimental social effect estimates and detailed survey information on observed social network structure will allow us to numerically simulate the effects of different public health program designs – for instance, targeting messages to teens versus pre-teens – in future research, hopefully providing useful information for policymakers.

Parents who are members of community groups – typically in western Kenya, women's farming and credit groups – have significantly more social links external to their child's school than other parents, with 0.70 (standard error 0.19) additional links on average (mean external parent links is 5.8, Table 1 Panel B). This potentially provides a justification for using community groups as focal points for the introduction of new technologies, as is already often done in practice. To the extent that the existence of community groups promotes more cross-school social contact[27], this finding also suggests that subsidizing the creation of additional community groups could speed the diffusion of new technologies, thus providing an additional rationale for promoting "social capital".

More educated parents have more external social contacts, 0.13 links per additional year of education (standard error 0.02). To the extent this relationship is causal, and symmetric (more educated individuals have more contact with other educated people), it suggests that subsidizing education may lead to faster diffusion of technology, even if the educated are no more receptive to new information than others. Measures of household asset ownership, as well as parent ethnic and religious characteristics, are generally not statistically significantly associated with additional external links (results not shown), though one exception is household latrine ownership, which is marginally statistically significant (0.40 additional links, standard error 0.22). Note that child gender, ethnic and religious characteristics are not significantly associated with the number of named contacts external to their school (not shown).

Within the set of rural schools in our sample, most of the variation in the number of external social contacts is at the individual rather than school level: school-level variation accounts for less than 10 percent of the total variation in external social contacts, suggesting that strategies to maximize information diffusion should target individuals with certain characteristics – for instance, teenagers or parents who are community group members or more educated – rather than particular types of schools.

7. Conclusion

We find evidence for social learning in the adoption of a new health technology – deworming drugs – in rural Kenya: individuals with exogenously more social links in program schools were significantly *less* likely to believe deworming drugs are effective and less likely to take them. Our finding of negative social effects implies actual learning took place, rather than simple imitation. We also find striking differences between experimental and non-experimental estimates, suggesting that the results of many existing non-experimental studies should be interpreted with caution due to the possibility of considerable omitted variable bias. The social networks of teenagers appear particularly influential at affecting take-up. We also find that teenagers, as well as educated parents and parents who are members of community groups, have especially large numbers of social contacts outside their own primary school, making them natural populations for public programs to target in order to promote rapid social learning.

[27] In other words, that measured community group membership is not simply proxying for some unobserved individual characteristic related to personal "sociability", which could lead to a spurious correlation between community group membership and the number of social links external to one's school.

While the learning framework we develop above (Section 4) is consistent with these main empirical results, some mystery remains as to why people are still not more likely to take the drugs than we find, given that more than 90 percent of children have worm infections of some sort and treatment is inexpensive. One possible non-Bayesian explanation is that people simply did not recognize the benefits of deworming: recall that in the traditional view in this area, worms are an integral part of the human body and necessary for digestion. To the extent that many people in this area believed that worms were not a serious health problem at the start of the program, and there are psychological fixed costs to "re-categorization" along the lines of Mullainathan (2002) – in other words, to begin believing that worms are a serious problem – the modest observed private deworming benefits might not have been large enough to justify shifting beliefs, further dampening take-up.

The existence of frequent health shocks from many sources (e.g., malaria, typhoid, cholera) in this area also complicates the signal extraction problem regarding the benefits of a new health treatment, especially given the timing of deworming costs and benefits: deworming entails immediate costs (i.e., the effort needed to obtain treatment and possible drug side effects) while benefits emerge only gradually as individual nutritional status improves in the months after treatment – potentially obscuring health gains and exacerbating commitment problems associated with time inconsistent preferences. The immediate drug side effects may be particularly salient for children. In contrast, social learning could conceivably lead to higher take-up for treatments for diseases like malaria, for which the negative health impacts are more acute. A final factor is that the individuals (parents) who provide consent for treatment, and pay for treatment in cost-sharing schools, are not the same people who directly benefit from it (their children); this may further reduce drug take-up to the extent that there is imperfect altruism and inefficient bargaining in the parent-child relationship.[28]

Our empirical finding of negative social learning, together with the results of a related project (Kremer and Miguel 2003), suggest that socially desirable health technologies may not diffuse on their

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[28] Udry (1996) presents evidence on inefficient within-household resource allocation in another rural African setting. Note that orphan status is not related to take-up in our setting (not shown).

own. Indeed, indefinite subsidies may be needed to sustain high drug take-up for diseases characterized by positive treatment externalities, like deworming – a finding especially important for Africa, where half the disease burden is associated with infectious and parasitic diseases (WHO 1999).

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9. Tables and Figures

Figure 1: Expected Benefits of Adoption and Early Treatment Links (N_{ij}^E) for $\phi_0 > \phi$

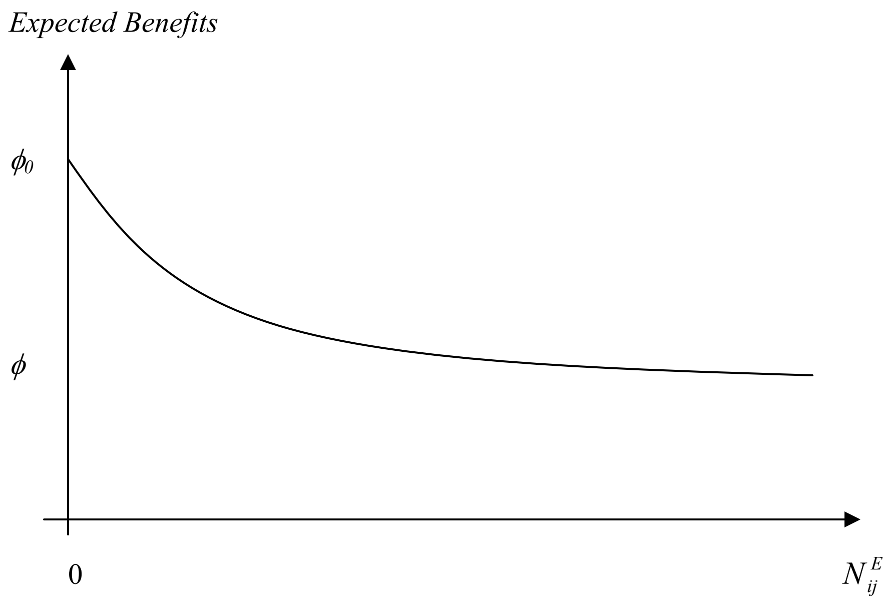

Figure 2: Non-parametric Fan regression (Epanechnikov kernel):
Effect of social links to early treatment schools (Group 1,2, not own school)
on 2001 Deworming Drug Take-up (residuals conditional on other covariates)

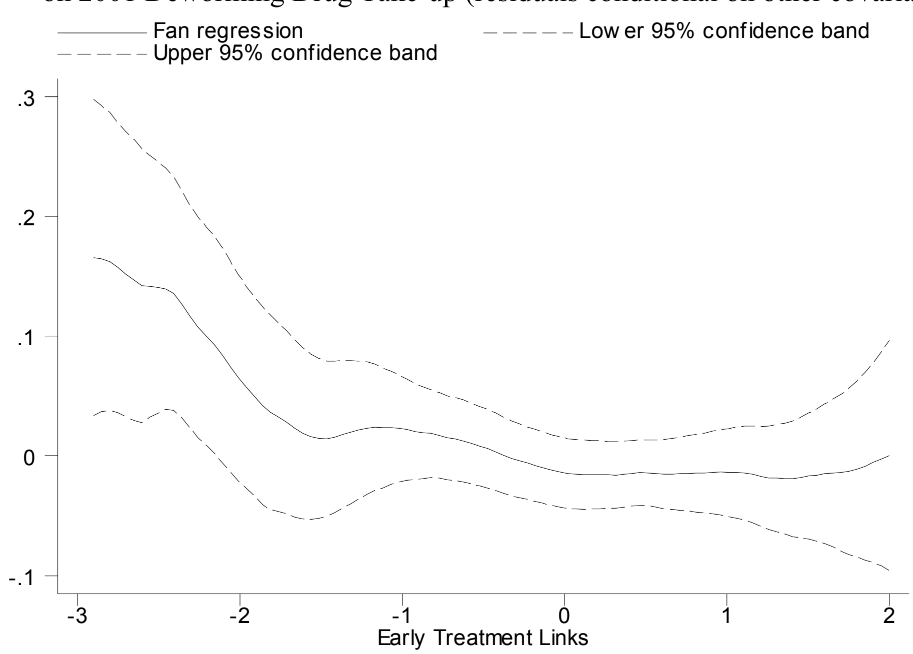

Table 1: Descriptive Statistics

	Mean	Std dev.	Obs.
Panel A: Deworming Treatment Take-up			
Took deworming drugs in 2001 (Group 2 and 3)	0.61	0.49	1678
Proportion deworming drug take-up in 2001, respondent's own school	0.61	0.28	1678
Took deworming drugs in 2001, free treatment schools	0.75	043	1251
Took deworming drugs in 2001, cost-sharing schools	0.19	0.39	427
Provided parental consent for deworming drugs in 2001	0.76	043	1678
Panel B: Parent Social Links (Round 1 and Round 2 Data)			
Total	10.2	3.4	1678
With children in own school	4.4	2.8	1678
With children not in Group 1, 2, or 3 schools	3.0	2.4	1678
With children in Group 1, 2, 3 schools – not own school	2.8	2.4	1678
With children in Group 1, 2 schools – not own school ("early treatment")	1.9	2.0	1678
With children in Group 1 schools – not own school	0.9	1.4	1678
Proportion with children in early treatment schools	0.66	0.37	1358
With children in early treatment schools, with whom respondent speaks at least twice per week ("Close Links")	1.2	1.6	1678
With children in early treatment schools, with whom respondent speaks less than twice per week ("Distant Links")	0.7	1.1	1678
With children in early treatment schools, high deworming take-up 1999	0.95	1.41	1678
With children in early treatment schools, low deworming take-up 1999	0.96	1.47	1678
With children in early treatment schools, large difference in school participation between treated and untreated 1999	0.99	1.47	1678
With children in early treatment schools, small difference in school participation treated and untreated 1999	0.90	1.36	1678
Panel C: Parent Social Links (Round 2 Data)			
With children in own school who received deworming	1.5	2.2	886
With children in early treatment schools who received deworming	0.39	1.02	886
With children in early treatment schools who received deworming and had "good effects" (according to respondent)	0.26	0.90	886
With children in early treatment schools who received deworming and had "side effects" (according to respondent)	0.02	0.20	886
With children in early treatment schools who received deworming, respondent does not know effects	0.09	0.39	886
With children in early treatment schools, respondent does not know whether they received deworming	1.89	2.06	886
With children in early treatment schools who did not receive deworming	0.16	0.57	886
Panel D: Child Social Links (Round 1 and Round 2 Data)			
Total	10.8	3.1	3164
Not in Group 1, 2, or 3 schools	0.6	1.1	3164
In Group 1, 2, 3 schools – not own school	3.2	2.5	3164
In Group 1, 2 schools – not own school ("early treatment")	2.1	2.0	3164
Proportion in early treatment schools	0.65	0.37	2673
Early treatment links, among teenagers (age $\geq$ 13 years)	2.2	2.1	1653
Early treatment links, among non-teenagers (age < 13 years)	1.9	2.0	1511

Notes for Table 1: From 2001 Parent and 2001 Pupil Questionnaires, and NGO administrative records. The "Proportion in early treatment schools" variables exclude respondents with no links to program schools (other than their own), hence the reduced sample.

Table 2: Validating the randomization (Group 2 and Group 3 households) – Parent Survey Data

	Dependent variable:						
	Respondent years of education	Community group member	Total number of children	Iron roof at home	Distance home to school (km)	Moderate-heavy infection, 2001	
	OLS	Probit	OLS	Probit	OLS	Probit	Probit
Explanatory variables:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Parent links with children in early treatment schools (Group 1, 2, not own school)	0.07 (0.08)	-0.008 (0.012)	-0.01 (0.06)	0.025** (0.012)	-0.18 (0.12)	-0.012 (0.018)	
# Parent links with children in early treatment schools, with whom respondent speaks **at least twice/week**							-0.018 (0.023)
# Parent links with children in early treatment schools, with whom respondent speaks **less than twice/week**							0.004 (0.021)
# Parent links with children in Group 1, 2, or 3 schools, not own school	0.11 (0.07)	0.013 (0.011)	-0.029 (0.054)	-0.009 (0.012)	0.14 (0.11)	-0.001 (0.015)	-0.002 (0.014)
# Parent links with children not in Group 1, 2, or 3 schools	0.07** (0.03)	0.018*** (0.006)	-0.043 (0.030)	0.009 (0.006)	0.02 (0.03)	-0.010 (0.009)	-0.009 (0.009)
# Parent links, total	0.13*** (0.04)	0.007 (0.006)	0.059** (0.028)	-0.011* (0.006)	-0.01 (0.03)	0.003 (0.008)	0.003 (0.007)
Cost-sharing school indicator	0.12 (0.30)	0.00 (0.04)	0.07 (0.23)	0.02 (0.06)	1.3 (0.9)	0.07 (0.11)	0.07 (0.11)
Group 2 school indicator	-0.46 (0.29)	-0.03 (0.04)	0.13 (0.19)	0.01 (0.05)	-0.03 (0.28)	-0.22*** (0.07)	-0.22*** (0.07)
Socio-economic controls (excluding dependent var.)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.06	-	0.01	-	0.10	-	-
Root MSE	3.8	-	2.3	-	1.9	-	-
Number of observations (parents)	1678	1678	1678	1678	1678	575	575
Mean (s.d.) of dependent variable	4.6 (3.9)	0.58 (0.49)	5.5 (2.3)	0.61 (0.49)	1.7 (2.0)	0.27 (0.45)	

Notes for Table 2: Data from 2001 Parent Survey, 2001 Parasitological Survey, and 2001 administrative records. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The socioeconomic controls include Respondent years of education, Community group member, Total number of children, Iron roof at home, and Distance from home to school (but when any of these is the dependent variable, it is not included as an explanatory variable).

Table 3: Experimental Social Effect Estimates (Groups 2 and 3) – Parent Networks

	Dependent variable: Child took deworming drugs in 2001		
	Probit	Probit	Probit
Explanatory variables:	(1)	(2)	(3)
# Parent links with children in early treatment schools (Groups 1 and 2, not own school)	-0.031**	-0.041**	
	(0.014)	(0.017)	
# Parent links with children in early treatment schools *** Group 2 school indicator**		0.018	
		(0.029)	
Proportion parent links with children in early treatment schools			-0.098**
			(0.045)
# Parent links with children in Group 1, 2, or 3 schools, not own school	0.013	0.013	-0.006
	(0.011)	(0.017)	(0.009)
# Parent links with children not in Group 1, 2, or 3 schools	-0.008	-0.008	-0.005
	(0.007)	(0.009)	(0.007)
# Parent links, total	0.019***	0.029***	0.021***
	(0.005)	(0.007)	(0.007)
Respondent years of education	0.003	0.003	0.002
	(0.003)	(0.003)	(0.004)
Community group member	0.029	0.033	0.038
	(0.025)	(0.026)	(0.029)
Total number of children	0.005	0.006	0.005
	(0.006)	(0.006)	(0.007)
Iron roof at home	0.010	0.007	0.010
	(0.027)	(0.027)	(0.032)
Distance home to school (km)	-0.018**	-0.018**	-0.014
	(0.009)	(0.009)	(0.010)
Group 2 school indicator	0.01	0.20**	0.01
	(0.05)	(0.09)	(0.05)
Cost-sharing school indicator	-0.62***	-0.62***	-0.62***
	(0.08)	(0.08)	(0.09)
Number of observations (parents)	1678	1678	1358
Mean of dependent variable	0.61	0.61	0.61

Notes for Table 3: Data from 2001 Parent Survey, and 2001 administrative records. Marginal probit coefficient estimates are presented. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Regression 2 also includes interaction terms (# Parent social links with children in Group 1, 2, or 3 schools, not own school)*(Group 2), (# Parent social links with children not in Group 1, 2, or 3 schools)*(Group 2), and (# Parent social links, total)*(Group 2). Regression 3 excludes parents for which (# Parent social links with children in Group 1, 2, or 3 schools, not own school) = 0, since the proportion of links is undefined in that case, leading to the reduction in sample size.

Table 4: The Effect of Different Types of Social Links – Parent Networks

Explanatory variables:	Dependent variable: Child took deworming drugs in 2001 Probit (1)	Probit (2)	Probit (3)
# Parent links with children in early treatment schools, with whom respondent speaks **at least twice/week**	-0.030* (0.016)		
# Parent links with children in early treatment schools, with whom respondent speaks **less than twice/week**	-0.033 (0.033)		
# Parent links with children in early treatment schools (Groups 1 and 2, not own school)		-0.002 (0.018)	0.006 (0.019)
# Parent links with children in early treatment schools * **Respondent years of education**		-0.0062* (0.0032)	
Respondent years of education		-0.016 (0.012)	
# Parent links with children in early treatment schools * **Respondent's child is a teenager**			-0.052*** (0.021)
Respondent's child is a teenager			-0.073 (0.102)
Social links, other controls	Yes	Yes	Yes
Number of observations (parents)	1678	1678	1678
Mean of dependent variable	0.61	0.61	0.61

Notes for Table 4: Data from 2001 Parent Survey, and 2001 administrative records. Marginal probit coefficient estimates are presented. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Social links controls include total number of parent links, number of parent links to Group 1, 2, 3 schools (not own school), and number of parent links to non-program schools. Other controls include respondent years of education, community group member indicator variable, total number of children, iron roof at home indicator variable, and distance from home to school in km, as well as the Group 2 indicator and Cost-sharing school indicator.

Regression 1 includes controls separately for the number of close links and distant links to Group 1, 2, or 3 schools (not own school). Regression 2 also includes interaction terms (# Parent social links with children in Group 1, 2, or 3 schools, not own school)*(Respondent years of education) and (# Parent social links with children not in Group 1, 2, or 3 schools)*(Respondent years of education); similarly, in Regression 3 there are analogous interaction terms with Child is a teenager.

Table 5: Non-Experimental Social Effect Estimates (Groups 2 and 3) – Parent Networks

Explanatory variables:	Dependent variable: Child took deworming drugs in 2001 Probit (1)	Probit (2)	Probit (3)
Proportion deworming drug take-up in 2001, respondent's own school (not including respondent)	0.84*** (0.11)		
# Parent links with children in respondent's own school whose children **received deworming**		0.015 (0.011)	
# Parent links with children in early treatment schools whose children **received deworming and had "good effects"**			0.004 (0.021)
# Parent links with children in early treatment schools whose children **received deworming and had "side effects"**			-0.112 (0.082)
# Parent links with children in early treatment schools whose children **received deworming and respondent does not know effects**			0.006 (0.046)
# Parent links with children in early treatment schools whose children **did not receive deworming**			-0.007 (0.028)
# Parent links in with children in early treatment schools, **respondent does not know whether they received deworming**			-0.013 (0.016)
Social links, other controls	Yes	Yes	Yes
Number of observations (parents)	1678	886	886
Mean of dependent variable	0.61	0.56	0.56

Notes for Table 5: Data from 2001 Parent Survey, 2001 Parasitological Surveys, and 2001 administrative records. Marginal probit coefficient estimates are presented. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Social links controls include total number of parent links, number of parent links to Group 1, 2, 3 schools (not own school), and number of links parent to non-program schools. Other controls include respondent years of education, community group member indicator variable, total number of children, iron roof at home indicator variable, and distance from home to school in km, as well as the Group 2 indicator and Cost-sharing school indicator.

Regression 1 presents results from Round 1 and Round 2 of the 2001 Parent Survey, and regressions 2-3 present results from Round 2 alone, since only Round 2 has detailed information on social links' deworming treatment impacts. In regression 3, we cannot reject that the coefficient estimates on (# Links with children in early treatment schools whose children received deworming and had good effects) and on (# Links with children in early treatment schools whose children received deworming and had side effects) are equal (p-value=0.22).

Table 6: Effects on Deworming Attitudes and Knowledge – Parent Networks

Dependent variable:	Estimate on # Parent links with children in early treatment schools [Experimental]	Estimate on # Parent links with children in early treatment schools **whose children received deworming** [Non-experimental]	Estimate on # Parent links with children in early treatment schools **with whom respondent spoke about deworming** [Non-experimental]	Mean dep. var.
Panel A: Attitudes				
1) Parent thinks deworming drugs "not effective"	0.017** (0.007)	0.013 (0.008)	0.009** (0.004)	0.12
2) Parent thinks deworming drugs "very effective"	-0.007 (0.010)	0.026** (0.013)	0.040*** (0.007)	0.43
3) Parent thinks deworming drugs have "side effects"	0.000 (0.003)	-0.001 (0.003)	0.003* (0.002)	0.04
4) Parent thinks worms and schisto. "very bad" for child health	-0.001 (0.006)	-0.004 (0.006)	-0.006* (0.003)	0.92
Panel B: Knowledge				
5) Parent "knows about ICS deworming program"	0.004 (0.011)	0.050*** (0.014)	0.055*** (0.011)	0.70
6) Parent "knows about the effects of worms and schistosomiasis"	-0.001 (0.013)	0.045*** (0.013)	0.039*** (0.009)	0.68
7) Number of infection symptoms parents able to name (0-10)	-0.006 (0.005)	0.018** (0.008)	0.010** (0.005)	1.8
8) Parent able to name "fatigue" as symptom of infection	-0.004 (0.010)	0.028*** (0.009)	0.021*** (0.006)	0.20
9) Parent able to name "anemia" as symptom of infection	0.005 (0.009)	-0.003 (0.011)	0.010** (0.005)	0.22
10) Parent able to name "weight loss" as symptom of infection	0.002 (0.006)	0.004 (0.005)	-0.001 (0.004)	0.06

Notes for Table 6: Data from 2001 Parent Survey, and 2001 administrative records. Marginal probit coefficient estimates are presented, and each entry is the result of a separate regression. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The ten possible infection symptoms include fatigue, anemia, weight loss, stunted growth, stomach ache, bloated stomach, blood in stool, worms in stool, diarrhea, and fever. Social links controls and other controls are included in all specifications. Social links controls include total number of parent links, number of parent links to Group 1, 2, 3 schools (not own school), and number of parent links to non-program schools. Other controls include respondent years of education, community group member indicator variable, total number of children, iron roof at home indicator variable, and distance from home to school in km, as well as the Group 2 indicator and Cost-sharing school indicator. The number of observations (parents) across regressions ranges from 1656 to 1678 depending on the extent of missing survey data for the dependent variable.

Table 7: Experimental Social Effect Estimates (Groups 2 and 3) – Child Networks

	Dependent variable: Child took deworming drugs in 2001		
	Probit	Probit	Probit
Explanatory variables:	(1)	(2)	(3)
# Child links in early treatment schools (Groups 1, 2, not own school)	-0.018	-0.016	
	(0.011)	(0.013)	
# Child links with children in early treatment schools *** Group 2 school indicator**		-0.002	
		(0.023)	
Proportion child links in early treatment schools			-0.073*
			(0.044)
# Child links in Group 1, 2, or 3 schools, not own school	-0.014	-0.009	-0.029***
	(0.009)	(0.013)	(0.008)
# Child links not in Group 1, 2, or 3 schools	-0.021*	-0.033*	-0.020*
	(0.012)	(0.018)	(0.013)
# Child links, total	0.030***	0.028***	0.030***
	(0.006)	(0.007)	(0.007)
Control variables	Yes	Yes	Yes
Number of observations (children)	3164	3164	2673
Mean of dependent variable	0.54	0.54	0.55

Notes for Table 7: Data from 2001 Pupil Survey, and 2001 administrative records. Marginal probit coefficient estimates are presented. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The control variables include: pupil age; pupil sex; the cost-sharing school indicator; Group 2 school indicator; and the pupil verbal commitment intervention indicator (this last term is discussed in Kremer and Miguel 2003). Regression 2 also includes interaction terms (# Child social links with children in Group 1, 2, or 3 schools, not own school)*(Group 2), (# Child social links with children not in Group 1, 2, or 3 schools)*(Group 2), and (# Child social links, total)*(Group 2). Regression 3 excludes children for which (# Child social links in Group 1, 2, or 3 schools, not own school) = 0, since the proportion of links to treatment schools is undefined in that case, leading to the reduction in sample size.

Table 8: Social Effect Estimates by Child Age (Groups 2 and 3) – Child and Parent Networks

	Dependent variable: Child took deworming drugs in 2001			
	Children ≥ 13 years old		Children < 13 years old	
	Probit	Probit	Probit	Probit
Explanatory variables:	(1)	(2)	(3)	(4)
# Child links in early treatment schools (Groups 1, 2, not own school)	-0.028** (0.012)	-0.022 (0.020)	-0.006 (0.014)	-0.006 (0.018)
# Parent links with children in early treatment schools (Groups 1, 2, not own school)		-0.009 (0.026)		0.010 (0.027)
Control variables	Yes	Yes	Yes	Yes
Child social links controls	Yes	Yes	Yes	Yes
Parent social links controls	No	Yes	No	Yes
Number of observations (children)	1653	651	1511	580
Mean of dependent variable	0.54	0.64	0.54	0.66

Notes for Table 8: Data from 2001 Pupil Survey, and 2001 administrative records. Marginal probit coefficient estimates are presented. Robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. The control variables include: pupil age; pupil sex; the cost-sharing school indicator; Group 2 school indicator; and the pupil verbal commitment intervention indicator (this last term is discussed in Kremer and Miguel 2003). Child social links controls include total number of child links, number of child links to Group 1, 2, 3 schools (not own school), and number of child links to non-program schools. Parent social links controls include total number of parent links, number of parent links to Group 1, 2, 3 schools (not own school), and number of parent links to non-program schools. Note that we cannot reject the hypothesis that the effect of early treatment links on take-up is the same for Children ≥ Age 13 and for Children < Age 13 (p-value=0.19) – regression not shown.

Appendix

Appendix Table A1: Primary School Deworming Project (PSDP) timeline, 1998-2001

Dates:	Activity:
1998	
January	75 Primary schools first stratified by geographic zone and previous NGO projects, and then randomly divided into three groups of 25 schools (Groups 1, 2, 3)
March-April	First round of 1998 treatment (albendazole, praziquantel) in Group 1 schools
November	Second round of 1998 treatment (albendazole) in Group 1 schools
1999	
March-June	First round of 1999 treatment (albendazole, praziquantel) in Groups 1, 2 schools
October-November	Second round of 1999 treatment (albendazole) in Groups 1, 2 schools
2000	
March-June	First round of 2000 treatment (albendazole, praziquantel) in Groups 1, 2 schools
October-November	Second round of 2000 treatment (albendazole) in Groups 1, 2 schools
2001	
January-March	2001 Parent Survey (Round 1) data collection in Groups 2, 3 schools. 2001 Pupil Survey (Round 1) data collection in Groups 2, 3 schools
March-June	First round of 2001 treatment (albendazole, praziquantel) in Groups 1, 2, 3 schools. Cost-sharing in 25 (randomly selected) Groups 1, 2 schools
May-September	2001 Parent Survey (Round 2), and household GPS collection in Groups 2, 3 schools. 2001 Pupil Survey (Round 2) data collection in Groups 2, 3 schools
October-November	Second round of 2001 treatment (albendazole) in Groups 1, 2, 3 schools. Cost-sharing continues in 25 (randomly selected) Groups 1, 2 schools

Appendix Table A2: Robustness of Social Effect Results – Parent Networks

	Dependent variable: Child took deworming drugs in 2001			
	Probit (1)	Probit (2)	Probit (3)	Probit (4)
# Parent links with children in early treatment schools (Group 1, 2, not own school)	-0.027* (0.014)	-0.029** (0.014)	-0.071*** (0.023)	-0.016 (0.014)
{# Parent links with children in early treatment schools}^{2}			0.0064** (0.0029)	
# Pupils in early treatment schools < 3 km from home (per 1000 pupils)				-0.20*** (0.07)
# Pupils in all schools < 3 km from home (per 1000 pupils)				0.14** (0.07)
Parent social links controls	Yes	Yes	Yes	Yes
Other household controls	No	Yes	Yes	Yes
Ethnic, religious controls	No	Yes	No	No
Number of observations (parents)	1678	1678	1678	1678
Mean of dependent variable	0.61	0.61	0.61	0.61

Notes for Appendix Table A2: Data from 2001 Parent Survey, and 1999 and 2001 administrative records. Probit estimation, robust standard errors in parentheses. Disturbance terms are clustered within schools. Significantly different than zero at 99 (***), 95 (**), and 90 (*) percent confidence. Parent social links controls include total number of parent links, number of parent links to Group 1, 2, 3 schools (not own school), and number of parent links to non-program schools. Other household controls include respondent years of education, community group member indicator variable, total number of children, iron roof at home indicator variable, and distance from home to school in km, as well as the Group 2 indicator and Cost-sharing school indicator. Ethnic controls include indicators for Luhya-Samia, Luhya-Nyala, Luo, Luhya-Khayo, Luhya-Marachi, and Teso groups, and an indicator for being a member of the largest ethnic group in the school (which is near zero and statistically insignificant). Religion controls include indicators for Catholic, Anglican, Pentecostal, Apostolic, Legio Mario, Roho, and Muslim faiths, and an indicator for being a member of the largest religious group in the school (which is negative and marginally statistically significant). In regression 1, no household controls are included as explanatory variables other than the standard social link controls.

Table A3: Deworming health externalities within and across schools, January to March 1999

Explanatory variables:	Dependent variable: Any moderate-heavy helminth infection, 1999 Probit (1)	Moderate-heavy schistosomiasis infection, 1999 Probit (2)	Moderate-heavy geohelminth infection, 1999 Probit (3)
Indicator for Group 1 (1998 Treatment) School	-0.25***	-0.03	-0.20***
	(0.05)	(0.03)	(0.04)
Group 1 pupils within 3 km (per 1000 pupils)	-0.26***	-0.12***	-0.12*
	(0.09)	(0.04)	(0.06)
Group 1 pupils within 3-6 km (per 1000 pupils)	-0.14**	-0.18***	0.04
	(0.06)	(0.03)	(0.06)
Total pupils within 3 km (per 1000 pupils)	0.11***	0.11***	0.03
	(0.04)	(0.02)	(0.03)
Total pupils within 3-6 km (per 1000 pupils)	0.13**	0.12***	0.04
	(0.06)	(0.03)	(0.04)
Grade indicators, school assistance controls, district mock exam score control	Yes	Yes	Yes
Number of observations (children)	2328	2328	2328
Mean of dependent variable	0.41	0.16	0.32

Notes for Table A3: From Miguel and Kremer (2003, forthcoming). 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. The local densities are constructed using GPS data on program schools.