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Return to Treatment in the Formal Health Care Sector: Evidence from Tanzania†

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Improving access to the formal health care sector is a primary public health goal in many low-income countries. But the returns to this access are unclear, given that the quality of care at public health facilities is often considered inadequate. We exploit temporal and geographic variation in the cost of traveling to formal sector health facilities to show that treatment at these facilities improves short-term health outcomes for acutely ill children in Tanzania. Our results suggest that these improvements are driven in part by more timely receipt of and better adherence to antimalarial treatment. (*JEL* I11, I12, I15, I18, J13, O15)

Expanding access to the formal health care sector is a primary goal for public health policy in many low-income countries. Yet the assumption underlying this priority—that access to the formal health care system improves the health (and ultimately the welfare) of marginalized populations—remains untested. Moreover, given that the quality of care at public health facilities in this setting is often considered inadequate (Das, Hammer, and Leonard 2008), it is unclear whether policies that enable or encourage access to formal sector health care could generate meaningful health improvements without additional public expenditure to increase quality (e.g., infrastructure development, supply chain improvements, and investment in human capital).

The main difficulty in estimating the returns to treatment in the formal health care sector is that individuals select into health care options (Cropper 1977, Selden 1993, Chang 1996, and Grossman 2000). The determinants of treatment choice—such as preferences, information, and the severity of illness—likely also affect health outcomes. Since these factors are not all observed, they will, in general, bias the estimate of the impact of formal sector treatment. Several recent studies in the United States have

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exploited natural experiments to estimate the returns to various categories of health care net of these biases. This literature has examined the effects of hospital quality (Geweke, Gowrisankaran, and Town 2003; Buchmueller, Jacobson, and Wold 2006), physician quality (Doyle, Ewer, and Wagner 2010), emergency care (Doyle 2011), and postnatal and postpartum care (Almond et al. 2010, and Almond and Doyle 2011).

In this study, we examine the effects of treatment following acute illness at formal sector health facilities on short-term health outcomes for young children using nationally representative data from Tanzania. Our empirical strategy exploits temporal and geographic variation in the costs of traveling to formal sector health facilities. Since health insurance is not present and the government heavily subsidizes the pecuniary costs of care, the most salient cost is related to traveling to the care option (Gertler, Locay, and Sanderson 1987; and Mwabu, Mwanzia, and Liambila 1995). This cost can be economically substantial, especially in remote areas.[1]

We use variation in this cost generated by the interaction of distance (to the nearest health facility) and rainfall to predict the choice of health care following acute illness. The intuition behind our interaction instrument is simple: rainfall generates random variation in the cost (or disutility) of traveling a given distance. Heavier rain should discourage individuals who live farther away *more* than individuals living closer to the nearest health facility. To account for the many direct effects of rainfall and remoteness on both the choice of care and health outcomes, we control for the main effects of rainfall and distance in both stages of the two-stage least squares estimation.

We find that the instrument is strongly predictive in the first stage: consistent with our prediction, the negative effect of distance on formal health care usage is exacerbated in rainy months. This effect is robust to a variety of additional controls and passes various falsification tests.[2] Using a sample of young children who were sick with fever in the two weeks preceding survey, we focus on two main health outcome variables: the incidence of fever and malaria on the day of survey. We find that the instrumental variable (IV) estimates are several times as large as the ordinary least squares (OLS) estimates, consistent with self-selection based on the severity of illness. Our overall finding is that formal sector health care greatly reduces the incidence of fever and malaria among children who sought treatment for acute illness.

Given the large magnitudes of the estimated effects, next we ask *why* formal sector health care is more effective. Our results suggest that these improvements are driven in part by more timely receipt of and better adherence to antimalarial treatment. In particular, we find that children using formal sector health care begin antimalarial treatment with less delay and are more likely to adhere to their antimalarial

[1] In the rural part of our sample from Tanzania, where health facilities are relatively densely located compared to the rest of East Africa, the average distance to the nearest health facility is 4.67 kilometers, which is most often traveled by foot.

[2] Additional controls include the historical mean and standard deviation of rainfall in a given locality; distance to the nearest market (which is a measure of remoteness of the household) and its interaction with rainfall; and the geographic (region fixed effects), demographic and socioeconomic characteristics of sample households. We provide falsification tests using rainfall in past and future months, and find no effects of their interactions with distance on health care choice. Additionally, we show that the instrument does not predict selection into the sick sample (reported fever in two weeks prior to survey) nor does it predict sickness (positive test for malaria) at the time of survey among the reportedly nonsick sample. These checks alleviate concerns that the instrument is picking up on unobservable, systematic differences between more or less remote locales that experienced more or less rainfall in the month of survey.

therapy regimens. Receipt of medications, both antimalarial and non-antimalarial, show only weak differences across formal vis-à-vis informal care. Taken together, these results suggest, at least in the Tanzanian context, that the outcome gradient across the formal and informal health care sectors is driven by differences in the receipt and appropriate usage of antimalarial treatment.

Our study makes two main contributions. First, we provide the first assessment, to our knowledge, of these returns in a developing country setting, using methods which account for the bias induced by self-selection into health care options. Despite a large number of studies in developing country contexts on the effects of health interventions—e.g., nutritional supplements, preventive technology, and treatments[3]—on outcomes, little is known about the causal effects of choosing formal sector health care following acute illness.[4] We add to this knowledge base by providing an estimate of these returns for young children.

Second, though disparities in quality of care (formal vis-à-vis informal health care options) have been well documented, the particular *mechanisms* through which improvements in health may be generated, in a causal sense, have not to our knowledge been explored. A long line of work in public health has highlighted the so-called "Last Mile Problem" in global health: even when effective health technologies exist, enabling access and acceptance in the population—going the last mile—is often difficult but has large returns.[5] Our results suggest that access to antimalarial treatments is only weakly better in the formal health care sector but that patients receive more timely treatment and adhere more when accessing formal sector health care, and that this subtle difference may generate large health returns, at least for the young children in our context.

The remainder of the paper is structured as follows. Section I describes a model of health care choice and outcomes to motivate the empirical analysis. Section II describes the data we use and the construction of important variables. Section III explains the empirical strategy. Section IV presents the results, and Section V concludes.

I. Model

In this section, we develop a simple model that relates health care choice to health outcomes, to better understand why comparing the health of individuals who do and do not choose formal sector health care produces a likely biased estimate of the effect of health care choice on outcomes. The model emphasizes the role of severity of illness, which simultaneously influences health care choice and outcomes and is unobserved to the researcher.

[3] See, for example, Strauss and Thomas (1998); Miguel and Kremer (2004); Bobonis, Miguel, and Puri-Sharma (2006); Thomas et al. (2006); and Thirumurthy, Graff Zivin, and Goldstein (2009).

[4] There are, of course, good studies documenting the variation in quality of care (Leonard and Masatu 2007) and the fact that patients bypass poor quality health facilities to reach higher quality ones despite the increase in distance (Klemick, Leonard, and Masatu 2009).

[5] See, e.g., Hogerzeil (2004), WHO (2004), and Zhu et al. (2008).

A. *Setup*

We consider a utility-maximizing agent who falls sick at random and must make a health care choice. His realized health outcome is determined by the inherent severity of his illness s, and by his choice of health care $h \in \{0, 1\}$. We will think of $h = 1$ as the choice of formal sector health care, and $h = 0$ as care outside the formal sector (or no care at all). Severity, which is observed by the individual, is randomly drawn from a distribution $F(s)$.

There are two health outcomes (represented as the random variable D, number of days ill) which may ensue: $D = D^g$ (good), or $D = D^b$ (bad), where $D^g < D^b$.[6] Health care choice and severity combine to determine the probability that the good health outcome occurs. Denote $\theta(s, h)$ as the function which maps severity and health care choice into a probability: $\theta(s, h) = \Pr(D = D^g \mid s, h) \in [0, 1]$. By definition of severity s, $\theta(s, h)$ is decreasing in s for all h. That is, for both choices of health care, a higher severity level decreases the probability of realizing the good health outcome.

We impose the following assumption: $\theta(s, 1) - \theta(s, 0)$ is increasing in s. Formal sector care is more comparatively effective at higher levels of severity: the differential benefit of visiting the health facility will be lower for low-severity illnesses than for high-severity illnesses. The intuition here is that self-treatment for the common cold, for example, is not likely to be very different from treatment in the formal sector; however, treatment for a more severe illness like pneumonia will likely be very different at a facility as compared to treatment in one's own home.

There are two main caveats to making this assumption. First, for extremely severe illnesses, the difference between formal sector care and informal care is likely to matter little (i.e., both $\theta(s, 1)$ and $\theta(s, 0)$ are likely to be close to 0). If this were true, then $\theta(s, 1) - \theta(s, 0)$ would be a nonmonotonic function of s; in particular, we would expect the difference in the effectiveness of care to increase up to a certain point in the severity distribution and then begin to decline. For our analysis, however, we are interested less in this extreme case. We focus on individuals in the portion of the severity distribution whose health care choices can be shifted through exogenous movements in the relative price of care. (Indeed, it is perhaps more policy-relevant to focus on this subgroup.) It is unlikely that these individuals are at either extreme of the severity distribution. Second, Leonard (2007) suggests, in a similar formulation, that this assumption may not hold for all illnesses. Nevertheless, the empirical application presented in this paper is specific to malaria-related symptoms and illness, which are unlikely to violate this assumption.

It is clear that for there to be a nontrivial tradeoff between quality and cost of health care, s must lie in a region of $F(s)$ such that $\theta(s, 1) > \theta(s, 0)$. That is, the probability of a good health outcome is larger at each level of severity if the agent chooses formal sector care. This establishes the comparative effectiveness of formal sector health care over other forms of care. Most evidence from both developed and

[6] Consistent with our empirical setting, we can interpret the D^b as the number of days a parent may have to spend caring for a young child with a prolonged bout of fever or malaria, and D^g as the number of days he or she would spend if the child recovered quickly.

developing countries suggests that the price of formal sector care is substantially larger than informal care, which establishes the tradeoff in price.

B. *Utility Maximization*

Individuals choose h in order to maximize their expected utility subject to a budget constraint. The utility maximization problem is the following:

$$\max_{h \in \{0,1\}} E(u(C)) - P(h) \quad \text{subject to } C \leq w(\Omega - D). \tag{1}$$

Here, C is consumption; $u(C)$ is the utility function; $P(h)$ is the price of health care at option h; w is the wage; and Ω is the amount of time an individual would work if fully healthy. The health outcome enters utility through its effect on the amount of time an individual is able to work (and thus the amount he can consume).

Since there are two possible states $\left(D = D^g \text{ or } D = D^b\right)$ with known probabilities $(\theta(s,h))$, when the budget constraint binds we can write the maximization problem as

$$\max_{h \in \{0,1\}} \theta(s,h)u\big(w(\Omega - D^g)\big) + (1 - \theta(s,h))u\Big(w\big(\Omega - D^b\big)\Big) - P(h). \tag{2}$$

Let us define $\bar{u} = u\big(w(\Omega - D^g)\big)$ and $\underline{u} = u\Big(w\big(\Omega - D^b\big)\Big)$ as the utilities in the good and bad state, respectively. Then, the expected utility of choosing $h = 1$ and $h = 0$, respectively, are:

$$U_1 = \theta(s,1)\bar{u} + (1 - \theta(s,1))\underline{u} - P(1),$$

$$U_0 = \theta(s,0)\bar{u} + (1 - \theta(s,0))\underline{u} - P(0).$$

The individual will choose $h = 1$ if and only if $U_1 - U_0 > 0$. Denoting $\Delta P = P_1 - P_0$ and $\Delta u = \bar{u} - \underline{u}$, we can express this inequality as

$$\theta(s,1) - \theta(s,0) > \frac{\Delta P}{\Delta u}. \tag{3}$$

Since we have assumed that the left-hand side of the above inequality is increasing in s, it follows that the function (let us denote $g(s) = \theta(s,1) - \theta(s,0)$) has an inverse $\left(g^{-1}\right)$. Thus, the utility maximization problem can be expressed as a simple cutoff rule:

$$\text{Choose } h = 1 \text{ if and only if } s > g^{-1}\left(\frac{\Delta P}{\Delta u}\right) \equiv K. \tag{4}$$

The individual will thus choose to use formal sector health care if the severity of his illness is greater than a cutoff, which is in turn a function of the model's parameters. The parameters enter in intuitive ways in the cutoff value. An increase in the relative price of formal sector care (ΔP) increases the cutoff, which in turn decreases the probability that an individual with randomly chosen severity uses formal sector care. On the other hand, an increase in the relative return to formal sector health care (Δu), for example due to an increase in the wage w, lowers the cutoff and thus increases the probability of choosing $h = 1$.

C. *Empirical Implications*

We are primarily interested in estimating the effect of formal sector health care usage on health outcomes. In doing this, we are essentially estimating the returns to formal sector care in the health production function. In this section, we investigate why comparing the average outcomes of individuals who used formal sector care with the outcomes of those who did not is an invalid strategy for estimating this effect. We then discuss the model implications for a valid identification strategy.

First, we calculate the true average treatment effect of formal sector health care on health outcomes over the entire distribution of severity. Denote $f(s)$ as the probability density function (pdf) of $F(s)$. The difference between the expected outcome under $h = 1$ and $h = 0$, which we denote $E(O_1 - O_0)$, is

$$\int_{-\infty}^{\infty} \Big(\big(\theta(s,1)D^g + (1 - \theta(s,1))D^b\big) - \big(\theta(s,0)D^g + (1 - \theta(s,0))D^b\big)\Big) f(s)\ ds.$$

We can rewrite this quantity as $\left(D^g - D^b\right) \int_{-\infty}^{\infty} (\theta(s,1) - \theta(s,0)) f(s)\ ds$. Since, $D^g < D^b$, and $\theta(s,1) - \theta(s,0) > 0$ for all s, we have that $E(O_1 - O_0)$ is negative, which indicates that the true average treatment effect of formal sector health care is an improvement in health outcomes. This fact arises rather trivially from the second assumption we made on p.

Second, we calculate the difference in health outcomes between individuals who chose to use formal sector care and those who did not. We know from the model's cutoff rule that individuals below the cutoff (K) in the distribution of severity will choose $h = 0$, while those with severity levels above it will choose $h = 1$. We can thus calculate the average outcome as:

$$\text{(5)} \qquad \int_{K}^{\infty} \Big(\big(\theta(s,1)D^g + (1 - \theta(s,1))D^b\big) f(s)\ ds$$

$$- \int_{-\infty}^{K} \big(\theta(s,0)D^g + (1 - \theta(s,0))D^b\big)\Big) f(s)\ ds.$$

Again, we can rewrite this quantity as $\left(D^g - D^b\right)\left(\int_K^{\infty} \theta(s,1) f(s)\ ds - \int_{-\infty}^{K} \theta(s,0) f(s)\ ds\right)$. Thus, the effect of formal sector health care calculated by simply comparing health outcomes for individuals who chose $h = 1$ and $h = 0$ has the following bias (calculated by subtracting the average treatment effect from this naively measured effect):

$$\text{(6)} \qquad \int_{K}^{\infty} \theta(s,0) f(s)\ ds - \int_{-\infty}^{K} \theta(s,1) f(s)\ ds.$$

It is clear that this bias term is not in general equal to 0. The direction of the bias depends on the shape of the severity distribution. For example, if we assume that severity s is uniformly distributed between 0 and B, such that $0 \leq K \leq B$ it is easy to see that the bias will be negative (given the assumptions made on p):

$$\int_K^B \theta(s,0)\ ds < \int_0^K \theta(s,1)\ ds. \tag{7}$$

The results presented in this paper are consistent with this attenuating bias due to severity. That is, the probability of a good health outcome appears to be higher under formal sector care, and the severity bias in the OLS estimates appears to be large and negative.

Finally, we discuss how the model motivates our instrumental variables strategy, which we will develop further in Section III. As we have shown, comparing outcomes for individuals who chose $h = 1$ and $h = 0$ generates a biased estimate of the effects of formal sector health care. The ideal experiment for estimating average treatment effect without bias would be to randomize the choice of health care for individuals across the severity distribution, and then compare outcomes for individuals who were randomly treated with formal sector care with those who were not.

In the absence of such an experiment, we consider the following strategy. If, on a random basis, individuals were to face different prices for formal sector care, the price variation would generate exogenous shifts in the cutoff K, and thus create treatment and control groups based on whether individuals were randomly exposed to higher or lower prices for formal sector care.

For example, suppose that randomly chosen individuals were exposed to a higher relative price of formal sector care ($\Delta P' > \Delta P$). This price shift would generate variation in the cutoff ($K' > K$), and would thus exogenously drive a portion of individuals (specifically, those who have severity levels between K and K') to choose $h = 0$ instead of $h = 1$, which is what they would have chosen if the cutoff had not changed. Thus, we can compare the health outcomes of individuals who would have chosen formal sector health care but were incentivized on a random basis not to by the change in price with those of the individuals who were not exposed to this random price incentive.

Note that this type of experiment elicits not an average treatment effect but rather a local average treatment effect (LATE), since the price experiment will generate variation in choice only in the region of the severity distribution between K and K'. We can express the LATE estimate as

$$\left(D^g - D^b\right)\int_K^{K'} (\theta(s,1) - \theta(s,0))f(s)\ ds. \tag{8}$$

The LATE estimate is the average treatment effect restricted to the region of the severity distribution between K and K'. In the following section, we discuss the particular instrument used to generate variation in the price of formal sectorformal sector health care, using the model to better understand threats to the validity of the instrument, and creating tests to evaluate empirically the extent to which these issues are present in our data.

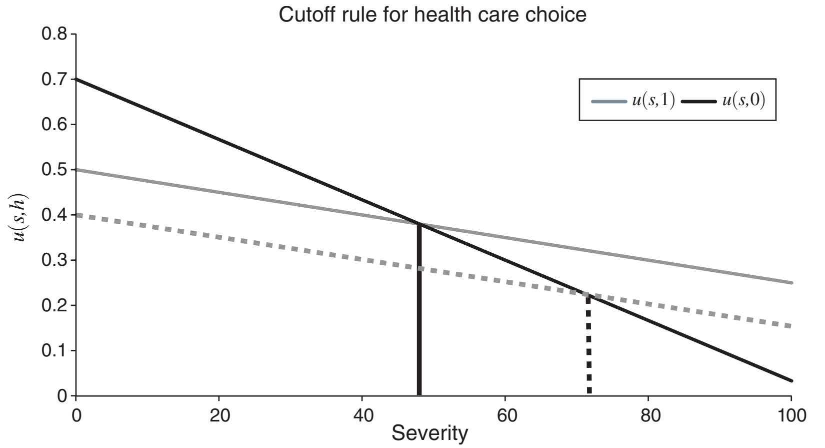

Figure 1. Price Increase Induces Rightward Shift in Cutoff Value

Figure 1 depicts the price variation we propose to use as an instrument in the context of the utility function presented above. Normalizing prices to the price of the informal option, a relative increase in the price of formal sector care results in a shift downwards of $u_1(s)$, the expected utility of choosing $h = 1$ as a function of s. For the sake of simplicity in the graphical representation, we have assumed that $\theta(s,h)$, and hence $u_h(s)$, is linear in s with $s \in [0,100]$. This is, of course, not necessary for the predictions of the model shown above to hold. The solid gray line corresponds to $u_1(s)$ before an increase in the relative price of formal sector care, and the dotted gray line below it represents the same utility after the price change. The black line, denoting the $u_0(s)$ is held constant, and thus the new cutoff value, where the new $u_1(s)$ and $u_0(s)$ meet, is to the right of the old in the severity distribution. We refer to the old cutoff as K (the solid vertical black line in Figures 1 and 2) and the new cutoff as K' (the dotted vertical black line in both figures).

Figure 2 depicts $\theta(s,1)$ and $\theta(s,0)$ under the linear functional form assumption along with the same shift in the severity cutoff as in Figure 1. Here we can see that the average treatment effect estimate from the OLS regression compares the average of $\theta(s,1)$ over $s \in (K,100]$ with the average of $\theta(s,0)$ over $s \in [0,K]$. On the other hand, the LATE estimate from the second stage IV regression compares the average of $\theta(s,1)$ over $s \in [K,K']$ with the average of $\theta(s,0)$ over $s \in [K,K']$.

II. Context and Data

A. *Context*

Health care markets in Tanzania are highly informal. Formal health facilities (defined here as hospitals, regional health centers, and drug dispensaries) provide care for less than half of all illness episodes. In rural areas, where the density of formal sector facilities is low, this fraction is smaller. Most patients seek care and purchase medication from (largely unregulated) pharmacies, drug shops, kiosks, traditional healers,

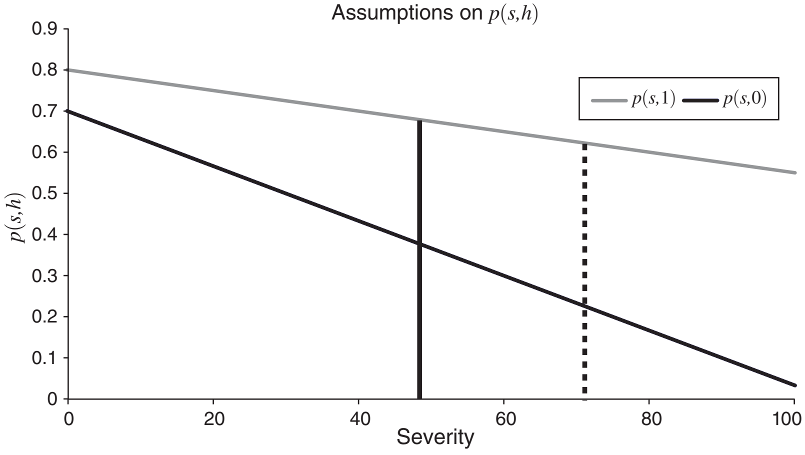

Figure 2. Price Shift, LATE and ATE

as well as family and friends. Most interaction with the health system is following episodes of acute illness—very little preventative health care is available/utilized in most areas of Tanzania. Among those seeking curative care, the most common symptom is fever; in children, this is due to the frequent incidence of malaria, pneumonia, and respiratory tract illnesses (Klemick, Leonard, and Masatu 2009).

Travel cost (utility cost and/or opportunity cost of time) is the most significant determinant of cost of care. This makes formal health facilities effectively more expensive than self-treatment (Gertler, Locay, and Sanderson 1987). Health care costs are financed out-of-pocket; formal insurance markets essentially do not exist. Infants and young children, pregnant women, the elderly, and severely ill patients tend to choose formal care following acute illness.[7] Care in the formal sector is delivered by health workers with varied training and experience. Fully trained doctors are few and far between, particularly in rural areas. Diagnosis and treatment are often low quality, both due to the lack of training of workers and the frequent absence of adequate diagnostic tools and medicine stocks (Klemick, Leonard, and Masutu 2009).

B. *Data*

We use the 2007–2008 Tanzania HIV/AIDS and Malaria Indicator Survey (THMIS), which is part of the Demographic and Health Surveys (DHS). The THMIS used a two-stage sampling frame. In the first stage, sample points (clusters) were selected based on enumeration areas designated by the 2002 Tanzanian Population and Housing Census; 475 clusters were selected. A household census within each cluster was then used to randomly select approximately 16 households from each cluster to be surveyed. Weighting factors are included in the data, so when weighted the sample is nationally representative. In sampled households, all men and women

[7] See, e.g., Cohen, Dupas, and Schaner (2013).

ages 15–49 were interviewed, and blood samples for malaria and anemia were collected for children under five years old (excluding children less than six months old).[8]

C. *Child-Level Variables*

The majority of our analysis is at the child level. Mothers were asked questions regarding the health of their children. We deal primarily with the sample of sick children, that is, children who were sick with fever or cough in the two weeks preceding the date of survey. Respondents who answered "yes" to this question were asked where they had sought care for the sick child, when they had sought care if they had, and whether the child was still sick (at the time of survey).

We construct variables corresponding to selection into the sick sample, health care choice, and health outcomes using answers to the survey questions mentioned above. If the respondent answered "yes" to the question about their child having a fever in the two weeks before survey, they are included in our sample of sick children. We then construct a binary variable h corresponding to the use of formal sector health care for the child; $h = 1$ if the respondent brought the sick child to formal sector health care, and $h = 0$ otherwise. Formal sector care is defined as a visit to a government or private hospital or health center.

We use two main health outcome variables. The first is a fever indicator variable, which equals 1 if the respondent reports that the sick child still has fever on the day of survey, and 0 otherwise. The second is an indicator variable which equals 1 if the child tests positive for malaria when surveyed, and 0 otherwise. We also construct and use binary variables for whether or not the sick child received any medicine; whether or not he received malaria medicine; days delayed before receiving malaria medicine; and whether or not he adhered to treatment regimen corresponding to these malaria medications. More detail on construction of the variables is provided in the online Appendix.

D. *Mother- and Household-Level Variables*

In subsequent analysis, we use the DHS's survey of mothers and women of child-bearing age to understand impacts of formal sector care usage on mothers' health care-related information. We construct indices corresponding to the amount of health-related information the mothers have, by aggregating yes-or-no questions on the definitions of diseases, disease transmission, and treatment. For example, if there were six questions about transmission of various diseases, the respondent was awarded 1 point for every correct answer, deducted 1 point for every wrong answer, and given a 0 for a response of "don't know" if applicable. The scores were then summed across the six questions yielding the disease transmission information score for that respondent. The same was done for a set of questions about the existence of various diseases and a set of questions about medical treatments for various diseases. A composite index equaling the sum across all of these information

[8] The Paracheck-Pf rapid diagnostic test used to detect malaria was found to be very reliable when measured against the current gold standard microscopy test for malaria in five districts in Tanzania (Mboera et al. 2006).

measures was also constructed. More detail on the construction of these indices is provided in the online Appendix.[9]

We obtain data on the distance to the nearest health facility and distance to the nearest market (both in kilometers) from the household questionnaire (a module of questions for household heads).[10] In addition, we include the following control variables from the child-level, mother-level, and household-level questionnaires in our regressions: indicator variables for age of the child (in years); region fixed effects; wealth index category fixed effects; mother's educational attainment in years; mother's age at marriage; year in which mother was married; indicators for the month of survey; household size; number of living children in household; number of children under age of five in household; an indicator variable for urban clusters; gender of the child; and household's altitude.

E. *Rainfall and Temperature Data*

We use the restricted version of the THMIS, which contains data on the latitude and longitude coordinates of each of the sampled clusters. We use these data to match clusters to rainfall and temperature data from the University of Delaware's Center for Climatic Research. The rainfall dataset is called "Terrestrial Precipitation: 1900–2008 Gridded Monthly Time Series (1900–2008) (Version 2.01)," and the temperature dataset is called "Terrestrial Air Temperature: 1900–2008 Gridded Monthly Time Series (1900–2008) (Version 2.01)."

The rainfall and temperature measures for a latitude-longitude node (on a 0.5° latitude by 0.5° longitude grid) combine data from 20 nearby weather stations using an interpolation algorithm based on the spherical version of Shepard's distance-weighting method. We matched the rainfall and temperature data to clusters by calculating the closest grid point to the latitude-longitude coordinates of each cluster.[11] The rainfall quantity we use for analysis is the one which corresponds to the month in which the individual was surveyed. Since the survey took place over a period of four months, each individual is matched based on the latitude-longitude coordinates of his household's cluster, and on the month of survey.[12]

In all our regressions, we control for the historical mean and standard deviation of rainfall and temperature in the month of survey at the closest grid point to the latitude-longitude coordinates of the household's cluster. We calculate these historical

[9] We also constructed principal component indices using the health-related information questions (composite, as well as separately for diseases, disease transmission, and treatment). The results are similar to the alternate constructions discussed above, and thus are not reported in the paper, but are available upon request.

[10] Note as a caveat that distances are self-reported, and thus likely have a degree of error that may be correlated with household characteristics (for example, one might imagine that the extent of reporting error is correlated with the household head's educational attainment). Nevertheless, in the absence of objective measures of distances, many studies use the self-reported measure (see, e.g., Gertler, Locay, and Sanderson 1987).

[11] We thank Seema Jayachandran for the Stata code that performs this calculation, which is based on the Haversine formula.

[12] The fact that the rainfall grid is created using only 20 stations likely implies that there is a great deal of measurement error in rainfall at the community level. Unfortunately, we cannot do much to correct for this problem. If measurement error is classical, we would expect attenuation of the rainfall impacts. We might imagine, though, that the most remote communities have the largest measure of error, because the rainfall stations associated with the grid points nearest to them are actually quite far away. If so, we would be leaning heavily on the quality of the interpolation algorithm to account for the density of rainfall stations around grid points.

variables by averaging over the rainfall quantities in the last 50 years in the particular month in question. So, for example, if an individual in cluster 1 was surveyed in January, the value corresponding to the historical mean of rainfall would be the average rainfall in January in his cluster over the last 50 years. The historical standard deviation would be the standard deviation from this mean in the last 50 years.

F. *Summary Statistics*

Table 1 presents means, standard deviations, and number of observations for select variables of interest to be used in the analysis below for the sample of children whose mothers reported them as having been ill in the two weeks prior to survey; sick children who received care at formal sector health facilities; and sick children who did not receive formal sector care. We also conduct *t*-tests for differences in means of these variables across children who visited formal care facilities and those who did not. We report the *t*-statistics and corresponding *p*-values in the last two columns of Table 1.

We see that 16 percent of the total sample of children under the age of five were reportedly ill with fever and/or cough, leaving a restricted sample of 1,200 children on which to perform the proposed analysis. Roughly 27 percent of this restricted sample still reported having fever at the time of survey and roughly 23 percent tested positive for malaria. It is important to note that these means are across the entirety of the restricted sample and that the relevant means against which to compare any local average treatment effect estimates from two-stage least squares regressions are those for the population on the margin. It is possible that these means are significantly different.

Columns 3–6 of Table 1 present means and standard deviations of the variables of interest across sick children who did and did not receive formal sector health care. We discuss these comparisons for the different sets of variables below.[13]

Children who did and did not receive formal sector care appear similar in terms of probability of anemia (both subsamples have 77 percent likelihood of severe anemia); we might expect this, given that anemia is an indicator for longer-term health status. On the other hand, the proportions of children with malaria and fever (at the time of survey) are substantially different. For both short-term health indicators, children who received formal sector care appear better off with nearly half the likelihood of both fever and malaria (32 and 35 percent likelihood of fever and malaria, respectively, for children who do not seek care vis-à-vis 17 and 21 percent likelihood among care-seeking children). These statistically significant differences are evident in the *t*-statistics and *p*-values presented in the final two columns of Table 1.

Across health care choices, sick children appear generally similar in terms of gender, but differ in terms of age, household size and rural residence. Those not receiving formal care are slightly older (1.75 versus 1.49 years of age), more likely

[13] We bear in mind in examining these differences in means that children in the two categories of care are, of course, likely different on many margins, not all of which can be observed (e.g., severity of illness, or the child's family's preferences for health). Thus, we cannot interpret these differences causally. We develop a methodology for causal identification of effects in Section III.

Table 1—Summary Statistics

Number of observations	
All children	7,502
Children reporting sickness in two weeks prior to survey	1,200
All mothers	4,910
Mothers reporting at least one child (<5) as acutely ill	1,071

	Sick children							
	All		Formal care		No formal care		Differences by care	
	Mean	SD	Mean	SD	Mean	SD	t-statistic	p-value
Sought care at a formal sector health care facility	0.573	0.495						
Instrument (cost-of-care shifters)								
Rain (mm/100)	0.918	0.610	0.881	0.631	0.967	0.579	2.41**	0.0161
Distance to nearest formal sector health facility (km)	4.418	6.027	3.352	4.789	5.853	7.131	7.21***	0.0000
Distance to nearest market (km)	26.293	29.585	22.127	27.652	31.927	31.168	5.64***	0.0000
Health status								
Tested positive for mild to severe anemia	0.770	0.421	0.777	0.417	0.761	0.427	−0.59	0.5538
Tested positive for malaria (at time of survey)	0.232	0.422	0.165	0.372	0.319	0.467	5.91***	0.0000
Fever now (self-reported, at time of survey)	0.272	0.445	0.213	0.409	0.351	0.478	5.37***	0.0000
Demographic characteristics								
Age	1.603	1.301	1.489	1.273	1.754	1.324	3.51***	0.0005
Female	0.496	0.500	0.483	0.500	0.513	0.500	1.01	0.3138
Rural	0.819	0.385	0.783	0.412	0.867	0.339	3.77***	0.0002
Household size	7.254	3.856	6.815	3.427	7.842	4.298	4.60***	0.0000
Medications								
Number of medicines received	1.491	0.863	1.761	0.756	1.122	0.863	−13.42***	0.0000
Number of medicines conditional on number >0	1.694	0.708	1.795	0.722	1.511	0.644	−6.28***	0.0000
Number of antimalarials	0.551	0.521	0.703	0.492	0.343	0.488	−12.39***	0.0000
Number of nonantimalarials	0.940	0.762	1.058	0.808	0.779	0.661	−6.28***	0.0000
Medication-related behaviors								
Adhered to at least 1 medication	0.629	0.483	0.639	0.481	0.602	0.491	−0.83	0.4095
Number of medications adhered to	0.634	0.492	0.643	0.489	0.608	0.502	−0.77	0.4398
Adhered to all medications	0.619	0.486	0.628	0.484	0.596	0.492	−0.71	0.4767
Number of days delayed before receiving medication	1.180	1.079	1.150	1.071	1.264	1.099	1.15	0.2505
Health information indices								
Composite	14.173	3.559	14.307	3.496	13.971	3.650	−1.42	0.1565
General disease-related	3.481	1.093	3.463	1.105	3.507	1.078	0.65	0.5156
Transmission-related	8.507	2.749	8.611	2.701	8.357	2.815	−1.48	0.1403
Treatment-related	1.909	1.298	1.996	1.275	1.778	1.324	−2.55**	0.0110

Note: Please see Data Appendix for details on the construction of variables.

to live in rural areas (0.87 versus 0.78 percent likelihood), and come from larger households (7.84 versus 6.8 household members).

Those who received formal sector care received more medicines on average, and the difference is apparent for the number of antimalarials (0.70 versus 0.34) as well as non-antimalarials (1.06 versus 0.78). Adherence, on the other hand, appears to be similar across health care choice groups (roughly 60 percent across all medicines and subsamples).

The information indices are constructed from responses to numerous questions regarding the existence, transmission, and treatment of various diseases. The information on disease index was constructed to range from -4 to 4 and has a mean value of just over 3. The information on transmission index was constructed to range from -13 to 13 and has a mean of just over 8. The information on treatment index was constructed to range from -3 to 3 and has a mean of roughly 1.9. The composite ranges from -20 to 20. It has a mean of just over 14. More information on the creation of these indices is provided in the online Appendix.

We see that the mean composite index (measured at the mother-level) is slightly larger for those who visited formal sector care, and that the separate indices related to diseases, transmission and treatment are all slightly larger for formal sector care users as well. These differences are fairly small as compared to the means and statistically insignificant except for treatment-related information.[14]

III. Empirical Strategy

In this section, we describe our instrumental variables (IV) strategy and present evidence related to the intuition behind and validity of our instrument. Our primary aim is to obtain unbiased estimates of the effects of accessing formal sector care on health outcomes. As detailed in the previous section, ordinary least squares (OLS) estimates of this effect will likely be biased by the severity of illness (among other unobserved factors), which is omitted from the regression and a determinant of both health care choice and health outcomes.

We propose instrumenting for health care choice using variation in one component of the relative price of health care in the formal sector. The most salient costs to health care in developing countries are those incurred through travel (Gertler, Locay, and Sanderson 1987). Since care at health facilities and hospitals is often free or heavily subsidized by the government, the most relevant factors contributing to costs are the time and general disutility associated with traveling to the source of care.

In countries with a low density of health facilities, and especially in sparsely populated rural areas, visiting a health facility for treatment often involves walking or riding public transportation for hours; individuals must incur the opportunity cost of this time and the disutility of strenuous travel if they choose to visit a formal health care provider. On the other hand, informal health care options—such as self-treatment with medicines obtained from a drug store or kiosk, a village health worker or a traditional healer—are often much more accessible in terms of distance.

Gertler, Locay, and Sanderson (1987) and others have used the distance to the nearest health facility as a proxy for the price of care. In our setting, using this variable as an instrument for the choice of care is likely invalid. Endogenous placement of health facilities—for example, allocating facilities to areas with very poor population health—is a primary concern. Moreover, more remote areas are less likely to have health facilities nearby; the manifold direct effects of living in a remote area on health would thus invalidate the exclusion restriction.

[14] Larger values of the index correspond to more knowledge in the particular dimension being measured.

We improve upon the use of variation in distance by interacting this variable with rainfall at the time the child fell sick, while controlling for the main effects of distance and rainfall in the first and second stages of the two-stage least squares estimator. That is, only the interaction is excluded; the main effects are allowed to have direct effects on both health care choices and health outcomes.

Let O_{ij} denote a health outcome for (sick) child i in cluster j; let h_{ij} denote the health care choice made for the child; and let $\mathbf{X}_{ij}$ denote a vector of child-, mother- and household-level characteristics. Denote the distance to the closest health facility as d_{ij}, and the quantity of rainfall in cluster j at the time of the child's illness as R_{ij}. Using this notation, the two-stage IV is specified as:[15]

$$\text{1st stage:} \quad h_{ij} = \alpha_1(d_{ij} \times R_{ij}) + \alpha_2 d_{ij} + \alpha_3 R_{ij} + \mathbf{X}'_{ij}\alpha_4 + \zeta_{ij} \tag{9}$$

$$\text{2nd stage:} \quad O_{ij} = \beta_1 h_{ij} + \beta_2 d_{ij} + \beta_3 R_{ij} + \mathbf{X}'_{ij}\beta_4 + \epsilon_{ij}. \tag{10}$$

A. *Intuition*

The intuition behind the instrument is the following. The main effect of distance should be negative; that is, traveling a greater distance should discourage the usage of formal sector facilities for individuals seeking care.[16] We posit that heavier rain should discourage individuals who live farther away *more* than individuals living closer to the nearest facility. Imagine for example that one household is located just next door to a facility, while another is located ten kilometers away. In times of dry weather, clearly the household next door will be more likely to choose health facility care than the one farther away. But in times of heavy rains, the rain should incrementally deter the farther household *more* than the one just next door.

Note also that in our baseline specification, we control for the historical average of rainfall in the individual's cluster. We do this to address the possibility that individuals living in wetter places might adjust their behavior (i.e., use different road networks or modes of transportation) in accordance with the prevalence of rainfall in their locality. We thus control for this "expected" amount of rainfall, and effectively use the "innovation" in rainfall realized in a particular month. The interaction instrument coefficient changes little, however, with and without the historical rainfall controls.

In section A in the online Appendix, we detail a variety of ways in which we address threats to this identification strategy. In particular, we address

- the possibility that rainfall might have differential effects across remote and nonremote locales;
- nonrandom selection into the self-reporting of recent illness;
- the possibility that the instrument directly drives changes in health;

[15] We use linear probability models throughout our analysis.

[16] We verify that this is the case, by omitting the interaction instrument from the first-stage regression and estimating the main effects of nearest health facility distance and rainfall on health care choice. The results of this estimation are reported in online Table A1, column 1. The estimated coefficients on distance and rainfall are both strongly negative.

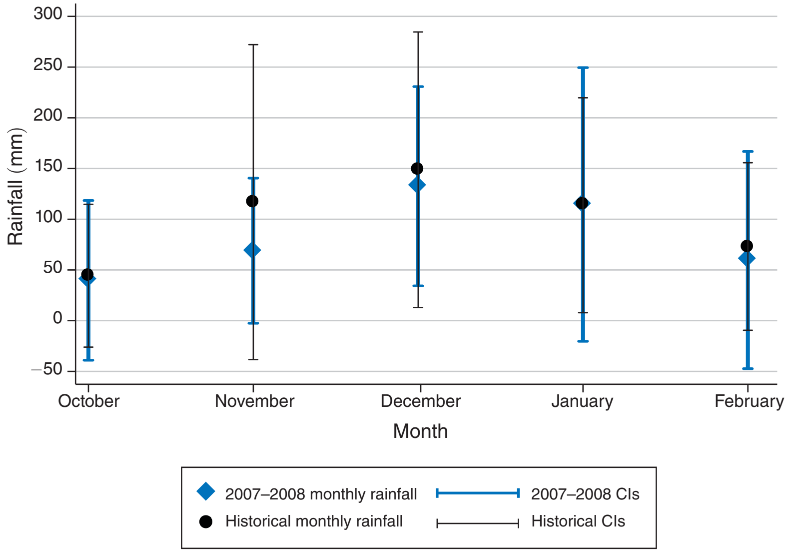

Figure 3. Survey Period and Historical Monthly Rainfall Variation

Note: Intervals depict geographic variation in mean rainfall within a month over the survey period (bold, diamond) and historically (thin, circle).

- the potential role of past and future rainfall on contemporaneous health care-seeking behavior;
- robustness to interactions of rainfall with a variety of covariates; and
- robustness to nonlinear distance terms. Details are provided in the online Appendix; in general we find strong support for the validity of the interaction instrument strategy in our setting.

B. *Preliminary Motivating Evidence*

Before employing this strategy below, we validate the intuition behind the instrument and its impact on health-seeking. We begin by showing that there is sufficient geographic variation in rainfall each month to drive variation in health care choice. We plot (in bold, diamond) the mean rainfall in each month of the survey period and the 95 percent confidence intervals around these means. For sake of comparison, we also plot the 50-year historical means and standard deviations of rainfall for each month. These plots are depicted in Figure 3.

We see in Figure 3 that there is, indeed, a great deal of geographic variation in rainfall each month, and that rainfall in the survey period conforms reasonably well to the historical distribution. Next, we check that the rainfall distributions, both during the survey period and historically, do not vary systematically by distance to health facility. We repeat the exercise from Figure 3 for subsamples of households with above and below median distance to nearest health facility; these plots are presented in the online Appendix (Figure A6).

Next, we explore the degree to which health care choice varies by distance to health facility. We plot the fraction of households seeking care as a smoothed

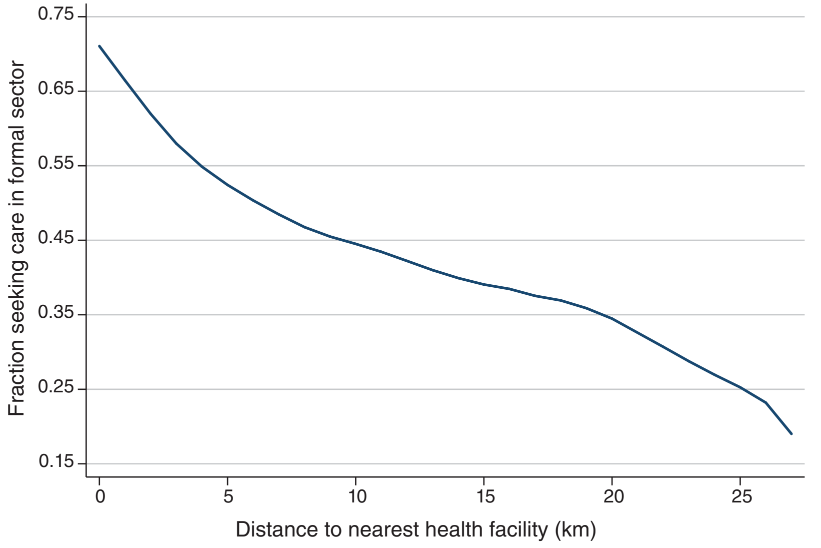

Figure 4. Health Care Choice by Distance to Facility

Note: Distance trimmed at 99th percentile.

function of the distance between the household and the nearest formal sector care facility. Figure 4 shows that, indeed, health-seeking is a monotonically decreasing function of distance to facility.

Finally, this leads us to explore the degree to which the slope of the health-seeking function is steeper at higher rainfall realizations (relative to the historical mean of rainfall). This is the core intuition behind the proposed instrument. We depict the identifying variation in the data in two ways. First, we repeat the exercise from Figure 4 for subsamples of households experiencing rainfall during the survey period above and below the historical mean rainfall for that month. These two curves are depicted in Figure 5 and show clearly that higher rainfall exacerbates the degree to which distance discourages health-seeking. This relationship is demonstrated through a contour plot in Figure A7 in the online Appendix.

IV. Results

A. *First-Stage Results*

Table 2 presents results from the first-stage regressions of health care choice on the interaction of rainfall and distance to nearest health facility for child level and mother level samples, respectively. All standard errors, here and in the results presented below, are clustered at the sampling cluster level. The first-stage effects are negative, significantly different from zero and robust to the inclusion of various controls.[17] Along with various demographic controls, we include region fixed

[17] We make a note here regarding the interpretation of the coefficient estimate on the main effect of distance. This coefficient estimate is slightly positive, because most of the negative effect of distance is absorbed in the interaction

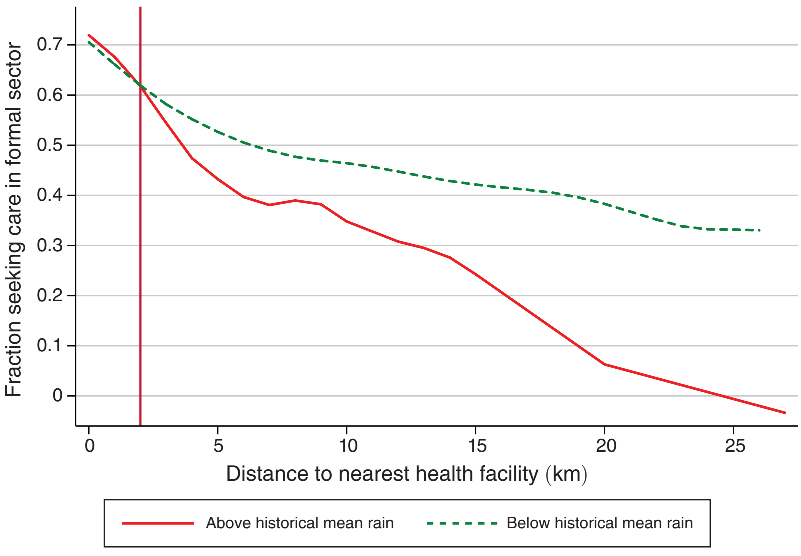

Figure 5. Health Care Choice by Distance to Facility and Rainfall

Notes: Vertical line depicts median of distance to nearest health facility. Distance trimmed at 99th percentile.

Table 2—First Stage: Interaction of Rain and Distance to Health Facility

	Child level	Mother level
Dependent var: 1(Sought care at a formal sector health care facility)		
Rain × Distance	−0.0204***	−0.0192***
	(0.00516)	(0.00534)
Distance	0.00569	0.00348
	(0.00489)	(0.00506)
Market	−0.00167	−0.00177
	(0.00130)	(0.00131)
Rain	−0.139*	−0.175**
	(0.0706)	(0.0724)
Rain × Market	0.000776	0.000912
	(0.00118)	(0.00122)
F-test: Rain × Distance = 0	15.60	12.98
Prob > F	9.37e-05	0.000358
Observations	1,081	963
R^2	0.178	0.177

Notes: Robust standard errors in parentheses. Standard errors are clustered at the sampling cluster level. All specifications include age, region, wealth, education, and month of survey group effects. Other controls include temperature, historical means and standard deviations of both rainfall and temperature, altitude, household size, number of living children, number of children under five, gender, mother's age at marriage, year in which mother was married, and a dummy for whether the household is located in a rural or urban area. For the sake of parsimony, all coefficients are not reported here, but are available upon request. Results from linear probability model estimations shown.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

of distance with rainfall. At the mean rainfall level we observe in the sample, the main effect of distance is indeed negative. This fact is reinforced by looking at the estimate of the main effect of distance when no interaction term is included—this estimate is reported in column 1 of online Table A1, and is negative and significantly different from 0.

effects representing the 26 regions in the data to ensure that the instrument is not picking up broad geographic variation across regions.

The specifications also control for demographic and geographic characteristics such as age and gender of child; household wealth; mother's education; mother's age at marriage; year in which mother was married; region, altitude, and size of household; number of living children and number of children under the age of five in the household and a dummy for whether the household is located in a rural or urban area.[18]

To the extent that rainfall and its subsequent effects on cost of travel are predictable, the ability of the instrument to predict health care choice in response to acute health shocks may be impaired. In order to account for this predictability of rainfall and the possibility that transportation infrastructure adapts to the predictable component of rainfall, we control for historical means and standard deviations of rainfall for the month of survey. Also, so as to ensure that the instrument is in fact reflecting specifically an increased cost of travel rather than a general impact of extreme weather on behavior, we control for average temperature in month of survey as well as historical mean and historical standard deviation. Online Table A1 columns 2–4 present results from the first-stage specifications with sets of controls added incrementally.

B. *Health Outcomes*

Table 3 presents the main results from regressions of health care choice on the incidence of fever and malaria among the sample of children who reported being ill with fever in the two weeks prior to survey. The first two columns in Table 3 report (endogenous) OLS regression estimates. We find that children who sought care in the formal sector are roughly 9 percentage points less likely to still have fever or malaria at the time of survey. Columns 3 and 4 of Table 3 report IV estimates. Effects on fever and malaria are negative and precisely estimated, around 62 and 40 percentage points, respectively.[19] The magnitudes of the estimated effects of care in the formal sector are quite large, particularly compared to the averages for fever and malaria prevalence in our sample. Of course, it bears mentioning that given that these impacts are not bound tightly around the point estimates, a fairly wide range of smaller impacts is possible.

What explains these magnitudes? We focus on two potential explanations. First, given that many fevers in young children in this context are malarial, treatment with effective antimalarials, particularly with Artemisinon-Based Combination Therapy (ACT), should essentially reduce the probability of fever to 0.[20] On the other hand, inappropriate treatment (with ineffective antimalarials or with antipyretics alone)

[18] We lose about 10 percent of observations due to missing values in control variables, thus the final sample sizes in these regressions are 1,081 and 963 for the child and mother samples, respectively.

[19] The fact that the OLS estimates are smaller than the IV is consistent with severity bias. The large difference in magnitudes between OLS and IV estimates is also consistent with related work. Online Table A8 reports a comparison of the estimates of severity bias found in this paper with those found in other work.

[20] It bears mentioning, however, that overdiagnosis of fever as malaria is very common in Tanzania and similar African settings, though the probability of misdiagnosis is increasing with age, so equating fever and malaria in young children in highly endemic malarial settings generates less egregious diagnostic error (see, e.g., Adhvaryu 2014).

Table 3—Effects of Health Care Choice on Health Outcomes

	OLS		Second stage IV	
	Fever	Malaria	Fever	Malaria
Formal health care	−0.0945***	−0.0961***	−0.619**	−0.398*
	(0.0315)	(0.0270)	(0.266)	(0.236)
Distance	0.00132	0.00228	−0.00442	−0.00102
	(0.00295)	(0.00236)	(0.00385)	(0.00286)
Market	−0.00123	0.000902	−0.00191	0.000706
	(0.00111)	(0.000940)	(0.00130)	(0.00105)
Rain	0.0640	0.0577	−0.0191	0.0152
	(0.0855)	(0.0599)	(0.106)	(0.0702)
Rain × Market	0.000494	−0.000885	0.000612	−0.000958
	(0.000943)	(0.000779)	(0.00108)	(0.000928)
Observations	1,073	934	1,073	934
R^2	0.110	0.294	−0.173	0.189

Notes: Robust standard errors are in parentheses. Standard errors are clustered at the sampling cluster level. All specifications include age, region, wealth, education, and month of survey group effects. Other controls include temperature, historical means and standard deviations of both rainfall and temperature, altitude, household size, number of living children, number of children under five, gender, and a dummy for whether the household is located in a rural or urban area. Sample sizes are reduced by some observations in these specifications due to missing values in the outcomes. Results from linear probability model estimations shown.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

should have a small or transient effect on fever.[21] Thus, we might expect that the treatment effect on children with malarial fevers would be large.

Second, as is the case with all LATE estimates, the appropriate mean by which to scale the size of the estimated effect is the mean of the dependent variable for the population *on the margin* of adoption. The characteristics of this subsample may be different than the sample of children on the whole. In Table 4, we explore heterogeneity by the relative wealth of the child's household and the child's gender in the effects of the instrument on health care choices in the first stage. The former dummy is constructed to equal 1 if the child's household is in the first or second quintile of the wealth distribution, for a wealth index generated using principal components analysis on household asset ownership data.[22]

The top panel of Table 4 shows that the coefficient on the interaction instrument is larger for children in lower-wealth households and for male children (though not significantly so across the fully stratified models). The bottom panel of Table 4 reports means of the dependent variables for the portion of this subsample that does not seek care. These means might more closely reflect the scope for improvement in health outcomes through formal care. A comparison of the magnitudes of the coefficients of interest from the re-reported in columns 2 and 4 to these means suggests that being exogenously driven to a formal sector health facility leads to nearly a full recovery from acute illness.

[21] See, for example, Cohen, Dupas, and Schaner (2013).
[22] The first quintile is the lowest relative wealth, and the fifth is the highest.

Table 4—First Stage Heterogeneity and Health Outcome Means by Subsample

	Household wealth		Child's gender	
	Higher wealth >= 3rd quintile	Lower wealth < 3rd quintile	Female	Male
Dependent var: 1(Sought care at a formal sector health care facility)				
Rain × Distance	−0.0194*	−0.0247***	−0.0180**	−0.0215***
	(0.0112)	(0.00591)	(0.00827)	(0.00659)
Distance	0.00933	0.00841*	0.000553	0.0109*
	(0.0127)	(0.00500)	(0.00699)	(0.00637)
Rain	−0.0365	−0.225**	−0.113	−0.197**
	(0.0973)	(0.109)	(0.0983)	(0.0872)
Observations	663	418	536	545
R^2	0.138	0.328	0.189	0.257
Means within subsample, no formal care				
Mean of fever	0.303	0.408	0.314	0.384
SD of fever	0.460	0.493	0.465	0.487
Mean of malaria	0.226	0.434	0.319	0.311
SD of malaria	0.419	0.497	0.467	0.464

Notes: Robust standard errors are in parentheses. Standard errors are clustered at the sampling cluster level. All specifications include age, region, wealth, education, and month of survey group effects. Other controls include temperature, historical means and standard deviations of both rainfall and temperature, altitude, household size, number of living children, number of children under five, gender, and a dummy for whether the household is located in a rural or urban area. Sample sizes are reduced by some observations in these specifications due to missing values in the outcomes. Results from linear probability model estimations shown.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

C. *Mechanisms of Impact*

Given the large estimates of effects on fever and malaria, we explore potential mechanisms through which care in the formal health care sector might improve these outcomes. While there are several possible mechanisms, we focus on three here due to data limitations.

Receipt of Medication.—We begin by estimating the effects of formal sector care on the number and type of medications taken by acutely ill children. Improving access to care in the formal sector may simply entail enabling access to appropriate medications. DHS contains self-reported medications bought or received for all children who reported being sick with fever in the two weeks preceding survey.

We look first at the quantity margin, namely, the total number of medications and the number of medications conditional on buying or receiving at least one medication. These results are reported in columns 1 and 2 of Table 5. The coefficient estimates on formal sector care (0.57 and 0.22, respectively) are positive and fairly large—about 38 and 13 percent of their respective means—but both are statistically insignificant. In columns 3 and 4, we examine the type of medications received, aggregating malaria and nonmalaria medications. Again, we find a similar pattern: the coefficient estimates on formal sector care are positive, large compared to their

Table 5—Effects of Health Care Choice on Number and Types of Medications Obtained

	Any medication		Medications by type		Quality of malaria medication	
	Number of meds (1)	Number of meds (conditional on number > 0) (2)	Number of malaria meds (3)	Number of nonmalaria meds (4)	Artemisinin-based combination therapy (ACT) obtained (5)	ACT conditional on any malaria medication (6)
Formal health care	0.572	0.218	0.283	0.290	0.299	0.308
	(0.410)	(0.345)	(0.279)	(0.356)	(0.245)	(0.273)
Distance	−0.000531	−0.00519	0.000886	−0.00142	−0.00368	−0.00760
	(0.00619)	(0.00549)	(0.00437)	(0.00546)	(0.00387)	(0.00582)
Market	−0.00227	−0.00242	−0.00231	3.12e-05	0.000107	0.00144
	(0.00243)	(0.00210)	(0.00143)	(0.00187)	(0.000969)	(0.00166)
Rain	0.0546	0.122	−0.0473	0.102	−0.133*	−0.171*
	(0.133)	(0.119)	(0.0837)	(0.125)	(0.0786)	(0.0911)
Rain × Market	0.000743	0.000493	0.000697	4.60e-05	−8.84e-05	3.05e-05
	(0.00185)	(0.00153)	(0.00121)	(0.00131)	(0.000650)	(0.00103)
Observations	1,049	931	1,049	1,049	1,078	565
R^2	0.205	0.117	0.290	0.146	0.209	0.237

Notes: Robust standard errors are in parentheses. Sample reductions in columns 1, 3, and 4 are due to missing observations in the outcomes. Further sample reduction in column 2 is due to conditionality in outcome. Standard errors are clustered at the sampling cluster level. All specifications include age, region, wealth, education, and month of survey group effects. Other controls include temperature, historical means and standard deviations of both rainfall and temperature, altitude, household size, number of living children, number of children under five, gender, and a dummy for whether the household is located in a rural or urban area. Sample sizes are reduced by some observations in these specifications due to missing values in the outcomes. Results from linear probability model estimations shown.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

respective means, but not statistically significant. Thus, if there was an increase in the number of medications, we unfortunately cannot detect it with confidence.

Finally, we look at one dimension of the quality margin: access to artemisinin-based combination therapy (ACT). ACT became the front-line antimalarial therapy in Tanzania in 2006 (a year before the DHS survey data we use were collected). The new therapy was much more effective than existing antimalarials at treating *falciparum* malaria, by far the most common type of malaria in Tanzania (Arrow, Panosian, and Gelband 2004).

ACT was initially available only in formal sector health facilities (Adhvaryu 2014) (though leakage to the private sector was common). Indeed, in our data, 32 percent of children visiting health facilities with recent fever received ACT, while less than 4 percent of children who sought informal care purchased ACT. Of course, these differences are not causally attributed to formal sector usage. We thus ask: did formal sector care enable differential access to ACTs? The evidence from columns 5 and 6 of Table 5 suggest the answer is yes, but our estimates of the impact of formal sector care on ACT access are imprecise. Column 5 uses the full sample, while column 6 reports results conditional on obtaining any malaria medication. Both estimates suggest a large impact (just under 30 percentage points), but both are not estimated with precision. We interpret these results as suggestive evidence on access to higher quality malaria medication for children in the formal sector.

Medication-Related Behaviors.—Policymakers often lament the "Last Mile" problem in global health: even when essential medicines are made accessible, promoting acceptance and proper usage in the population—the "last mile"—is often ignored or not incentivized. Since even the most effective medicine will not have its full effects if it is not taken up and used properly, solving this problem may have high health returns.

Accordingly, in addition to access to medications, we examine two important "last mile" behaviors: adherence to malaria therapy and delay before medicating. For each antimalarial reported as having been used as treatment, we construct indicators for whether the individual adhered to the full regimen for that particular antimalarial. We construct adherence measures based on self-reported number of doses of each therapy that was taken. We use the following variables: an indicator for adherence to at least one antimalarial; the number of antimalarials to which the patient adhered; and an indicator for adherence to all antimalarials taken. Details of the construction of these variables are provided in the online Appendix.

We begin by examining adherence not conditional on receipt of antimalarial. That is, we combine the indicators for receipt of antimalarial with adherence to the particular antimalarial(s) received. Thus, the dependent variables in these regressions are only equal to 1 (or greater than 0 in the case of the count variable) if an antimalarial was received *and* the dosing regimen was completed.

The second-stage results on the effects of formal sector care on these unconditional adherence measures are reported in columns 1–3 of Table 6. Across the adherence measures, the coefficient estimates on formal sector are positive, fairly large, and statistically significant. For example, formal sector care increases the probability of receiving *and* adhering to at least one malaria therapy by nearly 60 percentage points, or nearly 100 percent of the mean of this adherence measure. In columns 4–6, we run the same specifications as in the previous columns but restrict the sample to children who took at least one antimalarial. In these conditional regressions, the coefficient estimates on formal sector care are positive and of similar magnitude to the previous (unrestricted sample) estimates, though not statistically significant.

Finally, we estimate the effect of formal sector care on delay (number of days) before receiving malaria medication. We define this variable as the number of days after falling ill the child begins taking malaria therapy. Of course, the delay variable is only defined for children who eventually got at least one antimalarial. The coefficient estimate, reported in column 7, is negative and statistically significant: formal sector reduces delay to medication by about 1 day.

Overall, while we cannot conclude with confidence that access to malaria therapy matters in mediating the impact of formal sector care on health, the results described above indicate that formal care does improve access *and* appropriate usage of antimalarials, and that those who use the formal health care sector are less likely to delay malaria medication. Although data limitations restrict our ability to explore additional mechanisms, these results suggest that information regarding optimal medication-related behaviors is an important mediating mechanism for the impact of formal care on health outcomes.

These results seem sensible in light of two important facts. First, malaria is a known quantity in this context, and thus information regarding its causes, treatment,

Table 6—Effects of Health Care Choice on Adherence and Delay to Medication

	Unconditional			Conditional on receipt of malaria medication			
	Adhered to at least 1 med (1)	Number of meds adhered to (2)	Adhered to all meds (3)	Adhered to at least 1 med (4)	Number of meds adhered to (5)	Adhered to all meds (6)	Days delayed before medicating (7)
Formal health care	0.560**	0.506*	0.579**	0.427	0.343	0.440	−1.328**
	(0.277)	(0.285)	(0.278)	(0.314)	(0.322)	(0.315)	(0.578)
Distance	0.00287	0.00279	0.00321	0.00481	0.00420	0.00530	−0.00618
	(0.00477)	(0.00476)	(0.00471)	(0.00583)	(0.00588)	(0.00577)	(0.0119)
Market	0.000777	0.000771	0.000967	0.00446**	0.00453**	0.00471***	−0.00447
	(0.00139)	(0.00141)	(0.00136)	(0.00179)	(0.00179)	(0.00179)	(0.00417)
Rain	−0.0659	−0.0485	−0.0669	−0.0844	−0.0528	−0.0916	0.0551
	(0.0948)	(0.100)	(0.0950)	(0.103)	(0.113)	(0.104)	(0.238)
Rain × Market	−0.000731	−0.000761	−0.000877	−0.00232*	−0.00249*	−0.00255*	0.000279
	(0.00122)	(0.00128)	(0.00122)	(0.00140)	(0.00146)	(0.00140)	(0.00328)
Observations	1,049	1,049	1,049	565	565	565	554
R^2	0.065	0.096	0.042	0.036	0.073	0.024	−0.003

Notes: Robust standard errors are in parentheses. Sample reductions in columns 1 through 3 are due to missing observations in the outcomes. Further sample reduction in columns 4–7 are due to conditionality in outcomes as well as missing values. Standard errors are clustered at the sampling cluster level. All specifications include age, region, wealth, education, and month of survey group effects. Other controls include temperature, historical means and standard deviations of both rainfall and temperature, altitude, household size, number of living children, number of children under five, gender, and a dummy for whether the household is located in a rural or urban area. Sample sizes are reduced by some observations in these specifications due to missing values in the outcomes. Results from linear probability model estimations shown.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

and prevention are more or less well known. Yet, there is large heterogeneity here. For example, Björkman-Nyqvist, Svensson, and Yanagizawa-Drott (2013) report, from a study in Uganda, that 34 percent of the households have "severe" misconceptions about the transmission of malaria. Extrapolating a bit to the East African context, the data from Björkman-Nyqvist, Svensson, and Yanagizawa-Drott (2013) suggest that while most people have a basic understanding of malaria, a nuanced understanding of the disease, and of its treatment, may not be common. Second, while it does seem unlikely that people with frequent interaction with antimalarials forget how to take medication, it seems plausible that individuals do not know what the age-specific treatment regimen is for the particular antimalarial they take (e.g., a smaller dose is prescribed for younger children, and different combination therapies (ACTs) have different regimens) (Kachur et al. 2004); and salience might matter, so that a reminder from the doctor to finish the regimen may improve adherence even if the patient is familiar with the regimen already (see, e.g., Pop-Eleches et al. 2011).

Effects on General Health Knowledge.—Table 7 reports estimates from second stage IV regressions on indices measuring the mother's information about the existence of tuberculosis and sexually transmitted diseases including HIV/AIDS, the transmission of these diseases, and the existence of treatments for these diseases.[23]

[23] The corresponding first-stage regression results from this mother-level sample are reported in the second column of Table 2.

Table 7—Effects of Health Care Choice on Health-Related Information

		Information by topic		
	Composite (1)	Information about disease (2)	Information about transmission (3)	Information about treatment (4)
Formal health care	−0.421	−0.0276	−0.351	0.358
	(2.086)	(0.575)	(1.473)	(0.754)
Distance	−0.0510	−0.00666	−0.0546**	0.00664
	(0.0321)	(0.0111)	(0.0219)	(0.0134)
Market	−0.0188**	−0.000181	−0.0110*	−8.97e-05
	(0.00816)	(0.00310)	(0.00572)	(0.00332)
Rain	−0.457	0.0653	−0.262	−0.0253
	(0.693)	(0.207)	(0.451)	(0.262)
Rain × Market	0.0113*	−0.00178	0.00821**	2.53e-05
	(0.00582)	(0.00224)	(0.00401)	(0.00251)
Observations	845	963	943	854
R^2	0.258	0.157	0.215	0.170

Notes: Robust standard errors are in parentheses. Sample sizes are reduced by some observations in these specifications due to missing values in the outcomes. Standard errors are clustered at the sampling cluster level. All specifications include age, region, wealth, education, and month of survey group effects. Other controls include temperature, historical means and standard deviations of both rainfall and temperature, altitude, household size, number of living children, number of children under five, gender, and a dummy for whether the household is located in a rural or urban area. Sample sizes are reduced by some observations in these specifications due to missing values in the outcomes. Results from linear probability model estimations shown.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Column 1 of Table 7 reports estimates of the effect of a visit to formal care on the composite index of all this disease related information. We find no significant effects on health-related knowledge of the mother. Though we lack precision, the point estimate is quite small (less than 0.5) in absolute value, as compared to a mean of 14. Columns 2–4 show results on subindices that measure information on the specific topics of disease existence, transmission, and treatment, respectively. We do not find significant effects on information by topic, with similarly small point estimates. Overall, the evidence presented here indicates that formal sector care does not appear to affect mothers' knowledge about common diseases and their treatment.

D. *Instrument Checks*

Finally, we assess the importance of several threats to the validity of our empirical strategy. We describe these analyses in brief here and in more detail in the online Appendix.

Differential Effects of Rainfall by Remoteness.—We address the concern that fluctuations in rainfall might have differential effects across remote versus nonremote locales, and this disparity may drive differences in health outcomes across the two types of areas. Since distance to the nearest health facility is likely to be correlated with general remoteness, the interaction of distance with rainfall may not be excludable if the differential effects of rainfall by remoteness are not adequately accounted for. To address this problem, we include the interaction of rainfall with a proxy for remoteness (as well as including its main effect)—distance to the nearest marketplace. We are thus absorbing the variation in health facility distance that is associated with this proxy for remoteness.

As further evidence, we report in the first column of online Table A3 results from the first-stage regression including additional controls of interactions of rainfall with all other covariates of the household. If the interaction instrument is merely picking up on some unobservable remoteness or lack of access to resources of the household, which is then exacerbated by rainfall and ultimately predictive of health outcomes irrespective of health care choice, we should expect that controlling for the interaction of rainfall with demographic covariates such as wealth, education, household size, etc. would attenuate the coefficient on the instrument in the first-stage regression. We find, however, that the first stage, and in fact most of the second stage results reported in columns 2–6, are robust to these additional controls.

Selection into Self-Reporting of Acute Illness.—We address the concern that the instrument could change the self-reporting of acute illness. Perhaps in remote areas, high rainfall could result in differentially more sick individuals, or more individuals *perceiving* themselves to be acutely ill. This compositional shift could then account for part of the observed impact on health outcomes.

We measure the extent of this problem by regressing a dummy for reporting recent fever (the sample inclusion criterion) on the instrument, the main effects, and all controls as described above. The results are reported in column 1 of online Table A4. We estimate a small coefficient tightly bound around zero, indicating that the instrument does not appear to induce greater reporting of recent acute illness.

Falsification Tests.—We perform a variety of falsification tests. We answer the following questions: does the instrument drive changes in malaria for the nonacutely ill sample (no fever in the past two weeks); and does past (one month before acute illness) or future (one month after acute illness) rainfall have a differential effect on health care choice by health facility distance?

We report the results of these empirical tests in columns 2–4 of online Table A4, and discuss the specifications and results more in depth in the online Appendix. The results suggest that the answers to both the above questions are in the negative, lending further support to our identifying assumptions.

Robustness Tests.—We perform a variety of additional robustness tests. First, we ask if the coefficient on the instrument in the first stage is affected by the inclusion of interactions of past and future rainfall with health facility distance (online Table A5, column 1). Second, we ask whether the second stage results are affected by the inclusion of interactions of past and future rainfall with health facility distance (in both stages) (online Table A5, columns 2–6). Third, we test whether the first and second stage results are robust to the inclusion of nonlinear health facility distance terms (online Table A6). We find in each case that the results are robust to the inclusion of additional controls. A more in-depth discussion is contained in the online Appendix.

E. *Correlation of the Instrument with Severity*

Lastly, we check whether children living farther away from a health facility are more severely ill on average and require more rainfall to discourage their health-seeking. In order to address this concern, we control for measures of severity and their interaction with rainfall and check whether our main results are preserved. Online Table A7 reports results from first stage and select second stage regressions including additional controls of rainfall in the month of reported illness interacted with anemia and a dummy for whether the child is under the age of one as measures of severity as well as the main effects of these measures. The results suggest that the results are not driven by a correlation between the instrument and severity of illness.

V. Conclusion

This paper estimates the returns to treatment in the formal health care sector in a nationally representative sample of children in Tanzania. Using geographic and temporal variation in the cost of formal sector care to isolate exogenous variation in health care choice, we estimate large reductions in fever and malaria for sick children receiving treatment in the formal health care sector. These reductions are at least in part due to timely access to and appropriate usage of antimalarial therapy.

Our results underscore the importance of public sector health facilities in the management of fever for children in resource-poor environments. Policymakers and academics alike have, perhaps rightfully, levied criticism on the inadequate quality of health care at formal sector facilities. Nevertheless, we find that, at least for the population on the margin of treatment, these facilities do much better than self-treatment or traditional remedies.

Our results also speak to the importance of tackling the Last Mile Problem in global health. When we examine mechanisms of impact, we find the strongest effects of formal sector care on the receipt of *and adherence to* antimalarial therapy. Even the most effective treatments need to be taken properly to realize their full effectiveness: incentivizing patients to adhere to treatment regimens, either through the transfer of knowledge or through monetary incentives, may have large health returns.

In related work (Adhvaryu and Nyshadham 2012, 2014), we estimate the schooling and labor supply impacts of formal sector care access for children with fever, and examine how households' self-employment patterns change in response to treatment in the formal health care sector. Given the potential gains to improved access, future research should rigorously test the impacts of policy solutions that improve access to care. For example, governments could invest in transportation voucher programs in rural areas to subsidize the cost of traveling to the facility on common transportation forms, e.g., buses or rented bicycles. In particularly remote populations, where health facilities are effectively too far to travel to, policy could focus on equipping community health workers with proper diagnostics and treatment for common illnesses, and providing training that emphasizes timely treatment and adherence to treatment regimen.

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