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Labor Supply, Schooling and the Returns to Healthcare in Tanzania

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Labor Supply, Schooling and the Returns to Healthcare in Tanzania

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Abstract

We estimate the effects of higher quality healthcare usage on health, labor supply and schooling outcomes for sick individuals in Tanzania. Using exogenous variation in the cost of formal sector healthcare to predict treatment choice, we show that using better quality care improves health outcomes and changes the allocation of time amongst productive activities. In particular, sick adults who receive better quality care reallocate time from non-farm to farm labor, leaving total labor hours unchanged. Among sick children, school attendance significantly increases as a result of receiving higher quality healthcare, but labor allocations are unaffected. We interpret these results as evidence that healthcare has heterogeneous effects on marginal productivity across productive activities and household members.

Keywords: Labor supply, health shocks, schooling, Tanzania
JEL Codes: I10, J22, J43, O12

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1 Introduction

While much has been written regarding the effects of access to healthcare–via health insurance coverage–on individuals' labor force participation and labor supply in the United States[1], little is known about the effects of using better quality healthcare on labor outcomes in developing countries. This question is important, and its answer likely distinct across developed and developing contexts, for at least three reasons.

First, a large body of literature shows that better health increases labor supply, productivity and wages in developing countries.[2] Healthcare investment, as it mitigates the deleterious effects of health shocks, may thus have an important impact on the same labor outcomes for sick individuals. Second, populations in developing countries generally have lower stocks of health (measured along many dimensions) than those in the developed world. The theory of efficiency wages would imply that the link between health and labor is likely stronger in less healthy populations, a stipulation which is indeed borne out in data comparing developed with developing countries (Strauss and Thomas 1998) and the same countries over long periods of time (Costa 1996). Third, households in developing countries are often exposed to great risk from illness due to a lack of formal insurance (or other income-smoothing) mechanisms and the imperfection of informal insurance (Gertler and Gruber 2002). The use of high quality healthcare may mitigate this risk by improving not only health but labor outcomes as well.

This paper aims to estimate the effects of healthcare on labor supply and schooling following an acute health shock in Tanzania. While the effects of access to particular health interventions (Thomas et al. 2006; Thirumurthy et al. 2009), caloric consumption and nutritional status (Strauss 1986; Deolalikar 1988; Foster and Rosenzweig 1994), and health endowments (Pitt et al. 1990) on labor outcomes have been well documented, our study is different from this previous work in two ways. First, we focus on treatment for acute illness. Mitigating the effects of acute health shocks likely leads to a different time use response than nutritional or long-term care interventions.

[1] For an overview of this literature, see Currie and Madrian (1999).

[2] For a review of this literature, see Strauss and Thomas (1998) and Thomas and Frankenburg (2002).

Second, we focus on the effects of using formal sector healthcare on labor outcomes. The only other study to our knowledge which studies such effects in a developing country context is Dow et al. (1997), which evaluates a healthcare price experiment in Indonesia. While Dow et al. (1997) successfully link shifts in the localized price of healthcare to changes in labor outcomes, their analysis, as a product of the experimental design, is reduced-form: the study is not able to estimate the structural effect of *choosing into* higher quality healthcare amongst those on the margin. Our analysis is the first in the developing country setting to estimate the effects of choosing higher quality healthcare on the health and labor supply outcomes of sick individuals. An analogous study in the developed country context does not exist to our knowledge, and is indeed less relevant in that context given that the availability of health insurance largely shifts the notion of healthcare choice from *ex post* to *ex ante*.

Similarly, the effects of nutrition on school enrollment for children have been investigated in the developing country context (Glewwe and Jacoby 1995; Alderman et al. 2001). Neither nutritional status nor school enrollment, however, are short-term state variables. We examine the effects of *acute* illness on an *acute* schooling outcome, attendance, which is most likely to respond to such short-term fluctuations in health status. Moreover, though several papers have examined the tradeoff between child labor and school attendance (Beegle et al. 2006, 2009; Kruger 2007), our study is the first, to our knowledge, to explore the effects of variations in health, an important determinant of the relative productivity of the child in school and work, on the short-term allocations of time across both schooling and different types of labor.

Inasmuch as healthcare investments mitigate the negative effects of health shocks, we should expect to see (as has been shown in the United States) that using formal sector healthcare leads to improvements in health outcomes in acutely sick individuals. The nature of the labor supply response, however, is likely distinct from the developed country context, for at least two reasons. First, we measure labor responses to healthcare in the event of acute illness, whereas the evidence from the United States relates to longer-term shifts such as health insurance access (Currie and Madrian 1999) and chronic illness (Zhang et al. 2009). Second, there is much more informality in labor markets in developing countries, especially in agrarian societies. The joint determination of healthcare and labor allocations for individuals in these societies often involves not only the choice of how many hours to work, but also how to divide total time across several productive sectors–for example, farm labor, outside (non-farm) employment, self-employment and home labor (and in the case of children, time in school). One possibility in this context is thus that individuals reallocate labor from more strenuous to less strenuous sectors as an adjustment to illness, in addition to (or instead of) simply drawing down total labor hours.

We examine the labor supply and schooling effects of choosing formal-sector healthcare for acute illness in a region of northwest Tanzania. In order to overcome the well-known problem of self-selection into healthcare choices[3], we use an instrumental variables (IV) strategy which exploits exogenous variation in cost of formal sector care. Following Adhvaryu and Nyshadham (2010a), we propose an interaction instrument. We interact a dummy variable for the presence of a formal sector health facility in one's community with the number of days of rainfall in the month of the individual's sickness, and exclude only this interaction from the second stage, while controlling for the main effects of facility "existence" and days of rainfall in the first and second stages of a two-stage instrumental variables estimator. We find that the instrument is sufficiently predictive in the first stage; it is also robust to a variety of additional controls, and passes various falsification tests, all of which are discussed in detail in section 3.

Using this strategy, we first verify that higher quality healthcare, in fact, induces significant improvements in health outcomes for sick individuals. Then, we employ data on individual time use to estimate effects on individual labor supply and schooling. For the time use analysis, we split the sample into children (between the ages of 7 and 14) and adults (15 and over).

For adults, we find that there is no significant effect on total labor supply, though the coefficient suggests a slight increase in total labor hours as a result of using higher quality care. There is, however, a significant reallocation of time across sectors associated with receiving formal-sector care. In particular, when adults fall acutely ill, they shift away from strenuous farm labor and towards less strenuous home labor. When encouraged exogenously to visit formal health-

[3]Individuals with more severe illnesses are more likely to select into higher quality (and higher price) healthcare options. Thus, comparing outcomes across individuals who used different healthcare options will lead to a biased estimate of the impact of using higher quality healthcare.

care, adults shift their time use back towards farm labor. The magnitudes of the estimated substitution effects are large: individuals on the margin who visited formal-sector care worked nearly 40 hours more on the farm in the week prior to survey.

We replicate this analysis for children ages 7 to 14 (for whom time use data are recorded). We find no significant labor supply effects for children; moreover, the effect size estimates are small compared to their respective means. However, we do find a large increase in school hours as a result of better quality healthcare. Our results indicate that for children, acute sickness and corresponding investments in healthcare affect the marginal productivity of schooling to a greater extent than the marginal productivity of child labor.

This study contributes to the ongoing policy debate regarding the benefits of increased access to quality healthcare. Health policymakers seeking to estimate the effects of improving or adding to the stock of healthcare infrastructure or increasing access to existing infrastructure, for example, should take into account labor supply and human capital effects when making cost-benefit calculations. From a methodological perspective, our instrumental variables strategy may be useful in measuring the effects of healthcare choices (in response to acute illness) on short-term health and economic outcomes in other developing country contexts.

The remainder of the paper is laid out as follows. Section 2 describes our dataset. Section 3 presents our identification strategy and discusses its validity. Section 4 presents the empirical results. Finally, section 5 concludes.

2 Data

2.1 Overview

This study uses survey data from the Kagera region of Tanzania, an area west of Lake Victoria, and bordering Rwanda, Burundi and Uganda. Kagera is mostly rural and primarily engaged in producing bananas and coffee in the north, and rain-fed annual crops (maize, sorghum, and cotton) in the south. The Kagera Health and Development Survey (KHDS) was conducted by the World Bank and Muhimbili University College of Health Sciences (MUCHS). The sample consists of 816 households from 51 "clusters" (or communities) located in 49 villages covering all five districts of Kagera, interviewed up to four times, from Fall 1991 to January 1994, at 6 to 7 month intervals. The randomized sampling frame was based on the 1988 Tanzanian Census.[4]

KHDS is a socio-economic survey following the model of previous World Bank Living Standards Measurement Surveys. The survey covers individual-, household-, and cluster-level data related to the economic livelihoods and health of individuals, and the characteristics of households and communities. In the following paragraphs, we outline the variables we use in our analyses.

2.2 Health variables

In the health module of the KHDS, all household members are asked about chronic illnesses and acute illness episodes; care sought for these episodes; and current illness (at the time of survey).[5] As our main sample restriction, we use information on whether individuals were sick with an *acute illness*, i.e. one which began 14 days or less before the date of survey. We also restrict our attention to individuals at least 7 years of age, as data on time use were not collected for individuals younger than 7.

Table I shows summary statistics for the Kagera sample. Across the four waves of the survey, the data report, amongst individuals aged 7 years and above, nearly 4400 individual-year observations of sickness (that is, each time an individual reports being sick he is counted as an individual-year observation). The proportion of the sample of individuals aged at least 7 years reporting an acute illness that began in the two weeks preceding survey is 0.284. Of the acutely ill, roughly 41% report still being ill at the time of survey. About 22% of this sample sought formal sector healthcare for their illness episode, where formal sector healthcare is defined as care at a hospital, health center or dispensary (which includes government, NGO, and private

[4]A two-stage, randomized stratified sampling procedure was employed. In the first stage, Census clusters (or communities) were stratified based on agro-climactic zone and mortality rates and then were randomly sampled. In the second stage, households within the clusters were stratified into "high-risk" and "low-risk" groups based on illness and death of household members in the 12 months before enumeration, and then were randomly sampled.

[5]In the case of individuals below the age of 15, the primary caretaker of the child is asked to answer on the child's behalf.

facilities).

2.3 Labor variables

The time use module of the KHDS collects detailed information on various types of productive activity for all individuals seven years of age and older. Individuals are asked how many hours in the past 7 days they spent in each of a variety of activities. We construct a composite variable for total labor hours in the week preceding survey, as well as breakdowns into several important types of labor activities.

In particular, we first split total labor hours into farm hours and non-farm hours. Then, we further split non-farm hours into self-employment hours and home hours. Farm hours include time spent on the individual's own farm, on a community farm, on someone else's farm (as wage employment), herding time, and time spent making farm produce, among other farm-related activities. Self-employment includes any non-farm activities the profit from which accrues to the individual (as opposed to working for someone else's business). This may include household enterprise, production or sale of market goods, or owning another type of small business (restaurant, hotel, etc.). Home hours include time spent in household chores, and time spent collecting water and firewood.

We see in the summary statistics reported in Table I that among all sick individuals, labor hours are split roughly equally between farm and non-farm activities. Within non-farm labor, the majority of hours are spent performing home chores. Interestingly, we see also that among sick individuals, those who visited formal sector care and those who did not have almost exactly the same time allocations across sectors. Even the total labor supply is very similar across healthcare choice subsamples, with those who did not seek formal-sector care showing a slightly larger mean in total labor hours. This might correspond to the role of severity in healthcare choice (that is, those who chose not to seek formal care might be less severely ill on average); however, the differences in means are quite small in comparison to the standard deviations and so do not constitute strong evidence of anything in particular.

Among sick children, farm labor makes up roughly a third of total labor hours and non-farm makes up two-thirds. Again, non-farm labor is made up mostly of home chores. The labor hour levels are significantly smaller among the subsample of children as compared to the full sample discussed above. It appears that the remainder of the productive time of children is spent in school. We see large means in school hours of roughly 22 hours across the whole sample of sick children and subsamples of those who did and did not seek formal care. Similarly, there is very little variation in labor hour allocation across these subsamples.

This lack of variation in mean labor and school hours across subsamples of those who did and did not seek formal-sector care could be evidence of one of two possible cases: either formal-sector care has no effect on labor supply and school attendance, and perhaps even no effect on health outcomes in this population, or the choice of healthcare is endogenous (for example, on the basis of unobserved severity) rendering a simple comparison of means across healthcare choice subsamples and even ordinary least squares (OLS) estimates useless in investigating the effects of healthcare choice on health, labor supply, and schooling outcomes. As mentioned above, the presence of severity bias in OLS estimates is well-established in the literature. Therefore, in what follows, we will propose and employ an empirical strategy which accounts for this bias (see section 3).

2.4 Other individual-, household- and cluster-level variables

We use a variety of individual-, household-, and community-level demographic and socioeconomic characteristics in our regressions. The most important for the purposes of our analysis is the existence (or, to be precise, the lack of existence) of a formal healthcare facility in the cluster.[6] As Table I reports, about 61% of sick individuals lived in communities without a formal-sector healthcare facility. Among those who did not seek formal-sector care, 67% lived in a community without a formal-sector care facility; among those who sought formal-sector care, the percentage is much lower at only 42%.

[6]We use the lack of existence of a formal health facility (instead of simple existence) because we expect the effect of existence on the probability of visiting a formal-sector health facility to be *positive*, whereas we expect rainfall to have an incrementally *negative* effect for individuals living far from a facility. Thus, we construct the slightly awkwardly termed "non-existence" variable for ease of interpretation of the coefficient on the interaction instrument.

As we describe in Section 3, we accordingly control for the direct effect of the lack of a health facility in the community, along with a variety of other variables related to the existence of resources in one's community (existence of a daily market, periodic market, motorable road, public transportation, secondary school, bank, and post office/telephone). Table I shows that access to these resources in general appears to be greater for those who chose formal-sector care. Indeed, this fact is corroborated by the positive correlation between the existence of a health facility in one's community and the existence of the other above-mentioned resources.

We also control for the distance to various types of formal-sector care options if they are not in the individual's community; in particular, we include the distances to the nearest dispensary, health facility, and hospital (*n.b.*: if these options are in the individual's cluster, this variable equals 0).[7]

We include individual-level controls for the number of days before date of survey the individual's illness began (deciles); gender; years of completed schooling (quintiles); and age (cubic polynomial). We include household-level controls for household size (cubic polynomial); total assets owned by the household (quintiles of an asset index generated using principal components analysis); and year of survey (fixed effects). Finally, we include district fixed effects.

In the last four rows of Table I, we find no evidence of significant differences in demographic composition across healthcare choice subsamples. While the empirical strategy proposed below ought to be robust to such demographic differences, their absence is preliminary evidence of the relative importance of access to healthcare as a primary mover of healthcare choice and of healthcare choice as a primary determinant of health outcomes, at the least.

2.5 Rainfall data

We obtained monthly rainfall data from the Tanzania Meteorological Agency spanning from 1980 to 2004.[8] The data set includes the amount of rainfall (in millimeters) per month and total days with rainfall per month for 21 weather stations in Kagera region. The data set provides a

[7] In our empirical specification, we control for quintiles of the distance to each option separately.

[8] The data set is downloadable from the EDI-Africa website: http://www.edi-africa.com/research/khds/introduction.htm.

matching file which reports the closest and second closest weather station to each cluster in the KHDS sample. Two measures of "closest" have been used: a straight-line distance between each cluster and each rainfall station, and a distance measure which takes into account the location topology of the area. We use the straight-line measure definition of "closest," and use the number of days of rainfall in the month the individual was sick as the primary measure of rainfall in our regressions. Further, we match the rainfall observation to the sick individual by taking the rainfall value in the month the individual was surveyed, in the cluster of the individual's residence. If the rainfall value for this cluster-by-month observation is missing, we use the value at the second closest rainfall station to the cluster.

There appears to be, as shown in Table I, significant variation in the number of days of rainfall across all samples. While the means across healthcare choice subsamples are only minimally different, it is interesting to note that the mean days of rainfall is slightly larger on the subsample of sick children who visited formal-sector care, corresponding to a role of severity in healthcare choice. That is, if more rainfall corresponds to more severe illness, and the more severely ill, in turn, are more likely to choose formal-sector care, we would expect to see a larger mean number of days of rainfall in the sample of sick individuals who ultimately chose to visit formal care.

We also control for the number of days of rainfall in the month *prior to* the individual's sickness (as we discuss in Section 3); the historical mean and historical standard deviation of the distribution of rainfall in the given month, computed over all the years of available data for the month in question (quadratic terms of these variables are included as well); fixed effects for the closest rainfall station; deciles for the number of days of rainfall; deciles for the amount of rainfall (in millimeters) in the month the individual fell sick; and interactions of days of rainfall with the existence of resources variables defined in the previous sub-section. For further details on the construction of rainfall variables, please see the Data Appendix.

3 Empirical strategy

Our goal in this section is to propose and discuss the validity of an instrument for healthcare choice, and to discuss how we use the variation induced by the instrument to measure the effects of healthcare choices first on health outcomes and then on individual labor allocations and school attendance.

3.1 An instrument for healthcare choice

Let O_{ij} denote an outcome for individual i in cluster j, let h_{ij} denote the individual's healthcare choice, and let $\mathbf{X}_{ij}$ denote a vector of individual-, household- and community-level characteristics. Consider the following empirical model:

$$O_{ij} = \beta h_{ij} + \mathbf{X}'_{ij}\gamma + \epsilon_{ij}. \tag{1}$$

Measuring the relationship between healthcare choice and health outcomes as shown above in equation 1 likely results in a biased estimate of the effect of h on O, due to unobserved determinants of outcomes in the error term ϵ that are correlated with healthcare choice. In particular, the severity of the health shock likely influences the care option chosen (that is, individuals with higher-severity illnesses will choose into higher quality healthcare options) as well as the outcome (higher-severity illnesses will generate worse health, labor, and schooling outcomes).

To address these endogeneity concerns, we use an instrument for healthcare choice which exploits exogenous variation in the costs of formal-sector healthcare. The instrument builds on the methodology introduced in Adhvaryu and Nyshadham (2010a). A major point discussed in that paper is the fact that the largest costs of formal-sector care in developing countries are often those associated with the opportunity cost (or the direct costs) of travel to the care facility. Distance to the nearest facility (or alternatively, the presence of a formal care facility in one's community) is thus a large determinant of healthcare choice in developing countries, through its effects on costs (Gertler et al. 1987, Mwabu et al. 1995, Mwabu 2009).

Given the importance of proximity to formal-sector care, one might argue that this variable would be a good candidate for an instrument for healthcare choice, particularly in developing country settings. However it is likely, due to endogenous placement of facilities on the basis of a local population's health stock, that the existence of a facility in one's community and distance to the nearest facility are correlated with the error term in a second stage regression with health or labor supply outcomes as dependent variables. Later in this section, we present some evidence that this is the case in our context, as well.

Following Adhvaryu and Nyshadham (2010a), we propose an interaction instrument. Specifically, we interact a dummy variable for the absence of a formal-sector health facility in one's community with the number of days of rainfall in the month of the individual's sickness, and exclude only this interaction from the second stage, while controlling for the main effects of facility "existence" and days of rainfall in the first and second stages of a two-stage instrumental variables estimator.

The two stages of analysis are specified as follows. Define $NoFac_j$ to be a dummy variable which equals 1 if *no* formal-sector health facility exists in cluster j, and R_{ij} to be the number of days of rainfall in cluster j at the time of individual i's sickness.[9] The two-step estimator is written as follows:

$$\text{1st stage:} \quad h_{ij} = \alpha_1 \left(NoFac_{ij} \text{ x } R_{ij}\right) + \alpha_2 NoFac_{ij} + \alpha_3 R_{ij} + \mathbf{X}'_{ij}\alpha_4 + \zeta_{ij} \tag{2}$$

$$\text{2nd stage:} \quad O_{ij} = \beta_1 h_{ij} + \beta_2 NoFac_{ij} + \beta_3 R_{ij} + \mathbf{X}'_{ij}\beta_4 + \epsilon_{ij} \tag{3}$$

The intuition behind the instrument is simple. The main effects of facility non-existence and days of rainfall are likely both negative; that is, not having a facility in one's community and being exposed to more rainfall should, for the purposes of travel costs, discourage formal-sector health facility usage for individuals seeking care. Moreover, heavier rains should discourage individuals who live farther away from a health facility *more* than individuals living in a community with a health facility.

[9] We define the facility "existence" variable in the negative in order to make interpretation of the interaction coefficient easier; of course, changing this variable to reflect the existence of a health facility as opposed to the lack of existence has no effect on the estimation procedure or the results (barring changing the sign of the coefficients on the interaction term and the main effect of facility existence).

Imagine one household who lives next to a facility, while another is located many villages away. In times of dry weather, clearly the household in the community with a health facility will be more likely to choose formal-sector care than the one farther away. But in times of heavy rains, the rain should incrementally deter the farther household even *more* than it does the one just next door.

3.2 First Stage Results

Results from the first stage regressions are presented in Table II. In the preferred first stage specification in column 1, we regress a binary for whether the sick individual chose formal-sector care on the proposed instrument of the interaction of days of rainfall in month of survey and a dummy for the lack of a formal-sector healthcare facility in the individual's community. The results in column 4 of Table II show a significant reduction in the probability of a sick individual choosing formal-sector care when the interaction instrument increases. The F-stat on the instrument coefficient is above 15, with a p-value of just above 0.0001.

In columns 2 and 3 of Table II, we report first stage regression results from specifications identical to that corresponding to the results reported in column 1, but with varying sets of controls. The specification reported in column 3 only includes main effects of days of rainfall in month of sickness, the dummy for lack of a health facility in the community, linear and quadratic terms in historical mean and standard deviation of days of rainfall, quintiles in distances to hospital, health facility, and dispensary, and deciles in days of rainfall and amount of rainfall in month of sickness. The specification reported in column 2 additionally includes dummies for presence of resources in the community and interactions of these dummies with days of rainfall in the month of sickness. The preferred first stage specification reported in column 1 also includes demographic characteristics of the individual and household such as age, gender, household size, household assets, and education.

The instrument has a significant, negative effect on the probability of choosing formal-sector care in all three specifications shown in columns 1-3. This evidence suggests a general robustness of the first stage relationship between the instrument and formal-sector care use to variation in the sets of controls.

In column 4 of Table II, we present estimates of only the main effects of days of rainfall and lack of a health facility in the community on the binary for whether the sick individual chose formal-sector care. In this specification, the interactions between the dummies for the presence of resources in the community (including a health facility) and days of rainfall in month of sickness are not included. Similarly, for the sake of interpretability of the coefficients reported in column 1, the corresponding specification does not include quintiles in distances to hospital, health facility, and dispensary either. As is expected, the main effect of the lack of a formal-sector care facility in the community is negative on the probability of choosing formal-sector care.

Dummies for deciles in the days of rainfall and amount of rainfall in the month of sickness, however, are included to sufficiently control for nonlinear effects of rainfall on healthcare choice which might correlate with unobserved measures of remoteness. The inclusion of these terms renders the interpretation of the estimates of the effects of the linear term in days of rainfall difficult. As we see, though we would expect the effects of rainfall to be significant and negative, we find an insignificant positive effect on the probability of choosing formal sector care. This could be entirely due to the presence of the decile dummies in rainfall in the specification.

3.3 Instrument validity

Ideally, we would like variation in the instrument to be equivalent to experimental variation in the price of formal-sector care. That is, we would like to answer the question, "Holding all other prices constant, if we shift only the price of formal-sector care, how does the demand for this care change, and subsequently, how do these shifts affect health, labor supply, and schooling outcomes?" One crucial element of our argument is thus that the interaction instrument must induce price changes solely in the cost of formal-sector care, as opposed to shifting other prices which determine access to other resources, as well as directly influence consumption and labor allocations.

3.3.1 Controlling for general remoteness

It is plausible that fluctuations in rainfall induce shifts in the prices of non-healthcare goods and services differentially across communities with health facilities as compared with communities without. For example, suppose non-existence of a formal care facility was correlated with a community's general remoteness; that is, communities lacking health facilities lacked access to other important resources (commodity and labor markets, roads, schools, etc.). Since rainfall, through the interaction instrument, acts as a randomized amplifier of the costs of access to formal-sector care, rainfall would amplify the costs of access to these other resources as well. If this were true, the instrument would not be excludable.

To address this problem, we control for the existence of a variety of important resources, as well as the interactions of these variables with days of rainfall.[10] Controlling for these main effects and interactions ensures that the variation induced by the instrument is specific to the costs of formal-sector care.

3.3.2 Instrument does not predict chronic illness amongst non-acutely ill

The main reason we use an interaction instrument is that it improves on using facility existence alone as an instrument for healthcare choice, since, as mentioned earlier, endogenous allocation of health facilities to communities on the basis of the community's health stock would render invalid facility existence as an instrument. Here, we present evidence that this important distinction is relevant in our context.

First, we regress indicators for various chronic illnesses on the facility existence variable alone (along with the full set of controls used across all specifications). The results, reported in columns 1-4 of Table III, show that we fail to accept that facility existence is not correlated with measures of chronic illness in the non-acutely ill population. In particular, in column 2 we see that facility existence is a predictor of chronic weight loss at the 10 percent level of significance.

Second, we include the interaction term (along with the main effects and the full set of con-

[10] For example, we include existence of a daily market, motorable road, public transport, secondary school, and post office or telephone; for a full listing of the variables included, please refer to the note at the bottom of Table II.

trols), and verify that the interaction instrument, in contrast, is not significantly correlated with chronic illness measures. These results are reported in columns 5-8 of Table III. The results indicate that the instrument is not, in fact, a predictor of measures of chronic illness at conventional levels of statistical significance. The results reported in columns 1-8 are all from probit specifications. The extreme value distributions of chronic illnesses render linear probability OLS specifications inappropriate.

Finally, we regress the incidence of sickness (using a binary variable for being sick in the past 14 days) on the full set of regressors including the interaction instrument in a linear probability OLS regression. The results from this regression are reported in column 9 of Table III, and verify that the instrument does not predict selection into sickness.

3.3.3 Isolating transitory rainfall variation

Crucial to the interpretation of the instrument is the hypothesis that rainfall in the month of sickness induces a temporary, randomized amplifying effect in the costs of travel to formal-sector care. Moreover, rainfall generates a larger temporary effect in places where no formal-sector facility exists. To isolate this temporary variation from persistent high rainfall (which is a common phenomenon in our context given that rainy seasons in Tanzania last for months at a time and cause seasonal variation in incomes and opportunity costs of time), we control for the days of rainfall in the month *prior to* the individual's sickness, as well as the interaction of this variable with the facility existence dummy.

3.3.4 Nonlinear effects of endogenous distance

Finally, we allow for the possibility that distance enters the first and second stages nonlinearly. We do this to further preclude the possibility that the interaction instrument is only capturing a nonlinear effect of distance, rather than the interaction of distance with a randomized, transitory source of variation. To account for this concern, we include quintiles of the distribution of distance to the nearest health facility, hospital and dispensary in all regressions.

4 Results

4.1 Health Outcomes

The first column of Table IV presents results from the second stage instrumental variables regression of a binary for whether the individual was still ill at the time of survey on a binary for whether he visited a formal-sector healthcare facility. The results show a large and significant reduction in the probability of still being ill at the time of survey for those sick individuals driven exogenously to formal-sector care. For sick individuals on the margin, being exogenously driven to visit a health facility decreases the probability of still being ill at the time of survey by approximately 60 percentage points.

The magnitude of these results corresponds to the results found in Adhvaryu and Nyshadham (2010a), which applies a similar analysis to a nationally representative sample of children under five in Tanzania. Note that the marked attenuation in the OLS estimates reported in the second column of Table IV is also consistent with estimates from previous studies and corresponds to bias due to self-selection into formal-sector care on the basis of severity.

4.2 Adult Labor Supply

Now that we have established the fundamental links, first, between costs of formal-sector care and healthcare choice and, second, between higher quality care and improved health outcomes, we turn our attention to the effects of formal-sector care on the labor supply of sick adults aged fifteen and over. Specifically, if formal-sector care reduces the length of illness or in particular the probability that the individual is still ill on any subsequent day, then–to the extent that the illness had reduced the individual's total endowment of time, affected his marginal utility of consumption or leisure, or reduced his marginal productivity of labor–we should expect to see effects of formal healthcare on his labor supply.

Table V presents results from second stage IV regressions of adult labor hours on formal healthcare. Column 1 reports estimates of the effects of formal healthcare on total labor hours of the individual. Columns 2 and 3 of Table V report estimates of the effects of formal healthcare on farm and non-farm labor of sick individuals, respectively. Columns 4 and 5 show effects on subdivisions of non-farm labor: self-employment and home chores. Panel A shows estimates of the effects on labor hours in levels; panel B reports the same regressions on each type of labor as a proportion of total labor hours.

The results in column 1 of Table V show a moderate, but statistically insignificant, increase in total labor hours of sick individuals who chose formal-sector care. However, the results in columns 2-5 of both panels A and B show a large and significant substitution from non-farm labor back towards farm labor. That is, in the population of sick individuals on the margin, those driven exogenously to formal-sector care reallocate much of their labor hours towards farm labor.

These results suggest the possibility of a relationship between acute sickness and the capacity for effort in productive activities. That is, if we believe farm labor to require greater effort than the average self-employment or home chore activity, the pattern of labor reallocation found in the data might suggest that individuals substitute away from higher effort (and likely higher yield) productive activities, such as farm labor, when sick and toward lower effort self-employment or home chores. Then, as their health improves due to higher quality healthcare, individuals reallocate their productive time back towards higher-effort, higher-yield farm labor.

Preview studies have established links between nutrition and health and labor productivity (Strauss 1986; Deolalikar 1988; Foster and Rosenzweig 1994), and have explored heterogeneity across productive activities in the degree of sensitivity to health status (Pitt et al. 1990). Here we find evidence of heterogeneous effects of acute health shocks and corresponding investments in healthcare on productivity across activities.

4.3 Child Labor and Schooling

We might expect the effects of formal-sector care on labor supply to be different for children, a population for whom productive time is not necessarily allocated entirely toward various types of labor. In particular, we are interested in knowing how the effects for children of formal-sector care on labor supply compare to effects on school attendance.

In columns 1-5 of Table VI, we report results from regressions identical to those in Table V conducted on the subsample of children aged 7-14. Interestingly, we find no significant effects on total labor supply and labor allocations of children. The standard errors are large due to the reduction in sample size, so we cannot precisely measure the magnitude of the labor supply effects, but the point estimates are quite small as compared to the means of the dependent variables.[11]

On the other hand, in column 6 of Table VI we see a large and significant effect on hours spent in school. Sick children who are driven exogenously to seek formal-sector care spend nearly 24 hours more in school in the week prior to survey than those who are not. Compared to a mean of roughly 22 hours per week, these results amount to an entire week's worth of schooling gained through the use of higher quality care.

Previous studies have explored the effects of health and nutritional status on schooling outcomes (Alderman et al. 2001, Glewwe and Jacoby 1995). We find evidence here that school attendance responds to acute health shocks and corresponding healthcare investment as well. While, as discussed above, much work has been done on the relationship between health status and labor productivity, this study is the first, to our knowledge, to explore the degree to which this relationship differs across adults and children.

The non-zero means in child labor hours suggest that children in our sample do in fact spend time in farm and home labor. However, we find no effects of formal-sector care use on the child's total labor supply, nor on the allocation of labor hours to farm and non-farm activities. These results suggest that acute sickness may affect a child's capacity for effort in school to a greater extent than on effort or productivity in child labor.

Furthermore, it seems acute sickness and healthcare investment have less of an effect on the marginal productivity of child labor than they do on that of adult labor. This might be due to the specific nature of child labor activities. That is, if the productive activities in which a child engages on the farm and in the home require a lower degree of effort than those in which adults

[11]It is important to note that the means of the dependent variables reported in the Table V are for the entire sample on which the regression is conducted; however, the most relevant mean against which to compare the coefficient on formal-sector care is that for the population *on the margin*.

engage, we would expect the marginal productivity of child labor to respond less to a reduction in the capacity for effort as a results of acute sickness than that of adult labor.

5 Conclusion

Acute health shocks and corresponding investments in quality healthcare affect individual labor supply and school attendance. Accurate measurement of the benefits of improved access to quality healthcare should not only include first-order effects on health outcomes but also these second-order effects on labor and schooling outcomes.

The empirical results presented here first verify that higher quality healthcare in response to acute health shocks does in fact improve health outcomes. Second, we show that higher quality healthcare causes a significant shift in adult labor allocation away from non-farm labor such as self-employment or home chores and towards farm labor. We also see an increase in total labor hours, albeit statistically insignificant, among sick adults who sought formal-sector care.

This pattern of results provides suggestive evidence of a relationship between sickness and the capacity for effort in labor activities. That is, when individuals fall ill, they seem to be relatively less productive in strenuous farm activity which requires more physical effort, and consequently, relatively more productive in self-employment and home chores. Furthermore, the empirical evidence seems to suggest that acute sickness, and corresponding investments in healthcare, have larger effects on the relative marginal productivities of labor across farm and non-farm production than on the individual's time endowment or marginal utilities.[12]

Among the subsample of children, on the other hand, we do not find significant effects on total labor supply or labor allocation across productive activities, but rather a large increase in school attendance as a result of formal-sector care. These results suggest that negative shocks to a child's health reduce (and higher quality care consequently increases) the marginal productivity of a child's time in school. However, we find no evidence that the marginal productivity

[12]Models in which sickness reduces the individual's time endowment or reduces the marginal utility of consumption or leisure would predict a reduction in total labor supply as a result of acute sickness, or alternately an increase in total labor as a result of higher quality care.

of his labor is reduced to the same degree. The difference in results across adult and child labor emphasize the heterogeneity in the effects of acute sickness and corresponding healthcare investment on labor productivity within the household.

To the extent that the thinness of the labor market and the heterogeneity in quality across healthcare options in our context are similar to other settings in sub-Saharan Africa, we expect our results on health and labor outcomes to be generalizable to these other contexts. We expect the results on school attendance to be generalizable to many rural, developing settings in which a child's time is split between school and labor activities (especially in home enterprises).

Nevertheless, further research is required to understand the degree to which acute illness, and corresponding healthcare investment, affects productivity and labor supply in developed contexts, in particular when labor markets are more complete or perhaps there exists some degree of job security or frictions to hiring or firing. Additionally, the roles of chronic illness and disability in the household decision process are likely quite different than that of acute illness, particularly in developed contexts where health insurance and disability benefits are common.

Furthermore, in developing country settings in which extended households make resource allocation decisions jointly, especially when the household serves as both a consumptive and productive unit (as it does in rural agricultural households), the intra-household allocation of labor (and reallocation of labor in response to acute health shocks) will play a large role in the labor supply decisions of both sick and non-sick household members (see Adhvaryu and Nyshadham 2010b), and further, in school attendance of children in the household. Exploring the effects of acute illness and corresponding investments in quality healthcare in the context of a household resource allocation problem is thus an important area of further research.

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A Construction of variables

The following list describes the construction of variables used in analysis:

- $sick = 1$ if the individual was sick with an illness that began 14 days or less prior to the date of survey, $sick = 0$ otherwise.
- $h = 1$ if sick individual visited hospital, health center or dispensary (government, NGO or private); $h = 0$ otherwise
- $raindays$ equals the number of days of rainfall at the rainfall station closest to the individual's sample cluster, in the month and year that the individual was surveyed
- $histmean$ of rainfall is the number of days of rainfall in the month of survey averaged over all years in which rainfall data are recorded for that cluster in the particular month
- $histsd$ is calculated as the standard deviation of the historical distribution of days of rainfall in the month of survey, across all years in which rainfall data are recorded for that cluster in the particular month
- $histmeansq$ and $histsdsq$ are smooth polynomials to the second degree in historical mean days of rainfall and historical standard deviation of days of rainfall, respectively
- $raindayslast$ equals the number of days of rainfall at the rainfall station closest to the individual's sample cluster, in the month *before* that in which the individual was surveyed of the same year
- $decraindays$ and $decrainfall$ are categorical variables reporting which decile of the rain days and rainfall distributions, respectively, the rain in the survey month falls; fixed effects for each decile are included in all specifications
- $noexist$ is a binary variable which takes value $noexist = 1$ if neither hospital, health center, nor dispensary of (government, NGO or private) exists in the community, and $noexist = 0$ otherwise (Note: for waves in which these data were missing, the values were filled first

using the minimum from the waves in which the data were not missing for that cluster, and second using the minimum of non-missing values from clusters matched to the same rain station in the same wave; that is, if a facility of these types ever existed in that cluster or in very proximate clusters before or after the year in which the data are missing, we assumed it existed during this wave as well)

- For the following facilities/attributes (x), we calculate distances as $dist(x) = 0$ if the facility/attribute exists in the individual's village; $dist(x)$ equals the distance to the nearest such facility/attribute outside the individual's village if one does not exist in the village (Note: for waves in which these data were missing, the values were filled first using the mean from the waves in which the data were not missing, and second using non-missing data from clusters matched to the same rain station in the same wave)
  - Hospital
  - Health center
  - Dispensary
  - Daily market
  - Periodic market
  - Motorable road
  - Public transportation
  - Secondary school
  - Bank
  - Post office/telephone booth
- Categorical variables for the quintiles of the distributions of the above defined distances to hospital, health center, and dispensary were created and included in all specifications
- $hhsize$, $hhsizesq$, and $hhsizecub$ are smooth polynomials up to the third degree in the number of members of the household

- $age1$, $age2$, and $age3$ are smooth polynomials up to the third degree in the age of the respondent

- $assets$ is a categorical variable measuring the value of all assets of the household; fixed effects for these categorical values are included in all specifications

- $kid = 1$ if $age < 15$

- $someeduc$ is a binary for whether the individual has completed at least primary school education; it is included in all specifications

Table I: Summary Statistics

Summary Statistics for All Sick Individuals, and by Healthcare Choice

	All	Formal Care	No Formal Care
	Count	Count	Count
Individual-Year Observations *(illness in last 2wks, age>=7)*	4361	966	3395
Adult-Year Observations *(illness in last 2wks, subsample age>=15)*	2968	662	2306
Child-Year Observations *(illness in last 2wks, subsample aged 7-14)*	1393	304	1089

	All Mean	All SD	Formal Care Mean	Formal Care SD	No Formal Care Mean	No Formal Care SD
Health Status and Care						
Still Ill	0.412	0.492	0.370	0.483	0.424	0.494
Visited Formal Healthcare	0.222	0.415				
Time Use of Adults (age >=15, hours in week before survey)						
Total	33.595	25.341	32.811	27.715	33.820	24.620
Farm	16.487	15.384	16.748	18.048	16.412	14.532
Non-farm	17.109	20.884	16.063	21.896	17.409	20.580
Self-employment	4.940	18.718	5.137	19.669	4.884	18.440
Home	12.168	11.455	10.926	11.652	12.525	11.375
Time Use of Children (age 7-14, hours in week before survey)						
Total Labor	17.263	14.578	16.170	14.630	17.569	14.556
Farm	6.561	8.494	6.126	8.858	6.682	8.390
Non-farm	10.703	9.737	10.045	9.412	10.886	9.822
Self-employment	0.295	3.632	0.079	0.856	0.355	4.081
Home	10.407	9.100	9.966	9.288	10.531	9.047
School Hours	22.110	14.150	22.000	15.124	22.143	13.849
Costs of Healthcare (Instruments)						
# of days of rain in month of survey	7.913	5.294	8.268	5.351	7.812	5.275
No health facility in community	0.614	0.487	0.421	0.494	0.668	0.471
Resources in Community						
Daily market	0.626	0.484	0.642	0.480	0.622	0.485
Periodic market	0.336	0.472	0.322	0.467	0.340	0.474
Motorable road	0.962	0.190	0.968	0.176	0.961	0.194
Public transport	0.269	0.444	0.325	0.469	0.254	0.435
Secondary school	0.122	0.327	0.158	0.365	0.112	0.315
Bank	0.109	0.312	0.107	0.309	0.110	0.313
Post office/telephone booth	0.137	0.344	0.181	0.385	0.124	0.330
Demographic Characteristics						
Age	27.646	19.190	26.627	17.854	27.936	19.546
Household size	7.158	3.628	7.302	3.604	7.117	3.635
Female	0.544	0.498	0.514	0.500	0.552	0.497
Household assets (Deciles)	5.472	2.868	5.578	2.926	5.442	2.852

Notes: The sample, unless otherwise noted, is made up of individuals aged 7 and above who reported illnesses that began in the two weeks prior to survey.

Table II: First Stage and Robustness Checks

	First Stage Robustness			Main Effects
	All Controls	Distances to Resources x Days of Rainfall	No Controls	No Rain Interactions
Days of Rainfall x No Facility	**-0.0187***** **(0.00461)**	**-0.0156***** **(0.00468)**	**-0.00941***** **(0.00343)**	
No Facility	-0.104* (0.0617)	-0.203*** (0.0569)	-0.126** (0.0538)	**-0.188***** **(0.0247)**
Days of Rainfall	0.0204 (0.0179)	0.0287 (0.0188)	0.0145 (0.0178)	0.00555 (0.0179)
Observations	3879	4160	4176	3879
Mean of Dependent Variable	0.222	0.222	0.222	0.222
F-test: Rain x Distance=0	**16.43**	**11.13**	**7.516**	
Prob>F	**0.0000902**	**0.00112**	**0.00701**	

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the sampling cluster by year level. All specifications inlcude main effects of days of rainfall and "No Facility," assets, eduction, district, rain station, and year of survey group effects; as well as polynomials up to a third degree in age and household size, unless otherwise stated. Specifications also include, unless othewise stated, a dummy for gender; deciles of days of rainfall and levels of rainfall as well as for how long ago the illness started; and quintiles for distance to nearest hospital, healthcare facility, and dispensary. Dummies for the existence of a daily market, periodic market, motorable road, public transport, secondary school, bank and post office/telephone are included; along with interactions of these dummies with days of rainfall, unless otherwise noted. Other controls include historical means and standard deviations of both rainfall and quadratic terms of these; days of rainfall in month prior to survey and its interaction with "No Facility", unless other noted. Sample is restricted, unless otherwise noted, to all invividuals, aged 7 and above, with illnesses that began in the two weeks prior to survey. The specificaiton reported in column 2 excludes demographic characteristics such as household assets, age, education, and gender. The specificaiton reported in column 3 excludes demographic characteristics as well as all interactions of resource dummies with rainfall variables and rainfall in the month prior to survey. The specification reported in column 4 only excludes interactions between resource dummies and rainfall variables.

Table III: Instrument Checks

	"No Facility" Invalid as Instrument				Exogeneity of Instrument				Selection Into Sickness
	Chronic Illness	Chronic Weight Loss	Chronic Rash	Chronic Fever	Chronic Illness	Chronic Weight Loss	Chronic Rash	Chronic Fever	Illness (began last 2 weeks)
Days of Rainfall x No Facility					**0.000199**	**0.00147**	**-0.00206**	**0.000409**	**0.00165**
					(0.00219)	**(0.00165)**	**(0.00144)**	**(0.000716)**	**(0.00277)**
No Facility	**-0.0130**	**0.0126***	**0.00214**	**0.00525**	-0.0351	-0.00271	0.0235*	0.00984	-0.0149
	(0.0129)	**(0.00740)**	**(0.00783)**	**(0.00357)**	(0.0421)	(0.0187)	(0.0132)	(0.00648)	(0.0394)
Days of Rainfall	-0.0112	0.00113	-0.00457	-0.00237	-0.00487	0.00320	0.00467	0.00124	-0.00479
	(0.00825)	(0.00635)	(0.00520)	(0.00261)	(0.00989)	(0.00713)	(0.00589)	(0.00346)	(0.0153)
Observations	6797	6764	6765	6358	6797	6764	6765	6358	13699
Mean of Dependent Variable	0.0989	0.0508	0.0375	0.0210	0.0989	0.0508	0.0375	0.0210	0.284
F-test: Instrument = 0	**1.052**	**2.623**	**0.0732**	**1.878**	**0.00822**	**0.781**	**2.051**	**0.309**	**0.354**
Prob>F	**0.305**	**0.105**	**0.787**	**0.171**	**0.928**	**0.377**	**0.152**	**0.578**	**0.553**

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Columns 1-8 report marginal effects estimates from probit specifications. The extreme value distributions of chronic illnesses render linear probability OLS specifications inappropriate. All specifications reported in this table exclude dummies for deciles of how long ago the illness started. The specifications reported in columns 1-4 also exclude all interactions beteween rainfall variables and dummies for resources in the community. "Instrument" refers to the No Facility dummy in columns 1-4, and to the interaciton of this dummy and days of rainfall in the month of survey in columns 5-9. The samples on which the regressions reported in this table are run is restricted to all individuals aged at least 7 years who reported *not* being ill with an illness that started in the two weeks prior to survey

Table IV: Health Outcomes

Effects of Healthcare Choice on Probability of Still Ill

	Second Stage IV	OLS
	Still Ill	Still Ill
Formal Healthcare	**-0.595****	**0.0300**
	(0.291)	**(0.0236)**
Observations	3877	3877
Mean of Dependent Variable	0.412	0.412

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments.

Table V: Labor Supply of Sick Adults

Second Stage IV: Effects of Healthcare Choice on Labor Supply of Sick Adults

	Total Labor Hours	Farm Labor	Non-Farm Labor		
			Total	Self-Employment	Home
(Adults aged 15 and over with illness that began in the two weeks prior to survey)					
Panel A: Hours					
Formal Healthcare	**6.768**	**39.48****	**-32.72****	**-24.01***	**-8.710**
	(22.63)	**(18.03)**	**(15.32)**	**(13.16)**	**(7.112)**
Observations	2630	2630	2630	2630	2630
Mean of Dependent Variable	33.60	16.49	17.11	4.940	12.17
Panel B: Proportion of Total Labor Hours					
Formal Healthcare		**0.902*****	**-0.902*****	**-0.388****	**-0.513***
		(0.330)	**(0.330)**	**(0.183)**	**(0.275)**
Observations		2429	2429	2429	2429
Mean of Dependent Variable		0.504	0.496	0.0773	0.419

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Sample is restricted to adults aged 15 and over who reported being ill in the two weeks prior to survey.

Table VI: Labor Supply and School Hours of Sick Children

Second Stage IV: Effects of Healthcare Choice on Labor Supply and School Hours of Sick Children

(Children aged 7-14 with illness that began in the two weeks prior to survey)

	Total Labor Hours	Farm Labor	Non-Farm Labor			School Hours
			Total	Self-Employment	Home	
Formal Healthcare	**4.330**	**3.248**	**1.082**	**-1.482**	**2.564**	**23.92***
	(12.90)	**(8.293)**	**(7.282)**	**(1.371)**	**(6.887)**	**(13.74)**
Observations	1249	1249	1249	1249	1249	895
Mean of Dependent Variable	17.26	6.561	10.70	0.295	10.41	22.11

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). See Table II for additional comments. Samples are restricted to all individuals aged 7-14 who reported being ill in the two weeks prior to survey. The sample on which the regression reported in column 6 is run is further restricted to individuals aged 7-14 who also reported being enrolled in school.