Health Facility Characteristics and the Decision to Seek Care

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Health Facility Characteristics and the Decision to Seek Care[1]

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Abstract

Utilisation of healthcare facilities is low in many developing areas. One possible explanation is that treatment costs, in time or money, are high. Another is that parents perceive treatment benefits to be low. We combine Philippines DHS data with a subsequent facilities survey in order to examine these issues with respect to treatment for respiratory infections and diarrhoea in young children. Controlling statistically for the selectivity of the initiating illness, we find that the staffing level of nearby health facilities is a determinant of the probability that parents take their ill children for curative care.

Health Facility Characteristics and the Decision to Seek Care

In the presence of poverty, small changes in family resources or their allocation may lead to unfavourable consequences for children. A case may be made that parents in many cases are fair, even altruistic, allocators of resources under certainty. Even so, poverty coupled with income uncertainty may lead to unsustainably large family sizes, with malnutrition, morbidity, and mortality consequences for children. For example, Jensen and Ahlburg [2001] found that unwantedness was an important determinant of child morbidity in Indonesia and the Philippines. In every case, the acute respiratory infection (ARI) morbidity of a child reported unwanted[4] at conception was increased by at least 10% compared to a wanted child, and in many cases, there were similarly large, statistically significant impacts of unwantedness on diarrheal disease.

Jensen and Ahlburg argued that the underlying mechanism driving their result was malnutrition, as unwanted births forced parents to allocate fewer resources to children. If so, as family resource constraints were tested, one might reasonably expect to see reductions in other resource-intensive activities. One such activity might be the treatment of sick children. The analogy with the determinants of initial illness is not perfect. Providing children nutrition requires labour or some other source of income to buy food, or land to raise it, and providing children curative care may be waiting-time or travel-time intensive but, perhaps because of subsidies, not especially cash-intensive (at least when provided by the public sector). Nonetheless, the allocative impacts of resource scarcity will be greatest where income or the capability to travel is low, where resource demands within the family are high, or where curative care is expensive.

This leads to various income-constraint based comparative static predictions of changes in the likelihood that an ill child receives treatment. Unwanted children stretch family resources, and so, all else constant, they (and perhaps their siblings) should be less likely than children in families where all children are wanted to receive treatment. In similar fashion, children with many siblings should have fewer resources spent on them than would children with fewer siblings, and, if treatment or travel to a service delivery point requires a cash payment, children from higher income families are more likely to be treated than those from low income families (where children are normal goods).

To this point in our introductory discussion we have ignored benefits. In the standard Beck-Lewis [1973] framework, parents' actions are the results of comparing benefits and costs of competing actions on the margin. Pragmatically, it may be reasonable to avoid an explicit accounting of benefits in discussing nutrition, since the underlying transformation function of nutrition into health is not well understood. Contributors to the literature on behavioural models of morbidity or mortality therefore have focused on the presumptive outcome (morbidity or mortality) of allocative decisions. In shifting our emphasis to parents' decision to take a child in for treatment, it becomes simpler to incorporate benefits explicitly. While we still cannot observe directly the benefits perceived by parents, we are able to generate some rough measures of facility quality using observations from service provision points. Simply put, the benefits parents can expect to derive if they take a sick child in for treatment will depend on the quantity and quality of the resources in place at the source of care. A "high quality" facility might be one fully staffed with appropriate personnel, for example. If the parents correctly perceive quality differentials, then those provision points with objectively higher quality also will be perceived by parents to have higher treatment benefits[5]. All else equal, we expect that parents are more likely to seek care if they live in a community where more and better health care resources are available.

To undertake the task, we begin with the 1993 Demographic and Health Surveys (DHS) data [Philippines National Statistics Office and Macro International, 1994], also known as the Philippines National Demographic Survey (NDS). These data provide information about the family's characteristics and health care use patterns, but contain only limited information on treatment facilities. We then merge them with data collected by Stewart *et al.* [1997] in selected NDS clusters as part of a study of family planning delivery costs. The key variables contained in this study are staffing measures, and we are able to generate measures of resources per capita expended at public clinics. We also generate a rudimentary measure representing the level or quality of infrastructure, based on whether facilities have running water, working equipment, and so forth. We then use these measures, travel time (a measure of the time cost of treatment), and family and individual level variables to estimate multilevel regression models of treatment for ARI and diarrhoea, contingent on illness. The explanatory variables in these regressions represent characteristics of both family and health providers, but we emphasize that they share the theoretical justification of being elements of the cost-benefit calculation parents with a sick child undertake in deciding whether to take the child for medical treatment.

1. Within-family allocations

We make the claim that parents are sufficiently economically rational to withhold care from children where costs of treatment exceed benefits. To support the notion that parents are in fact rational, we begin by appealing to the literature on resource allocation within families. We pay particular attention to analyses of events that may be construed as natural experiments of sorts, such as an unwanted pregnancy, the birth of a girl or of twins, or the occurrence of famine. In doing so, we maintain the view that parents may be rational planners, but also may face unforeseen disruptions in their plans. If this is the case, their responses may reflect their preferences or constraints.

For example, the child mortality consequences of 1970s famine in Bangladesh were severe. They were especially so for girls. This is consistent with a rational resource allocation decision in response to an exogenous income shock, where preferences favour sons. Chen *et al*. [1981] discussed the mechanism by which this outcome obtained, showing that girls always were fed less than boys and that the deeper the famine, the more pronounced the mortality differential by sex became. Rosenzweig and Wolpin [1980] showed that the exogenous shock of a twin birth is followed by reduced educational attainment for Indian children, compared to children in families without twin births. Compared to those without twins in India, Indonesia and Malaysia, Rosenzweig [1990] found that siblings in families with twin births attain 17% fewer years of schooling if the twin birth is a first or second birth, but 34% fewer years of schooling if the twin birth is a third or fourth birth. This pattern is consistent with parents planning certain allocations, and being less able to accommodate exogenous disruptions to these plans later in their life cycles.

Using data from the Philippines and Indonesia, Jensen and Ahlburg [2001] showed that occurrences of unwanted births, another sort of exogenous shock, are associated with children who are sicker than those in families without unwanted births. In contrast to the results from South Asia [*e.g.*, Chen *et al.* 1981, Das Gupta 1987], and perhaps reflecting cultural differences between South and Southeast Asia, they did not find support for the notion that females might be disadvantaged. Myhrman *et al*. [1995] found that unwanted Finnish boys suffer little in terms of their educational attainment by age 20, but unwanted Finnish girls have less education than do wanted girls. Incomes are high enough in Finland that unplanned births would not be expected to carry health consequences for the children, but apparently, they cause some strain on family budget constraints nonetheless. Joyce *et al.* [2000] inject a cautionary note into the debate on the responsiveness of parents to unplanned events. They find little effect of unwantedness on child well being, measured in terms of birth weights and cognitive abilities through age 13, in the (US) National Longitudinal Survey of Youth. They attribute the commonly-observed correlation between unwantedness and birth outcomes to the joint effect of income, educational attainment and other variables on unwantedness and child well being.

The literature provides substantial justification for the notion that parents typically are rational decision makers who, when forced to respond to chance events, make some difficult choices regarding resource allocation. In this sense, exogenous shocks constitute strokes of statistical fortune to the researcher, because they serve to magnify existing behavioural differentials. But, the fundamental source of observed behavioural differences is response to varying degrees of resource scarcity, and with or without the occurrence of exogenous shocks, those who face a tight income constraint are more constrained in their choices than are those with more income (or fewer demands on that income). Thus, children from families in poverty display higher rates of malnutrition, morbidity and mortality, and are less likely to have expenditures for health or other reasons made on their behalf, than are other children. Because poverty is often defined in per-capita terms, poverty may be a reflection of low levels of family income or large family sizes, or both.

The impact of poverty, especially low levels of per-capita income within the family that result from large family sizes, is not completely clear-cut. Kelley [1995], for example, argues that parents may be able to forgo their own consumption in favour of their children. It is plausible that parents with a preference for larger families are those who see less need to educate their children, and rather than reflecting a pure causal relationship, the correlation between family size and children's educational attainment is based in part on both variables' response to parental tastes and resource constraints [Becker and Lewis 1973]. The unsuitability of empirical models that do not account for the endogeneity of fertility in parents' decisions lies at the heart of reviewers' criticisms of much of the work in the field [*e.g.*, King 1987, Kelley 1995]. Models that pay more careful attention to the issue of statistical identification (*e.g.*, Rosenzweig and Wolpin [1980], Behrman and Wolfe [1987]) tend to find small effects of exogenous variation in family size on household resource allocation. We risk erring in the direction of caution by including the number of siblings as well as a measure of household income as covariates in our empirical work.

In summary, we expect that, all else constant, the following simple comparative static effects will describe parents' decisions to take sick children in for treatment. Where cost is low as a share of income (*i.e.*, where prices are low or incomes high), treatment will be more likely. When a particular child is preferred, that child is more likely to receive curative care, compared to a less-preferred sibling. Finally, our central contribution in this paper: Where benefits of treatment are high, children are more likely to receive treatment.

2. The setting

The Philippines is an archipelagic, Southeast Asian country of approximately 65 million people. While Southeast Asia as a whole showed clear signs of social and economic development during the two decades preceding the surveys we employ, these generally were absent in the Philippine case. Per capita income actually declined during the 1980s due largely to political instability, capital flight, and continuously high levels of population growth. Structural transformation of the economy has been slow, as evinced by a constant or declining proportion of the labour force employed in manufacturing since the 1970s.

Significant health problems can be noted on a number of fronts [cf. Herrin, et al., 1993]. The Philippines was unable to achieve any measurable decline in infant mortality during the 1980s, a record that contrasts poorly with the other ASEAN countries. Malnutrition is common and infectious diseases continue to play a major role in the overall mortality and morbidity profiles. By the end of the 1980s government health statistics showed respiratory and diarrheal disease to both rank high among all causes of infant and childhood deaths. As of 1991, more than a third (34.2 percent) of all registered deaths to children less than five years of age were attributed specifically to one of these two conditions [Health Intelligence Service, 1994, Tables 17 and 23]. Major differentials in infant mortality also existed, with mortality rates being highest in rural areas (especially those located far from Metro Manila, the nation's capital), among poorer and less educated households, and for children born to older, high-parity women [Costello, 1988].

Results from the 1993 National Demographic Survey [National Statistics Office and Macro International Inc., 1994] showed several problematic areas with regard to the proximate determinants of child survivorship. More than a third of all Filipino households did not have a flush toilet or an electrical connection. Less than 40 percent had access to potable water. Fertility rates showed only a moderate decline in the preceding few decades; and usage levels for modern contraceptives were low (at about 25 percent of all currently married women in the childbearing ages) [Perez and Palmore, 1995].

Government efforts to control infectious disease concentrated largely upon a Primary Health Care program that emphasizes immunizations, oral rehydration therapy and a network of village-level clinics, known as Barangay Health Stations. This approach met with only moderate success, perhaps because of the limited resources made available to the health sector. Total expenditures on health did not exceed 1.7 percent of the gross national product during the 1980s while government spending on health, as measured in constant monetary units and on a per capita basis, failed to show a measurable increase throughout this same period [Herrin et al., 1993, Table 3.1 and Figure 1.19]. In recent years the Philippine health system has undergone a number of structural changes. These include the devolution, beginning in 1992, of primary health care service to local government and a major USAID program to use the NGO network to provide primary care through NGO sanctioned and supervised privately operated MCH/FP clinics.

3. Data and methods

The 1993 Philippines National Demographic Survey [Philippines National Statistics Office and Macro International 1994] is a nationally representative survey in the Demographic and Health Surveys (DHS) series, in which 15,029 women were interviewed. Of these women, 8,961 were married at the time of the survey, and those with children were asked about the recent health of their children. For births in the five years preceding the sample, detailed data on health were collected. There were 8,803 births to respondents in the five years preceding the survey. Mothers were asked if these children had experienced diarrhoea, or cough or fever in the two weeks preceding the survey, as well as what treatments the children were given. Since we are interested in resource constraints, including the intra-household effects of competition among siblings, only children with at least one surviving sibling are included in our sample. In our estimation subsample, 35% of children experienced ARI symptoms (fever or cough), and 7% experienced diarrhoea in the two weeks preceding the mother's interview.

Many variables are familiar, but some bear further explanation. The first is our measure of permanent income. DHS surveys do not collect direct data on income. Instead, they use a collection of questions about asset ownership (vehicles and appliances), and housing quality (roof and floor materials). We have combined the responses to many of these questions, using factor analysis, into a single factor allowing us to control for variations in a fairly large number of asset variables.[6] We then employ the resulting factor score as our measure of permanent income.[7] There are three variables we constructed as provincial-level means: the mean incidence of fever/cough and diarrhoea, and the mean travel time to health facilities. These were constructed using responses for children of every eligible respondent in the province except the reference birth, and therefore are indicative of the community-level conditions faced by the reference birth. Finally, the DHS question on wantedness comes in a section of the questionnaire extracting detailed information on recent births. The mother was asked whether she wanted the current birth at the time she became pregnant, whether she wanted the birth but would have preferred that it had come later, or whether she would have preferred that the birth had not occurred at all. Roughly 84% of births were classified as wanted at the time of pregnancy or at some future date, which is the definition of wantedness we employed. The unit of observation is the individual child.

The facilities survey of Stewart *et al.* [1997] provides information regarding the amount of resources devoted to and quality of health care service in the barangay containing the DHS cluster and the surrounding municipality. Conducted in the fall of 1996, it covers 253 facilities in 40 of the 750 clusters in the 1993 DHS. The clusters were clumped around Metropolitan Manila, Cebu City, and Cagayan de Oro City, because the staffs were based at universities in these cities. In a sampled municipality, all rural health units, at least one barangay health station, and any government hospital either within or nearby were included, as well as several types of private (for profit and NGO) providers. Facility managers were queried on staffing hours and wages, as well as other operational details.

The resulting facility labour costs and data on the number and types of public health facilities in each municipality were used to calculate the total expenditure on staff for each municipality.[8] Total expenditure on public facility health staff was then put on a per capita basis by dividing by the number of women of reproductive age living in the municipality. This variable provides a rough proxy measure of the amount of public health care available in the mother's municipality. [9] We also construct a facility quality score as a rough proxy for the level and quality of physical capital in place at publicly operated health facilities. We constructed the factor score based on five dummies for whether the facility has a diagnostic lab, a dental clinic, and working electricity, plumbing and telephone.[10]

Our merged data carry with them some problems. There is a reduction in sample size, because of the narrower coverage of the facilities survey. After merging these data with the DHS data, we have a sample of just over 800 children, or about 10% of the total DHS coverage. Because of the way in which the sample was selected, they are relatively more urban (70% compared to the original 50%), and may vary in other ways. We estimate identical models of treatment for the full DHS sample and our subsample below, and, while hardly conclusive on this point, the similarity in the estimated marginal effects between samples is reassuring on the representativeness of our sample.

An additional concern is our inability to say much about what is happening in the private sector. At least one of every type of public facility existing in each municipality was surveyed, and we have a count of the number of public facilities in each municipality. Our information on private facilities is somewhat spottier. Private facilities were sampled only by convenience, and we have no count of the total number of private facilities. We therefore have no sense of the overall level of expenditures at private facilities in particular municipalities. This is of potential concern, because of all ARI episodes in our data, 60% of those children treated were treated at public facilities, and the corresponding figure for diarrhoea episodes is 59%. Public facilities treated the majority of cases, but clearly, private facilities are important in treatment of either disease.

We will estimate our models of treatment using only the staffing levels of public facilities. In doing so, we are making the following assumptions. First, in a given location, a public alternative to a private provider always exists. Given the large number of public provision points, this seems likely to obtain. Second, public provision points must be less costly to use, and third, parents must view sites operated by the public sector as the lower quality providers. These also are in line with experience. We are examining parents' decision to treat an ill child, and motivating it with a discussion of resource constraints. Given our assumptions, parents who could not afford a private facility would not automatically rule out a public facility unless its quality was sufficiently low. That is, the decision not to treat is based on the perception of low quality at the provider of last resort. That provider is operated by the public sector. Therefore, the relevant measure of quality is the labour or capital employed by the public providers, for which we do have data.

Our estimates of the impact of quality at public provision points on the probability of treatment are likely to be conservative estimates of the true impact of quality. This is because we observe all treatment episodes, including those at private facilities. If poor quality public facilities may displace children to private providers, children are still being treated, even though the benefits of doing so at public facilities apparently are low. This would weaken the presumed link between treatment and quality.

A third issue is a potential timing problem with linking the NDS and Stewart *et al*. [1997] data. The NDS was administered in late 1993, but the facilities survey was done in mid-1996. Because we have dates at which surveyed clinics opened, it is straightforward to drop those that were surveyed by Stewart *et al*., but had not yet opened at the time the NDS was administered. Of 189 public facilities surveyed, 13 opened in 1994 or 1995, and these were dropped from our analysis. More problematic are those facilities that may have closed after the DHS was administered, but before the Stewart *et al.* facilities survey took place. In an era of growing demand, and given that the window of time between the surveys is fairly short, it seems unlikely that too many public facilities will have closed down, but we have no way of assessing how severe this problem may be.

The potentially knottiest problem is our use of 1996 data on prices for service, staffing patterns (e.g. whether there was a doctor on staff), services offered, etc., to explain 1993 behaviour. While chronologically closer to the 1998 Demographic and Health Survey, the facilities survey we employ relied on the earlier (1993) sampling frame, tying us to the earlier survey. The issue is how much change might have occurred, and if change happened, whether it was systematic. There is evidence of stasis in the health sector in the Philippines for years prior to the 1993 survey, and little change apparent between the two DHS surveys. The infant mortality rate was 64 per thousand in 1976, and had fallen only to 57 per thousand in 1990 [Philippine Institute for Development Studies 2002]. While the levels are different, due in part to differing data sources, the lack of clear trends in infant and child mortality also is apparent in the Demographic and Health Surveys. The 1993 NDS reported infant and under-five mortality rates of 33.6 and 54.3; comparable figures from the 1998 survey were 35.1 and 48.4 [Macro International 2002].

While there is little reason to expect sweeping change, it still seems unlikely that staffing would remain rigidly fixed in all facilities over the three-year interval between surveys. On the other hand, it seems likely that most facilities understaffed in 1993 were still understaffed in 1996, perhaps because they were in undesirable locations. If so, most of the variation in an imaginary panel that contained staffing data from both years would have been between, rather than within, delivery points. This suggests that 1996 staffing data are statistically noisy proxies for 1993 staffing data. Under the assumption that the deviation of 1996 from true 1993 staffing is random, standard measurement error techniques, such as instrumental variables estimation, yield consistent parameter estimates. We therefore will examine results based on instruments for expenditures, as well as non-instrumented results.

Brief definitions, means and standard errors for variables used in subsequent analysis appear in Table 1.

Table 1. Descriptive Statistics

Variable	Definition	Mean	**Std. Deviation**
ARI	Fever or cough in the 2 weeks preceding the survey	.346	.476
Diarrhoea	Diarrhoea in the 2 weeks preceding the survey	.073	.260
ARI Treated	Equals 1 if modern ARI treatment received, 0 else	.618	.487
Diarrhoea Treated	Equals 1 if modern diarrhoea treatment received, 0 else	.541	.502
Permanent income	Factor score for wealth based on asset ownership	.178	.806
Mother’s education	Mother’s education, in years	9.25	4.70
Mother’s age	Mother’s age, in years	30.00	6.27
Father’s education	Father’s education, in years	8.39	3.65
Wanted child	Equals 1 if the child was reported wanted, either then or later, by the mother at the time of conception	.877	.329
Child age	Child’s age, in years, at the survey date	2.40	1.39
Male	Equals 1 if the child was male	.52	.50
Water	Equals 1 if the household had access to piped water	.112	.316
Toilet	Equals 1 if the household had a flush toilet	.787	.410
Siblings	Number of living siblings (of the child)	2.16	2.20
Urban	Equals 1 if the household was in an urban area	.706	.456
Travel time	Cluster mean travel time, in minutes, from survey cluster to nearest health facility, as reported by mothers	33.44	29.51
Mean labour expenditures per area population, by facility type (pesos):			
BHS	Barangay Health Station	1.46	2.50
Hospital	Government hospital	2.97	8.61
RHU	Rural Health unit	2.70	4.92
MHC	Maternal Health Centre	2.46	2.71
BHC	Barangay Health Centre	2.99	4.02

4. Model

We estimate a model of treatment, conditional on illness, which incorporates information about health care facilities. To condition for possible non-random selection into illness, we also model the determinants of illness. Pragmatic concerns dictate the use of diarrhoea and respiratory illness in the two weeks preceding the survey, as these are the two illnesses most readily observed in the NDS data. However, we have already noted the sizeable contribution to infant and child mortality in the Philippines of these two diseases, so their policy importance is clear. We construct a model based upon the concept of a child-specific index of "child value", or parents' willingness to commit resources to a particular child. This index is posited to be a function of exogenous individual, household and community (including clinic characteristic) variables. Household resource commitments are measured directly by usage of health care, with associated monetary, time and other costs; and indirectly by the incidence of morbidity.

Define Z_j to be the index value for child *j*, where, for *F* a vector of family-specific variables (such as permanent income and mother's education) and C a vector of child-specific values (such as age, wantedness and sex)

$$(1)\ Z_j = f(F, C)$$

Define *R* to be a vector of underlying risks of illness, including disease prevalence and sanitation, *A* to be a vector of variables measuring access to health care, and let *Q* be a vector of quality measures for that care. Then the following conditions characterize the incidence of illness, curative care, and preventive care for living children:

$$(2)\ \ \textit{Illness observed}: I = 1 \textit{ if } Z_1^* \geq Z_1 \mid F, C, R$$
$$(3)\ \textit{Treatment observed}: T = 1 \textit{ if } Z_2^* \leq Z_2 \mid F, C, A, Q, I = 1$$

where Z_j* denote unobserved threshold variables. Illness occurs if the index of child value, conditioned on child and family specific covariates, falls below an unobserved threshold value, and curative treatment occurs if treatment value, conditioned on child and family specific covariates, access costs, facility quality, and illness, exceeds a minimum (unobserved) threshold. Child and family-level covariates associated with increased income or more-preferred children are expected to reduce illness and to exert positive impacts on treatment, and increases in accessibility or quality of care and risk of contagion are presumed to increase the odds of treatment and illness, respectively.

We employ a binary probit model with endogenous selection [van de Ven and van Praag 1981], to account for the non-random selection into child illness that must precede treatment. We identify the model with the exclusion of access and quality measures from the illness equation. The differing levels of aggregation of the data--individual and municipality--imply that some account should be taken of the multilevel nature of the error structure. It is well known that the incorporation of covariates representing varying levels of aggregation into linear models creates downward bias in the variance estimators of some coefficients [Moulton 1990, Goldstein 1995, Rodriguez and Goldman 1995, Bryk and Raudenbush 1992]. The key issue is that observations tied at some higher level of aggregation (*e.g.*, community characteristics in a model of individual behaviour) are likely to share certain unobserved characteristics, implying that the Gauss-Markov assumption of uncorrelated regression disturbances is likely to be violated. In a linear model, failure to account for such an error structure would lead to underestimates of standard errors and overstatement of *t* statistics. In a nonlinear model such as ours, all parameter estimators would be inconsistent. Therefore, in our statistical work, we explicitly allow for correlation between errors within municipalities. We employ instruments for our facilities measures, to allow for measurement error in these variables (and potentially other sources of inconsistency), and test for the appropriateness of this specification.

5. Results

We have two main findings. First, per capita expenditure on the staffing of public provision points typically is an important determinant of whether or not parents bring their ill children in for treatment. Parents living in municipalities with relatively high per-capita labour expenditures on clinic staff are those who, all else constant, are relatively likely to take children with ARI or diarrhoea to be treated at a health facility. The quality of physical facilities, at least in the crude way we measure it using a factor score based on rudiments like running water and working electricity, does not appear to play a role in the decision to treat either disease. Second, though the magnitudes of parents' responses to differences in travel times to the nearest health facility differ for fever/cough and diarrhoea treatment, the effect for both diseases is consistent with the hypothesis that increasing travel cost decreases the likelihood that treatment will be sought. These results are consistent with economically rational decision-making by parents.

Table 2 shows regression summaries for selected variables, for models of the treatment of ARI and diarrhoea symptoms. We transform the coefficients to marginal effects evaluated at sample means, accompanied by the associated probability values for two-tailed alternative hypotheses. The marginal effects are interpreted as the change in the probability that an ill child receives treatment resulting from a unit change in the covariate, all else constant.[11] The model is estimated as a two-equation probit model with endogenous selection (through illness) into risk of treatment.

Table 2. Selected determinants of treatment

		ARI treatment		Diarrhoea treatment	
		Actual expenditures and facility scores	**Instrumented expenditures and facility scores**	**Actual expenditures and facility scores**	**Instrumented expenditures and facility scores**
Facility characteristics	**Labour expenditure**	.002 (.05)	.004 (.01)	.006 (.04)	.010 (.22)
	Facility quality	.003 (.73)	.011 (.31)	-.035 (.63)	-.015 (.88)
	Travel time	-.0004 (.14)	-.0004 (.11)	-.003 (.01)	-.003 (.10)
Family characteristics	**Permanent income**	.068 (.01)	.068 (.01)	.097 (.19)	.117 (.29)
	Mother's education	.009 (.05)	.009 (.03)	-.002 (.87)	-.005 (.73)
	Number of siblings	.001 (.71)	-.001 (.75)	.018 (.22)	.025 (.34)
Child characteristics	**Male child**	.0001 (.95)	.003 (.83)	.077 (.18)	.123 (.21)
	Wanted Birth	.010 (.78)	.008 (.76)	-.140 (.18)	-.197 (.15)

Notes The body of the table presents marginal effects, evaluated at sample means, of a unit change in covariates on the probability of a child receiving treatment. The marginal effects are based upon the coefficients from the treatment regressions in a two-equation model of treatment, conditioned on illness. Probability values are presented in parentheses, and are appropriate for a two-tailed alternative hypothesis. A reported *p*-value of .01 means that the calculated *p*-value is less than .01. The underlying standard errors are robust, and based on a clustering correction reflecting the multilevel nature of the data. Results of the regressions used to generate the instrumental variables are presented in the Appendix, and the first-stage conditioning results by illness are presented in Table 3. Of 847 children with ARI data in the sample, 284 had ARI symptoms reported, and of these, 61% received treatment. Of 840 children with diarrhoea data, 61 had diarrhoea symptoms reported, and of these, 54% received treatment. Other variables included all specifications were the father's education, child's age and age squared, and a dummy for urban residence.

The expenditures and capital proxy instruments use district population size and dummy variables for whether the locality is in the Visayas or Mindanao. The results of the regressions are present in appendix Table A1; regarding those results, we note that both had R^2 values of at least 0.18, acceptably high in the sense of Bound *et al.* [1995]. When included as covariates in the structural equations for treatment, none were statistically significant. A more pressing question is whether the instrumental variables specification is the appropriate alternative, compared to simple maximum likelihood estimation with actual values of labour expenditures and facility scores. Unfortunately, it is difficult to resolve the question in this setting. In large samples, the basis for well-known tests of instrumented specifications (such as the Hausman test) is that the instrumented specification yields sufficiently different marginal effects from the simpler specification to allow rejection of the latter model. The test relies on the variance being (weakly) smaller for the simpler model, but there is no guarantee that this condition will obtain in finite samples. Using a standard Hausman test[12], we fail to reject the hypothesis that expenditures and facility quality are inconsistent without instrumenting. However, the estimated variances in the instrumented regression for fever/cough treatment too often are smaller than for the non-instrumented regression in our sample, violating the underlying assumption of the Hausman (and related) tests[13].

The issue is whether the results are sufficiently different in the competing specifications. In some sense, the point is moot, since the results appear very robust with respect to instrumenting. A possible exception is the labour expenditure marginal effect. The estimated marginal effect for expenditures in the instrumented specifications is roughly twice as large as in the non-instrumented specifications. This is consistent with measurement error in expenditures, as we have discussed, or for that matter with endogenous program placement or any of a host of other possibilities. However, except for the statistical insignificance resulting from the much larger estimated variance for the expenditures marginal effect in instrumented diarrhoea treatment equation (a specification that the Hausman test unambiguously does not support), the sign, rough magnitude and statistical significance of this marginal effect support the claim that parents are responsive to labour expenditures in deciding whether to take a sick child in for treatment. In practical terms, this renders the question of whether to employ instruments peripheral, and spares us a finite sample decision on model specification.

In Table 2, there are three clear determinants of treatment for fever or cough. At the facility level, labour expenditures matter, and within the family, income and mother's educational attainment affect treatment probabilities. Notably, no child characteristics influence the chance of receiving treatment. Mean labour expenditures are about 12 pesos per capita with standard deviation 10.6, so the marginal effect of 0.002 in the non-instrumented specification implies that a doubling of expenditures would cause treatment probabilities to increase by 0.024. The mean probability of treatment for fever or cough was 0.61, yielding an expenditure elasticity of treatment probability of 0.04, and an estimated differential in treatment probability of 3.5% between clients of facilities with a one standard deviation differential in labour expenditures per capita. Using the instrumented marginal effect, the estimated impact on treatment probability is 7%.

It is useful to put these figures in context. Labour expenditures increase with the number of facilities, and with increases in the level of staffing per facility. In this sense, labour expenditure measures both facility quality and quantity. The more pure measure of quantity probably is travel time, concentrating the residual quality variation in labour expenditures when both travel time and labour expenditures are used as regression covariates. Labour expenditure serves as an admittedly noisy measure of facility quality, and comparing the impacts of travel time reduction and labour expenditure increases on treatment is an exercise with implications for the impacts of quantity and quality improvements on treatment probabilities. A one standard deviation reduction in travel time is associated with an increase of 1.5% in the probability that fever or cough is treated. This is small, in comparison with the abovementioned impact of increasing labour expenditures, or with changing other covariates. For example, a one standard deviation increase in permanent income leads to an 8.9% increase in predicted treatment probability, and a one standard deviation increase in the mother's educational attainment a 6.9% increase in predicted treatment probability.

Two things are clear in this exercise. First, there is a fair amount of systematic variation in treatment probabilities. A woman in the bottom 15% of the income and education distribution (one standard deviation below the mean, assuming normality for both variables), faced with a facility in the bottom 15% of labour expenditures, is on the order of 20% less likely to take an ill child in for treatment of fever or cough than is woman facing mean values of these variables. Secondly, among these determinants, the policy lever available to health ministries is staffing, and the great variation in labour expenditures observed in our sample carries with it fairly substantial consequences for predicted probabilities of treatment of ARI.

An additional point of conjecture is the effect of income. Officially, services and even drugs dispensed from public facilities in the Philippines were virtually free during the period of time under study, and so it is somewhat puzzling that income should matter as much as it did in the treatment of ARI. One possibility is that the observed response was concentrated at the absolute bottom of the income distribution, and the expected 'tips' and other charges were beyond their means. Another possibility is that some parents, too poor to be able to afford private-sector treatment for their children but not necessarily the very poorest, chose not to have their children treated at all. There is too little variation within quartiles of the wealth score to allow us to resolve this question empirically by running the structural equations separately by quartile. However, simple cross tabulations of wealth quartiles and treatment show statistically significant differences in treatment. The children of parents in the top wealth quartile had treatment probabilities that were 25 points above full sample mean treatment (91% were treated); for the bottom wealth quartile, children were 26 points below the full sample mean, at 40% treated. This lends support to the idea that the poorest parents are unable to afford treatment.

Before discussing the results for treatment of diarrheal disease, it is worth stating at the outset that the treatment protocols for diarrhoea and fever/cough differ. The standard treatment protocol for cough and fever may include antibiotics for which a small fee is charged; for diarrhoea the treatment protocol is oral rehydration solutions (ORS), dispensed as a powder that parents mix with liquid and give to the child. This is very cheap and effective, though apparently not fully accepted by parents in the Philippines. Costello *et al.* [1994], for example, document the preference of parents for treatment of diarrhoea with (inappropriate) antibiotics. Therefore, as simple as the treatment of diarrhoea should be, there may still be a role for facility quality.

Turning to the diarrhoea treatment results, we see some similarities and some differences compared to ARI treatment. On Hausman test grounds, we have reason to prefer the non-instrumented specification. Here, both labour expenditures and travel time are statistically significant. Using the same calculations as for fever/cough, a one standard deviation in labour expenditures would yield an increase of 0.06 in treatment probabilities, which is an 11.7% increase from the mean probability (0.54) of diarrhoea treatment. A one standard deviation decrease in travel time is associated with a 36.9% increase in treatment probabilities. Thus, in contrast to the fever/cough results, we see a larger effect for quantity increases than for quality enhancements. If enough parents follow the treatment protocol, presenting their diarrheic child at the service provision point and leaving soon after with some ORS packets, this is a sensible finding. Furthermore, if parents are following the protocol, there are few demands on family resources, and so one would expect our finding that characteristics of family and child do not explain variation in the probability of diarrhoea treatment.

We note in closing our discussion of the diarrhoea treatment equation that the policy implications from the estimated labour expenditure and travel time marginal effects are very different. A one standard deviation increase in labour expenditures implies increasing expenditures per capita from 12.5 to 23.2 pesos. A one standard deviation decrease in travel times implies decreasing travel times from 33.4 to 4.9 minutes. The former undoubtedly would be difficult to achieve, but the latter is impossible. This limits the real-world potential of policy designed to increase treatment probabilities by decreasing travel times, and enhances the potential policy relevance of staffing or other quality improvements. It reinforces the point that simple access is not enough to ensure that services reach sick children.

Our rough measure of the infrastructure available at clinics performed well in neither diarrhoea nor respiratory disease treatment equations. It is sensible to think of infrastructure as an important observable element of parents' assessments of facility quality. However, the survey instrument Stewart *et al.* employed was designed to collect capital and equipment data in detail only for the family planning component of the various facilities' operations. We had only very limited information on capital more generally within the facility, and our constructed proxy was undoubtedly very noisy. We therefore can offer no evidence that capital matters in parent's treatment decisions. However, in the Stewart *et al.* [1999] analysis of family planning costs, of the 25.4% of costs that were nonlabor, the largest category was supplies, and imputed rent and annualised equipment together added up to only 10% of total facility costs. It may be the case that we estimate a small effect for capital not just because our proxy is noisy, but because there simply is not enough capital used in health care delivery in the Philippines for its variation to provide a quality signal to parents.

We present results from the selection equations in Table 3. We have two reasons for doing so. First is that we want to address issues of non-random sampling arising from our use of a convenience sample of DHS clusters. Second is that Heckman specifications of selectivity are notoriously sensitive to first-stage specifications, and disclosure is important. Because we have excluded facility characteristics from selection equations, it is possible to estimate the first stage with all non-missing cases from the full household survey. For each disease, we present two sets of results. The leftmost column for each disease represents the marginal effects for morbidity based on our estimation sample, and the column to the right the results from a fuller set of NDS observations.

Table 3. Morbidity determinants in estimation and full samples

	ARI		Diarrhoea	
	Observations with facilities information	**All non-missing NDS observations**	**Observations with facilities information**	**All non-missing NDS observations**
Number of siblings	-.007 (.40)	.002 (.58)	.001 (.91)	-.002 (.30)
Male child	.048 (.02)	-.004 (.73)	-.005 (.79)	-.003 (.70)
Wanted birth	-.060 (.49)	-.062 (.01)	.015 (.44)	-.019 (.06)
Child's age	.045 (.34)	.073 (.01)	-.006 (.77)	.015 (.10)
Child's age squared	-.018 (.05)	-.024 (.01)	-.0003 (.95)	-.005 (.01)
Mother's education	-.004 (.49)	-.005 (.01)	.002 (.41)	-.001 (.34)
Father's education	.007 (.81)	.003 (.18)	.001 (.73)	-.001 (.40)
Permanent income	-.091 (.01)	-.019 (.08)	-.038 (.05)	-.011 (.07)
Good water	-.010 (.88)	.064 (.01)	-.024 (.31)	.016 (.15)
Toilet facilities	.018 (.77)	-.003 (.87)	.004 (.87)	.011 (.20)
Prevalence	.830 (.01)	.611 (.01)	.707 (.01)	.581 (.01)
Sample size	829	8382	822	8258
Dependent variable mean	.35	.41	.07	.10

Notes: The body of the table presents marginal effects, evaluated at sample means; of probit estimates of the impact of a unit change is covariates on the probability of illness. Probability values are presented in parentheses, and are appropriate for a two-tailed alternative hypothesis. The underlying standard errors are robust, and based on a clustering correction reflecting the multilevel nature of the data. The full sample results are reproduced from Jensen and Ahlburg [2001, Table 2].

Our estimation sample is more urban than the full NDS, and of course much smaller. The regression results are comparable, however. There are some differences that may be small-sample anomalies, such as the large (positive) impact on ARI morbidity of being male and the large negative impact of permanent income on the probability of contracting either type of disease but, in general, the results are remarkably similar. One striking difference between samples is the much larger magnitude of the marginal effect of prevalence, for either disease. Prevalence is defined as the proportion in a geographic area (excluding the respondent) that is ill with the particular disease. Because our sample is relatively more urban than the NDS, this may reflect the greater importance of the contagiousness of these diseases in densely populated urban areas. We note that as a test of the identifying restrictions excluding access variables from the selection equations, we included the access variables in the illness equations (not presented). In no instance were their marginal effects statistically different from zero at the 5% level. Similarly, there were no cases in which variables included in the selection equations but omitted from the treatment equations (areal disease prevalence and household toilet facilities) were statistically significant, when inserted into the treatment equations.

6. Discussion

In the Philippines, as in many countries, utilization of health care is low. Only half of children aged 0-4 with diarrhoea in the two weeks preceding the survey we employ were taken for care, even though oral rehydration solution, the recommended treatment protocol for most diarrhoea, is very inexpensive and easy to administer. Only 61% of children aged 0-4 with symptoms of acute respiratory infection received medical care. Yet, the Philippines follows the common model of cheap, easily available primary care, which, all else constant, should lead to high utilization, as fees, travel time, and other costs to patients all are low. We find that access cost, as measured by travel time, does play some role for diarrheal disease treatment. However, travel times already are short, averaging 33 minutes. Coupled with the small and statistically insignificant effect of travel time on ARI treatment, the potential role that reductions in travel time might play in increasing health care utilization seems small.

Our measure of labour inputs, total labour expenditure, shows substantial impacts on the probability that a sick child is taken in for treatment of either ARI or diarrhoea. We caution that we calculate labour expenditures as municipality means. When high, they may reflect a high level of overall staffing, or a mix of personnel skewed in favour of more expensive personnel. Assuming that facilities are placed according to need and are of roughly equal size (both tenets of the Philippine system), density would largely be captured in the travel time variable. Hiring rules in the Philippines health system were highly standardized at the time our data were collected. Since more highly paid positions required more highly trained staff, it seems likely that a substantial component of the effect we estimate for labour expenditures is the potential quality of care delivered. Our results are consistent with the contention that staffing levels send a quality signal to parents, and this in turn affects utilization.

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Appendix

Table A1. Instrument generating equations

	Dependent variable	
Variable	**Expenditures per capita**	**Facility quality score**
Female population (100,000)	31.10 (.01)	.919 (.01)
Visayas dummy	-6.20 (.01)	.672 (.01)
Mindanao dummy	-11.04 (.01)	-.553 (.01)
Intercept	21.52 (.01)	-.145 (.01)
Number of observations	892	2336
R^2	.23	.18

Notes: The body of the table presents OLS coefficients. Probability values are presented in parentheses, and are appropriate for a two-tailed alternative hypothesis.

Endnotes

1 This project was funded by the United States Agency for International Development through the Measure-Evaluation Project. Correspondence to Jensen, Economics Dept., Box 8795, College of William and Mary, Williamsburg, VA 23187-8795 or eric_jensen@wm.edu. We acknowledge Dennis Ahlburg for much helpful discussion, the late Mike Costello for his contribution to our description of health delivery in the Philippines, and thank Alex Herrin and Brad Schwartz for graciously sharing data and anonymous referees for helpful comments.

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4 They use this term to refer to births that were never wanted, but not to mistimed births.

5 A close parallel between our strategy regarding curative care and models of morbidity or mortality driven by nutritional differences would be a situation where researchers constructing behavioral models of morbidity or mortality were to observe the quantity and quality of food, as in Chen et al. [1981]. The precise production relationship between food intake and health would remain unobserved, but a clear signal of parents' intentions is available.

6 The variables used in constructing this factor score are dummies for whether the household has electricity, a stove, a refrigerator, a television, a bicycle, a motorcycle, and a car.

7 Montgomery et al. [2000] examine the usefulness of scores such as this as proxies for expenditures (or income). They conclude that while noisy measures of income, such proxies nevertheless allow reasonably powerful tests of the null hypothesis that income does not matter in behavioral demographic models.

8 Where there were data on multiple facilities of the same type in a municipality (multiple barangay health stations, for example), the average staff expenditure was used.

9 Health care resources are notoriously heterogeneous and of necessity this measure contains elements of both quantity and quality. High per capita staff expenditures may be indicative of a staff that has a larger proportion of more highly skilled, and thus more highly paid, staff members. High expenditures may simply reflect more total staff per capita, and this also may capture a subtle quality aspect. Philippine primary health care centers are specifically situated to serve a client population, implying (roughly) equal labor expenditures per capita when fully staffed. Therefore, low labor expenditures may reflect incomplete staffing.

10 We experimented with multiplying this factor times the physical volume of the facility, measured in cubic feet per size of client population, as a way of capturing both quantity and quality of physical capital. The results were similar, although with slightly larger variances (perhaps reflecting covarying labor expenditures and facility sizes).

11 Note that unit change can be very large, as in the case of the wealth index or any of the categorical variables, or relatively small, as in the case of travel time.

12 We employed the "hausman" command in Stata to carry out this test.

13 While it is possible to carry out a less formal version of the Hausman test, using t-tests (or an F test) on the coefficients of the residuals from the instrument generating equations in treatment regressions that also contain the non-instrumented variables, the ARI treatment equation is not of full rank. We obtain a *t*-statistic significantly different from zero for the expenditures variable, but are hesitant to attach much significance to it.