Information is Power: Experimental Evidence of the Long Run Impact of Community Based Monitoring[*]

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Abstract

Poor quality plagues public service provision in many developing countries. In response, policies to enhance beneficiary involvement as way to strengthen demand-responsiveness and local accountability are becoming increasingly popular. Despite the enthusiasm for this approach, however, the evidence provides mixed results about its short-run effectiveness. Whether it can lead to sustained improvements in service provision is largely unknown. This paper presents the results of two field experiments on local accountability in primary health care in Uganda. Efforts to stimulate beneficiary control, coupled with the provision of information on the staff's performance, resulted in significant improvements in health care delivery and health outcomes in both the short and longer run. Efforts to stimulate beneficiary control without addressing users' lack of robust information on the clinic's performance, on the contrary, had no measurable impact on the quality of care. We investigate the role of information and provide suggestive evidence showing that informed users are better able to distinguish between health workers' actions and factors outside the workers' control, and thereby in a better position to identify and challenge (mis)behavior by the provider.

1 Introduction

Poor quality plagues public service provision in many developing countries.[1] In response, many have argued for policies to enhance beneficiary involvement; i.e., to strengthen local accountability and demand-responsiveness by empowering the users of public services. Despite the recent enthusiasm for this approach, however, evidence from randomized evaluations provides mixed results about its short-run effectiveness.[2] Whether it can lead to sustained improvements in service provision is largely unknown.

In this paper we first provide evidence of the longer run impact of a local accountability intervention in primary health care provision in Uganda. The *participation & information intervention* combined the dissemination of report card information on staff performance with efforts to enhance participation.[3] Four years after the initial intervention, we document significant improvements in both health care delivery and health outcomes in the treatment as compared to the control group. Thus, properly designed, enhanced beneficiary involvement can result in large and sustained improvements in both health service provision and health outcomes.

Second, we investigate why this particular community-based intervention resulted in such a large and sustained change in service provision while several other seemingly similar interventions have not had much of an impact. Our focus here is on the role and impact of information. Specifically, we designed an intervention (the *participation intervention*) replicating the *participa-*

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[1]Das et al. (2008) found that doctors in Tanzania completed less than 25% of the essential checklist for patients with malaria, a disease that is endemic in the country. Indian doctors asked an average of one question per patient ("What's wrong with you?"). Chaudhury et al (2006) found an average absence rate of 27% among primary school teachers and 37% of primary health center staff in Uganda. Primary students in urban schools in Tanzania spend about one quarter of the daily schedule in a classroom where the teacher is actually present. Roughly half of the primary school teachers in Senegal fail to demonstrate mastery of the curriculum their students are supposed to master. Every other primary health clinicians in Senegal were unable to detect a simple case of pneumonia and in total spend about a half hour per day counseling patients (Bold et al, 2011).

[2]Olken (2007) finds that grassroots participation in monitoring of a village road construction program in Indonesia had little average impact. Banerjee et al. (2010) show that a project giving local committees in India the tools to evaluate student performance had no effect. Casey et al. (2012) evaluate an infrastructure project in Sierra Leone involving both relatively large grants and the application of processes to enhance local empowerment and participatory governance in the planning and implementation phases. While the evaluation cannot separately assess the impact of the process from the impact of the grant, they do not find any evidence that the intervention led to fundamental changes in collective action at the village level. Duflo et al. (2012), on the other hand, find that a governance program that gave parents specific training on how to monitor and assess teachers' effort and performance in a contract-teachers program in Kenya resulted in significant improvements in learning. Björkman and Svensson (2009) find that providing users with information on health workers' performance and facilitating the development of an action plan, resulted in significant improvements in both health workers' performance and health outcomes in Uganda. For a review of the World Bank's experience with local participatory development programs, see Mansuri and Rao (2013).

[3]While lack of evidence of long-run impact is not specific to studies on beneficiary control, it is of particular concern here since public service providers can easily adjust their behavior for a short period of time given the large pre-existing X-inefficiencies in public service provision, but permanent changes in behavior may be harder to institute. Moreover, interventions aimed at increasing community participation are primarily meant to influence norms and collective actions and thus influence local decision making both in the short and in the longer run.

tion & information intervention but without providing users with information on the health clinic's performance. Most community empowerment projects focus on participation and the *participation intervention* can be viewed, from a process point of view, as being fairly representative of the standard Community Driven Development (CDD) approach promoted by the World Bank and other donors. Holding context constant, we evaluate whether differences in intervention design – the provision (or not) of information on the health clinic's performance – can help explain the mixed findings in the literature.[4] The impacts of the interventions with and without information differ markedly. Without information, the process of stimulating participation and engagement (the *participation intervention*) had little impact on the health workers' behavior or the quality of health care. In contrast, when community members are informed; i.e., have objective and quantitative information on the staff's behavior, the same type of processes resulted in significant improvements in health care delivery and health outcomes in both the short and longer run.

Using data from the implementation phases of the two interventions, we investigate why the provision of information appear to have played such a key role. A core component of both experiments was the agreement of a joint action plan outlining the community's and the provider's agreement on what needs be done, and by whom, in order to improve health care delivery. While the process of reaching an agreement looks similar on some observable measures in the two treatment groups – the same number of community members participated in the community meetings and on average the two groups identified the same number of actions to be addressed – the type of issues to be addressed differed significantly. In the *participation group*, the health provider and the community identified issues that primarily required third-party actions; e.g., more financial and in-kind support from upper-level authorities and NGOs. In the *participation & information group*, on the contrary, the participants focused almost exclusively on local problems, which either the health workers or the users could address themselves, including absenteeism, opening hours, waiting time, and patient-clinician interactions. These results are consistent with the hypothesis that lack of information on performance, by rendering it more difficult to identify and challenge (mis)behavior by the provider, constrains the community's ability to hold providers to account. That is, with access to information, user are better able to distinguish between health workers' effort and factors outside the health workers' control, and as a result turns their focus on issues that they can manage and work on locally.

Taken together, our results provide both encouraging, and less encouraging, news for those promoting greater beneficiary control. We find that an intervention that largely mimics the Community Driven Development (CDD) approach; i.e. the *participation intervention*, had little impact.[5] The

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[4]As pointed out in Banerjee et al. (2010), from the available evidence it is difficult to disentangle whether the mixed findings are driven by differences in the details of the intervention or context.

[5]The CDD approach has become one of the most common approaches for poverty reduction (broadly defined) in developing countries. Over the past decade, the World Bank alone has allocated close to $85 billion to local

technically more demanding (and more expensive) *participation & information intervention*, however, resulted in a more engaged community and large and long run improvements in both health service provision and health outcomes.

The next section describes the institutional setting for our study. Section 3 details our evaluation design, the features of the two interventions, and the data used to evaluate them. The long-run impact evaluation results of the *participation & information intervention* are presented in section 4, while the evaluation results of the *participation intervention* are discussed in section 5.[6] Section 6 presents some suggestive findings in support of the key mechanism; relate our findings to the mixed results in the literatures; and discusses the policy recommendations of our findings.

2   Institutional setting

The experiments were implemented in 75 rural communities served by a public primary health facility (or dispensary) in nine districts covering all four regions of Uganda. Dispensaries are in the lowest tier of the health system where a professional interaction between users and providers takes place. Most dispensaries are staffed by 6-10 workers – an in-charge or clinical officer (a trained medical worker), nurses, nursing aids and other assistants – and according to the government health sector strategic plan, the standard for dispensaries includes preventive, promotional, outpatient care, maternity, general ward, and laboratory services (Republic of Uganda 2000). Health services should be provided for free.

The health sector in Uganda is decentralized and a number of actors are responsible for supervision and control of the dispensaries. At the lowest tier, the Health Unit Management Committee (HUMC) is supposed to be the main link between the community and the facility. The baseline data, however, reveals that these institutions, including the HUMC, are not actively involved in supervision or support of the primary health care providers

As discussed in our previous work (Björkman and Svensson, 2009), the setting for our experimental study – rural Uganda – is characterized by poor public health service provision (large X-inefficiencies). For example, roughly 50% of the staff are absent from the clinic a typical day (based on observational data from unannounced visits); the average waiting time is more than two hour; and only four out of ten patients report that any equipment was used the last time the respondent (or the respondent's child) visited the clinic.[7]

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participatory development programs (Mansuri and Rao, 2013).

[6]The short-run evaluation results of the *participation & information intervention* are presented in Björkman and Svensson (2009).

[7]While we have no data on the clinicians' workload, other estimates from similar settings suggest a low workload. For example Bold et al. (2011) estimate, combining observational data on time spent per patient with facility records on number of patients treated per day, that the total time spent counseling patients per day and clinician in rural primary

3 Experimental design and data

3.1 Overview

The research project was initiated in 2004 and extended in 2007. Of the 75 rural communities and facilities, 50 facilities/communities were included in the first-phase of the project (the *participation & information intervention*) and 25 facilities/communities were added in 2007 (the *participation intervention*). The catchment area or community for each dispensary was defined as the households residing in the 5-km radius around the facility. A community in our sample has, on average, 2,500 households residing within the five-kilometer radius of the clinic, of which 350 live within a one-kilometer radius.

For the *participation & information* experiment, the units (facility/community) were first stratified by location (districts) and then by population size. From each block, half of the units were randomly assigned to the treatment group (25 units) and the remaining health facilities were assigned to the control group. A similar procedure was initiated in 2007; i.e., after stratifying on location and population size, the 25 new facilities were randomly assigned to a treatment group (13 units) and control group (12 units).

Trial sizes were set to detect effects on utilization and under-two mortality rates.[8] The trial sizes were also influenced by logistical and cost constraints and the anticipation of smaller long-run treatment effects. As a reference point, Bjorkman and Svensson (2009) show that enhanced community based monitoring (the *participation & information intervention)* reduced under-two mortality by an estimated 35% and increased utilization by 20-29% after one year. In each community we surveyed approximately 100 households. Thus, the sample for the *participation & information* experiment consists of 50 communities/health facilities and approximately 5,000 households. The sample for the *participation* experiment consists of 25 communities/health facilities and approximately 2,500 households. Mortality and utilization data are available for 2005 and these data were used to estimate study power. The estimated overall under-two mortality rate (number of child deaths divided by number of child observations) in the control group in 2005 was 0.056 (Björkman and Svensson, 2009). Assuming 270 child-years of observations in each cluster (three years, 2006-2008, and 90 child observation per year), 50 clusters and an estimated coefficient of variation of 0.20, the *participation & information intervention* had 80% power of detecting significant differences at the 0.05 [0.10] significance level if the intervention reduced mortality by 24% [21%]. Assuming 180 child-years of observations in each cluster (two years, 2007-2008, and 90

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health clinics in Tanzania and Senegal is only 26 minutes.

[8]We focus on under-two (and infant) mortality rate, as opposed to say under-five mortality rate, as we want to derive mortality rates using the DHS synthetic cohort life table approach and as we have data from from 2004 and onwards (see footnote 12).

child observation per year), 25 clusters, and a coefficient of variation of 0.20, the *participation intervention* had 80% power of detecting significant differences at the 0.05 [0.10] significance level if the intervention reduced mortality by 37% [33%]. Mean utilization in the control group in 2005 was 660 with a standard deviation of 175. The *participation & information intervention* therefore had a 80% power of detecting significant differences at the 0.05 [0.10] significance level if the intervention increased utilization by 21% [18%]. The *participation intervention* had a 80% power of detecting significant differences at the 0.05 [0.10] significance level if the intervention increased utilization by 30% [26%].

3.2 Interventions

The aim of the research project was twofold. First, to evaluate the long run impact of the community monitoring intervention initiated in 2004 – the *participation & information intervention*. Second, to assess, in these types of beneficiary control programs, what role and impact dissemination of quantitative information on the staff's performance has. To this end, we designed a new intervention in 2007 – the *participation intervention*.

Efforts to stimulate beneficiary involvement, like the Community Driven Development (CDD) approach, operate on the principles of local empowerment and participatory governance as mechanisms to strengthen demand-responsiveness and local accountability. The core of the strategy is the process through which problems and constraints are identified and how (local) decisions are made and executed. While there are variations across projects, in practice community driven development is achieved though facilitated meetings with, in this case, the community and the provider. Both the *participation intervention* and the *participation & information intervention* largely followed this approach. The key difference between the two interventions is that the *participation & information intervention* also included the dissemination of a report card with data on the health clinic's performance in various dimensions.

The research design allows us to estimate and compare three treatment effects, holding the context; i.e., health care provision in rural Uganda, constant:

- The short-run treatment effect of the *participation & information intervention* (reported in Björkman and Svensson, 2009);

- The long run; i.e., four years after the initial intervention, treatment effect of the *participation & information intervention*;

- The short-run treatment effect of the *participation intervention*.

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The design is illustrated in figures 1 and 2. Note that while the long-run evaluation covers the period 2006-2008, the short-run treatment effects are estimated over two consecutive time periods (2005 and 2007-2008, respectively).

The participation intervention

The *participation intervention* involved three types of meetings facilitated by staff from local community-based organizations (CBOs): (i) a community meeting – a two-day afternoon meeting with community members from the catchment area and from all spectra of society and with on average more than 150 participants per day and per community attending; (ii) a health facility meeting – a half-day event, usually held in the afternoon at the health facility, with all staff attending; and (iii) an interface meeting – a half-day event with representatives from the community and the staff attending.[9]

The main objective of these meetings was to encourage community members and health facility staff to develop a shared view on how to improve service delivery and monitor health provision in the community; i.e., to agree on a joint action plan or a community contract. In practice, the process of reaching an agreement was achieved in two steps. First, in the community and health facility meetings, using various participatory methods, including focus group discussions, community score cards, and role plays, the community and the health staff were asked independently to identify and prioritize the key problems and issues they viewed as the most important to address and how to address them within the current resource envelope.[10] Second, in the interface meeting, representatives from the community and health facility staff presented and discussed their suggestions and through facilitated meeting agreed on a set of issues that were viewed as most critical to address. These issues were put into a joint action plan. The action plan outlines the community's and the providers' joint agreement on what needs be done to improve health care delivery, how, when, and by whom.

In total, the process of reaching an agreement took five days. After the meetings, the communities themselves had the responsibility to monitor the implementation of the issues outlined in the joint action plan. In 2008, about a year after the initial intervention, there was a one-day repeat engagement on a smaller scale facilitated by the CBOs during which health workers and community members discussed suggestions for sustaining or improving progress on the issues outlined in the joint action plan.

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[9]Altogether, 18 CBOs, each receiving 10 days of training, implemented the two interventions.

[10]Focus group discussions were held with sub-groups (young, women, etc.) in the community meetings. The intention was to let each group voice their concerns so as to reduce the risk of elite capture. Community score cards are used as a method aimed at both identifying and aggregating issues to be addressed. In the community meeting, members scored the services provided by the facility on a scale from 0 to 100. In the health facility meeting, a similar exercise was implemented to identify key problems and constraints as viewed by the health staff, including a self-assessment of their performance. Role plays were used primarily in the interface meeting as a method to both illustrate and desensitize issues that the provider and the community had differential views on.

The participation & information intervention

The *participation & information intervention* mirrored the participation intervention with one important exception. Specifically, at the start of both the community and health facility meetings, the facilitators provided the participants with quantitative data on the performance of the health provider. These data were collected from facility and household surveys implemented prior to the intervention. A unique report card was established for each facility summarizing information that were identified from the baseline data as key areas subject to improvement, including utilization, access, absenteeism, and patient-clinician interaction. The report cards also included comparisons vis-à-vis other health facilities and with the national standard for primary health care provision. The report cards were translated into the languages spoken in the community and posters were designed to help the non-literate process the information being provided.

The *participation & information intervention* was initiated in the beginning of 2005 and was followed by small scale repeat engagements in the mid-2005, 2007, and 2008 as illustrated in figure 1. In between this fairly "minimal" intervention (a total of 10 days in 4 years), the communities were left to themselves to monitor the providers as outlined in the agreed upon action plan.

3.3   Data

Data collection was governed by two objectives. The first objective was to create report cards for the *participation & information intervention* on staff performance and how the community views the quality and efficacy of service delivery. The second objective was to rigorously evaluate the short and long-run impacts.

To meet these objectives, two types of surveys were implemented: a survey of health care providers and a household survey in the catchment areas of the facilities. A quantitative service delivery survey was used to collect data from the providers. Because health providers may have a strong incentive to misreport key data, the data were obtained directly from the records kept by facilities for their own need (i.e., daily patient registers, stock cards, etc.) rather than from administrative records. The former, often available in a highly disaggregate format, were considered to suffer the least from any incentive problems in record keeping. Data were also collected through visual checks by enumerators, including measuring absenteeism using unannounced visits.

Figure 3 provides details on the timing and scope of the data collection effort. The post-treatment survey collected data from 75 health facilities and roughly 7,500 households. While all surveys included a core set of modules, including socio-demographic characteristics, households' health outcomes and health facility performance as experienced by the household in the household survey, additional modules were added in the later rounds. For example, the household survey in 2006 included modules on child mortality and anthropometric measurements (height of children

under age 5 and weight of infants). The household survey in 2009 also included an additional module to capture the extent to which clinical guidelines for the examination of patients were followed, with a focus on prenatal and antenatal care.

In addition to data from facility and household surveys, we have information from the action plans for the two treatment group at the start of the intervention (in 2005 and 2007, respectively) and information from the treatment group in the *participation & information* experiment in 2007 when they were given the opportunity to revise their plans.

3.4   Outcomes and statistical framework

We divide our empirical investigation into two parts. We start by analyzing the impact on the main outcome of interest; i.e., whether the intervention resulted in improved health outcomes. We have four set of health measures: pregnancy, birth, child mortality and anthropometric measurements of children. We then turn to the related issue of quantity of health care. To measure utilization, we use data from the health facility (daily patient registers) as well as utilization data from the household survey. The second part focuses on the channels through which the intervention may have influenced health outcomes and the demand and supply of health care. That is, we assess changes in all steps in the accountability chain; i.e. whether there is evidence of increased monitoring activities from the communities and information exchanges, using information derived from direct observations at the facility and data collected at the household level, and whether there is evidence that the health facility staff responded through improved treatment practices and overall management of the health clinics.

We use the same set of outcome measures as those used in the short-run evaluation of the *participation & information intervention* (Björkman and Svensson, 2009). In addition, we report the findings on the extent to which clinical guidelines for the examination of patients were followed, with a focus on antenatal and postnatal care.[11]

To assess the causal effect of the intervention we compare mean outcomes, after accounting for stratification. That is we estimate

(1) $$y_{ijd} = \alpha + \beta T_{jd} + \theta_d + \varepsilon_{ijd}$$

where y_{ijd} is the outcome of household i (when applicable), in community/health facility j, in district d, T_{jd} is an indicator variable for assignment to treatment, θ_d are district fixed effects, and ε_{ijd} is an error term.

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[11]As discussed in section 3.3, these data were only collected in 2009.

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For a subset of variables, we can also stack the pre- and post-data and estimate the following difference-in-difference specification

(2) $$y_{ijt} = \gamma POST_t + \beta T_j \times POST_t + \mu_j + \varepsilon_{ijt}$$

where $POST$ is an indicator for the follow-up survey and μ_j is a facility or community fixed effect.

For some outcomes we have a group of related outcome measures. To assess the impact of the intervention on a set of K related outcomes, we follow Kling et al. (2004) and estimate a seemingly unrelated regression system, and derive average standardized treatment effects, $\tilde{\beta} = \frac{1}{K} \sum_{k=1}^{K} \frac{\hat{\beta}_k}{\hat{\sigma}_k}$, where $\hat{\beta}_k$ is the point estimate on the treatment indicator in the kth outcome regression and $\hat{\sigma}_k$ is the standard deviation of the control group for outcome k (see Duflo et al. 2007).

4 Long-run impact of the participation & information intervention

4.1 Balance at baseline

Tables 1 and 2, panel A, report mean pre-treatment characteristics for the treatment and control groups in the *participation & information intervention* and test statistics for equality of the means. We report both mean differences for a set of key outcome variables and differences in average standardized pre-treatment effects for each family of outcomes (utilization, utilization pattern, quality, catchment area statistics, health facility characteristics, citizen perceptions, supply of drugs, and user charges). There is no systematic difference between the treatment and the control group at baseline. Thus, overall the sample is balanced.

4.2 Health outcomes and quantity of care

Health outcomes

We consider five measures of health: births, pregnancies, child mortality, and height and weight of children. Number of births and pregnancies link directly to the quantity and quality of family planning and health education. Child mortality links to quality and quantity of a wide spectrum of services that should be provided by dispensaries. Many of these services, including improved pre-

natal care, health education, child preventive care, and child curative care, have also the potential to affect height and weight. Height can be viewed as a summary measure of health and nutrition since conception, while weight is an indicator of current nutrition and illness status, especially for younger children.

Table 3 reports the findings on births and pregnancies. The intervention reduced the incidence (column i) and the number of births since 2006 (column ii) by 14% – an effect roughly twice as large in absolute values compared to the short-run effect reported in Björkman and Svensson (2009). Columns (iii)-(iv) show that the effect is of the same order of magnitude – about two times larger in absolute values in the long (4 years) versus the short (1 year) run – when using the incidence and number of pregnancies as dependent variables.

Table 4 investigates the impact on child mortality. We start by reporting the results using the raw data; i.e., the number of deaths over child-years of observations. Column (i) show the mortality estimate for children under-five. The point estimate is negative, though our power to pick up an effect for older children is limited by a small sample size. As a large majority of under-5 deaths occur before the age of 2, our main focus is on under-two and infant mortality as reported in columns (ii) and (iii). The observed mortality reductions in children under-two is approximately 30% (p-value=0.061). The observed mortality reductions in children under-one is approximately 36% (p-value=0.053).

To estimate mortality rates, we follow the DHS synthetic cohort life table approach. This enables us to derive two mortality rates: infant (less than 1 year old) and under-two mortality over the period 2006-2009.[12]

The intervention reduced the mortality rate of children under the age of two in the longer-run (columns iv-v). The mortality reductions are substantial in magnitude: a 18.4 percentage points reduction in under-two mortality and a reduction of 13.7 percentage points for infant mortality. For the latter, this represents a 29.9 percent reduction in long-term infant mortality, while the 95 percent confidence interval on under-two mortality ranges from a 3% to a 64% reduction in under-two mortality in the treatment group.

Table 5, column (i), shows the age range of the mortality effects. Pooling data for the years 2006-2008, we have information on the birth year of all children (under age 5) alive at the be-

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[12]See Online Guide to DHS Statistics at http://legacy.measuredhs.com/help/datasets/. We collected birth year and death statistics from 2004 and onward. Therefore we can implement the DHS synthetic cohort life table approach to estimate infant (under 1 year) and under-two mortality rates. We cannot use this approach do derive, for example under-five mortality rates over the period 2006-2009, as this would require birth and death statistics from 2001 and onward. As we have mortality data for a longer period, our estimation approach provides a more accurate measure of child mortality than the one used to measure mortality rates in Björkman and Svensson (2009). They use data from 2005 only and derive an estimate of under-five mortality rate by summing the death rates for each cohort (0-1 year old, 1-2 year olds etc.) per community, expressed per 1,000 live births. The estimated infant mortality rate in the control group, 45.3, is somewhat lower than the infant mortality rate reported in official statistics (63 in 2010).

ginning of each year, and the number of children deceased by age for each year. In the sample, 74% of reported deaths occur in the first 11 months, and 88% of the deaths occur between 0-23 months. Children at age 36-59 month at the time of death, represent 6% of all reported deaths in the sample. Using these data we estimate equation (1), replacing the treatment indicator with a full set of cohort indicators and cohort-by-treatment interactions. We can then address the question: conditional on having a child of age x at the end of year j, or a child born in $j + 1$ (for the youngest cohort), where $j = 2005, 2006, 2007, 2008$, and $x = \{0 - 11\text{m}, 12 - 23\text{m}, 24 - 35\text{m}, 36 - 47\text{m}, 48 - 59\text{m}\}$, what is the probability that the child died in year $j + 1$? The coefficients on the cohort-by-treatment interactions gives use the average estimate for the three-year period 2006-2008. As evident, children under one year old drive the reduction in under-2 mortality. The point estimates implies a reduction in the probability of death in the first year of life of 29 percent in the treatment compared to the control group.[13]

Column (ii) focuses on infants only and assesses changes over time (2006-2008). All co-efficients are insignificant in this much smaller sample, but the point estimates suggest that the treatment effects have been of the same magnitude over all three years. Thus, the reduction in mortality appears to have been constant over the course of the program.

We measured the weight of children under-5 using portable scales and the height of children under-5 years using stadiometers. Table 6 reports the long-run intervention impacts on the two anthropometric measurements. We report results separately for infants (0-11 months) and children 12-59 months because reductions in illnesses tend to lead to gains in weight for infants while reductions in illnesses over time, and thus for older children, will tend to manifest in height. Columns (i-iii) depict the weight-for-age z-score results. Consistent with the findings in Cortinovis et al. (1997), and the short-run findings, Ugandan children have values of weight far lower than the NCHS/CDC international reference.[14] The treatment effect is 0.22 z-score in weight-for-age for infants and is fairly precisely estimated. Adding controls for age and gender does not change the results (column iii). There is no effect for children aged 12-59 months. As illustrated in figure 4, the difference in weight-for-age scores for infants appears to be driven by a positive impact of the intervention for underweight children. This finding is consistent with a positive treatment effect arising from improved access and quality of health care, as underweight children are at a higher risk of suffering from infectious diseases, or severe complications of infectious diseases, and therefore in higher demand of health care. The treatment effect is also quantitatively important.[15]

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[13]There are 2 reported deaths in the age cohort 48-59 months.

[14]Following Björkman and Svensson (2009), we omit observations with a recorded weight above the 90th percentile in the growth chart reported in Cortinovis et al. (1997). Since weight is measured by trained enumerators, the reporting error is likely due to misreported age of the child.

[15]As a reference, Jones et al. (2003) argue that interventions to improve the intake of complementary food for children, where a review of evidence shows a mean increase of 0.35 z-score, is one of the most effective preventive interventions feasible for delivery at high coverage in a low-income setting.

Columns (iv)-(vi) depict the results for height of children, using height-for-age z-score as dependent variable. To the extent health care provision in the treatment group has experienced a sustained improvement, one could plausibly expect an impact on height. There is a positive treatment effect, significant at the 10 percent level, on height for children 12-59 months old. Four years into the intervention, children 12-59 months old in the treatment group are 0.10 z-scores taller than the children of the same age in the control group. As illustrated in figure 5, the treatment effect is also increasing in the exposure to treatment. For example, the treatment effect for children that have lived 2-3 years in a treatment area is 0.12 z-score in height-for-age, corresponding to a roughly 10 percent increase compared to the similar aged cohort in the control group.

Utilization

Table 7 depicts the findings on utilization. Cross-section estimates based on equation (1) are given in Panel A. Estimates from a value-added specification, where we use average utilization for outpatient services and deliveries pre-intervention as a lagged variable, are shown in Panel B, and estimates from a difference-in-differences specification are reported in Panel C.[16]

Four year after the intervention started, we find large positive effects on all four utilizations measures. Although the point estimates are not individually significantly different from zero in the cross-section specification (see Panel A), the average standardized effect, reported in column (v), is significant. The precision improves and point estimates are larger in the value-added specification, in which utilization for general outpatient services and deliveries are significantly different from zero at the 10 percent and 5 percent level, respectively. For both the value-added and the difference-in-differences specifications, the average standardized effects are highly significant.

The impact on utilization is substantial. For outpatient services, the increase goes from 16 percent in panel A to 27% in panel B. For deliveries, the point estimate in panel B suggests a 50 increase in utilization. For antenatal care, the increase varies between 21 percent (in panel A) to 25 percent in panel B.

The last three columns in table 7, Panel A and C, report changes in utilization patterns based on household data. We collected data on where each household member sought care in case of illness that required treatment. As evident, households in the treatment communities switched from traditional healers and self-treatment, specification (vii), to the project facility, specification (vi), in response to the intervention. The average standardized treatment effects, reported in specification

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[16]We have pre-intervention data for outpatients and deliveries but not antenatal care and family planning. Thus, we use the average utilization for outpatient services and deliveries pre-intervention as lagged variable in the value-added model and estimate

$$y_{jst} = \alpha_{VA} + \beta_{VA} T_j + \lambda \bar{y}_{jt-1} + \varepsilon_j .$$

where y_{sjt} is utilization for service s in facility j at time t, and $\bar{y}_{jt-1}$ is the average utilization for outpatient services and deliveries in the pre-intervention period $t-1$. The difference-in-differences models can only be estimated on outpatient services and deliveries for which we have pre-intervention data.

(viii), are significant in both the cross-section and in the panel specifications.

4.3   Processes and health treatment practices

The findings in section 4.2 show that the intervention, which resulted in large improvements in health outcomes and utilization in the short-run (Björkman and Svensson, 2009), also resulted in long-run improvements in health. Below we try to identify possible channels through which these changes came about. The intervention was intended to improve health outcomes by strengthening the community's ability to monitor and hold providers to account, which in turn would influence health workers incentives to provide both more and better health care. We next turn to assessing the evidence for these two links.

Processes

In table 8 we use data on a set of process measures that proxy for the community's ability and extent of monitoring and engagement. The data in panel A were collected through visual checks by the enumerators during the post-intervention health facility survey. Four years after the initial intervention, the average standardized effect of having a suggestion box, numbered waiting cards, a staff duty roster, and posters informing patients' about their rights and that services are free is significantly positive. For two of the variables, having a suggestion box and a staff duty roster, the individual effects are also significantly positive and the point estimates, relative to the control group, are fairly large.

Panel B uses information collected at the household level. There are positive long-run effect on all individual indicators; i.e. whether the household has received information about the HUMC's role and responsibilities, whether the performance of health workers was discussed in Local Council meetings, whether a household member told the staff about their (dis)satisfaction with the health services received, whether they think that the staff at the facility works closely with the community, and whether they know somebody who monitored the health facility and staff performance.

Health treatment practices

Tables 9-12 focus on the health workers behavior; i.e. treatment practices and management.

Table 9 looks at a set of generic measures of behavior. The intervention had no significant long-run impact on the likelihood that any equipment was used during the patient's last examination, column (i), or waiting time before being examined, column (ii). Note, however, that as utilization increased in the treatment group (see table 7), the total number of patients examined with equipment increased in the treatment facilities. Column (iii) reports the impact on the absence rate, based on data from three unannounced visits. The point estimate is close to zero and insignificant. There is suggestive evidence, however, that the absence rate for staff residing in the community

fell in the treatment clinics (results available upon requests).

The general condition of the clinic – a measure of the conditions of the floor, walls and furniture and the smell of the clinic – remained significantly better in the treatment group also in the long run (column iv).[17] The effect is substantial, with the mean treatment facility having a 0.5 standard deviation higher score compared to the average control facility. There is also a significant decrease in the probability of drug stock-outs in the long-run, column (v), although there is no systematic difference in the supply of drugs between the treatment and control groups.[18]

Table 10 focuses on the extent to which clinical guidelines for the examination of patients were followed. We collected detailed data on antenatal and postnatal care. Four years after the initial intervention, health workers in the treatment communities appear to provide more effort, or better quality, in their interactions with pregnant women and infants. During antenatal care visits, pregnant women were more likely to be examined by a midwife; to have their weight taken; to have a blood sample taken; to have the fetus checked; and to receive information about pregnancy complications. Newborn children were also more likely to be checked at the facility in the first two months after delivery. For clinical guidelines that are seldom followed in the control group – checking the newborn in the first two months after delivery (a 24 percent increase in the treatment compared to the control group) and measuring weight and taking blood samples of pregnant women (a 22 percent increase in the treatment compared to the control group) – the treatment effects are substantial.

Table 11 includes result on health education on three of the most prevalent diseases in Uganda: HIV/AIDS, tuberculosis and malaria. Column (i) shows, not surprisingly given the mean in the control group of 0.99, that there is no impact on whether household members had heard of AIDS. But the intervention decreased the probability that the household members expressed stigmatization about AIDS (as measured by agreeing with the statement that people living with AIDS should be ashamed of themselves) and increased the knowledge that tuberculosis is spread through the air and the probability that children were sleeping under a treated mosquito net.

Table 12 reports impact on immunization by children's age group.[19] Two results stand out from the table. First, while the point estimates are positive, the treatment effects are small (between 3%-6%) and insignificantly different from zero. Second, coverage in the control group varies between

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[17]The enumerators visually checked the condition of the health clinics; i.e. whether floors and walls were clean, the condition of the furniture and the smell of the facility. From these data, we constructed a summary indicator using principal components analysis. The variable general condition of the clinic is the standardized (using control group mean and standard deviation) first component.

[18]See section 6.3.

[19]For each age group, we use information on how many times (doses) in total each child has received polio, DPT, BCG, and measles vaccines and vitamin A supplements. On the basis of the recommended immunization plan, we create indicator variables taking the value of 1 if child i of cohort (age) j had received the required dose(s) of measles, DPT, BCG, and polio vaccines, respectively, and 0 otherwise. We then estimate equation (3), for each age group, and calculate average standardized effects.

78%-94%, which is a doubling of the immunization rates since 2005. As almost nine out of ten children received immunizations according to the recommended immunization plan, there is little room for improvement. Moreover, the sample sizes in table 12 are insufficient to detect small significant treatment effects at standard levels of significance.[20]

5 Impact of the participation intervention

The short-run evaluation of the *participation & information* intervention showed that efforts to stimulate community monitoring can have large effects on both the quantity and the quality of service provision and in the end result in improved health outcomes (Björkman and Svensson, 2009). The results presented in Section 4 shows that the main results were sustained four years into the intervention. Collecting, assembling, and disseminating information on performance in a rural setting, however, is both technically complicated and costly. Moreover, as the intervention involved both information dissemination and various techniques to stimulate engagement and participation, it is unclear if and to what extent both components are crucial in order to initiate a successful process of community involvement and monitoring.

To address these issues, we designed a new intervention in 2007. The *participation intervention* was designed so as to mimic the *participation & information* intervention in all aspects, with the key exception being that the facilitators did not provide the participants with quantitative data on performance of the provider in the community and health facility meetings. Hence, in the *participation intervention* the communities' informational constraints were not addressed.

5.1 Balance at baseline

As evident from panel B in table 1, columns (iv)-(vi) and panel B in table 2, column (ii), there are no systematic differences between the treatment and the control group on any of the baseline characteristics. Thus, the sample is balanced. Comparing characteristics of the two control groups in the two different experiments at the same point in time (in 2006) also shows that there was no systematic difference across the two experiments (see Table A.1 for details).

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[20]The World Health Organization and other UN agencies have worked intensively with the Ministry of Health in Uganda on large-scale and country-wide immunization campaigns during the period of the study. This may explain the improvement in immunization coverage in both the treatment and the control group

5.2 Main outcomes: Health outcomes and quantity of care

Health outcomes

Table 13 reports the short-run findings on health outcomes. The control group means are similar to those reported in tables 3-5 for the *participation & information* intervention, but the treatment effects are small and insignificant. That is; there are no statistical differences in number of births (column i); number of pregnancies (column ii); rate of under-1 deaths (column iii); infant mortality (column iv); and weight-for-age and height-for age z-scores (columns v-viii) between the treatment and the control group.[21] Thus, based on health outcomes only, the participation intervention had no impact.

Utilization

Table 14 depicts whether the *participation intervention* had a significant effect on utilization. There is no clear evidence of impact. For general outpatient service, the point estimate goes from -100.2 (p-value=0.38) to -20.3 (p-value=0.89) depending on specification. For delivery there is a significant negative effect in two out of three specifications, while the treatment effects are positive, albeit insignificant, for antenatal care and family planning. The average standardized treatment effects are small and insignificantly different from zero in all three specifications (column v).

Columns (vi) and (vii) report changes in utilization patterns based on household data. There is some evidence that treatment households are less likely to use traditional healers and self-treatment, but the average standardized treatment effects are again insignificant.

5.3 Processes and health treatment practices

The *participation intervention* did not result in improvements in child health or change in the quantity of health care used. In the appendix, tables A.2-A.6, we show, using the same outcome variables as presented in section 4.3, that there is no systematic evidence that the intervention resulted in increased monitoring and information exchange in the treatment communities. Management of the facility and the extent to which clinical guidelines were followed also remained unchanged.

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[21]We can implement the DHS synthetic cohort life table approach to estimate infant (under 1 year) mortality but not under-two mortality rates, as the intervention started in 2007.

6 Mechanism and Discussion: The role of information

The results of the *participation* and the *participation & information* interventions differ markedly. Without information, the process of stimulating participation and engagement had little impact on health workers' performance or the quality of health care. In contrast, when community members are informed about the current state of the health facility's and staff's performance, the same type of a process intervention resulted in significant improvements in health care delivery and health outcomes in both the short and longer run.

In this section we investigate why the provision of information appeared to have played such a crucial role.[22] We start by presenting a stylized example of why information may matter in these kinds of programs. We then use process data from the two treatment groups to identify differences in decisions and actions taken in the two treatment groups (with informed and uninformed community members). We discuss alternative mechanisms and also review the literature on beneficiary control and argue that a key difference between successful and unsuccessful community empowerment interventions is indeed access to relevant information.

6.1 What role does provision of information play?

Why is access to information on the staff's performance a constraint to the community's ability to monitor and hold providers to account? Data from the baseline survey provides some suggestive evidence. For a set of indicators, like waiting time and absenteeism, we have data both from the in-charge of the facility and data collected either through visual inspections (by enumerators) or collected directly from the users. The comparison reveals a systematic pattern: the in-charge underreport performance problems. For example the mean waiting time as reported by the clinician was 2 minutes. The household survey data, however, revealed it was close to 2 hours. Data from unannounced staff surveys showed absence rate around 50 percent. In the in-charge survey, however, the clinic officers did not report problems with absenteeism and explained the low presence at time of the survey as the result of high, but unsubstantiated, level of staff training and outreach. Moreover, when listing key constraints facing the clinic, lack of funding, staff, material, and drugs were high on the agenda, while issues such as weak adherence to clinic guidelines, shirking, or mistreatment of patients were never listed.

Motivated by these examples, we provide a simple stylized model of how the provision of information on mean outcomes, by enabling users to better distinguish between health workers' effort and factors outside the health workers' control, can influence users' willingness to take

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[22]Lack of information was highlighted as a key constrain in the earlier work on community empowerment (see Samuel Paul 1987; Jenkins and Goetz 1999; and Goetz and Jenkins 2001).

actions (to monitor the provider) and in the end health workers' performance.

Consider a setup in which the quality of health service at each visit j, q_j, depends on two factors that are unobserved by the patient: the health worker's performance (p) and resources provided from the center (r_j) , where

$$q_j = p r_j$$

The health worker's performance is a function of the health worker's effort (e). With probability e, the performance is "appropriate" or "high" ($p = 1$) and with probability $1 - e$ the performance is "inappropriate" or "low" ($p = 0$). The health worker chooses e ("effort"), which costs him $c(e) = \frac{1}{2}e^2$. There are two types (T) of health workers: an honest type (H), who always chooses $e = 1$, and an opportunistic type (O), who chooses e so as to maximize his expected utility. Nature chooses type at the start of the period with worker of type O chosen with probability $\phi = 1/2$. As an honest type always chooses high effort, our focus is on the opportunistic type. The patient then seek service at the health facility. We assume that the resource flow from the center, r_j, is a random variable taking the value 1 with probability μ_j and 0 with probability $1 - \mu_j$.

Patients do not observe p_t, e_t, or r_j. However, they know the distribution of r_j and ϕ and observes q_j after each visit. Patients seek service at the health center and care about the quality of health service q_j. As our focus is on the role of information, we disregard collective action problem, and can then drop subscripts j, and just assume there is one representative community member (patient). We treat the representative consumer as myopic, in the sense that she does not aggregate information over visits. Instead we simply assume that the representative consumer observes $q = 1$ with probability $e\mu$ and $q = 0$ with probability $1 - e\mu$. Based on the outcome, the consumer can take local actions at a cost C, which inflicts a cost $\lambda(1 - e)$ on the provider. We can think of these costs as capturing the shame a low performing health worker would suffer if the community express their displeasure with his performance. The costs are proportional to $(1 - e)$ as the cost inflicted on the health workers is increasing in the extent of shirking. The parameter $\lambda < 1$ measures the health worker's sensitivity to local actions by the community. An opportunistic health worker's expected payoff π is

(3) $$\pi = w - c(e) - L\lambda(1 - e)$$

where w is the wage rate and L is a binary variable taking the value 1 if the community decides to take actions against the provider.

The sequence of events is as follows. Nature chooses type at the start of the period 1. The consumer then commits to a local action strategy. Thereafter the health worker makes an unobserved choice of effort. The consumer observes the quality of health service, q, and update beliefs about

the type of health worker. Depending on outcome, she then makes a decision whether or not to take local actions. We denote by $\varphi(\phi|q)$ or ϕ_q the posterior probability that the provider is an opportunistic type, given a realized outcome q and a prior probability ϕ.

Assumption 1: $\lambda\mu > C > \frac{1-\lambda\mu}{2-\mu-\mu\lambda}\lambda\mu$

Scenario 1: Suppose no information is provided. Then provided that assumption 1 holds, it is optimal for the patient to not take any local actions. The (opportunistic) health worker chooses $e = 0$ and $q = 0$.[23]

Scenario 2: Suppose patients receive information on the mean quality of health service; i.e. $E_q[q]$. As expected quality if the provider is an honest type is $E_q[q|H] = \mu$ and an opportunistic type sets $e \leq \lambda$ and consequently $E_q[q|O] \leq \lambda\mu$, the patient will realize that the health worker is an opportunistic type with probability 1. Thus, if assumption 1 holds, the community will take local actions and the health worker will respond by exerting effort $\hat{e}$. The expected quality of health service is $\hat{e}\mu$.

The provision of information solves consumers' aggregation problem. This enables users to distinguish between health workers' type or effort and factors outside the health workers' control. As a result, the return to local action increases. In equilibrium, consumers become more involved; i.e. take local actions, and the health worker responds by exerting higher effort. The likelihood that the community will take local actions is increasing in μ since both the posterior belief that the worker is opportunistic conditional on observing $q = 0$ (ϕ_0) and the opportunistic worker's effort in response to local actions are increasing in μ. An increase in λ; i.e., the health worker's sensitivity to local actions, has two effects. ϕ_0 falls making it less likely that consumer will take local actions after observing $q = 0$. On the other hand, the opportunistic worker's effort in response to local actions increases thus raising the return to local actions.

Assumption 1 does not hold for μ close to 0 and 1. In the first case, public service provision is so poor that the health worker's choice of effort has almost no bearing on health quality, so there are low returns to incentivize the provider. In the second case, the community member can tell with certainty, if observing $q = 0$, that the health worker is an opportunistic type so there is no aggregation problem. Moreover, the difference $\lambda\mu - \frac{1-\lambda\mu}{2-\mu-\mu\lambda}\lambda\mu$ has an inverse U-shape with a maximum at $\mu_{\max} = \frac{1}{1+\lambda}\left(2 - \sqrt{2(1+\lambda)}\right)$; that is, $\mu_{\max}$ is increasing in λ. Thus, if the health worker's sensitivity to local actions is relatively low, provision of information is most likely (i.e. the parameter space for which assumption 1 holds is the largest) to have an effect when public support to frontline providers is relatively poor (μ is relatively small).

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[23]See appendix for details.

6.2 Information and local actions

The example above shows how provision of information on mean outcomes, by enabling users to better distinguish between health workers' effort and factors outside the health workers' control, can influence the community members' willingness to take actions. In table 15, we use data from the implementation phases of the two interventions to corroborate this prediction.

A key component of both experiments was the agreement of an action plan. The action plan outlined the community's and the provider's joint agreement on what needs be done and by whom. As shown in table 15, the process of reaching an agreement looks similar on some observable characteristics. About the same number of community members participated in the community meetings (column i). The two treatment groups also identified roughly the same number of actions to be addressed (column ii). A closer look at the type of actions agreed upon, however, reveals that while the *participation group* identified mostly issues that required third-party actions; e.g. more financial and in-kind support from upper-level authorities and NGOs, and timely delivery of medicines from the center, the *participation & information group* identified almost exclusively (88 percent on average) local issues, which either the health workers or the users could address themselves, including absenteeism, opening hours, waiting time, and patient-clinician interactions (columns iii and iv). Even two years into the *participation & information intervention*, i.e. in 2007, when a shorter repeat engagement was implemented, more than four-fifth of the actions dealt with local issues (column v). Figure 6 illustrates the main result.

The difference in the content of the action plans across the two treatment arms, and the difference in the impact (health outcomes and staff behavior) of the two interventions, are consistent with the hypothesis that when the principal (i.e. community) is informed, efforts to stimulate beneficiary control results in an actionable plan that affects both staff behavior, health utilization and health outcomes. That is, the report card component helped build a reform agenda on the "true", as opposed to the perceived, status of service provision. But the findings do not rule out other explanations. To further examine the plausibility of the information and local actions channel as a key mechanism for the health utilization and health outcomes treatment effects, we exploit variation within the two treatment arms. Specifically, we use the difference in number of outpatients served before and after treatment in treatment clinics compared to the mean difference in the control clinics as dependent variable; i.e., $\bar{y}_j = \left(y_{tj}^T - y_{t-1j}^T \right) - \left(\bar{y}_t^C - \bar{y}_{t-1}^C \right)$, and regress $\bar{y}_j$ on the share of local issues raised in the action plan. The estimated relationship between the difference in number of outpatient served before and after treatment in treatment compared to control clinics, conditional on the share of local actions agreed upon in the action plan, is illustrated in figure 7. There is a consistent pattern across clinics and treatment arms. The *participation & information group* identified a significantly larger share of local actions and a higher share of local actions is associated with a larger, and significant, increase in number of outpatients served over time and

21

compared to the control group.

6.3 Alternative mechanisms

The findings of large treatment effects in the *participation & information* intervention are consistent with the community-based monitoring mechanism discussed above. But the findings do not rule out other explanations. In particular, other actors in the supply chain; e.g., district or sub-district management, may have changed their behavior or support in response to the intervention, for example by providing additional funding or other support to the treatment facilities. We do not find any evidence of this being the case (see appendix). The treatment facilities did not receive more drugs or funding from the sub-district or district as compared to the control facilities. Moreover, the level of supervision of providers by upper-level government authorities remained low in both the treatment and the control group. There is also no difference between treatment and control facilities in the number of staff that voluntarily left the facility.

6.4 Discussion

Over the past decade, the World Bank alone has allocated close to $85 billion to local participatory development programs. A recent comprehensive review of those programs (Mansuri and Rao, 2013) notes that few are systematically evaluated and that the enthusiasm for participatory initiatives is driven more by ideology and optimism than by analysis, either theoretical or empirical.

In this paper, we present the results of two field experiments. First, we assess the longer run impact of an intervention combining the standard package of facilitated meetings to enhance participation with the dissemination of report card information on the facility's performance. While lack of evidence of long-run impact is not specific to studies on beneficiary control, we believe it is of particular concern here since public service providers can easily adjust their behavior for a short period of time given the large pre-existing X-inefficiencies in public service provision. Permanent changes in behavior may be harder to institute. Moreover, interventions aimed at increasing community participation are primarily meant to influence norms and collective actions and thus influence local decision making both in the short and in the longer run.

Second, we assess the short-run impact of an intervention involving only the standard package of facilitated meetings to enhance participation. The focus on participation alone is typical for most Community Driven Development (CDD) programs.

The process of stimulating participation, when the community does *not* have access to information on performance, resulted in a joint agreement focusing on issues requiring third-party actions. The intervention had little impact on health workers' behavior or the quality of health care. In

contrast, with an informed community, the same type of a process intervention resulted in a joint agreement identifying almost exclusively issues that either health workers or the users could address themselves, and significant improvements in health care delivery and health outcomes in both the short and longer run.

The results of the two field experiments resonate well with the mixed findings in the literature. Banerjee et al. (2010) study beneficiary control in Uttar Pradesh, India. In one of their treatment arms, trained facilitators held small-group discussions with parents and provided them with simple tools to enable them to generate their own information about their children's learning outcomes. This information was later shared in a village meeting where the school teachers was also asked to provided general information about resources available at the school. No information was provided about the teachers' performance. While test scores are likely correlated with the teachers' effort, it is plausible that parents would (correctly) infer that the variation in test scores are to a large extent driven by child and household specific factors, rather than teacher effort, and they might, possibly incorrectly, infer that the lack of resources that have been highlighted in the meetings is also a quantitatively important factor. Thus, even if parents viewed these test scores as a credible and objective measure of what their children know, they might be skeptical about using these test scores as an indicator of the school's performance. As in the participation intervention evaluated above, the village meetings were dominated by discussions about third-party actions (e.g. the state government's scholarship program and a new school-meals program) not the teachers' performance or the learning environment. The intervention prompted no increased teacher effort and no improvement in education outcomes.

Olken (2007) evaluates different ways of monitoring corruption in a road construction project in Indonesia. In one of the experiments, invitations were sent out to village-level meetings where project officials documented how they spent project funds for local road construction. This provided villagers with new information, but at best only indirect information about the key outcome variable - corruption - as project officials and/or elite community member may be able to hide it when reporting on how funds were used.[24] Thus, it is unclear whether the non-elite community members were really more informed about corruption in the project and consistent with the results above, corruption problems were seldom discussed in the village meetings, and the intervention had little impact.

Duflo et al. (2012) evaluate a contract teachers program in Kenya. One of their treatment arms included School-Based Management (SBM) training of the PTA committee. Committee members were then given specific training on how to monitor and assess teachers' effort and performance and a set of parents were asked to perform attendance checks on the teachers on a regular ba-

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[24]Corruption is not easily observable and measurable, evidenced by the Olken (2007)'s novel, but burdensome strategy, to estimate it.

sis. A formal sub-committee of parents was formed to evaluate the contract teacher and deliver a performance report at the end of the year. While this intervention did not directly provide quantitative information on performance, it provided detailed training to the community on how to measure it throughout the year and how to combine the information in a performance report that could be shared with others. SBM training for PTA committees reduced teachers' absenteeism and increased student test scores.

Collecting data on performance using traditional survey methods is a costly and to some extent technically complex endeavor. This opens up important questions for future research. For example, is it possible to provide beneficiaries with tools to collect performance data and strategies to use them, for instance along the line of Duflo et al. (2012), instead of directly providing them with report cards also in the health sector? Can recent advances in ICT be used to identify ways to assemble and disseminate information more cheaply?

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7 Appendix 1: Details

Note first that if the (opportunistic) health worker faces local actions, the optimal level of effort (found by maximizing (3) with $L = 1$) is $e^*_{L=1} = \lambda$. If he does not face local actions, the optimal effort level is $e^*_{L=0} = 0$. Consider next the consumer. She observes q and revises her beliefs that the health worker is opportunistic $\phi_q = \frac{\Pr(q|O)\Pr(O)}{\Pr(q|O)\Pr(O)+\Pr(q|H)\Pr(H)} = \frac{1-\lambda\mu}{2-\mu-\lambda\mu}$ or honest $(1 - \phi_q)$, taking the worker's incentive into account. The consumer would want to commit to local actions if observing $q = 0$ if

$$\text{(A1)} \qquad \phi_0 \hat{e}\mu + (1 - \phi_0)\mu - L > (1 - \phi_0)\mu$$

where $\hat{e}\mu$ is the (ex ante) expected quality of health services if the health worker is opportunistic and the community takes local actions and μ is the (ex ante) expected quality of health services if the health worker is honest. The left-hand side of (A1) is the expected net gain of taking local actions, while the right-hand side of (A1) is the expected payoff of not taking local actions.

Note that (A1) can be simplified to

$$\text{(A2)} \qquad \frac{1 - \lambda\mu}{2 - \mu - \lambda\mu}\lambda\mu > L$$

That is, if assumption 1 holds, the community will not take local actions. As a result, the opportunistic worker will exert no effort.

FIGURE 1: TIMELINE FOR THE PARTICIPATION & INFORMATION INTERVENTION

2004	Early 2005	Mid-2005	2006	Early 2007	2008	Early 2009
Baseline data collection	**Participation & Information intervention** (5 days)	**1-day review**	Follow-up data collection: ➔ 1-year evaluation	**Repeat engagement** (3 days)	**1-day review** (1 day)	Second follow-up data collection ➔ Long-term evaluation: *Participation & Information intervention*
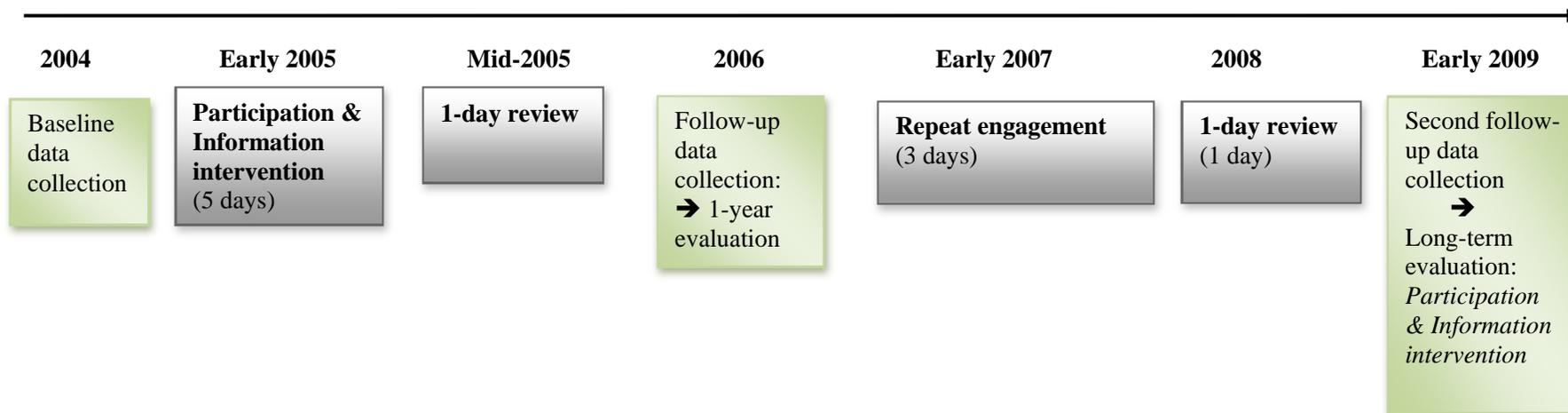
FIGURE 2: TIMELINE FOR THE PARTICIPATION INTERVENTION

2006	Early 2007	2008	Early 2009
Baseline data collection	**Participation intervention** (5 days)	**Mid-term review** (1 day)	Follow-up data collection ➔ Evaluation: *Participation intervention*
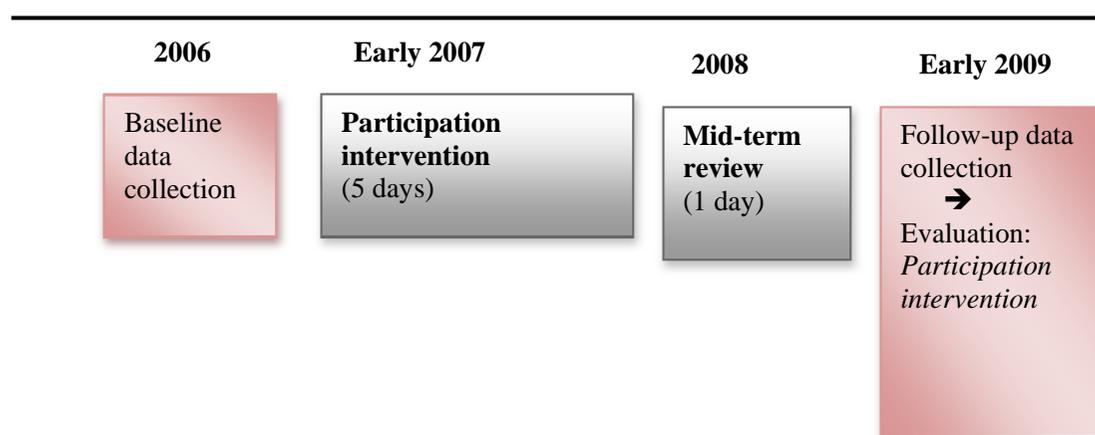
FIGURE 3: DATA COLLECTION

| 2004 | 2006 | 2009 |

Participation & Information experiment:
Baseline data collection:
- 50 facilities (25 treatment)
- 4978 households in catchment area (2543 treatment)

Participation & Information experiment:
Follow-up data collection: ➔
Short-term evaluation

Participation experiment:
Baseline data collection:
- 25 facilities (13 treatment)
- 2841 households in catchment area (1548 treatment)

Participation & Information experiment:
Second follow-up data collection: ➔
Long-term evaluation
- 50 facilities
- 5021 households

Participation experiment:
Follow-up data collection: ➔
Short-term evaluation
- 25 facilities
- 2590 households

FIGURE 4: DISTRIBUTION OF WEIGHT-FOR-AGE Z-SCORES FOR THE TREATMENT AND CONTROL GROUPS – PARTICIPATION & INFORMATION MODEL (2004-2008)
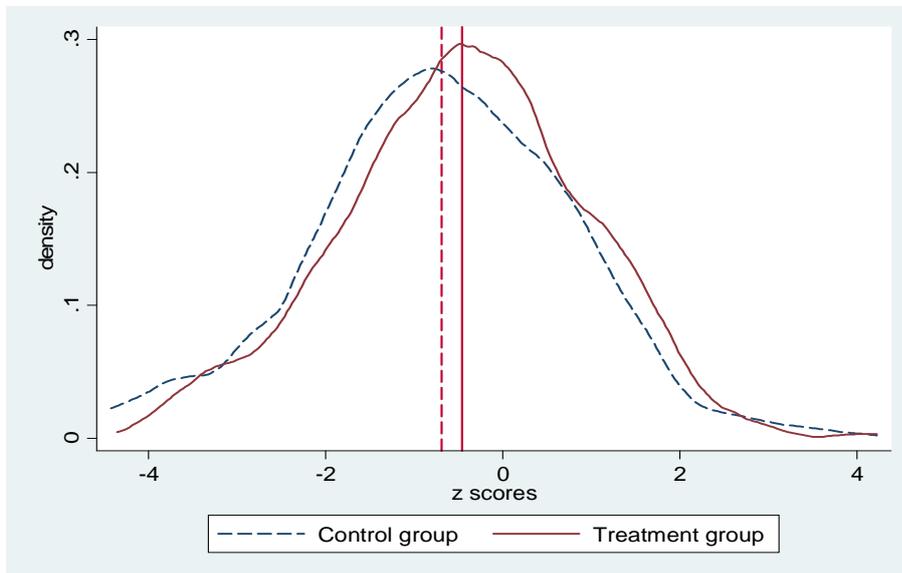
Notes: Weight-for-age z-scores for children under 12 months excluding observations with recorded weight above the 90th percentile in the growth chart reported in Cortinovis et al. (1997). Sample size is 669 children. Solid line depicts the distribution for the treatment group and dashed line the distribution for the control group. Vertical solid line denotes mean in treatment group; dashed line denotes mean in control group.

FIGURE 5: TREATMENT EFFECT ON HEIGHT-FOR-AGE Z SCORES CONDITIONAL ON TIME OF EXPOSURE - PARTICIPATION & INFORMATION MODEL
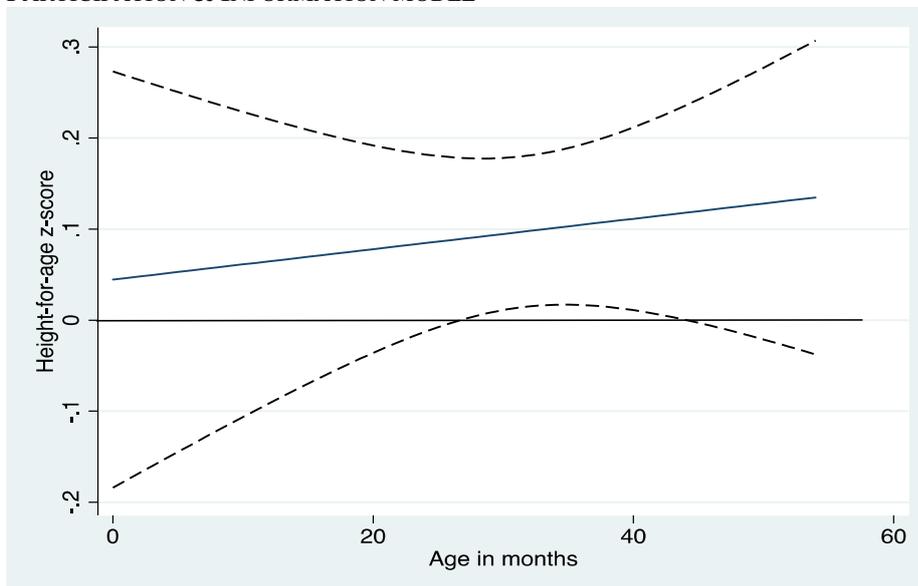
Notes: Figure plots the treatment effect conditional on months of exposure (solid line) and the 95 confidence intervals (dashed lines); i.e. the derivative $\partial z/\partial T$, where z is the height-for-age z-scores for children and T is the treatment indicator, of equation (1) augmented with age (in month) and $age \times T$ and with height-for-age z-scores for children under 60 months as dependent variable.
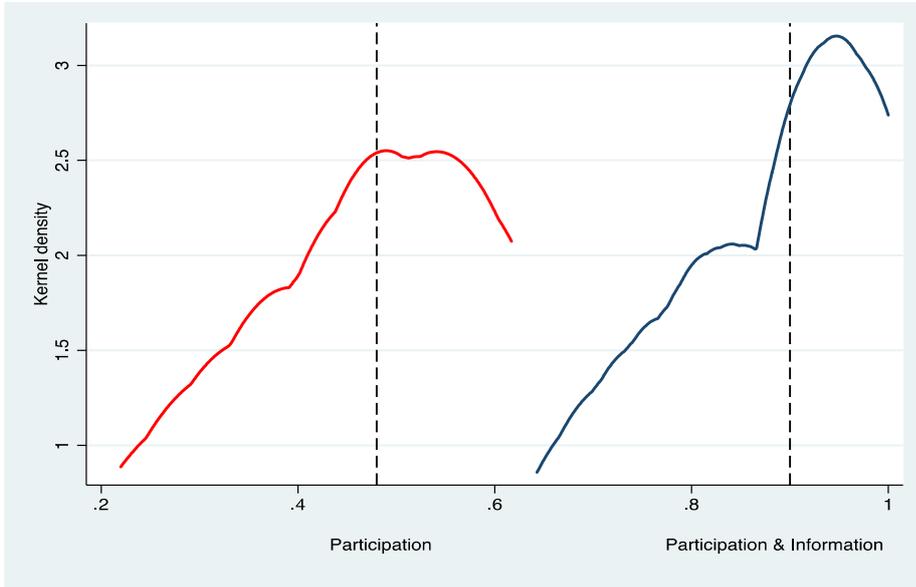
Notes: Figure plots the kernel density for the share of local actions for both treatment groups out of the total amount of actions listed in the joint action plan.

FIGURE 7: OUTPATIENT CARE CONDITIONAL ON THE SHARE OF LOCAL ACTION
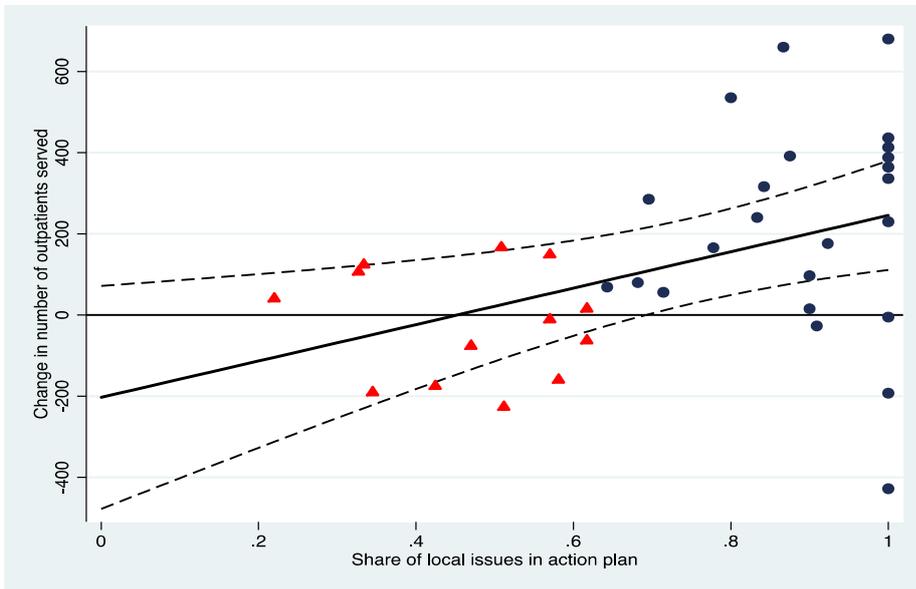
Notes: Figure plots the difference in outpatient served before and after treatment in treatment clinics (*participation intervention* in red triangles and *participation & information intervention* in blue dots) compared to control clinics, conditional on the share of local actions agreed upon in the action (solid line) and the 95 confidence intervals (dashed lines).

TABLE 1: PRE-TREATMENT CHARACTERISTICS IN THE PARTICIPATION & INFORMATION EXPERIMENT (2004) AND PARTICIPATION EXPERIMENT (2006)

	Panel A			Panel B		
	2004			2006		
	Participation & Information sample			Participation sample		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Variables	Treatment group	Control group	Difference	Treatment group	Control group	Difference
Outpatient care	593	675	-82	781	790	-9
	(75)	(57)	(94)	(83)	(95)	(127)
Delivery	10.3	7.5	2.8	12.4	15.9	-3.5
	(2.2)	(1.4)	(2.6)	(1.7)	(3.1)	(3.5)
No. of households in catchment area	2140	2224	-84.4	2850	2519	331
	(185)	(204)	(276)	(338)	(329)	(473)
No. of households per village	93.9	95.3	-1.42	121.9	118.1	3.75
	(5.27)	(6.32)	(8.23)	(10.6)	(12.0)	(16.0)
Drink safely today	0.40	0.32	0.08	-	-	-
	(0.10)	(0.10)	(0.14)			
Days without electricity	18.3	20.4	-2.12	-	-	-
	(2.95)	(2.90)	(4.14)			
Working water source	-	-	-	0.92	1.00	-0.08
				(0.08)	(0.00)	(0.08)
Functioning electricity at hf	-	-	-	0.46	0.42	0.04
				(0.14)	(0.15)	(0.21)

Notes: Key characteristics are catchment area/health facility averages for treatment and control group and difference in averages, with robust standard errors in parentheses. Columns (i)-(iii) corresponds to the 2004 baseline characteristics for the Participation and Information Experiment and columns (iv)-(vi) uses 2006 baseline data for the Participation Experiment. Description of variables: Outpatient care is average number of patients visiting the facility per month for outpatient care. Delivery is average number of deliveries at the facility per month. Number of households in catchment area and number of households per village are based on census data and Uganda Bureau of Statistics maps. Drink safely today is an indicator variable for whether the health facility staff at the time of the pre-intervention survey could safely drink from the water source. Number of days without electricity in the month prior to pre-intervention survey is measured for the last month out of 31 days. Working water source is an indicator variable for whether the health facility in 2006 had a functioning water source. Electricity at the health facility is an indicator variable for whether the main source of electricity health facility functioned during the survey.

	Panel A	Panel B
	2004	2006
	Participation & Information sample	*Participation sample*
Variables	Difference between treatment and control groups	Difference between treatment and control groups
Utilization from health facility records	0.11 (0.77)	0.18 (0.29)
Utilization pattern of the users	-0.48 (0.33)	-0.02 (0.03)
Quality of services according to users	-0.35 (0.84)	-0.02 (0.08)
Catchment area statistics	0.11 (0.66)	0.43 (0.39)
Health facility characteristics	0.14 (0.31)	-0.17 (0.19)
Citizen perceptions of treatment	0.37 (0.67)	0.03 (0.07)
Supply of drugs	0.73 (0.83)	0.47 (0.43)
User charges	-0.65 (0.63)	0.40 (0.30)

Notes: Robust standard errors in parentheses. Average standardized pretreatment effects are derived by estimating equation (1) on each family of outcomes. Column (i) corresponds to the 2004 baseline data for the Participation and Information Experiment and column (ii) uses 2006 baseline data for the Participation Experiment. Utilization from health facility records summarizes outpatients and deliveries. Utilization pattern of the users summarizes seven measures including use of the project facility, an NGO facility, a private-for-profit facility, other government facility, another provider, a traditional healer and self-treatment, reversing sign of traditional healer and self-treatment. Quality of services according to users summarizes the use of any equipment during the visit and waiting time, reversing sign of waiting time. Catchment area statistics summarize four measures about the number of villages in the catchment area and their distance to the health facility. Health facility characteristics uses different data in columns (i) and (ii) because of differences in data collected at the time of each baseline survey. For column (i) it summarize ten measures about the presence of piped water, access to a radio, a newspaper, the existence of a separate maternity unit, the distance to the nearest Local Council I and to the nearest public health provider, number of staff with advanced A-level education and with less than A-level education, drank safely today and days without electricity, reversing sign of days without electricity and distance to nearest local council. For column (ii) it summarizes six measures about the presence of piped water, working water source, functioning electricity, yellow star certification of the health facility, number of staff with advanced A-level education and with less than A-level education. Citizen perceptions of treatment summarize four measures about politeness, attention, freedom to express themselves and information about drug deliveries. Supply of drugs summarizes five measures about the availability of erythromycin, chloroquine, septrine, quinine and mebendazole. User charges summarize four measures about the existence of user charges for drugs, general treatment, injections and deliveries, reversing all signs.

TABLE 3: PROGRAM IMPACT ON HEALTH OUTCOMES (BIRTHS AND PREGNANCIES)
– PARTICIPATION & INFORMATION MODEL (2006-2008)

Dependent variable	Any birth	Number of births	Any pregnancy	Number of pregnancies
Specification	(i)	(ii)	(iii)	(iv)
Program impact	-0.048**	-0.061**	-0.051***	-0.071***
	(0.018)	(0.025)	(0.017)	(0.026)
Mean control group	0.35	0.43	0.39	0.51
Observations	5886	5886	5886	5886

Notes: Estimates from equation (1) with district fixed effects. Robust standard errors clustered by catchment area in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. Specifications: (i) indicator variable for whether any woman in the household has given birth to a child in 2006-2008; (ii) number of births in the household 2006-2008; (iii) indicator variable for whether any woman in the household is or was pregnant 2006-2008; (iii) number of pregnancies in 2006-2008. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

TABLE 4: PROGRAM IMPACT ON HEALTH OUTCOMES (MORTALITY)
– PARTICIPATION & INFORMATION MODEL (2006-2008)

Dependent variable Specification	Rate of under-5 deaths (i)	Rate of under-2 deaths (ii)	Rate of under-1 deaths (iii)	Under-2 mortality rate (iv)	Infant mortality rate (v)
Program impact	-0.0015 (.0010)	- 0.0036* (.0019)	- 0.0068* (.0034)	-18.39** (8.20)	-13.65* (8.02)
Mean control	0.007	0.012	0.019	54.3	45.3
Observations	50	50	50	50	50

Notes: Estimates from equation (1) with district fixed effects and robust standard errors in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. Specifications: (i) number of under-5 deaths over under-5 child-years of observations, 2006-2008; (ii) number of under-2 deaths over under-2 child-years of observations, 2006-2008; (iii) (i) number of under-1 deaths over under-1 child-years of observations, 2006-2008; (iv) Under-2 mortality rate in the community expressed per 1,000 live births (see text for details); (v) Infant or under-1 mortality rate in the community expressed per 1,000 live births (see text for details). *Significant at 10% level ; **Significant at 5% level ;***Significant at 1% level.

TABLE 5: PROGRAM IMPACT ON CHILD DEATHS – PARTICIPATION & INFORMATION MODEL (2006-2008)

Dependent variable Specification	Child death (i)	Infant death (ii)
Program impact×Age 0-11m	-0.015* (0.008)	
Program impact×Age 12-23m	-0.004 (0.003)	
Program impact×Age 24-35m	0.004 (0.003)	
Program impact×Age 36-47m	0.007 (0.005)	
Program impact×Age 48-59m	0.014* (0.008)	
Program impact×Year of birth 2008		-0.016 (0.016)
Program impact×Year of birth 2007		-0.012 (0.011)
Program impact×Year of birth 2006		-0.013 (0.012)
Mean control: Age 0-11months	0.049	
Mean control: Age 12-23 months	0.011	
Mean control: Age 24-35 months	0.003	
Mean control: Age 36-47 months	0.005	
Mean control: Age 48-59 months	0.000	
Mean control: Birth-year 2006		0.039
Mean control: Birth-year 2007		0.053
Mean control: Birth-year 2008		0.057
Observations	9116	2847

Notes: Data represents a panel with child-year observations. Estimates from equation (1) with district fixed effects and robust standard errors in parentheses, clustered by catchment area. Program impact measures the coefficient on the assignment to treatment indicator. Specifications: (i) The dependent variable is an indicator variable for child death and the regression includes a full set of cohort indicators; (ii) The dependent variable is an indicator variable for infant death and the regression includes a full set of year-of-birth indicators. *Significant at 10% level ; **Significant at 5% level ; ***Significant at 1% level.

TABLE 6: PROGRAM IMPACT ON HEALTH OUTCOMES (WEIGHT AND HEIGHT OF CHILDREN) – PARTICIPATION & INFORMATION MODEL (2006-2008)

Dependent variable	Weight-for-age z-scores (12-59m)	Weight-for-age z-scores (0-11m)	Weight-for-age z-scores (0-11m)	Height-for-age z-scores (12-59m)	Height-for-age z-scores (12-59m)	Height-for-age z-scores (0-11m)
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Program impact	0.01 (0.07)	0.22** (0.10)	0.20* (0.10)	0.09* (0.05)	0.10* (0.05)	0.04 (0.10)
Child age (log)	-	-	-0.51*** (0.08)	-	0.22*** (0.07)	-
Female	-	-	0.17 (0.12)	-	0.18*** (0.05)	-
Mean control	-0.92	-0.70	-0.70	-1.52	-1.59	-1.25
Observations	2839	669	669	2451	2451	565

Notes: Estimates from equation (1) with district fixed effects. Robust standard errors clustered by catchment area in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. Specifications: (i) Weight-for-age z-scores (WAZ) for children 12-59 months excluding observations with recorded weight above the 90th percentile in the growth chart reported in Cortinovis et al. (1997); (ii)-(iii) WAZ for children 0-11 months excluding observations with recorded weight above the 90th percentile in the growth chart reported in Cortinovis et al. (1997); (iv)-(v) Height-for-age z-scores (HAZ) for children 12-59 months excluding observations with recorded height above the 90th percentile in the growth chart reported in Cortinovis et al. (1997); (vi) HAZ for children 0-11 months excluding observations with recorded height above the 90th percentile in the growth chart reported in Cortinovis et al. (1997). *Significant at 10% level ; **Significant at 5% level ;***Significant at 1% level.

TABLE 7: PROGRAM IMPACT ON UTILIZATION/COVERAGE USING FACILITY AND HOUSEHOLD DATA– PARTICIPATION & INFORMATION MODEL (2006-2008)

Dep. variable	Out-patients	Delivery	Antenatal	Family planning	Average std effect	Use of Project facility	Self-treatment/ traditional healer	Average std effect
PANEL A: Cross section	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Program impact	97.5	6.03	18.0	4.89	0.37*	0.018*	-0.013*	0.049**
	(63.8)	(3.70)	(14.5)	(5.45)	(0.21)	(0.011)	(0.007)	(0.021)
Observations	50	50	50	50	50	4981	4981	4981
Mean control	598.7	13.0	84.8	26.4		0.30	0.11	-
PANEL B: Value added								
Program impact	110.1*	7.30**	21.4	4.86	0.43**			
	(62.4)	(3.67)	(15.3)	(5.30)	(0.21)			
Observations	50	50	50	50	50			
PANEL C: Panel data								
Program impact	162.4**	3.70			0.50***	0.037	-0.06*	0.17**
	(70.0)	(2.40)			(0.17)	(0.022)	(0.03)	(0.08)
Observations	100	100			100		9464	9464

Notes: Panel A reports program impact estimates from a cross-sectional model using data from the end line year (2008) with district fixed effects and robust standard errors in parentheses. Panel B reports program impact estimates from a value-added specification, using data from the end line year (2008) and average utilization for outpatient services and deliveries pre-intervention (2004) as lagged variable, with robust standard errors in parentheses. Panel C reports program impact estimates from a difference-in-differences specification with robust standard errors in parentheses. Program impact measures the coefficient on the assignment to treatment indicator in panels A and B, and the assignment to treatment indicator interacted with an indicator variable for 2008 in panel C. Specifications: First column is average number of patients visiting the facility per month for outpatient care; second column is average number of deliveries at the facility per month; third column is average number of antenatal visits at the facility per month; fourth column is average number of family planning visits at the facility per month; fifth column is average standardized effect of estimates in specifications (i)-(iv), respectively; sixth column is the households' share of visits to the project facility of all health visits; seventh column is the households' share of visits to traditional healers and self-treatment of all health visits; eighth column is average standardized effect of estimates in specifications (vi)-(vii), reversing the sign of use of self-treatment or traditional healers.

*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

TABLE 8: PROGRAM IMPACT ON MONITORING AND INFORMATION – PARTICIPATION & INFORMATION MODEL (2006-2008)

PANEL A:

Dependent variable (health facility level)	Suggestion Box	Numbered waiting cards	Staff duty roster	Poster informing free services	Poster on patients' rights	Average standardized effect[#]
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Program impact	0.21**	0.00	0.17*	-0.02	0.08	0.33**
	(0.10)	(0.07)	(0.09)	(0.08)	(0.07)	(0.16)
Mean control group	0.08	0.08	0.08	0.12	0.04	-
Observations	50	50	50	50	50	50

PANEL B:

Dependent variable (household level)	Knowledge about HUMC	Discuss health facility performance and staff in LC meetings[#]	Telling staff if dis/satisfied with quality of service	Health facility staff work closely with community	Knowing someone who monitored facility performance[#]
Specification	(vii)	(viii)	(ix)	(x)	(xi)
Program impact	0.07***	0.14***	0.04*	0.03**	0.10***
	(0.02)	(0.03)	(0.02)	(0.015)	(0.02)
Mean control group	0.14	-	0.25	0.15	-
Observations	5002	2526	3823	4294	4993

Notes: Robust standard errors in parentheses. Error terms are clustered by catchment areas in columns (vii)-(xi). Point estimates, standard errors, and average standardized effect, columns (i)-(vi), are derived from equation (1). Program impact measures the coefficient on the assignment to treatment indicator. Outcome measures in columns (i)-(vi) are based on data collected through visual checks by the enumerators during the post-intervention facility survey. Outcome measures in columns (vii)-(xi) are from the post-intervention household survey. Specifications: column (vi) average standardized effect of the estimates in columns (i)-(v); (vii) indicator variable for whether the household has received information about the Health Unit Management Committee's (HUMC's) roles and responsibilities; (viii) average standardized effect of variables related to whether the household discussed performance, staff and the functioning of the local health facility at the local council meetings during the past year; (ix) indicator variable for whether the household told health facility staff about dis/satisfaction with the quality of service; (x) indicator variable for whether the household thinks health facility staff work closely with the community; (xi) average standardized effect of variables related to whether the household knows community members who monitored facility performance. *Significant at 10%. **Significant at 5%. ***Significant at 1%. [#]denotes average standardized effect.

TABLE 9: PROGRAM IMPACT ON TREATMENT PRACTICES AND MANAGEMENT – PARTICIPATION & INFORMATION MODEL (2006-2008)

Variable	Equipment used	Waiting time	Absence rate	Condition of clinic	Drug stock-outs
Specification	(i)	(iii)	(v)	(vi)	(vii)
	OLS	OLS	OLS	OLS	OLS
Program impact	0.02	7.06	0.02	0.8*	-0.06*
	(0.03)	(6.74)	(0.03)	(0.45)	(0.04)
Mean control group	0.33	113.08	0.46	-0.36	0.57
Observations	3612	3829	50	50	41

Notes: Point estimates and standard errors are derived from equation (1) in the OLS specifications and equation (2) in the DD specifications. Robust standard errors clustered by catchment areas in parentheses. Program impact measures the coefficient on the assignment to treatment indicator in the OLS models and the assignment to treatment indicator interacted with an indicator variable for 2009 in the DD models. Specifications: (i) and (ii) indicator variable for whether the staff used any equipment during examination when the patient visited the health facility; (iii) and (iv) difference between the time the patient left the facility and the time the patient arrived at the facility, minus the examination time; (v) ratio of workers not physically present at the time of three unannounced visits during three months in 2009 to the number of workers employed.(see text for details); (vi) first component from a principal components analysis of four variables: condition of the floors of the health clinic, condition of the walls, condition of furniture, and smell of the facility, where each condition is ranked from 1 (dirty) to 3 (clean) by the enumerators; (vii) share of months in 2009 in which stock cards indicated no availability of drugs (see text for details). *Significant at 10%. **Significant at 5%. ***Significant at 1%.

TABLE 10: PROGRAM IMPACT ON ANTENATAL AND POSTNATAL CARE – PARTICIPATION & INFORMATION MODEL (2006-2008)

Dependent variable	Examined by midwife during antenatal visit at the health facility	Weight taken during antenatal visit	Blood sample during antenatal visit	Fetus checked during antenatal visit	Told about pregnancy complications during antenatal visit	Newborn checked at the health facility in the first two months after delivery	Average Standardized Effect
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Program impact	0.09**	0.13***	0.13**	0.03***	0.06**	0.09**	0.20***
	(0.03)	(0.04)	(0.05)	(0.01)	(0.03)	(0.03)	(0.05)
Mean control group	0.87	0.59	0.52	0.95	0.52	0.37	-
Observations	1152	1151	1146	1148	1148	1711	992

Notes: Point estimates and standard errors are derived from equation (1) with district fixed effects. Robust standard errors clustered by catchment areas in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. The data is from the household survey of women who experienced a pregnancy in the last 2 years. Specifications: (i) indicator variable for whether the woman was examined by midwife during the antenatal visit; (ii) indicator variable for whether the woman's weight was taken during the antenatal visit at the health facility; (iii) indicator variable for whether a blood sample was taken during the antenatal visit; (iv) indicator variable for whether the was checked during the antenatal visit; (v) indicator variable for whether the woman was told about pregnancy complications; (vi) indicator variable for whether the newborn was checked at the health facility in the first two months after delivery; (vii) average standardized effect of the estimates in columns (i)-(vi). *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.

TABLE 11: PROGRAM IMPACT ON HEALTH EDUCATION– PARTICIPATION & INFORMATION MODEL (2006-2008)

Dependent Variable	Have heard about AIDS	AIDS stigma	Knowledge that TB is spread through the air	Children sleeping under a treated mosquito net
Specification	(i)	(ii)	(iii)	(iv)
Program impact	0.00	-0.02***	0.04**	0.06**
	(0.00)	(0.01)	(0.02)	(0.02)
Mean control group	0.99	0.09	0.53	0.38
Observations	5002	4907	4880	2925

Notes: Point estimates and standard errors are derived from equation (1) with district fixed effects. Robust standard errors clustered by catchment areas in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. The data come from household interviews. Specifications: (i) indicator variable for whether the household member has heard about AIDS; (ii) indicator variable for whether the household member agree with the statement that people with AIDS should be ashamed of themselves; (iii) indicator variable for whether the household member knows that TB is spread through the air; (iv) indicator variable for whether the household's children are sleeping under a treated mosquito net. *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.

TABLE 12: PROGRAM IMPACT ON IMMUNIZATIONS – PARTICIPATION & INFORMATION MODEL (2006-2008)

Group Specification	Newborn (i)	Less than 1-year (ii)	1-year old (iii)	2-year old (iv)	3-year old (v)	4-year old (vi)
Average standardized effect	0.03 (0.03)	0.02 (0.03)	0.03 (0.04)	0.05 (0.04)	0.05 (0.04)	0.06 (0.05)
Mean control group	0.78	0.83	0.89	0.91	0.92	0.94
Observations	2535	2481	1894	1325	826	372

Notes: Average standardized effects with the dependent variables being indicator variables for whether the child has received at least one dose of measles, DPT, BCG, and Polio, respectively (see text for details) and with district fixed effects. Robust standard errors clustered by catchment areas in parentheses. Groups: (i) Children under 3 months; (ii) Children 0-12 months; (iii) Children 13-24 months; (iv) Children 25-36 months; (v) Children 37-48 months; (vi) Children 49-60 months. *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.

TABLE 13: PROGRAM IMPACT ON HEALTH OUTCOMES – PARTICIPATION MODEL (2007-2008)

Dependent variable	Number of births	Number of pregnancies	Rate of under-1 deaths	Infant mortality rate	Weight-for-age z-scores (0-11m)	Weight-for-age z-scores (0-11m)	Height-for-age z-scores (12-59m)	Height-for-age z-scores (12-59m)
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Program impact	0.00	-0.01	0.0015	-4.59	0.20	0.14	0.03	0.03
	(0.04)	(0.05)	(0.0085)	(10.98)	(0.11)	(0.10)	(0.08)	(0.08)
Child age (log)	-	-	-	-	-	-0.39***	-	0.45**
						(0.08)		(0.09)
Female	-	-	-	-	-	0.30**	-	0.11**
						(0.14)		(0.05)
Mean control group	0.43	0.52	0.030	34.1	-0.65	-0.65	-1.50	-1.43
Observations	3328	3328	25	25	423	423	1452	1821

Notes: Estimates from equation (1) with district fixed effects. Robust standard errors in parentheses (i-iv), clustered by catchment area (i-ii), (v-viii). Program impact measures the coefficient on the assignment to treatment indicator. Specifications: see notes to tables 3-5 for details.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

TABLE 14: PROGRAM IMPACT ON UTILIZATION/COVERAGE USING FACILITY AND HOUSEHOLD DATA– PARTICIPATION MODEL (2007-2008)

Dep. variable	Out-patients	Delivery	Antenatal	Family planning	Average std effect	Use of Project facility	Self-treatment/ traditional healer	Average std effect
PANEL A: Cross section	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Program impact	-100.2	-11.9**	9.6	3.0	-0.27	0.004	-0.022**	0.046
	(114.4)	(5.9)	(19.2)	(4.6)	(0.30)	(0.014)	(0.009)	(0.028)
Observations	25	25	25	25	25	2825	2825	2825
Mean control	649.6	23.9	106.9	18.6		0.32	0.11	-
PANEL B: Value added								
Program impact	-34.4	-5.7**	2.9	4.0	0.01			
	(77.7)	(2.6)	(14.2)	(4.0)	(0.14)			
Observations	25	25	25	25	25			
PANEL C: Panel data								
Program impact	-20.3	-5.9	0.32	5.4	-0.01	0.02	-0.03	0.09
	(150.4)	(7.5)	(26.3)	(6.8)	(0.41)	(0.03)	(0.03)	(0.09)
Observations	50	50	50	50	50	5657	5657	5657

Notes: Panel A reports program impact estimates from a cross-sectional model using data from the end line year (2008) with robust standard errors in parentheses. Panel B reports program impact estimates from a value-added specification, using data from the end line year (2008) and average utilization for outpatient services and deliveries pre-intervention (2006) as lagged variable, with robust standard errors in parentheses. Panel C reports program impact estimates from a difference-in-differences specification with robust standard errors in parentheses. Program impact measures the coefficient on the assignment to treatment indicator in panels A and B, and the assignment to treatment indicator interacted with an indicator variable for 2008 in panel C. Specifications: see notes in Table 7 for details. *Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

TABLE 15: INFORMATION PROCESSES AND LOCAL ACTIONS

Dependent variable	Participants in community meetings	Number of actions in contract	Share of upper-level actions in initial action plan	Share of local-level actions in initial action plan	Share of local-level actions in 2007
Specification	(i)	(ii)	(iii)	(iv)	(v)
Participation & information model	131.4	13.0	0.12	0.88	0.80
	(7.4)	(1.1)	(0.02)	(0.02)	(0.03)
Participation model	144.5	12.9	0.50	0.50	0.50
	(10.0)	(1.4)	(0.04)	(0.04)	(0.04)
Difference	-13.2	0.04	-0.39***	0.39***	0.30***
	(12.4)	(1.8)	(0.04)	(0.04)	(0.05)
P-value	0.30	0.98	0.00	0.00	0.00
Observations	38	38	38	38	38

Notes: Mean outcomes in the treatment group and the difference between the two treatment groups, with robust standard errors in parentheses. Specifications: (i) number of participants in the community meeting; (ii) number of actions agreed upon by community members and health facility staff in the joint action plan; (iii) share of upper-level actions; i.e. actions requiring third-party involvement out of the total number of actions agreed upon in the initial action plan (2005 for the Participation & Information experiment and 2007 for the Participation experiment); (iv) share of local-level actions; i.e. actions that either the health workers or the users could address themselves, out of the total number of actions agreed upon in initial action plan (2005 for the Participation & Information experiment and 2007 for the Participation experiment); (v) Share of local-level actions in the action plans conducted in 2007 (repeat action plan for the Participation & Information Model and initial action plan for the Participation Model).

TABLE A.1: COMPARISON OF CONTROL GROUP CHARACTERISTICS IN 2006

Variables	Participation & Information Control group	Participation Control group	Control group Difference	Difference between control groups
Specification	(i)	(ii)	(iii)	(iv)
Out-patient care	651.5	730.2	-78.7	
	(43.0)	(65.9)	(78.5)	
Delivery	12.3	19.9	-7.7	
	(2.5)	(3.9)	(4.6)	
No. households in catchment area	2224	2519	295	
	(206)	(325)	(385)	
No. households per village	95.3	118.1	-22.8*	
	(6.4)	(11.8)	(13.4)	
Working water source	0.96	1.0	-0.04	
	(0.04)	(0.0)	(0.04)	
Functioning electricity at hf	0.44	0.42	0.02	
	(0.10)	(0.15)	(0.18)	
	(7.9)	(14.4)	(16.4)	
Average standardized effects				
Utilization from health facility records				-0.49
				(0.33)
Utilization pattern of the users				-0.04
				(0.03)
Quality of services according to users				0.05
				(0.14)
Catchment area statistics				-0.22
				(0.25)
Health facility characteristics				-0.23
				(0.19)
Citizen perceptions of treatment				0.01
				(0.06)
Supply of drugs				-0.56
				(0.34)
User charges				-0.86**
				(0.32)

Notes: Key characteristics are catchment area/health facility averages for the two control groups (Participation & Information model and Participation model) in 2006 and difference in averages, with robust standard errors in parentheses. Description of variables: see Table 1 for details. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

TABLE A.2: PROGRAM IMPACT ON MONITORING AND INFORMATION – PARTICIPATION MODEL (2007-2008)

Panel A:						
Dependent variable (health facility level)	Suggestion box	Numbered waiting cards	Staff duty roster	Poster informing free services	Poster on patients' rights	Average standardized Effect[#]
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Program impact	-0.03	-0.08	0.08	-0.27[**]	-0.00	-0.12
	(0.12)	(0.11)	(0.11)	(0.11)	(0.13)	(0.17)
Mean control group	0.17	0.17	0.08	0.25	0.17	-
Observations	25	25	25	25	25	25
Panel B:						
Dependent variable (household level)	Knowledge about HUMC	Discuss health facility performance and staff in LC meetings[#]	Telling staff if dis/satisfied with quality of service	Health facility staff work closely with community	Knowing someone who monitored facility performance[#]	
Specification	(vii)	(viii)	(ix)	(x)	(xi)	
Program impact	0.02	0.09[***]	-0.03	0.01	0.06[**]	
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	
Mean control group	0.12	-	0.28	0.15	-	
Observations	2840	1507	2202	2505	2838	

 Notes: Robust standard errors in parentheses. Disturbance terms are clustered by catchment areas in columns (vii)-(xi). Point estimates, standard errors, and average standardized effect, columns (i)-(vi), are derived from equation (1). Program impact measures the coefficient on the assignment to treatment indicator. Specifications: see Table 6 for details. *Significant at 10%. **Significant at 5%. ***Significant at 1%. [#]denotes average standardized effect.

TABLE A.3: PROGRAM IMPACT ON TREATMENT PRACTICES AND MANAGEMENT– PARTICIPATION MODEL (2007-2008)

Variable	Equipment used	Equipment used	Waiting time	Waiting time	Absence rate	Condition of clinic	Drug stock-outs
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
	OLS	DD	OLS	DD	OLS	OLS	OLS
Program impact (Treatment*2009)	0.01 (0.04)	0.01 (0.05)	-2.83 (11.7)	-5.00 (12.0)	-0.04 (0.04)	-0.34 (0.46)	0.04 (0.07)
2009	-	-0.01 (0.03)	-	-7.68 (8.23)	-	-	-
Mean control group	0.38	0.38	129.03	134.21	0.55	0.23	0.54
Observations	2110	4034	2206	4274	25	25	19

Notes: The DD model is from equation (2). The OLS model is from equation (1) with district fixed effects. Robust standard errors, clustered by catchment areas in parentheses. Program impact measures the coefficient on the assignment to treatment indicator in the OLS models and the assignment to treatment indicator interacted with an indicator variable for 2009 in the DD models. Specifications: see Table 7 for details. *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.

TABLE A.4: PROGRAM IMPACT ON ANTENATAL AND POSTNATAL CARE – PARTICIPATION MODEL (2007-2008)

Dependent variable	Examined by midwife during antenatal visit at the health facility	Weight taken during antenatal visit	Blood sample during antenatal visit	Fetus checked during antenatal visit	Told about pregnancy complications during antenatal visit	Newborn checked at the health facility in the first two months after delivery	Average Standardized Effect
Specification	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Program impact	0.01	-0.05	0.10	0.00	-0.044	-0.08**	-0.05
	(0.02)	(0.06)	(0.07)	(0.01)	(0.03)	(0.04)	(0.07)
Mean control group	0.97	0.74	0.44	0.96	0.65	0.46	-
Observations	787	787	788	788	788	1033	664

Notes: The model is from equation (1) with district fixed effects. Robust standard errors, clustered by catchment areas in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. The data come from household interviews with women who experienced a pregnancy in the last 2 years. Specifications: see Table 8 for details. *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.

TABLE A.5: PROGRAM IMPACT ON HEALTH EDUCATION – PARTICIPATION MODEL (2007-2008)

Dependent Variable	Have heard about AIDS	AIDS stigma	Knowledge that TB is spread through the air	Children sleeping under a treated mosquito net
Specification	(i)	(ii)	(iii)	(iv)
Program impact	-0.002*	0.01	-0.01	-0.02
	(0.001)	(0.00)	(0.03)	(0.03)
Mean control group	1.00	0.06	0.54	0.28
Observations	2844	2796	2761	1766

Notes: The model is from equation (1) with district fixed effects. Robust standard errors, clustered by catchment areas in parentheses. Program impact measures the coefficient on the assignment to treatment indicator. The data come from household interviews. Specifications: see Table 9 for details. *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.

TABLE A.6: PROGRAM IMPACT ON IMMUNIZATIONS – PARTICIPATION MODEL (2007-2008)

Group Specification	Newborn (i)	Less than 1-year (ii)	1-year old (iii)	2-year old (iv)	3-year old (v)	4-year old (vi)
Average standardized effect	-0.04 (0.08)	-0.02 (0.06)	-0.02 (0.06)	-0.05 (0.07)	0.02 (0.08)	0.11 (0.09)
Mean control group	0.79	0.83	0.88	0.90	0.90	0.92
Observations	1498	1466	1097	756	467	226

Notes: Average standardized effects with the dependent variables being indicator variables for whether the child has received at least one dose of measles, DPT, BCG, and Polio, respectively (see text for details) and with district fixed effects. Robust standard errors clustered by catchment areas in parentheses. For more details see notes to Table 10. *Significantly different from zero at 90% confidence level; **Significantly different from zero at 95% confidence level; ***Significantly different from zero at 99% confidence level.