Reducing Child Mortality in the Last Mile

Author(s): Martina Björkman Nyqvist, Andrea Guariso, Jakob Svensson and David Yanagizawa-Drott

Source: *American Economic Journal: Applied Economics*, July 2019, Vol. 11, No. 3 (July 2019), pp. 155-192

Published by: American Economic Association

Stable URL: https://www.jstor.org/stable/10.2307/26727328

American Economic Journal: Applied Economics 2019, 11(3): 155–192
https://doi.org/10.1257/app.20170201

Reducing Child Mortality in the Last Mile: Experimental Evidence on Community Health Promoters in Uganda†

By Martina Björkman Nyqvist, Andrea Guariso, Jakob Svensson, and David Yanagizawa-Drott*

The delivery of basic health products and services remains abysmal in many parts of the world where child mortality is high. This paper shows the results from a large-scale randomized evaluation of a novel approach to health care delivery. In randomly selected villages, a sales agent was locally recruited and incentivized to conduct home visits, educate households on essential health behaviors, provide medical advice and referrals, and sell preventive and curative health products. Results after 3 years show substantial health impact: under 5-years child mortality was reduced by 27 percent at an estimated average cost of $68 per life-year saved. (*JEL* I12, I18, J13, O15, O18)

Despite significant reductions in child and infant mortality over the last few decades, about 1 in 13 children in sub-Saharan Africa still die before his or her fifth birthday (WHO 2017). Many, if not most, of these deaths can be avoided through simple preventative care and through simple, low cost treatments delivered at home. This means that an effective response to reduce child deaths is not out of reach. While health outcomes can be tied to a host of factors, both on the demand and supply side, there is limited evidence on effective and scalable solutions to the

* Björkman Nyqvist: Department of Economics, Stockholm School of Economics, P.O. Box 6501, Sveavägen 65, SE- 113 83 Stockholm, and CEPR (email: Martina.Bjorkman.Nyqvist@hhs.se). Guariso: Department of Economics, Trinity College Dublin, Arts Building, Dublin 2, Ireland, and LICOS (email: guarisoa@tcd.ie). Svensson: Institute for International Economic Studies, Stockholm University, 106 91 Stockholm, Sweden, and CEPR (email: jakob.svensson@iies.su.se). Yanagizawa-Drott: Department of Economics, University of Zurich, Schönberggasse 1, 8001 Zurich, Switzerland (email: David_Yanagizawa-Drott@econ.uzh.ch). Benjamin Olken was coeditor for this article. An earlier version of this paper has been circulated under the title "Effect of a micro entrepreneur-based community health delivery program on under-five mortality in Uganda: a cluster-randomized controlled trial." The trial was approved by the ethic committee of Fondazione IRCSS (D2291696), by the Harvard IRB (protocol P20141-101), by the Uganda National Council for Science and Technology (UNCST) (SS3195), and by the IRB Office of the Joint Clinical Research Center (JCRC) in Uganda. The trial was registered in the Pan African Clinical Trials Registry (PACTR201308000601715) and in the American Economic Association's registry for randomized controlled trials (AEARCTR-0000530). We gratefully acknowledge two anonymous referees for many valuable comments and suggestions. We appreciate comments on an earlier draft from May Sudhinaraset, Jenny Liu, Dominic Montagu, and Rebecca Weintraub. We thank Aletheia Donald and Charles Ntale for help during different phases of the evaluation, and the IPA-Uganda and its staff, specifically Jeff Alumai, Ezra Rwakazooba, Zahra Mansoor, Roselyn Mugide, and Douglas Kaziro. We thank Molly Christiansen, Joe Speicher, Chuck Slaughter at Living Goods, as well as Sharmin Sharif and a number of staff at the BRAC-Uganda office for insightful discussions over the years about the CHP program, and Anna Hakobyan and Amy Mayberry at the Children Investment Fund Foundation for support throughout the study. Financial support from the Children Investment Fund Foundation and the Swedish Research Council (421-2013-1524 and 2016-05615) is greatly appreciated.

† Go to https://doi.org/10.1257/app.20170201 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

problem (Dupas and Miguel 2016). In this paper, we focus on the role of delivery on basic health services and products to poor communities at the very end of the supply chain. How to combat child mortality in this space is arguably of first-order importance from a policymaking perspective. On the one hand, evidence shows that when households get access to very basic health products and services, free of charge, mortality is substantially reduced (Kumar et al. 2008, Baqui et al. 2008). On the other hand, even if such solutions are socially desirable in the long run, in the short run they imply severe feasibility challenges in countries with poor state capacity in general, and service delivery capacity in particular.

In this paper, we study the impact of a novel community health delivery program. The program relies on "Avon-like" networks of door-to-door mobile Community Health Promoters (CHP), whose main activities resemble the standard activities of any community health worker: conduct home visits within their community, educate households on essential health behaviors, provide basic medical advice, and refer the more severe cases to the closest health center. On top of this, the CHPs make a modest income by selling a diverse basket of basic health goods, ranging from anti-malaria drugs to soap and fortified foods. CHPs purchase these products directly from the nongovernmental organizations (NGOs) at wholesale price and earn a margin on each product sold. The idea is that these incentives on products, coupled with small financial incentives to encourage timely services, would not only move households down the demand curve but also motivate community health promoters to actively provide basic health services to mothers, newborn, and children.[1] Thus, the CHPs operated as micro-entrepreneurs with financial incentives to meet household demand and improve child health.

The program was randomized across 214 rural villages spread across Uganda and was fully operational in all treatment clusters, with at least 1 CHP locally recruited to the program, by the beginning of 2011.[2] Our results after 3 years show that the program resulted in a substantial health impact: in treatment villages, under 5-years child mortality was reduced by approximately 27 percent, infant (i.e., under 1-year) mortality by 33 percent, and neonatal (i.e., under 1-month) mortality by 28 percent, compared to control villages that did not receive the program.

While the evidence shows that households in treatment villages were significantly more likely to use products sold by the CHPs, including life-saving medical products such as insecticide treated bed nets and oral rehydration salts for treating diarrhea, we also provide suggestive evidence that the effects were not simply driven by access to cheaper, high quality medicines and health products. In particular, we document large increases in a number of maternal, newborn, and child health services, including follow-up visits and advice to seek professional medical care, in the treatment relative to the control group. We also show that households in the treatment group have better knowledge of basic health issues compared to households in the control villages. While we cannot rule out that these non-incentivized services

[1] The CHPs received small performance-based incentives to encourage registering of pregnant women and visits of newborns (∼$0.65 per registration/newborn visit).

[2] The rollout of the CHP program, including the trial clusters, was overseen by an advisory board including individuals with expertise in international public health and health service research, as well as officials from the Uganda Ministry of Health.

were provided for profit maximizing reasons, i.e., as a way to increase the demand for products sold by the CHPs, the findings are also consistent with the view that the CHPs also had nonpecuniary reasons to serve the community and that the pecuniary incentives did not (fully) crowd out nonpecuniary motivations. The findings also add to a small but growing literature that investigates the role financial incentives can play in motivating individuals engaged in pro-social activities (Ashraf, Bandiera, and Jack 2014; Olken, Onishi, and Wong 2014; and Luo et al. 2015).

To facilitate policymaking, we also perform a cost-benefit analysis. Our results indicate that the average cost per averted death over the three years of study is approximately US$4,000. As a comparison, the Guttmacher Institute estimates a cost per life saved from expanding a range of health services known to be effective at saving lives that is almost three times higher (Perry and Zulliger 2012). Taking into account that life expectancy is vastly improved conditional on surviving the first few years of life, the estimated average cost per life-year gained is $68. This figure compares favorably to existing estimates of various community health interventions, which range from $82 per life-year gained in expanding maternal and child health prevention and promotion activities in Kenya through community health volunteers, to about $3,400 per life-year gained in expanding basic health services in Indonesia through village midwives assisted by community health volunteers (Borghi et al. 2005, McPake et al. 2015).

The remainder of the paper is organized as follows. Details on the study setting, the research design, and the intervention are presented in Section I. Section II reports the main results. In section III, we perform a robustness analysis. Section IV presents the cost-effectiveness estimates, and section V concludes.

I. Intervention and Empirical Design

A. *The Program*

In 2007, Living Goods (LG), a US based NGO active in Uganda, in collaboration with BRAC Uganda began piloting a new community health delivery model intended to improve maternal, newborn, and child health. Unlike volunteer-based community health worker programs, the community health promoter (CHP) program harnesses the power of franchised direct selling to provide CHPs with incentives to increase poor households' access to low-cost, high-impact health products and basic newborn and child health services. The CHP program was organized into geographically based branches, managed by branch managers and supervised by the two NGOs (Living Goods and BRAC Uganda). Each CHP was assigned to a specific cluster, which in most cases corresponded to a village.

The CHP program is ongoing and by the end of the evaluation period in 2013, it was operating in 883 clusters (villages), organized in 29 branches, located in 23 districts, spread over all 4 regions of Uganda (see Figure 1).[3] The program has

[3] Figure A1 in the Appendix provides more detailed images by study district.

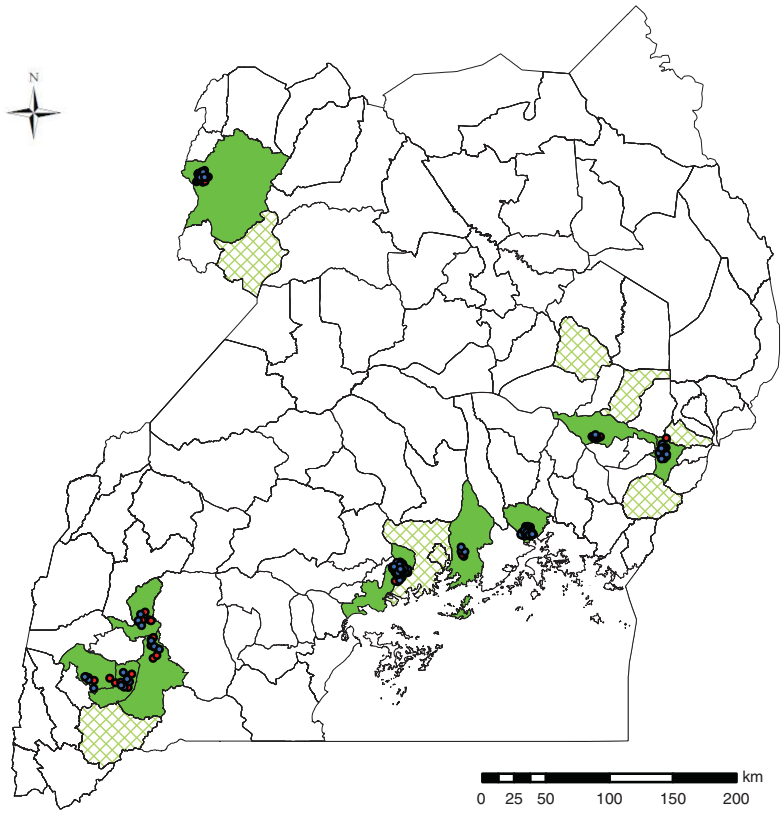


Figure 1. Map of Districts and Distribution of Clusters

Notes: Green fully colored areas indicate districts that were part of the study, while cross-hatched areas indicate districts excluded from the study, but in which the program was also implemented. Red and blue dots indicate, respectively, control and intervention clusters (villages) included in the study. Figure A1 in the Appendix provides a set of more detailed images by study district.

continued to expand and by the end of 2018 reached more than 7,000 villages, with a total population of over 5.5 million.

The CHPs were selected through a competitive process among female community members aged 18 to 45 who applied for the position in each village and who possessed basic writing and math skills.[4] Eligible candidates received two weeks of health and business training, covering preventing, diagnosing, and treating childhood illness, recognizing danger signs for referral, healthy pregnancy and newborn care, and nutrition.[5] At the end of the training, a skills test was administered to determine who would become an active CHP. In 2012, the NGOs—independently

[4] Basic characteristics on an average CHP working for the NGO are reported in Appendix Table A1. When comparing the CHPs to the average women from the same districts and in a similar age span, as reported in the 2011 Demographic and Health Surveys (DHS) data, we see that the CHPs are slightly more educated and more likely to be the head of household, but similar along all other dimensions.

[5] The specific topics covered in the initial training are: introduction and family health tools; sick child guide and malaria, diarrhea, and cough; newborn care; home health care and nutrition; pregnancy and birth; dosage guide and mapping; inventory/money management and branch visit; inventory presentations; sales and customer care; business tools; team practicum; work preparation and closing activities.

from the evaluation—surveyed a sample of 196 CHPs working in the study areas (not exclusively in the study villages). Appendix Table A2 summarizes the results from the knowledge-related questions that were administered. The average CHP is able to detect on average about half of the symptoms and signs related to diarrhea, malaria, maternal/neonatal health issues, and pregnancy/birth warning signs. When looking at the particular knowledge within each disease, the CHPs score higher on the most common signs. These test results confirm that the CHPs are clearly not medical specialists, but they are able to detect the most important danger signs at some reasonable level and provide key health support for the community. CHPs also attended a one-day refresher training each month to review and refresh key health and business topics and to get information on new product and services introduced to the program.

The CHPs' tasks were to conduct home visits to households with children under five years old, educate households on essential health behaviors, provide basic medical advice, refer the more severe cases to the closest health center, diagnose illness, and sell preventive and curative health products. The CHPs were also instructed to visit newborns within the first 48 hours of life and to encourage pregnant women to deliver in a facility or with professional assistance. The CHPs received small performance-based incentives to encourage registering of pregnant women and visits of newborns (∼$0.65 per registration/newborn visit).[6]

The product line the CHPs had at disposal included prevention goods (e.g., insecticide treated bed nets, water purification tablets, and vitamins), curative treatments (e.g., oral rehydration salts, zinc, and artemisinin-based combination therapies (ACTs)), as well as other health-related commodities (e.g., diapers, detergent, and hand soap) and durables with potential health benefits (e.g., improved cook stoves, solar lights, and water filters). The broad product mix and the pricing strategy had three potential benefits: driving up total sales and income for the CHPs; enabling the NGOs to cross-subsidize prices across products; and motivating agents to be out visiting households regularly, thanks to the presence of high-velocity items in the product mix.[7] The products were sold by the CHP generally below prevailing market prices. The retail price was indeed determined by country management with a target of keeping prices for preventive and curative products on average 10 percent lower than the prevailing local market prices. The CHPs, in turn, purchased these products directly from Living Goods or BRAC branches at wholesale prices on average 30 percent below market prices and therefore earned an income on each product sold.[8] The product list (as well as the exact pricing strategy) kept changing with the market conditions, but by the end of 2013 it included 152 different products. The average margin for the CHPs (based on the recommended price) across

[6] Similar health services in the public health literature are often labeled as iCCM (integrated community case management) and MNCH (maternal, newborn, and child health) services.

[7] The business training received by the CHPs explicitly stressed the importance of building up a customer-base by providing free services like health education, referrals, and newborn visits.

[8] The CHPs were given recommended retail prices, which they were expected to adhere to, although there were no strict enforcement policies in place. During field visits, the local branch managers would sometimes ask clients about the prices at which products were sold to them or would witness a CHP making a sale. However, we know from anecdotal evidence that CHPs sometimes offered discounts or even credit to households in need in order to build up a good reputation and a solid customer base within the community.

all products was 32 percent. However, when considering the actual purchases of the CHPs, average margin reduces to 20 percent. This suggests that most of the sales do not come from products with the highest margins. This is confirmed by Figure A2 in the Appendix, which illustrates the average margin the CHPs made across 13 macro-categories of products, together with their sales: products that provide the highest margins are not the most commonly bought by the CHPs. Overall, the CHPs operated as micro-entrepreneurs with financial incentives to meet household demand and improve child health. The two NGOs also managed to keep their own margins at 10 percent at least, by largely buying the products directly from the producers, national importers, or national distributors.

On average, CHPs bought products from the NGOs for 150,000 Ugandan shilling (∼$58) per month. With an average margin of 20 percent across the purchased products, this resulted to an average earning of roughly $174 per year, or roughly 32 percent of the average income per capita in Uganda in 2013. CHPs reported dedicating an average of 8 hours per week to meetings with households, and the vast majority of them had additional sources of income, with the CHP income representing roughly 50 percent of their total income. However, revenues varied across CHPs, due to a combination of population size (or market size) of their catchment area and effort put into to the task relative to other income generating opportunities. Figure A3 in the Appendix shows the distribution across CHPs of the monthly purchases from the NGOs in 2013.

B. *Comparison with Traditional Community Health Workers and Related Literature*

In many developing countries, as is the case in Uganda, the primary strategy to extend primary health care from facilities to underserved rural communities is community health workers (CHWs) (Singh and Sachs 2013). In contrast with the Living Goods model, traditional CHW programs lack explicit monetary incentives (Bhutta et al. 2010, Christopher et al. 2011, Gilmore and McAuliffe 2013, Haines et al. 2007, and Perry and Zulliger 2012). Specifically, community health work is often voluntary, which given competing opportunities such as paid work or home production, may lead them to devote less time to caregiving. Unsurprisingly, therefore, most of the evidence on the positive impact of CHW programs come from studies in settings with high quality supervision and support. Such a monitoring system, however, may not be achievable in routine field situations. How to incorporate incentives to motivate CHWs in large-scale CHW programs, and the impact these programs will have, are open questions.

Systematic reviews of existing studies show that CHWs can be impactful in promoting positive health behavior and in providing basic curative and health services (Bhutta et al. 2010, Christopher et al. 2011, Gilmore and McAuliffe 2013, Gogia and Sachdev 2010, Haines et al. 2007, Lewin et al. 2010, Naimoli et al. 2012, Okwundu et al. 2013, and Perry and Zulliger 2012). However, the findings from reviews of randomized controlled trials of CHW programs and CHW-led interventions are mixed (Lewin et al. 2010, Okwundu et al. 2013). Two proof-of-principle studies cited as evidence in the WHO and UNICEF home-visits strategy statement documented large

reductions in neonatal mortality (36–54 percent) (Kumar et al. 2008, Baqui et al. 2008). Five trials delivered in a program setting documented smaller (15 percent or lower)—and in four out of five trials not statistically significant—impact (Darmstadt et al. 2010, Bhutta et al. 2011, Bhandari et al. 2012, Kirkwood et al. 2013, Boone et al. 2016). Two studies assessed the impact of community-based training of mothers, of which one focused on teaching mothers curative treatments of malaria, finding a 40 percent reduction in under 5-years mortality (Kidane and Morrow 2000), and one focused on teaching childcare to expectant and postpartum women, finding instead no significant impact on neonatal and infant mortality (Sloan et al. 2008). Finally, two trials assessed the impact of Integrated Management of Childhood Illness program in Bangladesh (Arifeen et al. 2009) and Ethiopia (Amouzou et al. 2016), finding no significant effect on under 5-years mortality. Our study adds to this literature by evaluating whether a social entrepreneurial approach to health care delivery can lead to significant improvements in children's health. Although the experimental design does not allow us to pin down the specific contribution of the incentives, the results clearly indicate that a community health worker program, which embeds financial incentives for the health workers, can be highly effective in reducing child mortality.

Our study also contributes to the growing literature on the effect of incentives for agents engaged in pro-social activities. Most literature in this area has focused on the education sector and on the impact of performance incentives for teachers (e.g., Lavy 2002; Glewwe, Ilias, and Kremer 2010; Muralidharan and Sundararaman 2011; and Duflo, Hanna, and Ryan 2012). Recently, however, four studies have looked at the impact of incentives on the delivery of health services. Within the context of rural China, Miller et al. (2012) relied on a randomized trial to study the role of incentives for school principals to reduce anemia among their students, finding a modest effect.[9] In Indonesia, Olken, Onishi, and Wong (2014) studied a program that links aid disbursements for health and education to the performance of health services, finding significant short-run improvements in health indicators. In Zambia, Ashraf, Bandiera, and Jack (2014) evaluated the effect of both financial and nonfinancial incentives on the performance of agents recruited by a public health organization, finding, among other things, that both types of rewards are effective when their relative value is high and that the effect is stronger for pro-socially motivated agents. Finally, within the context of government childcare health workers in India, Singh (2015) showed that combining performance pay with information provision to mothers leads to a significant reduction in malnutrition, although individually the effects are negligible.

Two additional studies, reaching opposite results, study how incentives shape community health workers' performances in sub-Saharan Africa. Ashraf, Bandiera, and Lee (2016) experimentally varied the salience of career progression at recruitment stage for a new government health position in Zambia and found that more career-motivated individuals tend to perform better on the job, ultimately leading to

[9] In a new randomized controlled trial, performed in a similar setting, the authors find that large (but not small) incentives as well as larger unconditional grants succeed in reducing anemia substantially, with incentives being the most cost-effective solution. The authors also find that performance incentives and unrestricted grants work as substitutes, with the larger unconditional grants completely crowding out the positive effect of incentives (Luo et al. 2015).

better health outcomes. Deserranno (2019) experimentally varied expected earnings at the recruitment stage for new community health promoters in Uganda. Her findings show that when the job is expected to be more lucrative, it attracts less socially motivated individuals that tend to perform better on the job, thus ultimately leading to worse health outcomes.

C. *Study Design and Participants*

The study was a parallel group, stratified cluster randomized controlled trial, embedded in the rollout of the full CHP program. Two hundred and fourteen clusters (rural villages) took part in the trial. The clusters were located in 12 geographical zones spread across Uganda (see Figure 1). Each zone was associated to a different branch of the NGOs. Within each zone, the clusters were randomly divided into a treatment group and a control group. In 11 zones out of 12, the randomization was balanced (1:1). In 1 zone and for operational purposes the randomization was unbalanced (2:1). At least one CHP was assigned to each cluster in the treatment group.[10] No CHP was assigned to the control clusters. All clusters were enumerated at baseline. There were on average 237 households per cluster at baseline.

The main objective of the trial was to assess the impact of having a CHP working in the cluster on improving children's health. The evaluation design and implementation were independent of program implementation.

The outcomes of interest were measured through a cross-sectional household survey administered between September and December 2013; approximately three years after the CHPs began operating in the treatment clusters. Before implementing the survey, each cluster was enumerated. A random computer-generated sequence was then used to select 40 households to be surveyed in each cluster (if less than 40 eligible households were available, all were sampled). The analysis was based on a final sample of 7,018 households, and their 11,563 under 5-years children, that have lived in the same cluster throughout the trial. The final sample was slightly smaller than the cross-sectional household survey since households that migrated out from the baseline cluster were not included in the final analysis, nor were households that migrated into the trial clusters during the study period.

Sampled households were visited and asked for written informed consent to participate in the survey. The respondent was the female household head if available at the time of the interview or the primary female health care giver of the household. If neither could be found, or the household refused to participate, a replacement household was chosen (this happened in 7.2 percent of the cases, without any systematic difference between treatment and control clusters). Random back-checks confirmed that the protocol was followed.

The survey was implemented by Innovations for Poverty Action (IPA) Uganda with survey teams that were familiar with local customs and spoke the local language. Data collectors were masked to whether they were interviewing in a treatment or control cluster.

[10] One CHP was assigned to each cluster, with the exception of few large clusters, where two or three CHPs were assigned.

The trial was embedded in the rollout of the full CHP program (883 clusters) and there were no differences in program implementation between the treatment clusters (115 clusters) and the 768 clusters that were not part of the trial.

The CHPs were blinded to the trial status of the village they were assigned to avoid that the evaluation itself affected the CHPs behavior. As a consequence, no surveillance and monitoring system was put in place in the trial clusters and we did not track a predetermined set of households to avoid the CHP focusing their efforts on the households that were tracked at the expense of those who were not. Mortality rates were calculated based on cross-sectional household survey data collected at the end of the trial, using data from households that had resided in the same cluster throughout the trial. To ensure that these households were not systematically different in the two assignment groups, we tested for differential in- and out-migration during the trial period and checked for balance across assignment groups using pretrial determined observable household characteristics, and pretrial infant mortality rates, collected at the end of the trial period.

All households and especially households with children under five were potential recipients of visits from the CHPs. While the CHPs were recommended to focus attention on providing services to households living within their cluster, they were not prevented from selling or providing advice also to households outside the cluster, including control clusters. Similarly, households living outside the treatment clusters could visit a CHP in a treatment cluster.

Households in both treatment and control clusters could benefit from primary health care services provided by other actors, including private clinics, public primary health dispensaries, and Village Health Teams (a government community health worker program).

D. *Randomization*

Figure 2 describes the trial profile. As the full CHP program was rolled out over time, the randomization of clusters was also phased in over time. We began in 2009 with a sample of 200 clusters (villages) in 10 geographic zones (8 districts). The clusters were stratified by zone and size (below or above 400 households) and, within each stratum, half of the clusters were assigned to the treatment group and half were assigned to the control group through a simple randomization procedure (computerized random numbers) generated by the researchers. In 2010, a year before the evaluation began, a decision was taken to only include clusters with less than 400 households at baseline as the design of the trial was deemed less suitable for clusters where the CHPs only would be able to serve a small minority of the households. As a consequence, 10 strata with 94 clusters (47 treatment and 47 control) were deemed ineligible. Sixty clusters organized in one new geographic zone were added in the end of 2010. Half of these 60 clusters were randomly assigned to the treatment group and half were assigned to the control group, following the same procedure adopted for the other zones. An additional zone was added in the beginning of 2011. For operational purposes, one-third of the 48 clusters in the final zone were randomly assigned to the control group and the remaining two-thirds of the clusters were allocated to the treatment group. The final sample for the trial thus consisted

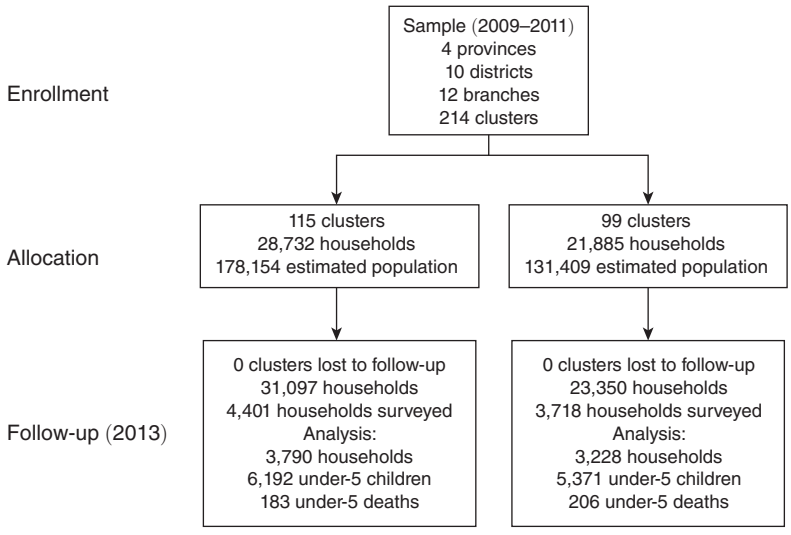


Figure 2. Trial Profile

of 214 clusters (115 treatment and 99 control) in 12 branches/zones (10 districts). Concerning the division between the two NGOs, 106 clusters (53 treatment and 53 control) were in BRAC-managed areas, while 108 were in LG-managed areas (62 treatment and 46 control). The program was fully operational in all treatment clusters in the beginning of 2011. An endline survey was conducted after three years, at the end of 2013.

E. *Outcomes*

The pre-specified primary outcome was under-five mortality rate (U5MR). Secondary outcomes were infant mortality rate (IMR) and neonatal mortality rate (NMR). All mortality rates were calculated using the sample household survey data collected at the end of the trial. The household survey recorded detailed birth and death information for all children under five living in the households at the time of the survey as well as for all children that died under the age of five in the previous three years. For each child, we defined the number of month of exposure to the risk of death during the trial period, defined as the difference between the birth date of the child, or the start date of the trial (January 2011) if the child was born before that date, and the date that the child turned five years old if that occurred during the trial period, or the date of the endline household survey if the child was less than five years old at that time, or the date of the death of the child (see Figure A4 in Appendix). Under-5 mortality was then calculated as number of under-5 deaths over the trial period per 1,000 child-years of exposure to the risk of dying under the age of five. Infant mortality was calculated as number of deaths during the trial period arising within the first year of life per 1,000 infant-years of exposure, with infant-years of exposure calculated in a similar way as the child-years of exposure

to the risk of death. Neonatal mortality was calculated as number of deaths during the trial period within the first month of life per 1,000 births.[11]

Additional secondary outcomes of interest were CHP interactions (to measure program coverage), health knowledge, under-five morbidity, preventive and treatment services, and integrated community case management (iCCM) and maternal, newborn, and child health (MNCH) services.[12] Data on all secondary outcomes were collected in the endline household survey.

F. *Sample Size*

The sample size was designed to detect a reduction in overall under-five mortality. In a community-based trial in 2009 with significant overlap in the regions covered to the CHP program studied here, U5MR was 18 deaths per 1,000 child-years with a coefficient of variation of the incidence rates of 0.32 (Björkman Nyqvist, De Walque, and Svensson 2017). On the basis of these data, and 120 child-years of observations in each cluster (i.e., three years and 40 child observations per year), a sample size of 214 clusters, of which 115 clusters are allocated to the treatment group and 99 clusters to the control group, would detect a 27 percent reduction in under-5 mortality with 80 percent power at the two-sided 5 percent significance level.

II. Results

A. *Balance at Baseline*

Table 1 reports balance tests using cluster-specific statistics before the program begun. There are no statistically different differences between the treatment group and the control group in terms of size, household characteristics, and distance to main roads, electricity transmission lines, and health facilities.

Household data was not collected at baseline by the research team.[13] We therefore use endline data to compute infant mortality for the two years preceding the intervention, i.e., in 2009 and 2010. Results in Table 2 show that there was no significant difference in infant mortality rates between the treatment and control prior to the intervention. IMR was 52.4 per 1,000 child-years in the treatment group compared to 50.0 per 1,000 child-years in the control group (p-value $= 0.83$).

[11] International organizations such as the United Nations and World Health Organization typically express mortality in terms of deaths per 1,000 live-births, using data collected over long periods of time and relying on a life-table approach to compute mortality as a probability. For an impact evaluation, when data is collected for a relatively short period (three years in our case), a more appropriate approach is to compute mortality as a ratio, following the steps described above, and to express it in terms of years of exposure. For completeness and in order to facilitate comparisons with other estimates, we also report results obtained using a life-table approach.

[12] Pneumonia was initially included as a secondary outcome but due to changes in the regulatory environment, there was a delay in the authorization to include antibiotics among the list of health products provided by the CHPs and we could therefore not study pneumonia-related outcomes in the end.

[13] Baseline data was collected for most BRAC clusters by BRAC itself. Although the analysis suggests a balanced sample, we do not include it in our discussion, due to its incomplete coverage and to the fact that data were collected by one of the implementing agencies.

Table 1—Baseline Characteristics

Variables	Treatment group	Control group	*p*-value
Number of clusters	115	99	
Households per cluster	250.0 (113)	221.0 (107)	0.226
Households with under-5 children per cluster	86.0 (47)	78.0 (46)	0.665
Distance to main road	5.6 (11.6)	6.8 (12.7)	0.126
Distance to electricity transmission line	1.8 (1.5)	1.8 (1.5)	0.707
Distance to health center	1.4 (1.1)	1.7 (1.2)	0.256
Number of health centers within 5 km	8.3 (5.0)	7.3 (5.2)	0.459
Distance to hospital	10.4 (8.5)	11.1 (8.5)	0.916

Notes: Cells report mean standard deviations across clusters included in the treatment or control group. A variety of sources were consulted to generate the dataset, including documents and maps from national utilities, regional power pools, and the World Bank. Information on households and households with under-5 children per cluster was collected from the enumeration of study villages at baseline. Data for medium and high voltage electricity transmission lines were obtained from the Africa electricity transmission network (AICD) study. Health centers takes into account facilities from HCIII (i.e., parish-level health centers, roughly one per 5,000 people) and above. Hospitals refer only to district/national hospitals (roughly 1 per 500,000 people). Distance measures are all expressed in kilometers.

Table 2—Baseline Characteristics of Households Not Lost to Follow-Up and Surveyed at Endline

Variables	Treatment group	Control group	*p*-value
Panel A. Infant mortality			
Years of exposure to risk of death under 1 year	1,927	1,743	
Deaths under 1 year	101	87	
Mortality rate per 1,000 years of exposure	52.4	49.9	0.833
Panel B. Households			
Number of households	3,789	3,228	
Household size	5.2 (2.4)	5.3 (2.3)	0.590
Age household head	36.4 (12.1)	36.7 (12.4)	0.616
Years of education household head	8.0 (0.2)	8.0 (0.1)	0.122

Notes: Cells report mean standard deviation from endline sample household survey data for households that have remained in the cluster throughout the trial, with values scaled back to baseline period.

Panel B of Table 2 shows that the pretrial determined observable household characteristics, such as household size at the start of the trial and age and years of education of the household head, were not statistically different between the treatment group and the control group for the households used in the analysis, i.e., households that had remained in the same cluster throughout the trial and surveyed in 2013.

B. *Mortality and Morbidity*

The primary outcome for the trial was reduction in child mortality. Reduction in child mortality links to a wide spectrum of child health specific services and health goods that CHPs could provide to households with children under the age of five. Specifically, Black, Morris, and Bryce (2003) estimates the distribution of causes of death for children under 5 years in sub-Saharan Africa to be: neonatal disorders (25 percent), malaria (22 percent), pneumonia (21 percent), diarrhea (20 percent), and AIDS (8 percent). Jones et al. (2003) identifies 21 effective child survival interventions, both preventive and treatment approaches, for these causes of deaths, judged to

Table 3—Program Impact on Child Mortality

	Number of deaths			Mortality per 1,000 years of exposure		Mortality per 1,000 births		
	Under-5 deaths	Infant deaths	Neonatal deaths	Under-5 mortality	Infant mortality	Neonatal mortality	Under-5 mortality	Infant mortality
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program impact	−0.58	−0.54	−0.29	−5.95	−18.87	−9.27	−19.86	−17.26
	(0.23)	(0.19)	(0.15)	(2.06)	(5.94)	(4.62)	(7.23)	(5.35)
Rate ratio				0.73	0.67	0.73		
				(0.09)	(0.09)	(0.11)		
Mean control	2.08	1.62	1.07	19.4	52.7	33.36	68.4	49.7
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	214	214	214	214	214	214	214	214
R^2	0.148	0.163	0.162	0.145	0.155	0.135	0.117	0.153

Notes: Program impact measures the coefficient on the assignment to treatment indicator, from a standard OLS regression. Dependent variables: (1) number of under-5 deaths; (2) number of infant deaths; (3) number of neonatal deaths; (4) number of under-5 deaths per 1,000 child-years of exposure to the risk of death; (5) number of infant deaths per 1,000 child-years of infant exposure to the risk of death; (6) number of neonatal deaths per 1,000 births; (7) number of under-5 deaths per 1,000 births; (8) number of infant deaths per 1,000 births. Rate ratios in specifications (4)–(6) are derived from a Poisson model, with branch fixed effects and standard errors clustered by village; the number of observations for those specifications are 11,342 (4), 8,808 (5), and 6,499 (6). Branch fixed effects are included in every regression. There are 12 branches in the sample. Robust standard errors are in parentheses. R^2 refers to the OLS regressions.

be feasible for high levels of implementation in low-income countries. We measure nine of these approaches for which there is sufficient evidence of a causal relationship between the intervention and reductions in cause-specific mortality: six prevention approaches (breastfeeding, clean delivery, newborn management, vitamin A supplementation, vaccination, clean water, insecticide-treated bed net) and two treatment approaches (oral rehydration therapy to treat diarrhea and anti-malarials (ACT) to treat malaria). We also measured a number of integrated community case management (iCCM) and maternal, newborn, and child health (MNCH) services, which were not included in Jones et al. (2003), and where more limited evidence of effect exists. Many of these services and goods, including health education, child preventative care, child curative care, and the medical drugs related to deworming, malaria, and diarrhea that were sold by the CHPs at affordable prices to the households also have the potential to affect other health outcomes. We therefore also collected measures of height, weight, and hemoglobin levels of all children under five living in the surveyed households.

We start, in Table 3, to report the findings on child mortality. The first 3 columns report the results using the raw data: i.e., the number of under-5, infant (under 12 months), and neonatal (under 1-month) deaths per year. In order to assess the impact of the program, here as well as in the remaining of the paper, we compare mean outcomes after accounting for stratification. That is, we estimate

$$Y_{ij} = \beta T_{ij} + b_j + \epsilon_{ij}, \tag{1}$$

where Y_{ij} is the outcome of interest (e.g., number of under-five deaths over the study period) in village i, located in the geographical zone associated to the NGO branch j; T is an indicator variable for villages assigned to the treatment groups, b_j are

branch fixed effects, and ϵ is an error term. In our analysis, we always cluster standard errors at the cluster/village level, given the likelihood of village level shocks and the assignment of treatment at the cluster level.

The first three columns of Table 3 show that the CHP program reduced the number of deaths in all three age categories. The number of under-5 deaths dropped by 28 percent (column 1); the number of infant deaths dropped by 33 percent (column 2); the number neonatal deaths dropped by 27 percent (column 3). The raw data clearly show that the CHP program had a large impact in reducing the number of child deaths. However, the reduction in number of deaths may not necessarily be driven solely by a reduction in the risk of child death, since cohort sizes may have been differentially affected by the intervention due to for example differences in fertility rates. Therefore, in columns 4–6 of Table 3, we estimate the mortality rate over the period of exposure, i.e., between January 2011 to December 2013.

The estimated rate ratio in column 4, i.e., the incidence of child deaths in the treatment relative to the control group, implies that the risk of under-5 deaths was reduced by 27 percent relative to the control group. The effect is of the same order of magnitude, and even more precisely estimated, using a linear model—a reduction of 5.95 deaths per 1,000 child-years from a control group mean of 19.4 deaths per 1,000. The reduction in infant mortality (column 5) is even larger—a 33 percent reduction in the risk of infant deaths in the treatment versus the control group—and even more precisely estimated. Finally, the number of children dying before reaching 1 month (per 1,000 live births) is 24.1 in the treatment group compared to 33.4 in the control group and the difference—a reduction of 28 percent in neonatal mortality—is significant at the 5 percent level.

In columns 7 and 8 of Table 3, we also report under-5 and infant mortality expressed in terms of deaths per 1,000 live births. In this case, we estimate the probabilities of deaths by using a life-table approach. Such an approach is more appropriate for data spanning longer time periods than the three years we are considering here. It provides, however, estimates that are more easily comparable with other reports of under-5 mortality. For example, WHO reports under-5 and infant mortality rates for Uganda in 2013 equal to 66.1 and 43.8, respectively (Hug et al. 2014). Our estimates for the control group are just slightly higher and equal to 68.4 and 49.7 deaths per 1,000 live births, respectively. Columns 7 and 8 show that the intervention led to a reduction in treatment village of 19.9 deaths in mortality under-5 (28.9 percent reduction) and 17.2 deaths in infant mortality (34.7 percent reduction). In both cases, the estimate is significant at 1 percent.

The main outcome of the CHP program was to reduce child mortality at all levels. The intervention, however, likely also impacted morbidity and health more generally. Table 4 reports the results on weight, height, and hemoglobin levels of children under 5 years old (panel A), distinguishing impact between children under 24 months of age (panel B) and children between 24 and 60 months (panel C). Weight was measured using portable weighing scales and height using stadiometers. Hemoglobin levels were measured through a hemocue machine.[14] Results

[14] A photometer that tests hemoglobin concentration using a single drop of blood taken from the child's finger.

Table 4—Program Impact on Child Weight, Height, and Hemoglobin Levels

Dependent variable	Height-for-age z-score (1)	Height-for-age z-score < -2 (2)	Weight-for-height z-score (3)	Weight-for-height z-score < -2 (4)	Hemoglobin level (5)	Hemoglobin level < 10g/dl (6)
Panel A. Children under 60 months						
Program impact	0.048	−0.019	−0.005	−0.003	0.128	−0.027
	(0.042)	(0.010)	(0.039)	(0.006)	(0.041)	(0.009)
Mean control	−1.166	0.280	−0.022	0.078	11.217	0.169
Observations	10,570	10,570	10,175	10,175	10,568	10,568
Panel B. Children under 24 months						
Program impact	0.027	−0.020	0.023	−0.008	0.117	−0.018
	(0.058)	(0.015)	(0.056)	(0.010)	(0.061)	(0.015)
Mean control	−1.010	0.280	−0.035	0.115	10.797	0.240
Observations	3,921	3,921	3,755	3,755	3,918	3,918
Panel C. Children 24–60 months						
Program impact	0.057	−0.020	−0.023	−0.002	0.147	−0.034
	(0.048)	(0.013)	(0.044)	(0.007)	(0.045)	(0.010)
Mean control	−1.255	0.279	−0.015	0.058	11.457	0.128
Observations	6,649	6,649	6,420	6,420	6,650	6,650
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Panel A considers the entire sample of children under five. Panel B only considers children under 24 months of age. Panel C only considers children between 24 and 60 months of age. *Program impact* measures the coefficient on the assignment to treatment indicator. Dependent variables: (1) height-for-age (common measure for stunting) expressed in terms of z-scores; (2) indicator for moderately stunted child (z-score < -2); (3) height-for-age (common measure for wasting) expressed in terms of z-scores; (4) indicator for moderately wasted child (z-score < -2); (5) hemoglobin level (common indicator for anemia) expressed in g/dl; (6) indicator for anemic child (hemoglobin level < 10g/dl). The z-score records the anthropometric value as a number of standard deviations below or above the reference mean value. Branch fixed effects are included in every regression. There are 12 branches in the sample. Robust standard errors are in parentheses, clustered at the cluster level. There are 214 clusters in the sample.

in columns 1 and 2 of Table 4 suggest that the program led to an improvement in height-for-age among children under 5 years old. The share of stunted children according to WHO standards (column 2) in the treatment villages decreased by 1.9 percentage points, corresponding to a 6.8 percent drop compared to children in the control group, and the effect is significant at 10 percent. Similar results are found in panel B and C when splitting the sample by age. Results in columns 3 and 4 show that the program had no detectable effect on wasting. Finally, columns 5 and 6 show a large and significant improvement in hemoglobin levels, with the share of anemic children (i.e., with a hemoglobin level below 10g/dl) in the treatment group decreasing by 2.7 percentage points, or by 16 percent compared to average prevalence in the control group.

C. *Intermediate Outcomes*

The community health promoters program was intended to improve child health by providing basic curative and preventative health services. Specifically, the CHPs' tasks were to conduct home visits, educate households on essential health behaviors, provide basic medical advice, refer the more severe cases to the closest health center, visit newborns, and encourage pregnant women to seek antenatal care, as well as to sell preventative and curative health products. Some of these tasks were directly incentivized, such as the sale of preventative and curative health products

and visiting newborns, but the majority of tasks the CHPs were asked to perform, including providing health education and health advice in case of sickness, were not explicitly incentivized. From a purely profit maximizing perspective, the CHPs could still find these tasks worth performing, however, in order to build up reputation and a loyal customer base within the community—a point also emphasized in the business training the CHP received. The presence of high-velocity items in the product mix, such as soap, was meant to incentivize CHPs to regularly visit households.

We have shown that the program had an impact on the primary outcome—child mortality—and we now turn to assessing evidence on intermediate outcomes that relates to better child health.

Interaction and Knowledge.—We chose not to have a surveillance or monitoring system in place in the study villages. We therefore cannot directly measure the intensity or the quality of the CHPs interactions with the households throughout the trial period. Consequently, we base our analysis here primarily on self-reported data from the surveyed households at the end of the trial. However, we have also access to complementary information, collected directly by the NGOs, on the CHPs' reported activities. Specifically, Appendix Table A3 shows that the average CHP spends roughly 2 days per week working as a health promoter, conducting 10 household visits per day and working on average 8 hours per week. The CHP revisits 13 percent of the households once per month, and 48 percent of the CHPs say that they visited a new household in the previous month. The CHPs arrange on average 1.5 health education meetings per month in their villages.

In Table 5, using household survey data, we assess the quantity, and indirectly, quality of the CHPs' interactions with households. We start by documenting the extent of interactions. Column 1 shows that almost 23 percent of the households in the treatment clusters had been visited by a CHP in the 30 days preceding the survey. While there is evidence of spillovers—5.4 percent of the households in the control group have also been visited by a CHP—households in the treatment group were more than 4 times as likely to have benefited from such a visit.

In columns 2–7 of Table 5, we assess changes in households' health knowledge. Health education is typically considered as one of the key tasks of community health workers and could also be viewed as the first link in a chain linking behavioral changes to improvement in child health outcomes. Health knowledge is also relatively easy to quantify and provides insights into the intensity and quality of the CHPs' interactions with the households. As evident, households in the treatment group are better informed about causes and treatments of diarrhea as well as about causes for malaria: compared to the control area, they were 11 percent more likely to know that diarrhea is transmitted by drinking untreated water; 16 percent more likely to know that zinc is effective in treating diarrhea; and 38 percent more likely to know that mosquito bites are the only cause of malaria. They were also 4.7 percentage points (control mean is 59.1 percent) more likely to have heard of food with added vitamins or nutrients. Knowledge about bed nets and the importance of professional assistance when giving birth did not differ between control and treatment groups, although in these cases there was limited room for

TABLE 5—PROGRAM IMPACT ON CHP INTERACTION AND HEALTH KNOWLEDGE

Dependent variable	HH visited by a CHP in last 30 days (1)	Diarrhea from drinking untreated water (2)	Zinc is effective against diarrhea (3)	Mosquito bites are the only cause of malaria (4)	Aware of food with added nutrients (5)	Bed nets can help prevent malaria (6)	Women should deliver at hospital (7)	Average standardized effect (2)–(7) (8)
Program impact	0.175	0.041	0.036	0.027	0.047	0.001	0.000	0.065
	(0.021)	(0.012)	(0.012)	(0.009)	(0.016)	(0.002)	(0.001)	(0.014)
Mean control	0.054	0.373	0.227	0.071	0.591	0.991	0.998	
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,018	7,018	7,018	7,018	7,018	6,977	7,018	
R^2	0.158	0.035	0.084	0.056	0.065	0.005	0.005	

Notes: Program impact measures the coefficient on the assignment to treatment indicator. Dependent variables are indicators taking value one if: (1) household (HH) was visited by a CHP in the previous 30 days; (2) respondent knows that diarrhea is transmitted by drinking untreated water; (3) respondent believes that zinc is effective in treating diarrhea; (4) respondent believes that mosquito bites are the only cause of malaria; (5) respondent has ever heard of food with added vitamins or nutrients; (6) respondent believes that bed nets can help prevent catching malaria; (7) respondent believes a woman giving birth should deliver at a hospital or health facility. Results in columns 1 to 7 are obtained from a standard OLS regression. Column 8 reports average (standardized) effect size across outcomes (2) to (7), using the seemingly unrelated regression framework to account for covariance across estimates. Branch fixed effects are included in every regression. There are 12 branches in the sample. Robust standard errors are in parentheses, clustered at the cluster level. There are 214 clusters in the sample.

improvement, as even in control villages more than 99 percent of respondents displayed such knowledge.

In the last column of Table 5, we report average standardized effects of the health knowledge outcomes, i.e., we estimate a seemingly unrelated regression system

$$\Upsilon = [I_n \otimes T]\beta + \mu, \tag{2}$$

where Υ is a vector of n related health knowledge outcomes, I_n is an n by n identity matrix, and T is a vector of assignment to treatment group indicators. We derive an average standardized effect, $\tilde{\beta} = \frac{1}{n}\sum_{n=1}^{N} \hat{\beta}_n/\hat{\sigma}_n$, where $\hat{\beta}_n$ is the point estimate on the treatment indicator in the nth outcome regression and $\hat{\sigma}_n$ is the standard deviation of the control group for outcome n (see Kling et al. 2004; Duflo, Glennerster, and Kremer 2007). The average standardized effect is positive and highly significant, confirming that overall the intervention improved health knowledge among households.

Preventive and Treatment Services.—Table 6 presents the findings on the nine preventive and treatment approaches we measured and for which there is sufficient evidence in the medical literature of a causal relationship between the intervention and reductions in cause-specific mortality. The results for the preventive approaches are reported in columns 1–7. For three out of the seven preventive approaches we observe a significant difference between the treatment and the control group. Households in the treatment group were 5 percent, or 3.8 percentage points, more likely to have treated their water before use and their children were 13 percent, or 5.1 percentage points, more likely to have slept under an insecticide-treated bed net, compared to households in the control group. Households in the treatment

Table 6—Program Impact on Preventive and Treatment Services

Dependent variable	Child delivery at HF (1)	Follow-up visit first week post delivery (2)	Child was breastfed (3)	Child took vitamin A (4)	Child under treated net last night (5)
Program impact	0.010	0.081	−0.005	0.001	0.051
	(0.020)	(0.020)	(0.003)	(0.012)	(0.014)
Mean control	0.846	0.114	0.985	0.73	0.402
Branch FE	Yes	Yes	Yes	Yes	Yes
Observations	1,955	1,925	10,953	10,953	10,953
R^2	0.054	0.074	0.012	0.006	0.227

Dependent variable	Treat water before drinking (6)	Child fully immunized (7)	Child w/diarrhea treated w/ORS/zinc (8)	Child w/malaria treated w/ACT (9)	Average standardized effect (1)–(9) (10)
Program impact	0.038	−0.002	0.053	0.004	0.061
	(0.015)	(0.013)	(0.020)	(0.015)	(0.017)
Mean control	0.774	0.91	0.328	0.668	
Branch FE	Yes	Yes	Yes	Yes	Yes
Observations	7,013	2,041	2,686	5,422	
R^2	0.190	0.019	0.019	0.016	

Notes: Program impact measures the coefficient on the assignment to treatment indicator. Dependent variables are indicators taking value one if: (1) the respondent gave birth in a hospital or health facility (HF) (question only asked to women that delivered in the previous 12 months); (2) the household received a follow-up visit by a health care provider in the first week after delivery (question only asked to women that delivered in the previous 12 months); (3) the child (under-5) was breastfed; (4) the child ever took vitamin A; (5) the child slept under a treated bed net the previous night; (6) the respondent treats water before drinking it; (7) the child was fully immunized (question only asked for children born in the previous 12 months); (8) the child with diarrhea was treated with ORS/zinc (question only asked for children that fell sick with diarrhea in the previous 3 months); (9) the child with malaria was treated with ACT drugs for (at least) 3 days (question only asked for children that fell sick with malaria in the previous 3 months). Results in columns 1 to 9 are obtained from a standard OLS regression. Column 10 reports average (standardized) effect size across outcomes 1 to 9, using the seemingly unrelated regression framework to account for covariance across estimates. Branch fixed effects are included in every regression. There are 12 branches in the sample. Robust standard errors are in parentheses, clustered at the cluster level. There are 214 clusters in the sample.

group with a newborn baby were also 71 percent, or 8.1 percentage points, more likely to have received a follow-up visit in the first week after birth compared to households in the control group.[15] There is no significant difference between assignment groups in breastfeeding, whether the child was delivered in a clinic, whether the child received vitamin A, and whether the child is fully immunized. For three of these outcomes, however, there was again limited room for improvement, with 98 percent of children reported being breastfed, 85 percent of children being delivered in a clinic, and 91 percent of the children reported being fully immunized.

Columns 8–9 of Table 6 report the results for the two treatment approaches we measured: treatment with effective anti-malarials (ACT) in case of malaria, and treatment with oral rehydration therapy (ORS) in case of diarrhea. Self-reported morbidity in malaria and diarrhea did not differ between control and treatment groups. Conditional on falling sick with diarrhea, children in the treatment group

[15] As expected, the increase is driven by visits from CHPs. See Table A4 in the Appendix for more details.

were 16.2 percent, or 5.3 percentage points, more likely to have received treatment with ORS/zinc, column 8; while conditional on falling sick with malaria, children in the treatment group were as likely as children in the control group to have received treatment with ACTs for at least 3 days, column 9.

Finally, column 10 of Table 6 reports the average standardized effect of the nine preventive and treatment approaches. The point estimate is positive and highly significant, confirming that overall the intervention increased access or coverage to a set of proven medical services.

To what extent can the increased coverage of these services help account for the reduction in child mortality? To make some progress in answering this question, we use the nine preventive and treatment services in Table 6 as independent variables in an Oaxaca decomposition of the child mortality findings between the treatment and the control group. The results from such a decomposition are clearly suggestive, not least since the decomposition have a causal interpretation only under a set of strong assumptions, but also because there are many potential channels through which the CHPs' activities could reduce mortality, and we do not measure all conceivable channels.[16] Keeping those concerns in mind, the Oaxaca decomposition suggests that 34 percent of the treatment effect on child mortality can be accounted for by differences in means across the nine preventive and treatment services, i.e., in changes in the quantity of health service coverage. By contrast, 66 percent can be accounted for by differences in coefficients, i.e., by changes in the relationship between coverage and mortality, or by other factors that we cannot measure. Our preferred interpretation is that this is likely to reflect that the quality of products and services provided by the CHPs is higher, compared to what is offered in the status quo.[17] Further suggestive evidence that this is the case can be seen by running regressions of child mortality on all of the nine preventive and treatment services jointly, but separately for the treatment and the control group. This analysis indicates that the slope relationships between child mortality and the quantity of these services are not the same in treatment and control villages. In particular, in treatment villages, each individual coefficient enters with the right sign, consistently implying a negative relationship between quantity of these services and child mortality, and they are jointly highly statistically significant ($p < 0.001$). By contrast, in control villages, there is no apparent systematic relationship indicating preventive and treatment services are associated with lower child mortality—the point estimates fluctuate around zero with both positive and negative coefficients—and a test of joint statistical significance cannot reject the null hypothesis ($p = 0.14$).[18]

[16] A key assumption underlying the decomposition is the ignorability condition, namely that selection on observables (in this case with respect to the prevention and treatment measures) is the same in the treatment and control group. While this is a weaker condition than the standard zero conditional mean assumption, it is arguably a strong one since the presence of a CHP may influence what types of households seek and receive health services. For a general discussion of decomposition methods, see Fortin, Lemieux, and Firpo (2011).

[17] One example of improved quality of drugs in the treatment area is Björkman Nyqvist, Svensson, and Yanagizawa-Drott's (2014) study that shows that 37 percent of the local drug outlets in the control areas sell fake anti-malarial drugs. Treatment villages are provided higher quality drugs since the CHPs are selling authentic drugs.

[18] We do not include the analysis here, for brevity, but Appendix Figure A5 plots the estimated relationships for all nine prevention and treatment services across the two samples.

To probe further, we can conduct a simple back-of-the-envelope calculation where we estimate the predicted reduction in child mortality in the control group as if the control group had the same coverage as in the treatment group; i.e., same means for the nine explanatory variables. The result of such an exercise shows that child mortality in the control group is estimated to be reduced from 19.4 to 18.4 deaths per 1,000 years of exposure, i.e., a predicted reduction of a mere 5.2 percent. Analogously, assuming the treatment group had the same means as the control group, predicted mortality in treatment villages is 9.9 percent higher. As a comparison, in the treatment group, child mortality was reduced to 14.2, or by 27 percent relative the control group mean. Thus, while this analysis is merely suggestive, it indicates that differences in the quantity of life-saving approaches can explain quite a bit of the observed mortality effect, but not the bulk of it, whereas the quality by which these services were provided seemingly played an important role.

iCCM and MNCH Services.—Community health workers can also improve health outcomes through the provision of integrated community case management (iCCM) and maternal, newborn, and child health (MNCH) services. Some of these services are primarily intended to increase the coverage of effective preventive and treatment approaches, including those reported in Table 6. There is limited evidence of a direct effect of these services on reducing under-five mortality from trials delivered in a program setting. However, it is also the case that most trials in a program setting have struggled to achieve significant increases in intervention coverage so as to generate measurable reductions in child mortality—a fact that also motivated the design of the CHP program evaluated here.[19]

Table 7 presents the findings on the six iCCM and MNCH services we measured: follow-up visits of children fallen sick in malaria and diarrhea, referrals to professional care for sick children in malaria and diarrhea, encouragement to give birth with professional assistance, and family planning. For all of these services, except for referrals to professional care for children reported sick in malaria, we document large and significant differences between the treatment and the control group. Households with a child under-5 in the trial areas that fell sick with malaria or diarrhea were, respectively, 73 percent and 62 percent more likely to have received a follow-up visit compared to households with sick children in the control areas. A 10 percent higher share of women in the treatment group had been advised to give birth with professional assistance and 4 percent more of the households in the treatment as compared to the control group has used (any form of) family planning.

The results reported in Tables 6 and 7 also speak to the concern of multitasking. Of the nine preventive and treatment approaches reported in Table 6, five—follow-up visit in the first week after birth, insecticide-treated bed net, clean water, treatment of ORS in case of diarrhea, and ACT in case of malaria—were directly incentivized, as the CHPs were incentivized to visit newborns and sell water purification tablets and insecticide treated bed nets. For four out of these five outcomes, we observe a significant difference between the treatment and control groups. For the approaches not

[19] See discussion in Section IB.

Table 7—Program Impact on ICCM and MNCH Services

Dependent variable	Follow-up after child sick w/ diarrhea (1)	Child w/diarrhea referred to HF (2)	Follow-up after child sick w/malaria (3)	Child w/malaria referred to HF (4)	Advised to deliver in HF (5)	Ever used family planning (6)	Average standardized effect (1)–(6) (7)
Program impact	0.043	0.015	0.061	−0.004	0.059	0.025	0.104
	(0.017)	(0.009)	(0.014)	(0.008)	(0.024)	(0.014)	(0.026)
Mean control	0.069	0.041	0.084	0.083	0.577	0.683	
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,228	2,687	5,335	5,418	1,942	7,018	
R^2	0.077	0.008	0.096	0.008	0.039	0.019	

Notes: Program impact measures the coefficient on the assignment to treatment indicator. Dependent variables are indicators taking value one if: (1) the household received a follow-up visit by a health care provider after a child under-5 fell sick with diarrhea (question only asked for children that fell sick with diarrhea in the previous three months); (2) the child under-5 sick with diarrhea received a referral to the health facility (HF) (question only asked for children that fell sick with diarrhea in the previous three months); (3) the household received a follow-up visit by a health care provider after a child under-5 fell sick with malaria (question only asked for children that fell sick with malaria in the previous three months); (4) the child under-5 sick with malaria received a referral to the health facility (question only asked for children that fell sick with malaria in the previous three months); (5) woman was advised to deliver with the assistance of a doctor, nurse, or medical officer (question only asked to women that delivered in the previous 12 months); (6) respondent ever used family planning. Results in columns 1 to 6 are obtained from a standard OLS regression. Column 7 reports average (standardized) effect size across outcomes 1 to 6, using the seemingly unrelated regression framework to account for covariance across estimates. Branch fixed effects are included in every regression. There are 12 branches in the sample. Robust standard errors are in parentheses, clustered at the cluster level. There are 214 clusters in the sample.

directly incentivized, there are no significant differences between the groups, but also little room for improvement due to high coverage in both groups. Nevertheless, for the six iCCM and MNCH services, only family planning was incentivized through the inclusion of condoms in the product list. However, we still observe significant difference between the treatment and control group in all but one indicator. Overall, therefore, the findings suggest that the CHPs acted on the incentives they faced and as a result, utilization of a number of life-saving preventive and treatment services increased (see Table 6). But our findings also suggest that child mortality fell not only due to the increased coverage of the nine approaches we measure, but also for other reasons, including possibly the quality of the services and the products CHPs provided. In the end, though, whether the CHPs viewed providing higher quality services and/or non-incentivized services, such as follow-up visits and referrals (results in Table 7) as a way to increase the demand for health products in their portfolio, or they provided some of these services, and others such as health knowledge (results in Table 5) because of nonpecuniary reasons, we simply do not know. We do know, however, that there is no evidence that the intervention led to unintended negative health consequences, implying we can rule out more extreme implications that could result from the multitasking problems.

III. Robustness

Our study has some limitations. First, the choice not to have surveillance or monitoring systems in place in the study villages implied that we had to rely on

Table 8—Population Data and Flows

	Intervention group (115 clusters)	Control group (99 clusters)	p-value
Rate of in-migration	0.16 (0.12)	0.15 (0.11)	0.521
Rate of out-migration	0.07 (0.13)	0.07 (0.13)	0.991
Share of migrants	0.14 (0.09)	0.13 (0.08)	0.614

Notes: Data are mean standard deviations estimated by combining data from baseline census, endline census, and endline sample household survey. p-values are adjusted for the stratified randomized design. Rate of in-migration is i_j/b_j and rate of out-migration is o_j/b_j, where $i_j = \hat{\theta}_j \times e_j$, $o_j = b_j - (i - \hat{\theta}_j) \times e_j$, b_j is the number of households residing in cluster j at baseline, e_j is the number of households residing in cluster j at endline, and the share of migrants $\hat{\theta}_j$ is an estimate of the share of households in cluster j that moved into the cluster during the trial period, out of the total number of households living in the cluster at endline, based on the household sample survey.

retrospective recall information. We used standardized data collection methods, and any potential recall lapses were expected to affect the treatment and control groups equally. The fact that we identify highly statistically significant effects, despite the potential measurement error in the dependent variables, brings further credibility to our conclusions. Second, as we use the end of trial sample survey to define baseline residence and thus the core sample for the analysis, selective out-migration by assignment groups could have caused some confounding bias in our main estimates. In Table 8, we test whether there was selective in- and out-migration using enumeration data at baseline and endline combined with data from the household survey.

At baseline, 50,617 households were residing in the trial cluster, 4,132 of whom were estimated to have migrated out from the baseline cluster by the end of the trial. The average rate of out-migration per cluster was 7.1 percent (not statistically different between the treatment group and control group ($p = 0.991$)). An estimated 7,962 households moved into the trial clusters during the study period. The average rate of in-migration per cluster was 15.3 percent (not statistically different between the treatment group and control group ($p = 0.478$)). The share of sampled households that has moved into the cluster during the trial period, out of the total number of sampled households, was not statistically different between the treatment group and control group ($p = 0.614$). Hence, measured in- and out-migration into the study clusters were similar across assignment groups. Moreover, as shown in Table 2, baseline household characteristics of the sampled households that had lived in the same cluster for the whole study period were not statistically different between the treatment group and the control group. Overall, these results indicate that the results are unlikely to be biased by migration patterns.

Third, the possibility of contamination is plausible because the study clusters, within each zone, were geographically close. Analysis of behavioral data (interaction with CHP in the control sites) also suggested that some, although low, contamination occurred, most likely causing us to estimate a lower bound on the impact of the CHP program on child mortality.

Fourth, charging for preventive and curative products, even when prices are low, could disproportionately benefit the less-poor households. We therefore study whether the impacts of the CHP program differ depending on the wealth of the

households. One important caveat here is that we only have eight asset-related variables to use for the wealth index.[20] We do not find the impact of the program to be stronger in relatively poorer villages. In Figure A6 in the Appendix, we see that child mortality was reduced in 9 out of the 12 geographic zones that were part of the RCT. When sorting the 12 zones in terms of wealth, the three that did not experience any reduction in child mortality rank second, sixth, and tenth.[21]

However, it might still be the case that within a given village, CHPs focus more on the relatively wealthier households, as they might be more likely to purchase more products from them. One way to check this is to divide the sample of households within each study village in wealth quantiles and run the analysis by quantiles. One severe limitation with this approach is that child death is a relatively rare event and when we split the sample, results become very sensitive to few extra deaths. This problem becomes even more severe when we look at infant and neonatal mortality, as the number of deaths decreases further. For this reason, we limit this analysis on under-five mortality. Table A5 in the Appendix shows the results when the sample is divided in different wealth quantiles. Although the point estimates indicate reduction in mortality across the entire wealth spectrum, the results suggest that the impact is strongest for households in the middle of the wealth distribution.[22]

Finally, with the data at our disposal, we can indirectly explore whether and how CHPs' activity is affected by consideration on distance and on household characteristics. For this analysis we can rely on detailed GPS information that we collected during the survey. Results are reported in Table A6 in the Appendix. We find that a household is more likely to be visited by a CHP the closer it is to the CHP house and the further away it is from the house of Village Health Team (VHT) members (although in this case, the results are only weakly significant). We also find suggestive evidence that priority households—narrowly defined as households that recently experienced a delivery or pregnancy—are more likely to be visited by a CHP. Finally, when considering the full sample, we find some evidence of substitutability between different types of community health workers: households in treatment clusters are significantly more likely to interact with CHPs and significantly less likely to interact with VHTs.

[20] Ownership of durable assets (two sets of clothes for each household member, mobile phone, radio, and television), infrastructure and housing characteristics (electricity, roof, and floor material), and consumption habits (number of meals containing fish or meat served in a week).

[21] More formally, we observe a negative correlation (−0.73) significant at the 1 percent level between under-5 mortality and wealth across control villages. However, we find no clear link between wealth and program impact: the correlation between the reduction in mortality and the wealth index is negative (−0.44), but not significant. Moreover, if we run a regression with an interaction term between the village treatment status and the average wealth in the village, the estimated coefficient on the interaction terms is close to zero and far from being statistically significant.

[22] Important to note is that at the most severe level of poverty, clearly, there must be households who cannot afford to buy products from the CHPs. We do not have enough detailed data to investigate whether this is the case and if these poor households are not provided any services from the CHPs. We looked at the intermediate outcomes by wealth quartiles and we did not observe any clear pattern. For most outcomes, the control group means are lower for the poorest quartile, as would be expected, but we still observe increases that are generally of the same order of magnitude as in the other wealth quantiles (results available on request).

IV. Cost-Effectiveness Analysis

So far, we have focused on presenting the health impacts of the CHP program. We next turn to its cost-effectiveness. Community health worker programs have typically been promoted as an effective and relatively cheap method to deliver health services in low- and middle-income countries (Dahn et al. 2015). Remarkably, there is a dearth of information on the actual cost-effectiveness of such programs (Bhutta et al. 2010, Vaughan et al. 2015). Moreover, when cost-effectiveness is discussed, the impact of the program is often imputed based on a set of assumptions related to the coverage of the interventions and their impact (e.g., McPake et al. 2015).[23] By contrast, we can compute the cost-effectiveness of the CHP program by taking into account the actual impact observed in the study.

In estimating cost-effectiveness, we focus on the budget provided by Living Goods—one of the two NGOs involved in the project—whose operations exclusively focused on the implementation of the CHP program.[24]

During the study period, there were an estimated 5,339 children under 5 in the 46 Living Goods control clusters and 194 of these children died before reaching the age of 5. By applying this mortality rate to the 8,306 children located in the 62 Living Goods treatment clusters, we should have expected 302 deaths under-5 in this group of villages in the absence of the intervention. Our estimates above indicate that the program resulted, on average, in a 27 percent reduction in child mortality, or 81.5 averted deaths in the Living Goods treatment clusters over the 3-year trial period.[25] Given that overall 79 CHPs operated in the Living Goods treatment clusters during the study period, we estimate an average of 0.34 deaths averted per CHP per year.

In performing the cost-effectiveness analysis, we take the perspective of a public agency running the CHP program. We therefore take into account the total Uganda-based costs borne by Living Goods, add the deadweight loss associated with the increased taxes that would be required to pay the monetary cost of the program, and subtract the revenues that were generated by selling the health products to the CHPs.[26] Table 9 illustrates the evolution of the costs per death averted over time, starting in 2010 when Living Goods started its operations in Uganda, under the assumption of constant effectiveness of the program. As evident, the analysis suggests dramatic improvement in terms of cost-effectiveness during the first years of operations, as new CHPs got added to the program. This is due to the presence of

[23] The Lives Saved Tool (LiST) is a software often used to estimate such effects. LiST estimates how a projected change in selected inputs—related to a range of possible health interventions—can translate into a reduction in child mortality, for given assumptions on the coverage of and impact of the interventions.

[24] As explained above, the program was run by two NGOs—Living Goods and BRAC. While Living Goods only operates the CHP program, BRAC has a vast portfolio of activities. Given the presence of synergies across programs, it is difficult to properly isolate the portion of costs to be attributed to the CHP program. Still, we obtained an estimate of the costs that BRAC attributes to the CHP program and using these cost estimates provides cost-effectiveness estimates that are just slightly lower than those discussed in the text.

[25] When splitting the sample between the two NGOs, mortality reduction appears somewhat higher in Living Goods clusters, compared to BRAC clusters (31 percent (p-value $= 0.06$) versus 23 percent (p-value $= 0.08$)), but the difference is not statistically significant.

[26] Estimates of the marginal cost of public funds vary considerably. Auriol and Warlters (2012) estimates the average marginal cost across 38 African countries to be 1.21. According to their estimates, Uganda is the country with the third highest marginal cost, equal to 1.42. We therefore assume the marginal cost of public funds to be 1.42 (i.e., deadweight loss is 0.42). This is slightly higher than the value typically used for policy analysis (Olken 2007).

Table 9—Cost-Effectiveness Analysis

Year	2010	2011	2012	2013
Total costs (US dollars)	362,853	419,190	476,815	729,653
Deadweight loss (US dollars)	152,398	176,060	200,262	306,454
Sales revenues (US dollars)	87,939	180,936	253,762	407,364
Number of CHPs	128	223	437	477
Estimated number of deaths averted	44	76	149	162
Estimated number of life-years saved	2,589	4,511	8,841	9,650
Cost per death averted (US dollars)	9,819	5,464	2,849	3,877
Cost per life-year saved (US dollars)	165	92	48	65

Notes: Figures are based on the yearly budgets provided by Living Goods, one of the two NGOs that implemented the program.

significant fixed costs (such as the installation of new branches) and economies of scale (as for instance staff transportation costs are limited when the program expands to villages close to those already served).[27] During the first two years, the number of CHPs increased by more than 70 percent per year, and cost per averted death dropped by more than 50 percent per year. The expansion slowed down in 2013, when cost-effectiveness slightly worsened compared to the previous year.[28] Overall, in the three years of the evaluation, the average cost per death averted was $4,063.[29]

As a comparison, the Guttmacher Institute estimates, combining a range of available cost-effectiveness estimates, a cost per life saved from expanding a range of health services known to be effective at saving lives to $12,000, or almost three times higher than the estimate for the CHP program (Perry and Zulliger 2012). Overall, however, it is difficult to put the $4,063 estimate in perspective, given the lack of estimates of CHW programs based on rigorous evaluations.

A complimentary method to assess cost-effectiveness is to consider how much each life saved would have contributed in economic activity over his or her lifetime. Dahn et al. (2015), for instance, estimates the net present value of the contribution in economic activity that a child under-5 in SSA will provide over his or her lifetime to be approximately $65,000.[30] Based on this estimate, the CHP program provides an economic return of about 16:1.

[27] The fixed cost of opening a new branch office is estimated to be around $12,500 to cover licenses, design, furniture, and equipment. The yearly variable costs of an average branch are estimated to be around $19,000, with the largest item (about 38 percent) being staff travel expenses.

[28] From conversations with the NGO, costs in 2013 increased for two reasons. First, it was an important expansion year for the NGO, with a number of new hires in key positions and new investments. Among other things, the NGO also started its operations in Kenya, and while Kenya-based costs are not included in the figure, this expansion had some repercussions also on the organization in Uganda. Secondly, figures in 2013 also include part of the costs used to support the activity of a partner organization. Unfortunately, it is not possible to clearly identify and exclude those costs from the budget.

[29] We also obtained an estimate of the total costs attributable to the CHP program in 2013 from BRAC. Overall, total costs equal $823,363, while sales revenues were equal to $418,537. Taking into account the deadweight loss and the fact that BRAC had 644 CHPs operating in 2013, the estimated cost-effectiveness for BRAC in 2013 is $3,428, which is just slightly lower than the estimate we obtained for Living Goods for 2013.

[30] The estimate is based on the following assumptions: GDP per capita $1,738, expected GDP per capita growth of 2.5 percent per annum, child will enter the workforce at age 18 and exit the workforce at age 56, and discount rate of 5 percent to calculate the net present value of the future cash flows from projected lifetime earnings.

In Table 9, we also report the cost of the program per life-year saved. The estimates assume a life expectancy at age 5 to be equal to 59.5, in line with the 2013 WHO figures for Uganda. Our figures compare favorably to most existing estimates, which range from $82 per life-year gained in expanding maternal and child health prevention and promotion activities in Kenya by relying on community-health volunteers, to $3,396 per life-year gained in expanding basic health services in Indonesia by relying on village midwives assisted by community health volunteers (Borghi et al. 2005, McPake et al. 2015). While cost per life saved or per life-year gained are commonly used metrics to assess the cost-effectiveness of a health intervention, there are additional dimensions that could be taken into account when considering the welfare impact of the program. For instance, the fact that the program allows households to rely on primary health care in their own village, instead of traveling far to reach a health facility, is likely to bring a range of benefits to the society: it lowers the time and transportation costs for the patients,[31] while also reducing the workload of the health facilities, freeing up resources that can be better used for more urgent cases. Moreover, the presence of financial incentives provides all CHPs and their families with an additional source of income.

As the program keeps expanding, reaching more villages and recruiting more CHPs, the costs per CHPs are expected to decrease even further, making the program even more cost-effective.

V. Conclusion

We estimate that the CHP program in Uganda reduced the under-five mortality rate by 27 percent, infant mortality rate by 33 percent, and neonatal mortality rate by 27 percent after three years. These effects are supported by changes in health knowledge, increased utilization of preventive and treatment approaches, as well as increased maternal, newborn, and child health service coverage. We also estimate the cost-effectiveness of the program and find the estimated cost per averted death under five years old during the study period to be approximately $4,000.

While a growing body of evidence has identified effective interventions that can be delivered by community health workers, a key consideration for the success and sustainability of such program is how high-quality performance by community workers can be achieved and maintained. This study is the first impact evaluation of a community health delivery intervention based on an incentivized approach. Unlike previous studies that have primarily focused on the impact of specific interventions that could be delivered effectively in a community setting, our focus is on how to ensure that community health workers successfully implement a set of interventions proven to be effective if delivered and the impact that may have on child health.

In the CHP program, community health workers operated as micro-entrepreneurs earning an income on the sale of preventive and curative products. A concern with such a scheme is that it may encourage overuse of medications and inappropriate

[31] By exploiting a randomized evaluation of spring protection in Kenya, Kremer et al. (2011) estimate the average value of time to be about 7 percent of the mean local hourly wage. In the context of Mozambique, Jeuland et al. (2010) estimate the opportunity cost of time to be approximately 28 percent of the mean hourly wage.

treatment at the expense of prevention and referrals. However, the provision of free services like health education and follow-up visits was viewed as strategy to build up a loyal customer base. More generally, whether extrinsic incentives in some domains have positive or negative impacts on intrinsic motivation in other domains is an empirical question. The data do not suggest that the program only had an impact on incentivized services, with evidence of increases in the promotion of healthy behavior and improved health knowledge. More specifically, while we document large increases in some incentivized tasks, like visits of newborns, we also document large increases for tasks that were not directly incentivized, such as follow-up visits of children sick in malaria and diarrhea. Although the experimental design does not allow us to clearly separate out the mechanisms, the findings suggest that providing financial incentives on one dimension of the desired CHP behavior did not crowd out the allocation of effort into other margins.

Our analysis has shown no significant impact on malaria prevalence and treatment. However, a similar treatment pattern does not necessarily imply a similar quality of treatment. Among other things, the CHPs sell authentic ACT drugs. There is growing evidence that the private market for anti-malarial medicines is plagued by counterfeit and substandard (fake) products, with recent estimates suggesting that as much as a third of the anti-malarial drugs sold contain too little or no active pharmaceutical ingredients (Nayyar et al. 2012). Uganda is no exception: a smaller study conducted in the same research areas one year into the program estimated that 37 percent of the retail outlets were selling substandard anti-malarial drugs (Björkman Nyqvist, Svensson, and Yanagizawa-Drott 2013). Poor quality is not specific to ACTs but is a generic problem in the largely unregulated market for preventive and curative health products in many developing countries. The CHPs market share for ACT drugs and ORS were 11.3 percent and 14.1 percent, respectively. Under the assumption that every third dose of ACT treatment sold in the private market is fake and that authentic drugs are provided in the public sector (about 40 percent of the market share), children in the treatment group are 19 percent less likely to be treated with a fake ACT medicine.

With the accumulated know-how we have today, few would question the potential of community health care provision. How to best ensure that CHW deliver timely and appropriate services is, however, largely an open question and motivates the continued search for innovative approaches. The CHP program we studied here harnesses the power of franchised direct selling (business-in-a-bag) to provide community health providers with incentives to increase access to low-cost, high-impact health products and basic newborn and child health services. As of the end of 2018, the program was active in more than 7,000 clusters, with a total population of over 5.5 million. The impact of the CHP program was conditional on existing facility based professional health care as availability of referral services is a crucial component to the program. Thus, the findings should encourage government and nongovernment organizations to continue improving their facility based care, but also points to the importance of integrating the program into the existing health service provision strategy. The process of integrating the CHP program we have evaluated here into the overall health care system is currently underway.

Appendix

Table A1—CHWs' Characteristics

Personal characteristics	CHP women	DHS women
Age	37.4	36.4
Married	75%	77%
Respondent is head of household	67%	29%
Number of household members	7.5	6.1
Number of children (0–5)	1.8	1.1
Have children under age 5	67%	68%
Have school children (age 6–18)	89%	94%
Years of education	9.5	6.1
Finished O-level education	43%	32%
Previous experience as health worker	29%	*NA*
Electricity in the household	5%[1], 59%[2]	11%[1], 53%[2]
Brick home	26%[1], 73%[2]	67%[1], 75%[2]

Notes: Figures are taken from the report "Assessment of Activities by the Community Health Promoters of BRAC and Living Goods in Uganda" produced by BRAC and Living Goods in 2012. The sample covers 196 CHPs working in the study area. The DHS women sample is based on women living in the study districts and in the age span 30–45 (326 observations).

[1] Indicates the rural share of people with electricity and brick homes.

[2] Indicates the urban share of people with electricity and brick homes.

Table A2—CHPs' Knowledge

Topic	Percent correct answers
Diarrhea	30.1
Malaria	68.9
Maternal and neonatal health	43.2
Pregnancy/birth warning signs	45.1

Notes: Figures are taken from the report "Assessment of Activities by the Community Health Promoters of BRAC and Living Goods in Uganda" produced by BRAC and Living Goods in 2012. The sample covers 196 CHPs working in the study area. Scores for *Diarrhea* are based on ability to detect signs and symptoms, correct treatment, prevention, and what to do when identifying someone with the disease. Scores for *Malaria* are based on detecting signs and symptoms, correct treatment, prevention, what to do when identifying someone with malaria, and correct dosage of ACT to children under the age of two and five. Scores for *Maternal and neonatal health* are based on detecting signs and symptoms of pregnancy, danger signs during pregnancy, what drugs to use when treating malaria, what to do when identifying a pregnant woman, what to do when identifying a woman near to giving birth, and what to do when there is a newborn in the home. Scores for *Pregnancy/birth warning signs* are based on detecting hemorrhage, convulsions/seizures, prolonged labor, obstructed labor, severe headache, blurred vision, hyperpyrexia, and water breaks without labor starting.

TABLE A3—CHPs' ACTIVITIES

Question	Average answer
Days per week in visits	2.3
Hours per day in visits	3.4
Household visits per day	10.6
Household visited more than once per month	13.0
Percent CHPs who visited someone new last month	47.9
Number of community health meetings per month	1.4
Average attendance in community health meetings	16.8

Notes: Figures are taken from the report "Assessment of Activities by the Community Health Promoters of BRAC and Living Goods in Uganda" produced by BRAC and Living Goods in 2012. The sample covers 196 CHPs working in the study area.

TABLE A4—PROGRAM IMPACT ON HEALTH VISITS

	Follow-up visit...			
Dependent variable	… first week post delivery (1)	… after child sick with malaria (2)	… after child sick with diarrhea (3)	Average standardized effect (4)
Panel A. Any health care provider				
Program impact	0.081	0.061	0.043	0.215
	(0.020)	(0.014)	(0.017)	(0.048)
Mean control	0.114	0.084	0.069	
Panel B. CHPs				
Program impact	0.069	0.057	0.043	0.437
	(0.016)	(0.013)	(0.015)	(0.101)
Mean control	0.027	0.011	0.017	
Panel C. Any other health care provider				
Program impact	0.012	0.005	0.000	0.021
	(0.014)	(0.008)	(0.009)	(0.026)
Mean control	0.087	0.073	0.051	
Branch FE	Yes	Yes	Yes	Yes
Observations	1,925	5,335	2,228	

Notes: Program impact measures the coefficient on the assignment to treatment indicator. Dependent variables are indicators taking value one if the household received a follow-up visit by a health care provider: (1) in the first week after delivery; (2) after a child under-5 fell sick with malaria; (3) after a child under-5 fell sick with diarrhea. Panel A considers follow-up visits performed by any health care provider (this includes CHPs, doctors, nurses, other health workers, and traditional healers). Panel B restricts the focus to follow-up visits performed by CHPs from Living Goods or BRAC. Panel C restricts the focus to follow-up visits performed by any other health care provider. Columns 1 to 3 report the results of separate OLS regressions. Column 4 reports average (standardized) effect size across outcomes 1 to 3, using the seemingly unrelated regression framework to account for covariance across estimates. Branch fixed effects are included in every regressions. There are 12 branches in the sample. Robust standard errors are in parentheses, clustered at the cluster level. There are 214 clusters in the sample.

Table A5—Under-5 Mortality by Wealth Quantiles

	Under-5 mortality per 1,000 years of exposure					
	50–50 split		Quartiles			
	Poorest (1)	Richest (2)	Poorest quartile (3)	Others (4)	Richest quartile (5)	Others (6)
Program impact	−5.67	−6.14	−4.40	−6.81	−5.90	−6.12
	(2.95)	(2.90)	(3.83)	(2.38)	(4.38)	(2.26)
Rate ratio	0.78	0.68	0.83	0.69	0.69	0.74
	(0.12)	(0.12)	(0.18)	(0.10)	(0.16)	(0.10)
Mean control	19.84	18.77	18.83	19.93	20.41	19.24
Branch FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	214	214	214	214	214	214
R^2	0.082	0.124	0.045	0.150	0.047	0.126

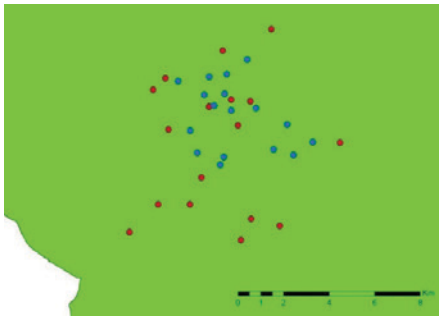
Notes: Program impact measures the coefficient on the assignment to treatment indicator, from a standard OLS regression. The dependent variable is the number of under-5 deaths per 1,000 child-years of exposure to the risk of death. Rate ratios are derived from a Poisson model, with branch fixed effects and standard errors clustered by village. Each regression refers to a different sample, selected on the basis of a wealth index, computed using Principal Component Analysis (PCA) on eight variables capturing ownership of durable assets (two sets of clothes for each household member, mobile phone, radio, and television), infrastructure and housing characteristics (electricity, roof, and floor material), and consumption habits (number of meals containing fish or meet served in a week). In columns 1 and 2, the sample is divided within each village between the poorest and the richest halves. In columns 3 to 6, the sample is divided within each village in wealth quartiles: column 3 considers the poorest quartile; column 4 considers the other three quartiles (excluding the poorest one); column 5 considers the richest quartile; column 6 considers the other three quartiles (excluding the richest one). Branch fixed effects are included in every regression. There are 12 branches in the sample. Robust standard errors are in parentheses. R^2 refers to the OLS regressions.

Table A6—CHPs' Activity

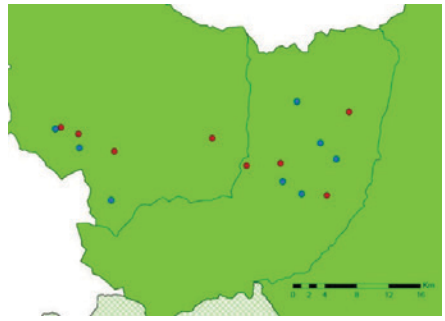
	Treatment clusters								Full sample	
	HHs ever visited by a CHP				HHs visited by a CHP in last 30 days				Ever interacted with CHP	Ever interacted with VHT
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance from CHP	−0.061		−0.064		−0.037		−0.041			
	(0.017)		(0.019)		(0.018)		(0.021)			
Distance from VHT		0.042	0.045			0.029	0.029			
		(0.023)	(0.021)			(0.030)	(0.021)			
Priority HH				0.054				0.045		
				(0.015)				(0.014)		
Program impact									0.215	−0.032
									(0.025)	(0.015)
Branch FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Number of clusters	104	104	95	115	104	104	95	115	214	214
Observations	3,434	3,431	3,138	3,790	3,434	3,431	3,138	3,790	7,018	7,018
R^2	0.307	0.297	0.312	0.294	0.190	0.182	0.191	0.182	0.098	0.178

Notes: Dependent variables are indicators taking value one if: (1)–(4) households were ever visited by a CHP; (5)–(8) households were visited by a CHP in the previous 30 days; (9) households interacted with a CHP at any point in time for any health-related issue (including receiving advice or medical products); (10) households interacted with a VHT at any point in time for any health-related issue (including receiving advice or medical products). In columns 1 to 8, the sample is restricted to villages in treatment areas (115 clusters and 3,790 households). Clusters in which the geolocation of the CHP or of the VHT could not be recorded are missing. *Priority households* are defined as households where a woman delivered in the previous six months and/or a woman is currently pregnant. *Program impact* measures the coefficient on the assignment to treatment indicator. Robust standard errors are in parentheses, clustered at the cluster level.

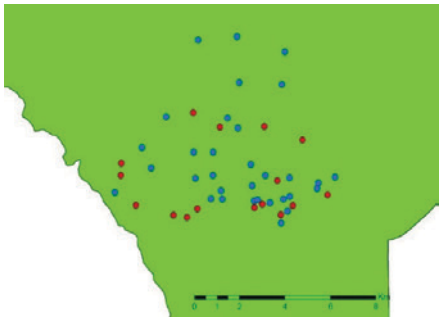
Panel A. Arua district



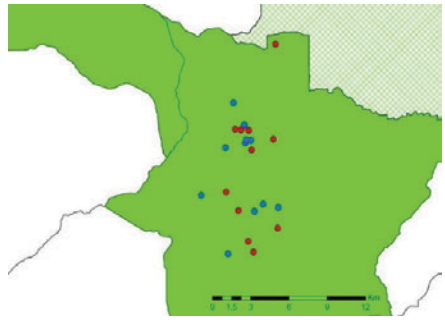
Panel B. Bushenyi (West)/Sheema (East) districts



Panel C. Jinja district



Panel D. Mbale district



Panel E. Mpigi district

Panel F. Mukono district

Panel G. Pallisa district

Panel H. Ibanda (North)/Mbarara (South) district

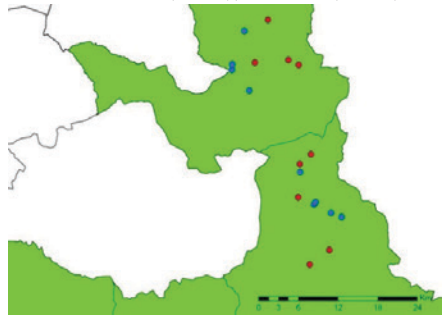


FIGURE A1. MAPS BY STUDY DISTRICT

Notes: These figures are expansions of the map reported in Figure 1. Green fully colored indicate districts that were part of the study. Red and blue dots indicate, respectively, control and intervention villages included in the study.

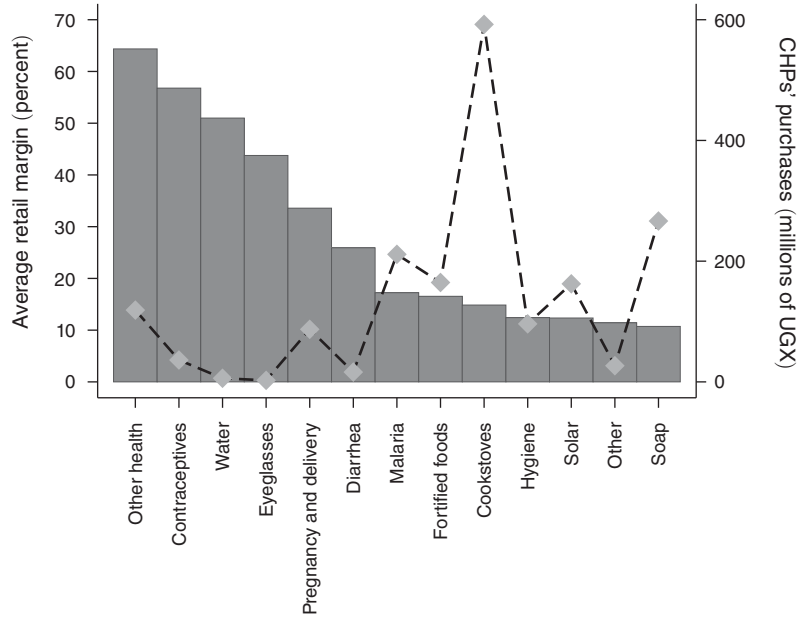


FIGURE A2. YEARLY CHP PURCHASES AND MARGINS ACROSS PRODUCTS

Notes: The figure illustrates the average margin the CHPs made across 13 macro-category of products (152 different products in total) in 2013, together with the amount they purchased from the local branches in 2013.

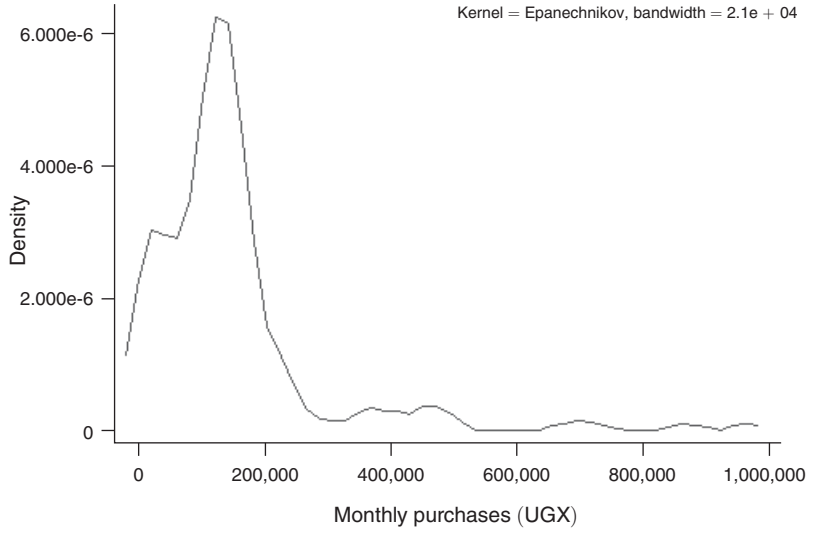


FIGURE A3. CHP MONTHLY PURCHASES

Notes: The figure illustrates the kernel distribution across CHPs of the monthly purchases from the NGOs in 2013. Values range from 0 to 950,000 UGX (mean: 150,000; standard deviation: 140,000). Information was provided by the two NGOs, Living Goods and BRAC.

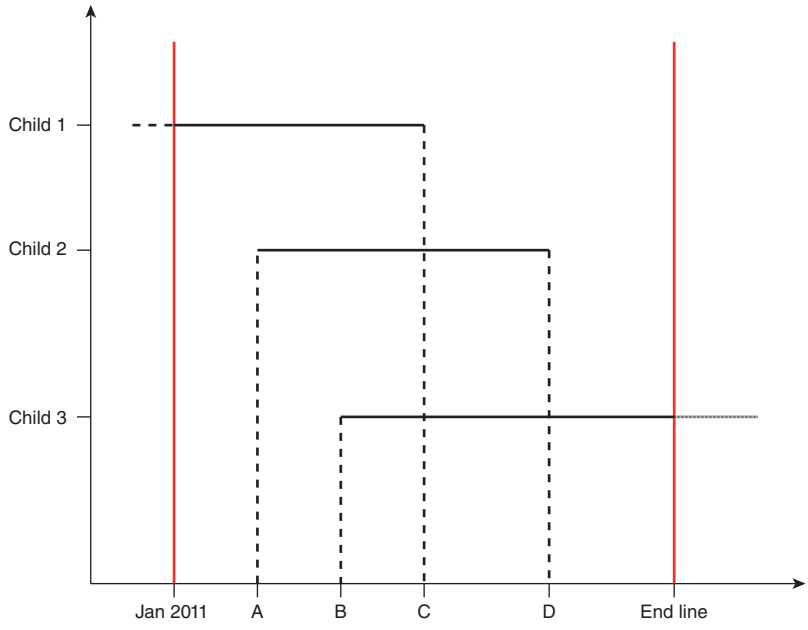


FIGURE A4. COMPUTATION OF THE MONTHS OF EXPOSURE TO THE RISK OF DEATH UNDER-5

Notes: For each child, the number of months of exposure to the risk of death under-5 during the study period is computed as the number of months between the birth date of the child, or the start date of the program (January 2011) if the child was born before that date, and the date that the child turned five years old if that occurred during the study period, or the date of the end line household survey if the child was less than five years old at that time, or the date of the death of the child. The figure illustrates these different possibilities using the example of 3 children: child 1 was born before January 2011 and turned 5 at time C (the same computation would hold if the child died under age 5 at time C). Hence, the exposure to the risk of death under-5 for child 1 is represented by the (rounded) number of months between January 2011 and time C. Child 2 was instead born at time A, during the study period, but died at time D. Hence, in this case, the exposure to the risk of death under-5 is represented by the (rounded) number of months between time A and D. Finally, child 3 was also born during the study period, at time B, and was still alive at the time of the end line. In this case, the exposure to the risk of death under-5 is represented by the (rounded) number of months between time B and the time of the end line survey.

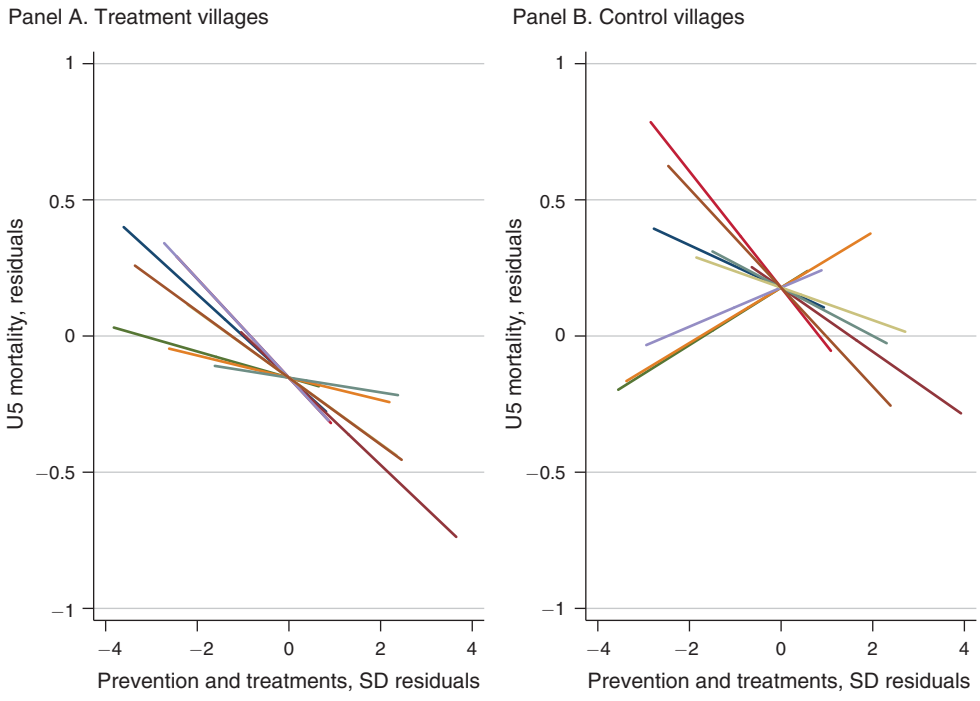


FIGURE A5. CORRELATIONS BETWEEN CHILD MORTALITY AND INTERMEDIATE OUTCOMES

Notes: The two graphs above plot the slope relationships between child mortality and the nine preventive and treatment services, separately in treatment and control villages. For ease of interpretation, all variables are standardized in the full sample. The underlying variation consists of the residuals from a regression of child mortality on all the nine variables jointly, and thus each slope captures the residual relationship holding constant the other eight variables. The graph shows that in treatment villages, all nine relationships are negative (joint p-value < 0.001), implying that greater coverage of the prevention and treatment services are associated with lower mortality; whereas in control villages, there is no apparent systematic relationship (joint $p = 0.14$). Our preferred interpretation is that these relationships indicate that not only quantity, but also quality, of these services matters and that the quality of services provided by the CHPs was greater compared to the status quo.

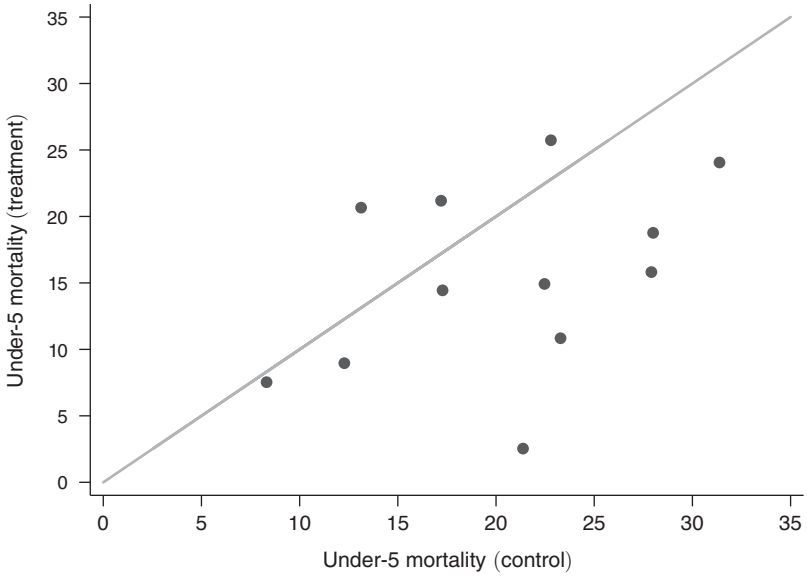


FIGURE A6. PROGRAM IMPACT ACROSS GEOGRAPHIC ZONES

Notes: The figure illustrates program impact across the 12 different geographic zones, each one associated to a different NGO's branch. Each dot represents one zone. Points below the 45-degree line indicate that under-5 mortality in the treatment villages in that zone was lower than under-5 mortality in the control area. Reduction in child mortality was achieved in 9 out of 12 study zones.

REFERENCES

Amouzou, Agbessi, Elizabeth Hazel, Bryan Shaw, Nathan P. Miller, Mengistu Tafesse, Yared Mekonnen, Lawrence H. Moulton, et al. 2016. "Effects of the Integrated Community Case Management of Childhood Illness Strategy on Child Mortality in Ethiopia: A Cluster Randomized Trial." *American Journal of Tropical Medicine and Hygiene* 94 (3): 596–604.

Apltauer, Kathleen Wetzel, and Munshi Sulaiman. 2012. "Assessment of Activities by the Community Health Promoters of BRAC and Living Goods in Uganda." Unpublished.

Arifeen, Shams E., D.M. Emdadul Hoque, Tasnima Akter, Muntasirur Rahman, Mohammad Enamul Hoque, Khadija Begum, Enayet K. Chowdhury, et al. 2009. "Effect of the Integrated Management of Childhood Illness Strategy on Childhood Mortality and Nutrition in a Rural Area in Bangladesh: A Cluster Randomised Trial." *Lancet* 374 (9687): 393–403.

Ashraf, Nava, Oriana Bandiera, and B. Kelsey Jack. 2014. "No Margin, No Mission? A Field Experiment on Incentives for Public Service Delivery." *Journal of Public Economics* 120: 1–17.

Ashraf, Nava, Oriana Bandiera, and Scott S. Lee. 2016. "Do-Gooders and Go-Getters: Career Incentives, Selection, and Performance in Public Service Delivery." https://scholar.harvard.edu/files/lee/files/scott_lee_do-gooders_latest.pdf.

Auriol, Emmanuelle, and Michael Warlters. 2012. "The Marginal Cost of Public Funds and Tax Reform in Africa." *Journal of Development Economics* 97 (1): 58–72.

Baqui, Abdullah H., Shams El-Arifeen, Gary L. Darmstadt, Saifuddin Ahmed, Emma K. Williams, Habibur R. Seraji, Ishtiaq Mannan, et al. 2008. "Effect of Community-Based Newborn-Care Intervention Package Implemented through Two Service-Delivery Strategies in Sylhet District, Bangladesh: A Cluster-Randomised Controlled Trial." *Lancet* 371 (9628): 1936–44.

Bhandari, Nita, Sarmila Mazumder, Sunita Taneja, Halvor Sommerfelt, and Tor A. Strand. 2012. "Effect of Implementation of Integrated Management of Neonatal and Childhood Illness (IMNCI) Programme on Neonatal and Infant Mortality: Cluster Randomised Controlled Trial." *BMJ* 344: Article e1634.

Bhutta, Zulfiqar A., Zohra S. Lassi, George Pariyo, and Luis Huicho. 2010. *Global Experience of Community Health Workers for Delivery of Health Related Millennium Development Goals: A Systematic Review, Country Case Studies, and Recommendations for Integration into National Health Systems*. Geneva: World Health Organization.

Bhutta, Zulfiqar A., Sajid Soofi, Simon Cousens, Shah Mohammad, Zahid A. Memon, Imran Ali, Asher Feroze, et al. 2011. "Improvement of Perinatal and Newborn Care in Rural Pakistan through Community-Based Strategies: A Cluster-Randomised Effectiveness Trial." *Lancet* 377 (9763): 403–12.

Björkman Nyqvist, Martina, Damien De Walque, and Jakob Svensson. 2017. "Experimental Evidence of the Long-Run Impact of Community Based Monitoring." *American Economic Journal: Applied Economics* 9 (1): 33–69.

Björkman Nyqvist, Martina, Andrea Guariso, Jakob Svensson, and David Yanagizawa-Drott. 2019. "Reducing Child Mortality in the Last Mile: Experimental Evidence on Community Health Providers in Uganda: Dataset." https://doi.org/10.1257/app.20170201.

Björkman Nyqvist, Martina, Jakob Svensson, and David Yanagizawa-Drott. 2013. "The Market for (Fake) Antimalarial Medicine: Evidence from Uganda." https://pdfs.semanticscholar.org/1218/f721296f51150bcc858a1c606bfdc38a474f.pdf.

Black, Robert E., Saul S. Morris, and Jennifer Bryce. 2003. "Where and Why Are 10 Million Children Dying Every Year?" *Lancet* 361 (9376): 2226–34.

Boone, Peter, Diana Elbourne, Ila Fazzio, Samory Fernandes, Chris Frost, Chitra Jayanty, Rebecca King, et al. 2016. "Effects of Community Health Interventions on under-5 Mortality in Rural Guinea-Bissau (EPICS): A Cluster-Randomised Controlled Trial." *Lancet Global Health* 4 (5): 328–35.

Borghi, Josephine, Bidur Thapa, David Osrin, Stephen Jan, Joanna Morrison, Suresh Tamang, Bhim Prasad Shrestha, et al. 2005. "Economic Assessment of a Women's Group Intervention to Improve Birth Outcomes in Rural Nepal." *Lancet* 366 (9500): 1882–84.

Christopher, Jason B., Alex Le May, Simon Lewin, and David A. Ross. 2011. "Thirty Years after Alma-Ata: A Systematic Review of the Impact of Community Health Workers Delivering Curative Interventions against Malaria, Pneumonia and Diarrhoea on Child Mortality and Morbidity in Sub-Saharan Africa." *Human Resources for Health* 9: Article 27.

Dahn, Bernice, Addis Tamire Woldemariam, Henry Perry, Akiko Maeda, Drew von Glahn, Raj Panjabi, Na'im Merchant, et al. 2015. *Strengthening Primary Health Care through Community Health Workers: Investment Case and Financing Recommendations*. World Health Organization. Geneva, July.

Darmstadt, Gary L., Yoonjoung Choi, Shams E. Arifeen, Sanwarul Bari, Syed M. Rahman, Ishtiaq Mannan, Habibur Rahman Seraji, et al. 2010. "Evaluation of a Cluster-Randomized Controlled Trial of a Package of Community-Based Maternal and Newborn Interventions in Mirzapur, Bangladesh." *PLOS One*: e9696.

Deserranno, Erika. 2019. "Financial Incentives as Signals: Experimental Evidence from the Recruitment of Village Promoters in Uganda." *American Economic Journal: Applied Economics* 11 (1): 277–317.

Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2007. "Using Randomization in Development Economics Research: A Toolkit." In *Handbook of Development Economics*, Vol. 4, edited by T. Paul Schultz and John A. Strauss, 3895–3962. Amsterdam: North-Holland.

Duflo, Esther, Rema Hanna, and Stephen P. Ryan. 2012. "Incentives Work: Getting Teachers to Come to School." *American Economic Review* 102 (4): 1241–78.

Dupas, Pascaline, and Edward Miguel. 2016. "Impacts and Determinants of Health Levels in Low-Income Countries." National Bureau of Economic Research (NBER) Working Paper 22235.

Fortin, Nicole, Thomas Lemieux, and Sergio Firpo. 2011. "Decomposition Methods in Economics." In *Handbook of Labor Economics*, Vol. 4A, edited by Orley Ashenfelter and David Card, 1–102. Amsterdam: North-Holland.

Gilmore, Brynne, and Eilish McAuliffe. 2013. "Effectiveness of Community Health Workers Delivering Preventive Interventions for Maternal and Child Health in Low- and Middle-Income Countries: A Systematic Review." *BMC Public Health* 13: Article 847.

Glewwe, Paul, Nauman Ilias, and Michael Kremer. 2010. "Teacher Incentives." *American Economic Journal: Applied Economics* 2 (3): 205–27.

Gogia, Siddhartha, and Harshpal Singh Sachdev. 2010. "Home Visits by Community Health Workers to Prevent Neonatal Deaths in Developing Countries: A Systematic Review." *Bulletin of the World Health Organization* 88 (9): 658–66.

Haines, Andy, David Sanders, Uta Lehmann, Alexander K. Rowe, Joy E. Lawn, Steve Jan, Damian G. Walker, et al. 2007. "Achieving Child Survival Goals: Potential Contribution of Community Health Workers." *Lancet* 369 (9579): 2121–31.

Hug, Lucia, David Sharrow, Kai Zhong, and Danzhen You. 2014. *Levels and Trends in Child Mortality*. UN Inter-agency Group for Child Mortality. New York.

Jeuland, Marc, Marcelino Lucas, John Clemens, and Dale Whittington. 2010. "Estimating the Private Benefits of Vaccination against Cholera in Beira, Mozambique: A Travel Cost Approach." *Journal of Development Economics* 91 (2): 310–22.

Jones, Gareth, Richard W. Steketee, Robert E. Black, Zulfiqar A. Bhutta, and Saul S. Morris. 2003. "How Many Child Deaths Can We Prevent This Year?" *Lancet* 362 (9377): 65–71.

Kidane, Gebreyesus, and Richard H. Morrow. 2000. "Teaching Mothers to Provide Home Treatment of Malaria in Tigray, Ethiopia: A Randomised Trial." *Lancet* 356 (9229): 550–55.

Kirkwood, Betty R., Alexander Manu, Augustinus H. A. ten Asbroek, Seyi Soremekun, Benedict Weobong, Thomas Gyan, Samuel Danso, et al. 2013. "Effect of the Newhints Home-Visits Intervention on Neonatal Mortality Rate and Care Practices in Ghana: A Cluster Randomised Controlled Trial." *Lancet* 381 (9884): 2184–92.

Kling, Jeffrey R., Jeffrey B. Liebman, Lawrence F. Katz, and Lisa Sanbonmatsu. 2004. "Moving to Opportunity and Tranquility: Neighborhood Effects on Adult Economic Self-Sufficiency and Health from a Randomized Housing Voucher Experiment." Princeton University Industrial Relations Section Working Paper 481.

Kremer, Michael, Jessica Leino, Edward Miguel, and Alix Peterson Zwane. 2011. "Spring Cleaning: Rural Water Impacts, Valuation, and Property Rights Institutions." *Quarterly Journal of Economics* 126 (1): 145–205.

Kumar, Vishwajeet, Saroj Mohanty, Aarti Kumar, Rajendra P. Misra, Mathuram Santosham, Shally Awasthi, Abdullah H. Baqui, et al. 2008. "Effect of Community-Based Behaviour Change Management on Neonatal Mortality in Shivgarh, Uttar Pradesh, India: A Cluster-Randomised Controlled Trial." *Lancet* 372 (9655): 1151–62.

Lavy, Victor. 2002. "Evaluating the Effect of Teachers' Group Performance Incentives on Pupil Achievement." *Journal of Political Economy* 110 (6): 1286–1317.

Lewin, Simon, Susan Munabi-Babigumira, Claire Glenton, Karen Daniels, Xavier Bosch-Capblanch, Brian E. van Wyk, Jan Odgaard-Jensen, et al. 2010. "Lay Health Workers in Primary and Community Health Care for Maternal and Child Health and the Management of Infectious Diseases." *Cochrane Library* 3: Article CD004015.

Luo, Renfu, Grant Miller, Scott Rozelle, Sean Sylvia, and Marcos Vera-Hernández. 2015. "Can Bureaucrats Really Be Paid Like CEOs? School Administrator Incentives for Anemia Reduction in Rural China." National Bureau of Economic Research (NBER) Working Paper 21302.

McPake, Barbara, Ijeoma Edoka, Sophie Witter, Karina Kielmann, Miriam Taegtmeyer, Marjolein Dieleman, Kelsey Vaughan, et al. 2015. "Cost-Effectiveness of Community-Based Practitioner Programmes in Ethiopia, Indonesia, and Kenya." *Bulletin of the World Health Organization* 93: 631–39.

Miller, Grant, Renfu Luo, Linxiu Zhang, Sean Sylvia, Yaojiang Shi, Patricia Foo, Qiran Zhao, Reynaldo Martorell, Alexis Medina, and Scott Rozelle. 2012. "Effectiveness of Provider Incentives for Anaemia Reduction in Rural China: A Cluster Randomised Trial." *BMJ* 345: Article e4809.

Muralidharan, Karthik, and Venkatesh Sundararaman. 2011. "Teacher Performance Pay: Experimental Evidence from India." *Journal of Political Economy* 119 (1): 39–77.

Naimoli, Joseph F., Diana E. Frymus, Estelle E. Quain, and Emily L. Roseman. 2012. *Community and Formal Health System Support for Enhanced Community Health Worker Performance: A U.S. Government Evidence Summit*. USAID. Washington, DC, December.

Nayyar, Gaurvika M.L., Joel G. Breman, Paul N. Newton, and James Herrington. 2012. "Poor- Quality Antimalarial Drugs in Southeast Asia and Sub-Saharan Africa." *Lancet Infectious Diseases* 12 (6): 488–96.

Okwundu, Charles I., Sukrti Nagpal, Alfred Musekiwa, and David Sinclair. 2013. "Home- or Community-Based Programmes for Treating Malaria." *Cochrane Database of Systematic Reviews* 5: Article CD009527.

Olken, Benjamin A. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia." *Journal of Political Economy* 115 (2): 200–249.

Olken, Benjamin A., Junko Onishi, and Susan Wong. 2014. "Should Aid Reward Performance? Evidence from a Field Experiment on Health and Education in Indonesia." *American Economic Journal: Applied Economics* 6 (4): 1–34.

Perry, H., and R. Zulliger. 2012. *How Effective Are Community Health Workers? An Overview of Current Evidence with Recommendations for Strengthening Community Health Worker Programs to Accelerate Progress in Achieving the Health-Related Millennium Development Goals*. Baltimore: Johns Hopkins Bloomberg School of Public Health.

Singh, Prabhjot, and Jeffrey D. Sachs. 2013. "1 Million Community Health Workers in Sub-Saharan Africa by 2015." *Lancet* 382 (9889): 363–65.

Singh, Prakarsh. 2015. "Performance Pay and Information: Reducing Child Undernutrition in India." *Journal of Economic Behavior and Organization* 112: 141–63.

Sloan, Nancy L., Salahuddin Ahmed, Satindra N. Mitra, Nuzhat Choudhury, Mushtaque Chowdhury, Ubaider Rob, and Beverly Winikoff. 2008. "Community-Based Kangaroo Mother Care to Prevent Neonatal and Infant Mortality: A Randomized, Controlled Cluster Trial." *Pediatrics* 121 (5): Article 3.

Vaughan, Kelsey, Maryse C. Kok, Sophie Witter, and Marjolein Dieleman. 2015. "Costs and Cost-Effectiveness of Community Health Workers: Evidence from a Literature Review." *Human Resources for Health* 13: Article 71.

World Health Organization (WHO). 2017. "Children: Reducing Mortality." *WHO Fact Sheet*, September 19. http://www.who.int/mediacentre/factsheets/fs178/en/.