Community Health Educators and Maternal Health: Experimental Evidence from Northern Nigeria *

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Abstract

The slow pace of improvement in service delivery and health outcomes for pregnant women and newborns in developing countries has been a major concern for policy makers in recent decades. This paper presents the results from a randomized controlled trial of a community health worker program designed to enhance uptake of child and maternal health services in northern Nigeria. Three interventions were evaluated: the deployment of community health educators; health educators with the provision of safe birth kits; and health educators with community dramas. The results suggest that the interventions increased utilization of antenatal, postnatal, and infant care. Maternal and newborn health practices improved as well as health knowledge. In addition, the community health worker program was more effective when supplemented with additional program components.

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1 Introduction

Enhancing health outcomes for pregnant women and newborns in developing countries has been a key policymaking goal over the last twenty years, but progress has been stubbornly slow. The World Health Organization estimates that more than 300,000 women and more than two million newborns die each year during pregnancy and childbirth (WHO, 2016). The majority of maternal and newborn deaths can be prevented via the appropriate provision of antenatal care in pregnancy, skilled care during childbirth, and postnatal care after delivery (Goodburn and Campbell, 2001). However, availability and utilization of these services remains low in much of the developing world (WHO, 2016). Barriers to utilization of maternal health care include an inability to access services due to transportation, logistical or financial constraints, disagreements within a household over care utilization, or low trust in the quality of facilities and providers (Gabrysch and Campbell, 2009; Simkhada et al., 2008).

In this study, we evaluate a community-based health intervention designed to increase utilization of maternal health services and enhance maternal and neonatal health in rural northern Nigeria. The base program relies on voluntary female community members who are recruited, trained and deployed as community resource persons (CORPs). CORPs conduct door-to-door visits to pregnant women in order to provide health information, encourage the utilization of facility-based care, and promote safe pregnancy and infant health practices.

The basic CORPs program was supplemented with two additional health programs in two separate treatment arms. The first additional program was safe birth kits distributed by the CORPs to pregnant women. The kits contained sterile supplies that could be used during delivery at home or in a facility. The objective of the birth kits intervention was twofold: its provision may reduce barriers to delivering in a facility for women who avoided facilities due to the perception of stockouts or the fear that they would be required to purchase supplies.[1] In addition, women who preferred to give birth outside a facility could utilize the birth kit to reduce their risk of infection during delivery. The second additional program was community dramas implemented by a professional theatre group that sought to address misperceptions in the community and knowledge around maternal and child health. The dramas were conducted quarterly, promoting themes of safe motherhood and targeting a broad audience including men, elders, and traditional leaders.

The study was a randomized controlled trial implemented in 96 rural villages/clusters in northern Nigeria between 2012 and 2016. The design included three treatment arms: the basic CORPs intervention, CORPs in conjunction with safe birth kits, and CORPs in conjunction with community drama. The evaluation sample included 7,000 women of reproductive age. Following the baseline survey, births were monitored on an ongoing basis for approximately two years and in 2016, a comprehensive endline survey was conducted. This evaluation was conducted in the state of Jigawa, an overwhelmingly rural area characterized by extremely low levels of health care utilization at baseline.

Our results show that the basic community health worker program was able to deliver services even in an area that is characterized by ongoing civil unrest, though they did not reach the majority of target beneficiaries. In our core sample of around 4500 births observed during the two-year intervention period, 24% of pregnant women in treatment communities reported having had a visit from a CORP; 10% of respondents in the birth kit arm reported receipt of a birth kit, and 36% of respondents in the community drama arm report that a drama was conducted in their community. Our findings also reveal that the community based health interventions had positive effects on a range of variables capturing care utilization and health practices, and the effects are proportionally large. We observe an increase in the utilization of any antenatal care of 6–8 percentage points relative to 64% in the control group, and an increase in the utilization of postnatal care of 3 percentage points relative to a control group mean of 7%. There is also evidence of greater intensity of use of antenatal care, and an increased probability that infants receive immunizations and check-ups in the first two months of life. There is, however, no significant effect on the probability of skilled attendance at birth, and no evidence of any detectable effects on self-reported maternal and neonatal morbidity and neonatal and infant mortality.

This paper makes several contributions. First, in the existing literature around community based health worker programs, relatively few are from sub-Saharan Africa, and even fewer from West Africa (Scott et al., 2018). Second, this paper contributes to the literature by analyzing how additional health interventions interact with a basic community health worker program, seeking to understand whether these additional interventions add value and contribute to enhanced program intensity. Finally, this evaluation provides experimental evidence around the efficacy and feasibility of the distribution of birth kits outside of the formal health system, and evidence on whether community dramas increase the take-up of formal health care services.

2 Institutional setting

This program evaluation was conducted in 96 rural communities in Jigawa state in northern Nigeria, an environment that ranks among the most challenging in the world for maternal and child health. Though Nigeria accounts for only 2% of the world's population, it accounts for 10% of worldwide maternal deaths (Hogan et al., 2010).

Jigawa is a poor, rural state with an overwhelmingly Muslim and Hausa-speaking population (EIU Canback, 2016), characterized by extremely poor health outcomes. The maternal mortality rate in Jigawa state in particular is estimated to be around 1,012 per 100,000 live births, compared to 576 per 100,000 nationwide (Sharma et al., 2017a). In the 2013 Demographic and Health Survey, Jigawa reported the third lowest rate of facility births in Nigeria; 7% of women in the state reported their most recent birth was in a health facility, compared to a national average of 36% (NPC and International, 2014). In addition, this region of Nigeria has been particularly affected by the ongoing violence linked to the Boko Haram rebellion. Between 2012 and 2016, the Armed Conflict Location and Events Dataset (ACLED) reported 23 violent events in Jigawa, of which at least six are linked to Boko Haram, and the close neighboring states of Yobe and Bauchi reported 224 and 116 violent events respectively.

A survey of 24 primary health centers serving the evaluation sample was conducted to provide additional information about the availability of formal health care services in the area, and summary statistics are reported in Table S1 in the supplementary material. Primary health centers provide basic preventive and curative care. While a majority of facilities report having access to electricity and water over the last six months, only 4% report access to a telephone, and 21% report access to an ambulance. Eight of the 24 primary health centers reported having a general doctor on staff, but no health center was staffed by an obstetrician or other specialist.

3 Intervention and empirical design

3.1 The program

This project evaluates the impact of a basic community-based health worker intervention implemented in conjunction with two additional health programs in northern Nigeria. These interventions were implemented in areas that had benefited from the Midwives Service Scheme (MSS), a program launched by the Nigerian government in 2009 to fund the deployment of midwives to rural primary health centers (Okeke et al., 2016).

In northern Nigeria, our partner organization, the Planned Parenthood Federation of Nigeria (PPFN), rolled out these three interventions to stimulate utilization of maternal health services that were newly available under the MSS.[2] The core intervention entailed the deployment of community health educators, designated community resource persons (CORPs). CORPs were defined as "a bridge/link crucial to foster trust, confidence and acceptance between the midwives and their clients and . . . increased access to maternal health services" (PPFN, 2012b). Women between the ages of 20 and 45 who were married, widowed, or divorced and possessed a minimum of primary school education were eligible to serve as CORPs. Recruitment was managed by PPFN.

CORPs were supposed to provide health information to pregnant women in a series of up to six home visits. The objective was to conduct visits at different points during pregnancy in order to monitor women's health status, encourage them to seek formal health care if needed, and promote the benefits of delivery at the primary health center. Importantly, the CORPs are not health workers and do not provide any health services (i.e., they cannot provide antenatal, delivery, or postnatal services). Moreover, qualitative data collection implemented by the research team as well as ongoing monitoring by the intervention team suggests there is no evidence that CORPs were simultaneously serving as traditional birth attendants.

At the launch of the intervention in 2012, the CORPs received a one-week training, and a small monthly stipend of 2000 naira (about $5) thereafter; the absence of any substantial financial incentive is consistent with the evidence that community health workers are not highly responsive to financial incentives (Ashraf et al., 2014). Each CORP was responsible for roughly 150 households, and given the estimated birth rate, would conduct approximately 15 household visits to pregnant women each month.

In the first treatment arm, the basic CORPs intervention was implemented in isolation, but in the other two treatment arms, it was supplemented by additional interventions. In the second treatment arm, CORPs were provided with birth kits (also known as safe delivery kits or clean delivery kits) to distribute at no cost to all pregnant women in their third trimester. The objective was for women to utilize the sterile birth kits during delivery either at home or in the health facility. The kits included a plastic sheet for the woman to lie on during delivery, surgical gloves for the birth attendant, a sterilized razor and cord clamps to cut and tie the umbilical cord, methylated spirit, clean gauze, swabs and perineal pads to be used by the mother after birth, a gallipot, a mechanical suction tube to clear secretions from the baby's airways, and a wrapper and diapers (PPFN, 2012a). All materials are packaged in a single sterile unit, and had an estimated cost per kit of $8.[3] The kits distributed in this intervention were comparable to those promoted by the World Health Organization as a promising strategy to strengthen hygienic standards for deliveries conducted at home and in low-capacity facilities (Hundley et al., 2012).

The goal of the birth kits intervention is twofold: first, utilization of the sterile materials by a birth attendant will reduce infection risk and thus reduce morbidity and mortality for mothers and infants. Second, provision of materials may increase utilization of health services by ensuring that women are confident that appropriate supplies will be available for their delivery (if they bring the kit to the facility), and by reducing the perceived risk that they will be requested to pay for materials. This second channel of increased utilization may also reduce maternal and neonatal morbidity and mortality risk.

In the third treatment arm, the basic CORPs program was implemented in conjunction with a series of community dramas conducted in order to promote the importance of safe motherhood to a broader audience. The dramas were conducted quarterly by a local drama organization called YARAC (Youth, Adolescent, Reflection and Action Center), and included a performance followed by a community forum. The objective of this intervention was to shift norms around the utilization of maternal health care, and target community members who are primary decision makers but might not be well-informed about maternal health challenges, especially men but also elders and traditional leaders. The implementing organization reported that the dramas were widely attended by both men and women in the villages, and that was also confirmed in video recordings from some sample villages.

All three interventions were targeted to address the challenges around maternal and child health observed at baseline. Given low utilization rates of health services and perceived low levels of trust between communities and facility staff, the CORPs intervention was designed to provide information about the newly available midwife services, increase knowledge about the benefits of utilizing these public health services, and increase confidence in service quality. The CORPs also sought to enhance maternal and neonatal health by providing information about recommended health practices including nutrition during pregnancy, danger signs during pregnancy, delivery and the postpartum period, and breastfeeding and infant care. The birth kits intervention was designed to provide a safer, more sterile environment for women during delivery, regardless of whether they delivered at home or in the health facility. Finally, the drama intervention aimed to change the social norms around safe motherhood among men and other community stakeholders, given the evidence that health care decision-making in this context is dominated by men.

The basic CORPs intervention and the birth kit intervention were launched in January 2013, and the drama intervention was launched in the third quarter of 2013. Data collection was conducted between 2012 and 2016. A timeline is provided in Figure 1.

3.2 Related literature

There is increasing evidence around the effects of community health worker programs, and systematic reviews of randomized controlled trials of these interventions have found mixed evidence, reflecting the wide heterogeneity in supervision and incentives provided to the health workers themselves (Lewin et al., 2010; Okwundu et al., 2013). Björkman Nyqvist et al. (2019) analyze a micro-entrepreneurship program in Uganda in which community health workers were incentivized to provide education and sell health products during home visits to households with children younger than five years old, and report a significant reduction in child mortality over a three year period. Comfort et al. (2016) and Comfort et al. (2019) analyze the effect of programs that engage community health workers in distributing pregnancy tests on utilization of both antenatal care and contraceptive services. Our paper contributes to this broad literature by analyzing the effects of community health workers in a region where utilization of formal maternal and child care is particularly low, and in a context of ongoing civil unrest, making utilization of health services even more challenging.

There is also a small evidence base around the use of safe delivery kits, an intervention widely promoted by the international health community (including the WHO) in recent years (WHO, 1996). The majority of the evaluations are small-scale and non-experimental, and focus on safe birth kits distributed via the formal health system

(Winani et al., 2007; Darmstadt et al., 2009; Hundley and Avan, 2012). This study adds to this literature by evaluating take-up and usage of safe birth kits distributed via community channels to women at their home, and in a context where delivery outside the facility is the norm.

Finally, our paper also joins a small but growing debate around the returns to utilizing formal health care for delivery. A recent global analysis estimates mortality due to low-quality care and finds that poor quality of health care is a larger contributor to excess mortality globally than low utilization of care (Kruk et al., 2018). Okeke et al. (2016) found that the expansion of the Midwives Service Scheme in Nigeria itself had no significant effect on maternal or neonatal outcomes in a quasi-experimental evaluation.[4] In this paper, we are exploring a community-based strategy designed to enhance maternal and child health: training volunteers with no formal education as community health workers in order to encourage pregnant women to utilize the existing formal health system.

3.3 Evaluation design and sample

The study was a parallel-group, stratified cluster randomized controlled trial, where randomization was conducted at the community level. The evaluation design called for the inclusion of 96 clusters (communities), with 24 clusters in each of four arms and where each community included around 500 households. The surveyed sample comprises 15% of households reporting a woman of reproductive age at baseline. Births among the sampled women are monitored continuously for a two-year period of intervention implementation, and all women are surveyed again at endline. Given this design, the evaluation was powered to detect a 25% decrease in the maternal morbidity and neonatal mortality rates with 90% power to detect an effect in a one-sided test at a ten percent significance level.[5] Ethical approval for the trial was provided by the Massachusetts Institute of Technology and the Jigawa State Operations Research Advisory Committee (ORAC).

In order to identify the sample communities, we first identified local government areas (a subdivision analogous to a district). Jigawa state has 27 local government areas (LGAs) and out of those, 24 LGAs containing a primary health center receiving services under the Midwife Services Scheme were included in the evaluation. Within each of the 24 LGAs, we identified all villages of the target size that were located in the catchment area of the MSS facility. Thereafter, a random subset of 96 communities in total were selected to be included in the evaluation sample. Sampling procedures are described in more detail in Section S1, but in brief, we selected any community in the target facilities' catchment areas that had a reported population between 500 and 850 households for inclusion in the sample. If there were more than four communities of this size, we randomly selected a subset of these communities; if there were too few communities of the target size, we identified clusters of adjacent villages and jointly treated them as a cluster.

In sum, each of the three treatment arms and the control arm include 24 clusters and approximately 1600 women in fertile age were surveyed in each arm. Figure 2 provides the trial profile. Finally, we conducted a partial census in all communities that was employed as the basis for baseline sampling.[6]

3.4 Data

The evaluation included three phases of data collection: baseline, ongoing data collection (surveys conducted 3 days and 28 days after birth in addition to a sample-wide audit of recent births), and an endline survey, as summarized in the timeline in Figure 1.[7] The baseline survey was conducted between March and June 2012 and included 7069 households with a female household member of reproductive age (corresponding to roughly 15% of the listed households). More details are provided in Panel A of Table S2 in the supplementary material. The respondent of the baseline survey was a woman in the household of reproductive age (between 15 and 49). If more than one eligible woman was present, the respondent was randomly selected utilizing an on-the-field randomization protocol.

Ongoing data collection entailed continuous monitoring of births among the baseline sample during the two-year study period. Female monitors in each community were recruited and trained to provide a cardboard chip to all baseline households and send a simple text message by SMS to the survey team following a birth or infant death in the baseline households. The SMS messages were redirected to one of our survey enumerators, who had the responsibility of following up on the SMS messages by identifying the household and conducting surveys 3 days and 28 days after birth.[8] Continuous monitoring and data collection was initiated in November 2013 and continued until November 2015. Unfortunately, this process was incomplete given that female monitors in the villages did not fully comply and failed to identify some births in the baseline sample households. More specifically, in total 1791 3-day and 28-day surveys were conducted, corresponding to 41% of all births reported for the sample during this period. The survey team also conducted an audit between January and March 2015; approximately 70% of baseline households were reached in the audit survey (4674 households), and enumerators conducted detailed surveys on 802 additional births in 802 distinct households; the majority of these surveys were for births that had not been included in the ongoing data collection.

Finally, the endline survey was conducted between February and July 2016.[9] We successfully re-surveyed 90% of the baseline sample, or 6350 households. Some households that could not be reached during the endline survey (households that had migrated, divorced etc.) had already been surveyed previously in the post-birth or audit surveys. Therefore, the total number of women observed in any follow-up data collection was slightly larger than the endline sample (6494 women, or 92% of the original sample). Anthropometric data collection was conducted in all surveys, and was implemented using SECA portable stadiometers, digital baby scales and measuring mats and MUAC measuring tapes following standard procedures (Cogill, 2003).

Given that the entire state was significantly affected by ongoing violence and instability linked to Boko Haram attacks during the evaluation period, we regard the observed pattern of attrition as low. In the analysis, we find no evidence that the correlation between baseline characteristics and a dummy for attrition differs significantly across treatment and control arms. More details on attrition are found in Section S6.

3.5 Outcomes and statistical framework

We analyze the impact of the interventions on five primary categories of outcomes: intervention exposure, utilization of maternal health care, maternal and newborn health practices, child anthropometrics, and maternal and child morbidity and mortality, as well as secondary outcomes related to knowledge and attitudes and fertility. These variables were all included in the endline survey as well as in the post-birth survey (3-day or 28-day).

We begin by analyzing patterns of household exposure to the community health educator program, and identify whether the intensity of the CORPs program was different when additional interventions — safe birth kits and community dramas — were implemented in conjunction with the basic CORPs program. In particular, we report whether women who experienced a pregnancy during the follow-up period had received any visit from a CORP member, and how many visits. For the birth kits intervention, we report whether the woman had received a kit, if she knows how to use the kit, whether she can name at least one item in the kit, and whether she reports utilizing the kit in her most recent delivery. Finally, we report whether the woman and her spouse attended the community drama.

Thereafter, we analyze the impacts of the intervention on health service utilization for maternal health care. Variables of interest capturing the utilization of maternal health care include the respondent's reported antenatal care visits, the number of visits, an index of antenatal care quality, and whether the woman received a postpartum check-up within two months. For delivery care, variables of interest include whether the program impacted facility-based delivery or skilled attendance at birth. We also analyze intervention effects for maternal and newborn health practices: whether the respondent developed a birth plan, breastfeeding practices, and immunizations and infant check-ups. We further analyze an index of maternal morbidity (during pregnancy, delivery, and postpartum), an index of neonatal morbidity and infant and neonatal mortality. Finally, we analyze child health outcomes as captured by height-for-age, weight-for-age, and MUAC-for-length, measured at endline for all children under five in households reporting a birth during the intervention period. In addition to these five primary outcome categories, we also analyze a set of additional secondary outcomes capturing knowledge and attitudes and fertility.

3.5.1 Sample for primary analysis

The sample for the analysis of the primary outcomes linked to maternal and newborn health is restricted to the subset of women who reported a birth (live birth or stillbirth) during the intervention period, or women who were pregnant at endline. It is important to note that women who had not been pregnant during the intervention period do not report any pregnancy or delivery-related outcomes, and are thus not included in the analysis.[10] Specifically, 4420 births corresponding to 4290 women are observed in the intervention period; thus 61% of the original sample reported at least one birth.

Selection into this subsample is potentially a source of bias if the intervention affected choices around fertility or birth timing. Given the relatively short evaluation timeline, the vast majority of women delivered only once, and thus there is very limited scope to observe any effect on timing following the first birth. Only 130 women, or 2%, reported two births during the period. Moreover, since CORPs did not interact with women until they were pregnant, there is similarly very little scope for an effect on birth timing for the first birth during the evaluation period.[11]

3.5.2 Econometric specification

The primary specification used to estimate the effect of the interventions on an outcome variable X_{bicg} for birth b observed for respondent i in cluster c in LGA g can be written as follows:

$$X_{bicg} = \beta_1 CORPS_{cg} + \beta_2 Kits_{cg} + \beta_3 Drama_{cg} + \chi_{icg}\gamma + \mu_g + \epsilon_{bicg} \quad (1)$$

$CORPS_{cg}$, $Kits_{cg}$ and $Drama_{cg}$ are indicator variables denoting treatment assignment, and χ_{icg} is a vector of baseline control variables.[12] All regressions include LGA fixed effects μ_g, and standard errors clustered at the cluster level.

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

In addition, we report at the bottom of each table tests of equality across β_1, β_2, and β_3. In addition, we estimate a separate regression including a dummy variable for assignment to the basic CORPs only arm and a dummy variable for assignment to any "enhanced intervention" arm (CORPs and birth kits, or CORPs and dramas), and report a test $\beta_1 = \beta_{enhanced}$, where $\beta_{enhanced}$ denotes the effects of the enhanced interventions. This test allows us to identify whether the effect of the arms including enhanced interventions is significantly larger. Most important, we report a joint test of the impact of any intervention $\beta_1 = \beta_2 = \beta_3 = 0$ to evaluate whether any intervention including the CORPs program had a statistically significant effect.

4 Results

4.1 Balance at baseline

Tables S3 and S4 in the on-line supplementary material present mean pre-treatment characteristics for the treatment (CORPs only, CORPs and birth kits, and CORPs and community dramas) arms as well as the control arm. We report in the tables the p-value on the joint test $\beta_1 = \beta_2 = \beta_3 = 0$. The average respondent is age 28 at baseline, with a reported age at marriage of 15; around a third live in polygamous households, and only about 17% of the respondents report ever attending school. The baseline values of the main outcome variables of interest are reported for the subset of 4007 women who reported having had a birth during the two years preceding the baseline survey. At baseline, around 65% of women report having utilized antenatal care in their most recent pregnancy, but only 9% of women reported their most recent delivery was in a health facility.

In the last row of Table S4, we report the p-value corresponding to the joint F-test of the hypothesis $\beta_1 = \beta_2 = \beta_3$, testing across all variables examined in both tables of baseline characteristics. From this joint test we can conclude that there are no significant differences in observable characteristics in aggregate when comparing across treatment arms.

4.2 Intervention exposure, care utilization and health practices

We first analyze the evidence around pregnant women's exposure to the interventions, patterns of utilization of maternal health services, and health practices. Table 1 reports the results of estimating equation (1) for variables capturing intervention exposure. Pregnant women in all three treatment arms were significantly more likely to report a visit by a CORP during their pregnancies compared to pregnant women in the control group, despite the fact that we observe some spillovers in the control arm.[13] Intervention coverage was 37% higher in the CORPs-only communities compared to the control group, when coverage is defined as the percentage of women reporting a birth who had a visit from a CORPs.

We find even higher coverage in the treatment arms that had the CORPs program implemented in conjunction with an additional health program: a 12.6 percentage increase in birth kits communities and a 10 percentage point increase in drama communities. The difference in program coverage between the basic CORPS program and the enhanced CORPs program is statistically significant at the one percent level, suggesting that CORPs seem to have been more motivated to identify and visit pregnant women when other interventions entailing provision of health inputs or community education were also implemented. On average in the three treatment arms, 22% of women reporting a birth interacted with a CORP during the program period, and the difference between the treatment and the control arms is statistically significant. Conditional on reporting a CORPs visit, the average respondent reported 1.6 visits in total. Within the sample of women who reported a CORP visit, the most common topics reported discussed were the importance of antenatal care (70%) and healthy pregnancy practices (69%), followed by the importance of delivering at a clinic (63%).

Next, Column (3) in Table 1 reports results for the birth kit arm. Here, we observe that among the 28% of women who reported they received a visit from a CORP, only 8.8% reported they received a birth kit. When asked specific knowledge questions about the birth kits, 7% of respondents stated they knew how to use the kit and 8% could name at least one item included in the kit. Clearly, the birth kit intervention was not as inclusive as intended, as only a small fraction of pregnant women received and used the kits, and the reason for this pattern is unclear.

By contrast, the drama intervention was more successful in achieving coverage, as reported in Columns (7) and (8) in Table 1. We find that 36% of respondents in the drama arm state that events were conducted in their community, and 27% state that they attended. Among respondents who attended a drama, 32% report their husbands also attended, and 51% report that they discussed the content with a relative or friend. Conditional on attending at least one drama, the average number attended was 1.6, and the effect is statistically significant at the one percent level. We also observe some spillovers of the drama intervention in the other arms, as 8-9% of the respondents in the control arm and 19% of women in the birth kits arm report exposure to a drama in their community.

Summing up, it is clear that the CORPs program did provide services to beneficiaries, but did not reach the majority of pregnant women. However, the intervention was more effective when it was accompanied by additional interventions (birth kits or dramas). Supplementary data from a qualitative analysis suggests that CORPs encountered hostility from women and particularly their husbands if they made household visits without the provision of birth kits or other hoped-for incentives or material benefits (Sharma et al., 2020). Accordingly, one hypothesis that would be consistent with the observed pattern is that CORPs are more active in the birth kits and drama arms because the additional services provided rendered beneficiaries more receptive to engagement. This is also consistent with existing literature suggesting non-financial incentives are highly effective in motivating health workers (Ashraf et al., 2014; Deserranno, 2019).

4.2.1 Utilization of maternal health care

Table 2 reports results analyzing women's utilization of maternal health care services during pregnancy. For antenatal care, we observe in Columns (1) and (2) an increase in the probability of using antenatal care in the birth kit and drama treatment arms of 6.3 to 8.6 percentage points that is statistically significant. Relative to the control arm, these effects correspond to a 11% increase. In the same treatment groups, we also observe an increase in the number of antenatal care visits of around 10% that is also statistically significant. In order to capture the quality of antenatal care, we construct an index, and the estimated coefficients reported in Column (3) suggest there was also an increase in the quality of antenatal care in the birth kits and drama arms.[14] In addition, Column (4) reports an increase in the probability of utilizing postnatal care of around 2–3 percentage points in all treatment arms, though this coefficient is significant only for the basic CORPs and drama arms. Given the control mean of 7%, this is a proportionally large effect of 45%. Column (5) reports the average standardized treatment effects for these four outcome variables. Here, we observe a 0.12 standard deviation increase in utilization of antenatal and postnatal care, statistically significant for the birth kits and drama treatment arms. The enhanced interventions (where the CORPs program is implemented in conjunction with another health program) had larger impacts on antenatal and postnatal care compared to the treatment arm with the basic CORPs program, and the difference is statistically significant at the one percent level.

In Columns (6) through (8), we report treatment effects for the probability of a facility delivery, skilled attendance at birth, or the probability of delivering with another person present. Here, we find a uniformly null effect.[15] In Column (9), we report the average standardized effect for the outcomes capturing utilization of maternal health services (i.e. Columns 1-4 and 6-8); the estimated coefficients suggest there was an increase in care utilization of around 0.07 standard deviations that is statistically significant in the birth kits arm and drama treatment arm. Again, the health educator program was more effective when implemented in tandem with other health interventions.

The joint tests of the effects of any intervention reported at the base of the table reinforce this evidence. The effect of any CORPs intervention is significant and positive for the three antenatal care variables and narrowly insignificant ($p = .110$) for postnatal care. The hypothesis that the average standardized effect across any intervention is equal to zero can be rejected at the one percent level for non-delivery care utilization measures, and at the five percent level for the full set of utilization measures including facility births.

4.2.2 Maternal and newborn practices

A second objective of the CORPs program was to enhance health practices, and Table 3 reports the estimated effects of the intervention on these outcomes. Two of the CORPs' specific learning objectives included encouraging pregnant women to develop a birth plan and providing information about the importance of breastfeeding. In Column (1), we find evidence of a significant and positive effect on the probability the respondent reported a birth plan in all treatment arms (an increase of between 21 and 40 percent relative to the control mean of 11%). We do not find that the intervention had any impact on breastfeeding behavior.

We also find a significant increase in the number of immunizations administered to newborns in the first month of life in the birth kit and drama arms (an additional 0.1 immunization administered on average, relative to a control mean of one), and a significant increase in the probability of an newborn check-up in the first month (an increase of around 12 percentage points, relative to the control mean of 29%). These are proportionally large effects, suggesting an increase of roughly 40% in infant check-ups. Again, we report the average standardized treatment effect for these variables in Column (7), and observe that there is an enhancement in maternal and newborn health practices of roughly 0.06 standard deviations compared to the control group, and this impact is significant in all three treatment arms.[16] The joint tests of the effects of any intervention reported at the bottom of the table suggest that any CORPs intervention had a significant and positive effect on the utilization of birth kits and postnatal infant check-ups, and the average standard treatment effect for any intervention is similarly positive and significant at the five percent level.

4.3 Maternal and child morbidity, mortality and anthropometrics

The interventions did not seem to have any effect on self-reported measures of maternal morbidity during pregnancy, delivery, and the post-partum period or neonatal morbidity, as reported in Table S5 in the supplementary material. We do observe a decline in maternal morbidity in the birth kits arm, and this pattern would be consistent with some limited use of the birth kit yielding benefits in terms of maternal health.[17] However, the average standardized effect for the morbidity indices reported in Column (3) shows no significant effects. We also fail to identify any significant impact on the probability of stillbirths and infant and neonatal mortality; the mortality rates are calculated at the cluster level, yielding a sample of 96 observations.

We also evaluate the effects of the interventions on the anthropometric status of all children under five for respondents reporting a birth in the intervention period.[18] The final sample thus includes 5332 children with data on anthropometric measures reported at endline. In Table 4, we report the results of estimating equation (1) for height-for-age, weight-for-age, and mid-upper-arm-circumference-for-age. Here, we find evidence of a somewhat heterogenous pattern across indicators, but the standardized effects reported in Column (5) are generally insignificant. The reports for the subsample of children under one are reported in the on-line supplementary material, Table S6.[19]

4.4 Knowledge and attitudes

In addition to the primary outcomes enumerated above, we analyze a set of additional secondary outcomes: knowledge and attitudes around facility delivery care, and knowledge about infant care and fertility. The results of estimating equation (1) for these variables are reported in Table 5.[20] We find that the interventions increased knowledge of relative risk and pregnancy complications and enhanced attitudes toward health facility use. The average standardized effect for the attitudinal variables, reported in Column (6), shows increases of around 0.05 standard deviations that are statistically significant in the birth kit and drama treatment arms. Results for the subsample of respondents reporting a birth during the intervention period are reported in the on-line supplementary material, Table S7, and are broadly similar.

We also analyze whether the intervention generated any effects on fertility. The results are reported in Table S8 in the on-line supplementary material and the coefficients of interest are small in magnitude and insignificant, consistent with the hypothesis that the intervention did not generate any differential patterns of selection into the subsample of women reporting a birth.

4.5 Treatment on the treated

To evaluate the effects of the interventions on women who did in fact receive the interventions, we estimate treatment on the treated specifications. We focus on the CORPs intervention given that this intervention was implemented across all three treatment arms.

The specification of interest is as follows:

$$X_{icg} = \beta_1 CORPsDummy_{cg} + \chi_{icg}\gamma + \mu_g + \epsilon_{icg} \quad (2)$$

where χ_{icg} denotes the same vector of control variables utilized in the intent-to-treat estimates, and the dummy variable for exposure to the CORPS intervention is instrumented by a dummy variable for assignment to any treatment arm.[21]

The results of two-stage least squares estimation for care utilization, health practices, and attitudes and knowledge — the primary outcomes for which significant ITT estimates are observed — are reported in Table S9 in the on-line supplementary material.[22] The estimated treatment on the treated coefficients are large in magnitude; the probability of accessing antenatal care services increases by more than 50 percentage points, and the probability of a postnatal care visit increases by 32 percentage points. These results provides suggestive evidence that a community health worker program that was more aggressively implemented could have very large effects on care utilization even in an extremely low-resource setting.

5 Conclusion

Over the last twenty years, progress in reducing maternal and neonatal deaths in developing countries has largely stagnated, particularly in sub-Saharan Africa. Accordingly, identifying interventions that can increase utilization of maternal health services and enhance health outcomes, particularly in challenging regions characterized by extremely low baseline human capital outcomes and persistent violence, is a key priority for policymakers and researchers.

This evaluation adds to the body of evidence around community-level interventions designed to improve maternal and child health in low resource settings. The results suggest that the interventions reached approximately 22% of women reporting a birth during this period, and generated increased utilization of antenatal care, postnatal care, immunizations, and newborn check-ups in communities exposed to the intervention. We also find positive shifts in attitudes around facility-based delivery care. The community health program was most effective when implemented in conjunction with additional health programs — the birth kits and the drama interventions.

However, the increases in service utilization and health practices did not yield any detectable improvements in anthropometric outcomes for infants or young children or any decreases in maternal or neonatal morbidity or mortality. The persistently low quality of health services available through the MSS may have limited both the intervention's effectiveness in increasing utilization of services and its effect on health outcomes. Previous literature about the MSS did not identify any positive effect of the deployment of these services on health outcomes, and found significant quality challenges (Okeke et al., 2016). Additional qualitative research conducted as part of this project similarly identified the absence of health providers (particularly female health providers), the absence of drugs and equipment, and unexpected facility closures as significant barriers to the use of facilities (Sharma et al., 2017b, 2019).

Our results add to an existing evidence base suggesting that while community health worker programs can be effective in enhancing health outcomes in developing countries, there is huge heterogeneity in their effects (Scott et al., 2018; Lewin et al., 2010; Okwundu et al., 2013). It is important to note that the evaluation has limitations, and questions linked to external validity of the intervention for other contexts must be carefully considered. This trial was conducted in one state in northern Nigeria characterized by very low utilization of care and poor human capital outcomes ex ante; in addition, ongoing patterns of conflict, described in more detail above, contributed to very limited penetration of the CORPs program and associated interventions in the study communities. In other, more stable contexts, interventions such as the CORPs program may be able to achieve higher penetration rates in communities and more effectively reach target women. On the other hand, receptiveness to information provided by actors such as the CORPs may vary unpredictably in different communities, and if supply-side barriers are salient, demand-side interventions may not shift utilization of maternal health care significantly.

Further research, both qualitative and quantitative, may productively explore the conditions under which community health worker programs can effectively shift maternal and neonatal health outcomes. Ultimately, the observed pattern here is consistent with the hypothesis that demand-side interventions such as community health educator programs can effectively stimulate increased utilization and change health behaviors in a challenging context such as Jigawa state.

Notes

[1]In practice, MSS-served facilities were in fact characterized by frequent stock-outs (Okeke et al., 2016).

[2]Founded more than 25 years ago, PPFN is now one of the oldest indigenous organizations in Nigeria offering sexual and reproductive health services; however, the interventions proposed as part of this evaluation were new to the organization.

[3]The kits were identical to those available to midwives working in the MSS primary health care centers.

[4]Chari and Okeke (2014) conclude that a policy-induced shock to the supply of institutional deliveries did not have a significant effect in reducing newborn mortality in Rwanda. Evidence from Malawi suggests a government ban on the use of traditional birth attendants did result in a significant increase in utilization of formal sector care, but no overall decline in newborn deaths (Godlonton and Okeke, 2016).

[5]Power calculations are conducted following Hayes and Bennett (1999). This is assuming a baseline maternal morbidity rate of 35%, and a baseline infant mortality rate of 47 deaths per 1000 births. In addition, the design assumed a birth rate of 46 per 1000 population, and accordingly 21 sampled births observed per year per community of 3000 individuals given that 15% of households are sampled. The coefficient of variation between clusters k was assumed to be .2.

[6]The trial was registered with the American Economic Association registry (Leight et al., 2016), and registered at clinicaltrials.gov. The protocol number is NCT01487707.

[7]All respondents provided informed written consent, and all data was collected by electronically by trained same-sex enumerators using ODK software.

[8]In the event an enumerator became aware of a birth more than three days after the birth, she was

still instructed to conduct the three-day survey as soon as possible, and then return for the 28-day survey. In the event she became aware of a birth more than 28 days after birth, she was instructed to conduct the survey up to three months after birth.

[9]Some additional intensive data collection targeted to minimize attrition continued until October 2016.

[10]No systematic data was collected on miscarriages, other than one question posed at the endline as to whether the respondent experienced at least one miscarriage in the preceding two years.

[11]More evidence around the hypothesis that there is no selection into the subsample of women reporting births can be found in section S6 in the supplementary material.

[12]The control variables employed include all those reported in the balance tests in Panel A of Table S3: a dummy variable for whether the respondent is married, the number of co-wives, age at marriage, the number of marriages reported, age, a dummy for whether the respondent has ever attended school, a dummy variable for whether the respondent reads Hausa, a dummy variable for Muslim, current birth parity, and a wealth index. We also include dummy variables equal to one if the respondent is observed in the 3-day and 28-day surveys, and in the audit survey.

[13]There is no evidence that spillovers are higher in control communities that are geographically more proximate to treatment communities.

[14]The antenatal care index is equal to the mean of indicator variables for receiving important components of antenatal care: utilizing care in the first trimester, receiving more than half of available ANC services, receiving iron folic pills and the tetanus vaccine, and receiving advice on danger signs during pregnancy. The observed intervention effect is largely driven by increases in the receipt of iron pills and tetanus vaccines, as well as reported counseling about pregnancy danger signs.

[15]In practice, facility delivery and skilled attendance at birth are almost equivalent, given that health personnel generally do not attend home births in this region.

[16]Given that the effect is observed in all three treatment arms, it is plausible to hypothesize that it primarily reflects the effects of the CORPs, rather than the ancillary interventions.

[17]It should be noted that the sample for neonatal morbidity is restricted, given that this information was reported only in the 28-day survey. We also collected limited data on miscarriages at endline, and find no significant effect of the interventions on the rate of miscarriage, defined as the loss of a pregnancy in the first two trimesters. Data on induced abortions was not collected, but this could be another potential channel for selection in the sample of households reporting a birth.

[18]Endline anthropometric data is missing for children corresponding to births observed only in ongoing surveys (133 observations), as well as for an additional 650 observations; for the latter subsample, the adult respondent was surveyed in the endline, but the enumerator assigned to follow up with a separate

anthropometric survey did not locate the household, the respondent declined to provide consent for measurement, or the child was not available.

[19]It is important to emphasize that the results should be interpreted cautiously given that the children observed in the anthropometric data are drawn from a subsample of respondents. However, we present evidence in Section S6 that respondents observed only in the endline and those observed in ongoing surveys are not characterized by significant differences in observable characteristics, and thus we cannot reject the hypothesis that the respondents observed in the anthropometric data constitute a random subsample.

[20]All the indices are coded such that a higher value indicates more knowledge or more positive attitudes.

[21]In the first stage, the coefficient on the treated dummy is .101, significant at the one-percent level.

[22]Estimating the treatment on the treated specifications for the outcomes reported in Table S5 shows null effects. Limited power is available to estimate treatment on the treated for the anthropometric variables given the reduced sample.

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Table 1: Intervention exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any CORPs visit	Number CORPs visits	Kit received	Kit knowledge	Name an object	Kit used	Dramas conducted	Dramas attended
CORPS only	.048***	.115***	.005	.003	.005	.007	.003	.011
	(.018)	(.044)	(.008)	(.006)	(.007)	(.006)	(.018)	(.017)
CORPs + Birth kits	.126***	.188***	.088***	.066***	.078***	.052***	.096***	.082***
	(.022)	(.046)	(.018)	(.012)	(.016)	(.012)	(.023)	(.021)
CORPs + Dramas	.100***	.152***	.002	.005	-.003	-.003	.250***	.209***
	(.018)	(.030)	(.010)	(.007)	(.009)	(.007)	(.026)	(.025)
Test $\beta_1 = \beta_2$	.000	.145	.000	.000	.000	.000	.000	.001
Test $\beta_1 = \beta_3$	.006	.328	.717	.84	.313	.131	.000	.000
Test $\beta_2 = \beta_3$	.214	.373	.000	.000	.000	.000	.000	.000
Test $\beta_1 = \beta_{enhanced}$	.000	.162	.001	.000	.003	.023	.000	.000
Joint test: any intervention	.000	.000	.000	.000	.000	.000	.000	.000
Control mean	.13	.16	.01	.01	0	0	.09	.05
Obs.	4420	4420	4420	4420	4420	4420	4420	4420

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The sample includes all respondents reporting a pregnancy during the intervention period who were interviewed in at least one follow-up survey. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic arms and the arms including additional interventions (drama and birth kits) $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table 2: Utilization of maternal health care

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any ANC visits	Number ANC visits	ANC quality index	Postnatal care	Avg. std. effect Cols. 1-4	Facility birth	Skilled attendance	Birth accompanied	Avg. std. effect Cols. 6-8
CORPS only	.006	-.100	.002	.023*	.018	-.027	-.009	.010	-.001
	(.036)	(.168)	(.024)	(.013)	(.055)	(.021)	(.018)	(.023)	(.039)
CORPs + Birth kits	.063*	.281*	.053**	.026	.125	-.003	.018	-.003	.077
	(.034)	(.160)	(.021)	(.017)	(.050)**	(.024)	(.021)	(.022)	(.038)**
CORPs + Dramas	.086***	.352**	.059***	.033**	.154	-.026	.005	-.029	.072
	(.033)	(.171)	(.021)	(.013)	(.052)***	(.020)	(.019)	(.027)	(.037)**
Test $\beta_1 = \beta_2$	.084	.015	.016	.834	.036	.328	.213	.606	.047
Test $\beta_1 = \beta_3$	.017	.009	.01	.383	.012	.951	.453	.184	.059
Test $\beta_2 = \beta_3$	.461	.67	.753	.676	.552	.339	.56	.341	.896
Test $\beta_1 = \beta_{enhanced}$	.021	.004	.006	.553	.009	.491	.218	.271	.026
Joint test: any intervention $\beta_1 = \beta_2 = \beta_3 = 0$	.019	.016	.004	.110	.004	.45	.644	.606	.047
Control mean	.63	2.66	.38	.07		.18	.12	.59	
Obs.	4420	4420	4420	3684		4009	3649	3649	

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The sample includes all respondents reporting a pregnancy during the intervention period who were interviewed in at least one follow-up survey and who report the indicator of interest. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table 3: Maternal and newborn health practices

	(1) Birth plan	(2) Breast-feeding	(3) Breastfeeding exclusively	(4) Breastfeeding duration	(5) Immuni-zations	(6) Infant check-up	(7) Avg. std. effect
CORPS only	.046***	-.028	.016	-1.283	.015	.117***	.056**
	(.017)	(.018)	(.014)	(1.727)	(.064)	(.041)	(.027)
CORPs + Birth kits	.023	-.005	.004	-1.657	.091*	.119***	.067**
	(.014)	(.016)	(.018)	(1.854)	(.052)	(.044)	(.027)
CORPs + Dramas	.045***	-.014	.022	.006	.131*	.054	.073**
	(.014)	(.017)	(.017)	(1.672)	(.070)	(.042)	(.030)
Test $\beta_1 = \beta_2$	.17	.143	.458	.818	.234	.965	.670
Test $\beta_1 = \beta_3$	.957	.403	.697	.375	.133	.097	.568
Test $\beta_2 = \beta_3$	.111	.579	.37	.323	.523	.100	.839
Test $\beta_1 = \beta_{enhanced}$	.418	.197	.788	.746	.137	.415	.538
Joint test: any intervention	.005	.398	.516	.679	.176	.013	.044
Control mean	.11	.89	.58	14.22	1.03	.29	
Obs.	4152	3051	3516	2803	1385	1317	

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The sample includes all respondents reporting a pregnancy during the intervention period who were interviewed in at least one follow-up survey and who report the indicator of interest. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table 4: Child anthropometrics: Children under five

	(1) Height-for-age	(2) Weight-for-age	(3) MUAC-for-age	(4) Avg. std. effect
CORPS only	.230**	.008	-.024	.038
	(.097)	(.056)	(.043)	(.031)
CORPs + Birth kits	.228***	.003	.020	.058
	(.085)	(.059)	(.046)	(.028)**
CORPs + Dramas	-.022	-.120*	-.062	.035
	(.095)	(.063)	(.049)	(.033)
Test $\beta_1 = \beta_2$	.979	.853	.366	.727
Test $\beta_1 = \beta_3$	.013	.029	.422	.027
Test $\beta_2 = \beta_3$	.004	.047	.113	.014
Test $\beta_1 = \beta_{enhanced}$	.061	.296	.819	.275
Joint test: any intervention	.004	.121	.414	.070
Control mean	-1.54	-1.19	-.75	
Obs.	5332	5332	5332	5040

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The sample includes children of respondents born in the intervention period who have anthropometric measurements collected at endline. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table 5: Health knowledge and attitudes

	Delivery preference	Knowledge relative risk	Knowledge compli-cations	Infant. care know.	Attitudes toward facility	Avg. std. effect
	(1)	(2)	(3)	(4)	(5)	(6)
CORPS only	.023	-.006	.006	.001	.026**	.025
	(.014)	(.018)	(.007)	(.010)	(.011)	(.035)
CORPs + Birth kits	-.00003	.032**	.008	.002	.022**	.035**
	(.011)	(.013)	(.006)	(.010)	(.009)	(.024)
CORPs + Dramas	-.004	.026	.011*	-.004	.014	.049*
	(.012)	(.016)	(.006)	(.009)	(.011)	(.029)*
Test $\beta_1 = \beta_2$	.204	.019	.749	.949	.723	.305
Test $\beta_1 = \beta_3$	.06	.089	.432	.582	.41	.501
Test $\beta_2 = \beta_3$	.417	.638	.567	.528	.489	.711
Test $\beta_1 = \beta_{enhanced}$	.087	.030	.538	.804	.525	.357
Joint test: Any intervention	.305	.026	.299	.917	.040	.103
Control mean	.75	.86	.62	.55	.45	
Obs.	6350	6350	6350	6350	1393	

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Figure 1: Timeline

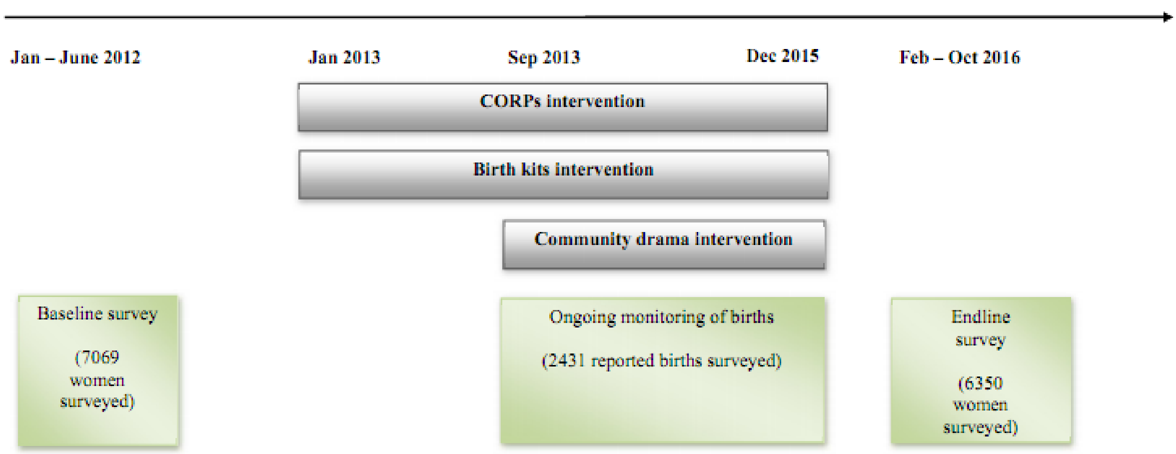
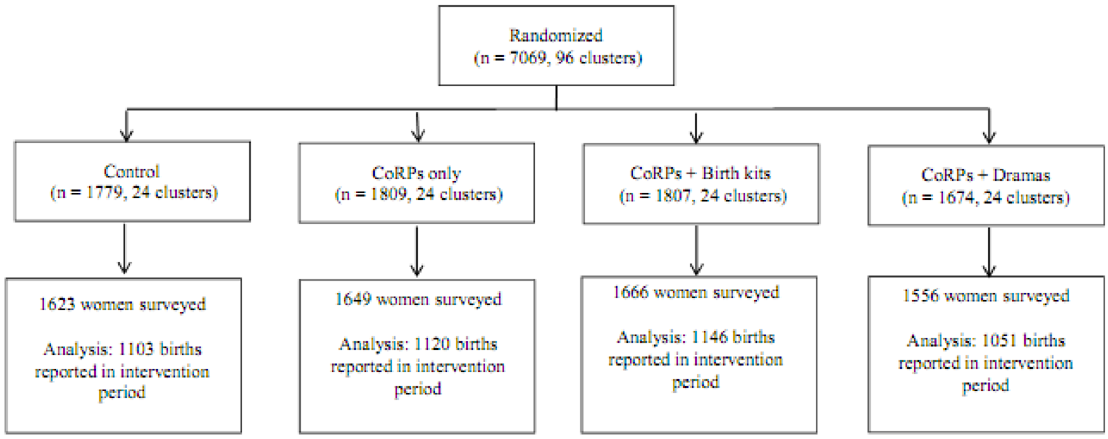


Figure 2: Trial profile



Supplementary material

S1 Sampling strategies

In order to identify the sample communities, we utilized the following strategy. First, we selected the sample local government areas and associated primary health centers. The local government areas of Gumel, Hadejia, and Jahun did not receive any MSS-funded services; anecdotally, this primarily reflects the fact that these three LGAs included larger population centers that were already served by well-equipped health facilities or hospitals. Accordingly, these three LGAs were excluded from the evaluation. In the remaining 24 LGAs, there were 35 facilities included in the MSS as of 2012; in those LGAs that had more than one MSS-served PHC, the PHC that had a larger number of communities of the target size in the catchment area was selected. Effectively, this strategy identified the health center located in a more densely populated area.

Second, we defined a catchment area around each PHC. Health authorities at the federal or state level had no systematic enumeration of the catchment areas of the health facilities of interest. While some PHCs did informally define their own catchment areas, not all facilities consistently provided this information. Accordingly, we imposed our own criteria informed by some health centers' estimate of their own catchment areas, using a radius of 20 kilometers from the PHC's location (calculated employing a straight-line distance).[23] This was the minimum radius that would allow us to identify sufficient sample communities of the target size.

Third, we selected any community in the defined catchment area that had a reported population between 500 and 850 households for inclusion in the sample. If there were more than four communities of this size, we randomly selected a subset of these communities. In the majority of LGAs, however, we could not identify four communities of the target size. In these cases, we sought to identify clusters of adjacent villages that could be jointly treated as a unit. The sampling procedure then entailed identifying 'anchor villages' having a population of at least 150 households, and evaluating whether there were additional villages within 3 kilometers of the anchor village. The objective was to identify a cluster utilizing the smallest number of communities possible: accordingly, we first identified clusters including two separate villages, then three, and then four. If there were more than the target number of clusters within a certain subset, we randomly selected clusters for inclusion. Given that our sampling units frequently included more than one distinct village, we adopted the term 'clusters' to describe them. The final sample included 30 clusters (31% of the total) that were comprised of one village; 47 (49%) that were comprised of two separate but adjacent villages; 14 (15%) that were comprised of three separate villages; and 5 (5%) that were comprised of four villages.

Randomization of the sampled clusters into the four experimental arms was conducted by the research team using Stata.

Following the identification of the 96 target clusters, we conducted a partial census in the sampled communities to verify the population estimates provided by the NPC, and to generate a household roster that could be the basis of sampling for the baseline survey. In order to conduct the partial census, the survey team worked with traditional leaders in each village (known as lambas, bulamas, or mai' angwans) to identify key informants knowledgeable about the population, typically assistants or relatives of the village chief. Leaders also assisted in subdividing the community into non-overlapping and easily recognizable areas to facilitate the partial census; enumerators then worked in pairs to walk on foot through their assigned area in conjunction with informants. As they walked, they created a map and a list of households, identifying each house and asking the informant to provide the name of the household head and the names and approximate ages of his wives, if applicable.

Following the conclusion of the partial census, the lists of households in each cluster were digitized. The sampled households for the baseline were then identified as follows: first, all households that informants had designated as including only single men or elderly couples were dropped. Second, we selected a simple random sample of 15% of households including women of reproductive age (15-49) as target households for the baseline; an additional 3.75% of households were selected to serve as substitutes.

During the partial census, the number of households identified in our census often fluctuated substantially relative to the number postulated by the NPC. While on average the enumerated population was around 90% of the projected population, in some LGAs this was substantially lower (as low as 50% or 75%), and in some much higher (as high as 150%). These discrepancies presumably primarily reflect the fact that the census data was collected six years prior to the start of the evaluation. When the population identified was so low as to render a cluster ineligible for inclusion in the evaluation, two strategies were employed. The first strategy was to expand the cluster by including additional, adjacent settlements that had not originally been identified for inclusion. If this was infeasible, however, the cluster would be dropped and replaced with another cluster that had previously been identified as eligible, but had not been randomly selected to be part of the evaluation sample.

Some variation across LGAs in the average cluster size, and thus in the number of households sampled in each cluster, could not be eliminated. The total number of households in the four sample clusters in each LGA, and the total number of households sampled for the baseline survey within those clusters, is reported in Panel A of Table S2. While the experimental design estimated that the total population of the four sampled clusters in each LGA would be 2000 households, in practice this number varied between 1600 and 2400.

S2 Pregnancies observed in each survey

The evaluation design called for each pregnancy in the baseline sample to be observed in both immediate post-birth surveys (conducted 3 days and 28 days after birth), as well as the endline survey. However, the monitoring and follow-up system designed to identify pregnancies on an ongoing basis did not identify all pregnancies. This was primarily due to poor performance by monitors and enumerators, who could not be directly supervised due to their dispersion over a wide geographic area. Frequent absences by both monitors and enumerators due to travel, pregnancy, or illness rendered prompt follow-up impossible, and in some of the most remote LGAs, recruiting sufficient (female) staff with adequate skills was challenging.

During the two-year period of continuous data collection, 55% of the pregnancies were thus observed in at least one of the ongoing surveys (3-day or 28-day survey or the audit). There are 1989 pregnancies, or 45% of the total sample, observed only in the endline.[24] This information is also summarized in Panel B of Table S2.

S3 Selection into women reporting births

We provide two sources of evidence consistent with the hypothesis that there is no selection into the subsample of women reporting births. First, we analyze whether there is a treatment effect on fertility, and find no evidence that the treatment shifted the probability that a respondent reports giving birth, or the number of births. These results are reported in Table S8. Second, we evaluate baseline balance in the subsample of respondents who report a birth. These results are reported in Tables S10 and S11, and also here we find no evidence of imbalance across arms in this subsample. We conclude that bias is not introduced by evaluating the sample of women who have given birth or are currently pregnant.[25]

Within the sample of 4420 births, 55% were observed in both a post-birth survey (conducted 3 and 28 days after birth) as well as in the endline survey. However, in 45% of cases, one survey was missing. For the births for which one survey is missing, some outcomes of interest are not reported. In addition, 411 of the pregnancies reported had not ended in a live birth at the point of the endline (either the woman was pregnant at the time of the endline survey, or she had suffered a stillbirth). For these cases, data on labor and delivery (for contemporaneous pregnancies), postnatal morbidity, anthropometrics, and newborn care is correspondingly unavailable. Our analysis shows that baseline characteristics do not predict patterns of selection into the different surveys, and we therefore conclude that there is no bias introduced by missing data.[26] Further details are provided in the next two sections.

S4 Missing data

The objective of this section is to provide a brief overview of patterns of missing data and the reasons for these patterns in the primary results. First, intervention exposure (as reported in Table 1 is reported for all pregnancies observed during the intervention period, a sample of 4420 births.

For care utilization variables, as reported in Table 2, variables for utilization of antenatal care are reported for all pregnancies. Care conditional on complications during pregnancy (Column (4)) is reported for the subsample of 1571 women who did report a complication during pregnancy. Delivering in a facility (Column (5)) is reported for the full sample of pregnancies that had concluded in a live birth prior to the endline, excluding the 411 pregnancies corresponding to stillbirths and pregnancies ongoing at endline. Dummy variables for postnatal care, skilled attendance at birth, delivering accompanied by another individual, and postnatal care (in Columns (5), (8) and (9)) are available for a slightly reduced sample, given that these variables were not reported in the 28-day survey or the audit.

For health practice variables, as reported in Table 3, the sample is limited for various reasons. Data in Columns (1) and (2) is missing for pregnancies reported in the audit, as well as for some pregnancies recorded only in the endline due to non-response. Data in Columns (3) and (4) is available in the 3-day survey and the endline, while data in Column (5) is reported only in the endline. Some respondents also stated that they could not recall their breastfeeding decisions at endline, and this response was coded as missing. Data in Columns (6) and (7) is available only in the 28-day survey.

For variables capturing maternal and neonatal morbidity, as reported in Table S5, variables capturing maternal morbidity and stillbirth are reported for all pregnancies in the sample. However, the variable capturing neonatal morbidity (reported in Column (4)) corresponds to data reported only in the 28-day survey, and is thus available for a reduced sample.

For variables capturing child anthropometrics, as reported in Table 4, the sample is restricted to households included in anthropometric data collection at endline reporting a child born in the intervention period.

For variables capturing additional outcomes around attitudes, knowledge, and fertility, as reported in Table 5, data availability is as follows. The variables reported in Columns (1) through (4) capturing knowledge and attitudes were reported in the endline, and are available for the 6350 women observed in the endline. Attitudes toward the health facility (Column (5)) was collected in the 28-day survey, and is available for the reduced sample of women included in that survey. Fertility variables as reported in Columns (6) and (7) are available for all 6494 women observed in follow-up data collection.

S5 Variables reported vis-a-vis pre-analysis plan

Prior to initiating the analysis, the outcome measures of interest were identified and registered in a pre-analysis plan (Leight et al., 2016). There are four variables included in the analysis plan that were not included in the analysis here. Birth weight can be analyzed only for the subsample of births where the newborn was weighed within 72 hours given that this was ultimately a small subsample, we do not report these results, though we will note their consistency with other anthropometric results below. The maternal mortality rate is not reported given that the verbal autopsies that would be required to identify the cause of death for women of reproductive age who died were ultimately not collected fully for all reported deaths. The perinatal mortality rate (calculated using data on stillbirths and deaths within the first seven days of life) was not calculated because information on the exact date of birth and death of newborns was frequently missing, rendering it challenging to identify a perinatal death rate. The under-five mortality rate was not calculated as the choice was made to report under-one mortality; however, both variables yield null results for the effect of the interventions estimated.

S6 Robustness checks

Here, we report two robustness checks on the primary results. We analyze whether we observe non-random selection into the 3-day, 28-day, and audit surveys, and also present additional evidence around attrition.

Selection into the survey sample While 90% of the baseline sample was re-surveyed in the endline survey, the surveys conducted during the ongoing data collection period (3-day, 28-day, and audit surveys) are available for smaller samples. In addition, a reduced sample of households is represented in the endline anthropometrics data. Accordingly, we can compare the baseline characteristics of women included in these various data sources to evaluate whether they differ significantly on observables. We also analyze whether the probability of inclusion differed across treatment arms.

To implement this test, we utilize the full sample of all births observed and estimate the following specifications, regressing dummy variables for whether the birth was observed in a given survey ($Survey_{picg}$) on the three treatment dummies and two sets of baseline characteristics: baseline demographics as reported in Panel A of Table S3, and baseline care utilization as reported in Panel A of Table S4. The baseline care utilization variables are available only for women who reported a recent birth (within the last two years) at baseline.

$$Survey_{bicg} = \beta_1 CORPS_{cg} + \beta_2 Kits_{cg} + \beta_3 Drama_{cg} + \beta \chi_{icg} + \mu_g + \epsilon_{bicg} \quad (3)$$

Table S12 presents the results. We can observe in Columns (1) to (3) that women in the CORPs-only arm were slightly less likely to be surveyed during ongoing data collection, while educated and married women characterized by higher birth parity at baseline are more likely to be surveyed. This pattern may reflect these women's higher status within the community; enumerators relied on local informants to assist in identifying the women in the sample, and they may have found it easier to locate women who were better known. In Columns (4) to (6), the results suggest that only significant differences between women who were and were not included in anthropometric data collection at endline is age and the probability of Muslim identity. Importantly, however, there is no evidence that baseline care utilization predicts selection into any form of data collection.

Data from the ongoing surveys was used to construct outcome measures employing two strategies. First, whenever the same outcome was measured in both the endline and an ongoing survey, we preferentially employed the latter report given that this data was collected closer to the birth and is presumably characterized by more limited recall bias. Second, for women who were observed only at endline, the outcome variables were constructed using the endline data. Given the characteristics of the women interviewed during the ongoing data collection, we can conclude that recall bias may be slightly lower for married and educated women who had slightly larger families. However, given that these women do not show evidence of significantly different care-seeking behavior at baseline, and all regressions include a large set of baseline demographic controls, we do not anticipate these differences will generate significant bias in the primary results.

Attrition As previously noted, 10.2% of baseline respondents (719 women) were not observed in the endline survey, and 8.1% of respondents (575 women) were never observed again after the baseline survey. Given the challenging setting, characterized by ongoing civil unrest and violence, these attrition rates are relatively low. However, attrition may pose a threat to internal validity if respondents attriting from the sample in treatment communities differ systematically from those attriting in control communities. We conduct two tests to evaluate whether differential attrition could be a source of bias.

First, we report in Tables S13 and S14 the percentage of respondents reached at follow-up in each of the four experimental arms, as well as a comparison of baseline characteristics among non-attriters across arms. As before, we first analyze a series of basic socioeconomic characteristics, and then analyze the baseline values of the primary outcome variables of interest. It is evident that there are no significant differences between the proportion of respondents reached at follow-up across treatment arms. In addition, as with the balance analysis of the entire sample, there are in general very few significant differences in baseline characteristics across treatment arms, and the joint F-test again fails to reject the hypothesis of balance.

Second, we estimate the following specification to test whether baseline characteristics predict attrition from the sample, and whether this relationship differs significantly in treatment and control arms. A dummy for a respondent attriting from the sample $Attritted_{icg}$ is regressed on a treatment dummy, baseline characteristics, and the interaction between the two, again conditional on LGA fixed effects and using standard errors clustered at the cluster level. Again, we focus on baseline demographic characteristics and baseline reports of care utilization.

$$Attritted_{icg} = \beta_1 Treated_{cg} + \beta_2 BaseX_{icg} + \beta_3 Treated_{cg} * BaseX_{icg} + \mu_g + \epsilon_{icg} \quad (4)$$

The results are reported in Table S15. Some demographic characteristics are predictive of attrition on average. In general, however, there is limited evidence of any differences between attriters in treatment and control arms; only one estimated coefficient β_3 is significant at conventional levels, for postnatal care as reported in Column (5) of Panel C. This suggests the scope for bias due to differential attrition is limited.

In addition, we can construct bounds for the primary treatment effects of interest. We proceed as follows: given that only 61% of sampled baseline women reported a birth during the follow-up period, we assume that 61% of attrited respondents similarly would have given birth and entered the subsample of interest, and randomly select this proportion to be included in this analysis. We then construct bounds for the treatment effects of interest as follows. First, we assume negative selection into attrition, assigning to all attrited respondents the 25th percentile for continuous outcome variables, or zero for binary outcome variables; second, we assume positive selection into attrition, and assign to all attrited respondents the 75th percentile for continuous outcome measures of interest, or one for binary outcome variables.[27] For concision, we then estimate the average standard treatment effects for each family of outcomes, and report these effects in Table S16. In general, the estimated bounds reinforce the primary treatment effects previously reported: there is evidence of a significant average standard treatment effect for health utilization and health practices, and little evidence of effects on health outcomes.

Supplementary material tables

Table S1: Characteristics of sample health facilities

	Mean	St. dev.	Min.	Max.
Facilities reporting access to electricity (last 6 months)	0.67	0.48		
Facilities reporting access to water (last 6 months)	0.92	0.28		
Facilities reporting access to refrigerator (last 6 months)	0.63	0.49		
Facilities reporting access to telephone (last 6 months)	0.04	0.20		
Facilities reporting access to ambulance (last 6 months)	0.21	0.41		
Facilities reporting ability to perform assisted vaginal delivery	0.92	0.28		
Facilities reporting ability to perform neonatal resuscitation	0.92	0.28		
Facilities reporting access to a blood bank for transfusions	0.38	0.49		
Facilities reporting ability to conduct a caesarean section	0	0		
Number of labor and delivery beds	2.6	2.9	1	16
Number of postpartum beds	3.6	4.6	0	22
Number of newborn beds	2.1	2.3	0	8
Number of deliveries in last 12 months	532.1	401.6	19	2109
Number of sick newborns treated in last 12 months	112.1	327.7	0	1554

Notes: This table reports summary statistics for the 24 primary health centers that serve the local government areas included in the primary sample.

Table S2: Sample composition

Baseline sample by LGA

LGA name	Total households in sampled clusters	Total sampled households	Average villages per cluster
Dutse	1782	307	3
Gwaram	1727	294	2.25
Miga	2211	355	2.8
Birniwa	1979	261	2.33
Kaugama	1509	183	1.67
Mallam Madori	1673	257	2.25
Babura	1689	288	2.25
Gagarawa	1677	278	2.33
Garki	1744	259	2.25
Maigatari	1694	223	2.56
Ringim	2127	351	2.78
Roni	1818	323	2.60
Birnin Kudu	2505	353	1
Buji	2230	332	2.25
Kiyawa	2379	347	1.67
Auyo	2218	289	1.86
Guri	2543	315	1
Kafin Hausa	1964	284	1.86
Kirikasama	2189	313	1.86
Gwiwa	2038	272	3
Kazaure	1851	316	3
Sule Tankarkar	2286	257	1
Taura	2203	337	1.67
Yankwashi	2359	275	1.86

Pregnancies observed by source

Endline - only	1989
Ongoing survey - only	67
Audit - only	57
Endline and ongoing	1562
Endline and audit	583
Audit and ongoing	9
All three surveys	153
Overall total	4420

Notes: In Panel A, we report for each LGA the total number of households identified in the four sampled clusters; the total number of sampled households within each cluster; and the average number of villages constituting each cluster. In Panel B, we report the number of pregnancies observed in the surveys conducted.

Table S3: Baseline socioeconomic characteristics

Variables	Control (N=1779)	CORPs only (N=1809)	CORPs + birth kits (N=1807)	CORPs + drama (N=1674)	Joint p-value	Obs.
Panel A: Demographic characteristics						
Married	0.99	0.99	1.00	0.99	.50	7069
Number of other wives	0.33	0.38	0.35	0.34	.36	7069
Age at marriage	15.02	15.17	15.19	15.23	.68	7052
Number of marriages	1.24	1.24	1.18	1.23	.00	7063
Age	27.99	27.67	27.79	27.98	.53	7069
Ever attended school	0.16	0.15	0.19	0.18	.20	7069
Literate in Hausa	0.08	0.09	0.11	0.10	.33	7069
Muslim	1.00	1.00	1.00	1.00	.01	7069
Birth parity	4.19	4.17	4.23	4.28	.60	7069
Wealth index	-0.03	0.04	-0.05	0.05	.71	7069
Panel B: Household income and consumption						
Head attended school	0.27	0.31	0.32	0.33	.89	6625
Head's highest educ. (primary)	0.13	0.14	0.13	0.14	.75	6625
Head's highest education - second. (secondary)	0.10	0.11	0.11	0.12	.59	6625
Head's occupation (Own cultivation)	0.51	0.51	0.51	0.49	.93	7031
Head's occupation (Non-farm self-emp.)	0.28	0.29	0.29	0.30	.49	7031
Head's occupation (Outside employment)	0.11	0.12	0.13	0.12	.94	7031
Owns land	0.95	0.94	0.94	0.94	.73	7069
Number plots	3.74	3.63	3.63	3.50	.50	6679
Cultivates any millet	0.90	0.89	0.87	0.88	.59	7069
Cultivates any guinea corn	0.87	0.85	0.84	0.84	.70	7069
Cultivates any beans	0.77	0.74	0.74	0.74	.91	7069
Cultivates any ground nut	0.48	0.44	0.40	0.48	.06	7069

Notes: This table reports the mean values of household demographic characteristics as reported at baseline for households in each experimental arm. We also estimate a regression in which each demographic characteristic is regressed separately on three dummy variables for assignment to each treatment arm, as well as LGA fixed effects; standard errors are clustered at the cluster level. The reported p-value is the p-value on the joint test $\beta_1 = \beta_2 = \beta_3 = 0$, where the three coefficients refer to the coefficients on each treatment dummy variable.

Table S4: Baseline health care utilization, health practices and health outcomes

Variables	Control (N=1779)	CORPs only (N=1809)	CORPs + birth kits (N=1807)	CORPs + drama (N=1674)	Joint p-value	Obs.
Panel A: Utilization of maternal health care						
Any antenatal care visit	0.47	0.53	0.56	0.57	.55	4007
Number of antenatal visits	1.95	2.23	2.42	2.41	.46	4007
Antenatal quality index	0.26	0.31	0.34	0.34	.41	4007
Care-seeking for complications	0.47	0.47	0.52	0.53	.26	2625
Facility delivery	0.07	0.10	0.09	0.10	.63	3742
Skilled attendant at birth	0.08	0.11	0.10	0.13	.16	3742
Post-natal check-up	0.25	0.25	0.30	0.28	.10	3742
Panel B: Maternal and newborn health practices						
Male involvement in pregnancy	0.57	0.56	0.59	0.59	.56	3837
Infant breastfed (first day)	0.82	0.82	0.82	0.83	.75	3642
Excl. breastfeeding (first three days)	0.31	0.30	.34	0.28	.05	3642
Panel C: Infant and child anthropometrics						
Weight-for-age (< 1 year)	0.15	-0.21	-0.36	0.21	.01	2042
Height-for-age (< 1 year)	0.53	0.62	0.33	0.87	.36	2009
MUAC-for-age (< 1 year)	-0.74	-0.80	-0.90	-0.92	.41	1590
Weight-for-age (< 2 year)	-0.43	-0.68	-0.76	-0.30	.00	3382
Height-for-age (< 2 year)	0.29	0.24	0.16	0.58	.06	3312
MUAC-for-age (< 2 year)	-0.89	-0.86	-0.91	-0.94	.45	2915
Panel D: Maternal morbidity						
Any complication (during pregnancy)	0.55	0.57	0.56	0.54	.30	4007
Any complication (during delivery)	0.06	0.06	0.08	0.08	.17	3742
Any complication (during postpartum)	0.21	0.25	0.22	0.22	.59	3742
Joint F-test (All baseline characteristics in Table S3 and S4)					.59	

Notes: This table reports the mean values of household characteristics as reported at baseline for households in each experimental arm; the variables reported are constructed to be identical to the outcome variables of interest subsequently analyzed in Tables 2 through 4. We also estimate a series of regressions in which each demographic characteristic is regressed separately on three dummy variables for assignment to each treatment arm, as well as LGA fixed effects; standard errors are clustered at the cluster level. The reported p-value is the p-value on the joint test $\beta_1 = \beta_2 = \beta_3$, where the three coefficients refer to the coefficients on each treatment dummy variable.

Table S5: Maternal and neonatal morbidity and mortality

	(1)	(2)	(3)	(4)	(5)	(7)
	Maternal morbidity	Neonatal morbidity	Avg. std. effect Cols. 1-2	Stillbirth	Under 1 mortality rate	Neonatal mortality rate
CORPS only	-.012	-.003	-.015	.005		
	(.018)	(.030)	(.037)	(.006)		
CORPs + Birth kits	-.033**	.054	.019	.003		
	(.017)	(.034)	(.035)	(.005)		
CORPs + Dramas	.010	-.028	-.017	.001		
	(.019)	(.034)	(.039)	(.005)		
Treated					4.291	6.761
					(7.226)	(5.985)
Test $\beta_1 = \beta_2$	.216	.075	.370	.794		
Test $\beta_1 = \beta_3$	.256	.44	.958	.533		
Test $\beta_2 = \beta_3$	.023	.024	.380	.696		
Test $\beta_1 = \beta_{enhanced}$	.98	.475	.583	.636		
Joint test: any intervention	.097	.135	.770	.812		
Control mean	.59	.5		.02	43.892	18.557
Obs.	4420	1288		4420	96	96

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The sample includes all respondents reporting a pregnancy during the intervention period who were interviewed in at least one follow-up survey. The control variables in each regression are identical to those reported in Table 1. The morbidity indices are defined as dummy variables equal to one if the respondent reported any of a series of enumerated symptoms in each specified period (pregnancy, delivery and postpartum), or if she reports symptoms for her infant. For pregnancy, the enumerated symptoms include convulsions, swelling of legs, body, or face, excessive fatigue, vaginal bleeding, trouble with vision during daylight, night blindness, high blood pressure, and any other complication. For delivery, the enumerated symptoms include excessive bleeding, fits or convulsions not caused by fever, labor longer than 12 hours, headache / blurred vision / high blood pressure, a high fever with bad-smelling vaginal discharge, the baby's hands or feet coming out first, and any other complication. For the postpartum period (the first two months after birth), the enumerated symptoms include bleeding, convulsions, swelling in the legs, face or hands, blurring of vision, unconsciousness, high fever, abnormal or smelly vaginal discharge, and serious abdominal pain. For neonates, the enumerated symptoms include loose watery stools, blood in the stool, persistent vomiting, rash, high fever, cough, difficulty breathing, weight loss, convulsions, discharge from the umbilicus, and any other complication. The stillbirth dummy is a dummy variable equal to one if the respondent reports a stillbirth during the intervention period. Infant and neonatal mortality rates are defined at the cluster level as the total number of deaths observed over the total number of births observed in the sample households over the study period. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table S6: Child anthropometrics: Children under one

	(1) Height-for-age	(2) Weight-for-age	(3) MUAC-for-age	(4) Avg. std. effect
CORPS only	.431**	.020	-.096	.026
	(.202)	(.118)	(.095)	(.031)
CORPs + Birth kits	.504***	.077	.128	.037
	(.170)	(.120)	(.084)	(.033)
CORPs + Dramas	.234	.047	.003	-.049
	(.235)	(.118)	(.098)	(.034)
Test $\beta_1 = \beta_2$	.810	.304	.005	.467
Test $\beta_1 = \beta_3$	.683	.381	.088	.914
Test $\beta_2 = \beta_3$	.499	.912	.239	.409
Test $\beta_1 = \beta_{enhanced}$	.044	.341	.019	.740
Joint test: any intervention	.045	.537	.029	.218
Control mean	-1.23	-1.42	-.84	
Obs.	1614	1614	1266	

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The sample includes children of respondents born in the intervention period (in Panel A) or under age one at endline (in Panel B) who have anthropometric measurements collected at endline. The control variables in each regression are identical to those reported in Table 1. The variables reported are Z-scores for height-for-age, weight-for-age, and mid-upper-arm circumference-for-age (MUAC-for-age) constructed using anthropometric data collected at endline and the World Health Organization child growth standards. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table S7: Health knowledge and attitudes: Women reporting pregnancies during the study period

	Delivery preference	Knowledge relative risk	Knowledge compli-cations	Infant. care know.	Attitudes toward facility	Avg. std. effect
	(1)	(2)	(3)	(4)	(5)	(6)
CORPS only	.029*	-.020	.006	-.003	.026**	.009
	(.017)	(.020)	(.008)	(.010)	(.011)	(.037)
CORPs + Birth kits	-.013	.027*	.010*	.003	.022**	.067**
	(.016)	(.015)	(.006)	(.011)	(.009)	(.026)
CORPs + Dramas	-.002	.017	.007	-.012	.014	.028
	(.015)	(.018)	(.007)	(.011)	(.011)	(.031)
Test $\beta_1 = \beta_2$	.035	.007	.523	.641	.723	.080
Test $\beta_1 = \beta_3$	.164	.071	.896	.446	.41	.614
Test $\beta_2 = \beta_3$	.385	.505	.565	.227	.489	.121
Test $\beta_1 = \beta_{enhanced}$	.048	.015	.679	.897	.525	.225
Joint test: any intervention	.206	.031	.413	.642	.040	.037
Control mean	.76	.87	.63	.56	.45	
Obs.	4161	4161	4161	4161	1393	

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The control variables in each regression are identical to those reported in Table 1. The first index measuring preference for delivery location is equal to the simple mean of seven dummy variables taking the value one if the woman said she would deliver at a facility in a series of hypothetical scenarios. These scenarios include if there was funding, if the husband said the choice was hers, if the mother-in-law said the choice was hers, if a female midwife was available at the facility, if the midwife was welcoming, if she was sure the facility was open at any hour, or if she was sure the facility was properly equipped. The second index measuring relative risk perception is an average of dummy variables equal to one if the woman reported that an unattended delivery is more dangerous than delivery with a traditional birth attendant (TBA), home delivery is more dangerous than delivering at a facility, and delivery with a TBA is more dangerous than delivering at facility. The third index measuring knowledge of pregnancy complications is a mean of three variables counting the number of complications correctly identified for the pregnancy, delivery and postpartum period, and of three dummy variables equal to one if the respondent reports a woman could die from said complications. The fourth index measuring attitudes around the use of a health facility is equal to the mean of 11 dummies coded to indicate positive attitudes toward the facility, its staff, and individuals who use the facility. The fifth index measuring knowledge of infant health practices is equal to the mean of dummies for correct answers to a series of questions; this includes whether an infant should be breastfed immediately after birth, when complementary feeding should be initiated, how much food and water should be provided to an infant or child suffering from diarrhea, how many immunizations an infant should receive in the first two months of life, and what the signs of pneumonia are in an infant. In Panel A, the sample includes all women surveyed in the endline survey. In Panel B, the sample is restricted to women who reported a birth during the intervention period. Accordingly, the fertility variables are omitted, as there is essentially no variation in fertility in this subsample. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table S8: Fertility

	Number of births	Any births
CORPS only	.013	.007
	(.015)	(.014)
CORPs + Birth kits	.002	-.006
	(.015)	(.014)
CORPs + Dramas	-.0005	-.008
	(.017)	(.016)
Test $\beta_1 = \beta_2$	.394	.319
Test $\beta_1 = \beta_3$	.374	.338
Test $\beta_2 = \beta_3$	.886	.904
Test $\beta_1 = \beta_{enhanced}$	.309	.251
Joint test: any intervention	.747	.720
Control mean	.67	.66
Obs.	6494	6494

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level. The control variables in each regression are identical to those reported in Table 1. The dependent variables include a dummy variable for whether the respondent reported a birth during the intervention period, and the number of births reported. In Panel A, the sample includes all women surveyed in the endline survey. In Panel B, the sample is restricted to women who reported a birth during the intervention period. Accordingly, the fertility variables are omitted, as there is essentially no variation in fertility in this subsample. We report tests of equality across the estimated coefficients; a test of the hypothesis that the joint effect of treatment is zero ($\beta_1 = \beta_2 = \beta_3 = 0$; and a test of equality of impact across the basic and enhanced arms $\beta_1 = \beta_{enhanced}$. Asterisks denote significance at the ten, five and one percent level.

Table S9: Treatment on the Treated estimates: Care utilization, health practices and health knowledge and attitudes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Care utilization							
	Any ANC visit	Number ANC visits	ANC quality index	Care for complications	Postnatal care	Facility birth	Skilled attendance	Birth accompanied
CORPs	.561*	1.923	.411**	-.301	.315**	-.190	.054	-.084
	(.314)	(1.485)	(.201)	(.328)	(.150)	(.184)	(.178)	(.215)
Obs.	4420	4420	4420	1570	3684	4009	3649	3649
	Panel B: Maternal and newborn health practices							
	Birth plan	Breast-feeding	Breastfeeding exclusively	Breastfeeding duration	Immunizations	Infant check-up		
CORPs	.446***	-.181	.157	-12.920	.695	.806***		
	(.165)	(.167)	(.144)	(19.269)	(.442)	(.309)		
Obs.	4152	3051	3516	2803	1385	1317		
	Panel C: Health knowledge and attitudes							
	Delivery preference	Knowledge relative risk	Knowledge compli-cations	Infant. care know.	Attitudes toward facility			
	(1)	(2)	(3)	(4)	(5)			
CORPs	.107	.295	.147	-.002	.174**			
	(.189)	(.215)	(.092)	(.128)	(.070)			
Obs.	6350	6350	6350	6350	1393			

Notes: All regressions include LGA fixed effects and standard errors clustered at the cluster level; the CORPs dummy variable is instrumented by a dummy variable for assignment to a treatment arm, and the specifications of interest are estimated employing two-stage least squares. The sample in Panels A and B includes all respondents reporting a pregnancy during the intervention period who were interviewed in at least one follow-up survey and who report the indicator of interest; the sample in Panel C includes all respondents surveyed in the endline, and the intervention exposure variable is coded zero for those who did not report a pregnancy during this period. The control variables in each regression are identical to those reported in Table 1. Asterisks denote significance at the ten, five, and one percent level.

Table S10: Baseline socioeconomic characteristics *for households reporting a birth during intervention period*

Variables	Control (N=1623)	CORPs only (N=1649)	CORPs + birth kits (N=1666)	CORPs + drama (N=1556)	Joint p-value	Obs.
Percentage of respondents observed at follow-up	.912	.912	.923	.929	.233	7069
Panel A: Respondent demographic characteristics						
Married	0.99	1.00	1.00	1.00	.43	4290
Number of other wives	0.32	0.36	0.35	0.31	.31	4290
Age at marriage	15.10	15.27	15.29	15.39	.45	4282
Number of marriages	1.19	1.21	1.15	1.17	.03	4288
Age	25.97	25.76	26.21	25.93	.17	4290
Ever attended school	0.16	0.17	0.20	0.20	.33	4290
Literate in Hausa	0.08	0.10	0.12	0.11	.78	4290
Muslim	1.00	1.00	1.00	1.00	.03	4290
Birth parity	3.76	3.61	3.86	3.77	.03	4290
Wealth index	-0.05	0.09	-0.07	0.09	.33	4290
Panel B: Household income and consumption						
Head attended school	0.30	0.34	0.34	0.36	.82	4007
Head's highest educ. (prim.)	0.15	0.14	0.14	0.15	.87	4007
Head's highest educ. (sec.)	0.11	0.13	0.12	0.15	.29	4007
Head's occupation (Own cultivation)	0.49	0.48	0.49	0.48	.96	4280
Head's occupation (Non-farm self-emp.)	0.31	0.29	0.30	0.31	.65	4280
Head's occupation (Outside employment)	0.12	0.15	0.13	0.12	.62	4280
Owns land	0.95	0.94	0.94	0.93	.49	4290
Number plots	3.72	3.66	3.52	3.33	.10	4045
Cultivates any millet	0.90	0.89	0.88	0.86	.27	4290
Cultivates any guinea corn	0.87	0.84	0.85	0.84	.99	4290
Cultivates any beans	0.76	0.74	0.75	0.73	.61	4290
Cultivates any ground nut	0.46	0.44	0.40	0.47	.14	4290

Notes: This table reports the mean values of household demographic characteristics as reported at baseline for households in each experimental arm; the sample is restricted to households reporting a birth during the intervention period. We also estimate a regression in which each demographic characteristic is regressed separately on three dummy variables for assignment to each treatment arm, as well as LGA fixed effects; standard errors are clustered at the cluster level. The reported p-value is the p-value on the joint test $\beta_1 = \beta_2 = \beta_3$, where the three coefficients refer to the coefficients on each treatment dummy variable.

Table S11: Baseline health care utilization, health practices and health outcomes *for households reporting a birth during intervention period*

Variables	Control (N=1623)	CORPs only (N=1649)	CORPs + birth kits (N=1666)	CORPs + drama (N=1556)	Joint p-value	Obs.
Panel A: Utilization of maternal health care						
Any antenatal visit	0.47	0.53	0.58	0.60	.28	2661
Number of antenatal visits	1.96	2.24	2.49	2.52	.32	2661
Antenatal quality index	0.26	0.31	0.35	0.36	.17	2661
Care-seeking for complications	0.45	0.45	0.52	0.52	.12	1782
Facility delivery	0.07	0.11	0.08	0.10	.39	2528
Skilled attendant at birth	0.07	0.11	0.09	0.12	.25	2528
Post-natal check-up	0.25	0.24	0.29	0.29	.05	2528
Panel B: Maternal and newborn health practices						
Male involvement in pregnancy	0.57	0.56	0.60	0.60	.19	2584
Infant breastfed (first day)	0.82	0.83	0.83	0.85	.49	2464
Excl. breastfeeding (first three days)	0.31	0.31	0.34	0.28	.19	2464
Panel C: Maternal morbidity						
Index of complications (pregnancy)	0.56	0.58	0.57	0.53	.27	2661
Index of complications (delivery)	0.05	0.05	0.07	0.08	.07	2528
Index of complications (postpartum)	0.21	0.24	0.21	0.21	.67	2528
Joint F-test (All baseline characteristics in Table S10 and S11)					.52	

Notes: This table reports the mean values of household characteristics as reported at baseline for households in each experimental arm; the variables reported are constructed to be identical to the outcome variables of interest analyzed in Tables 2 through 4, and the sample is restricted to households reporting a birth during the follow-up period. We also estimate a series of regressions in which each demographic characteristic is regressed separately on three dummy variables for assignment to each treatment arm, as well as LGA fixed effects; standard errors are clustered at the cluster level. The reported p-value is the p-value on the joint test $\beta_1 = \beta_2 = \beta_3$, where the three coefficients refer to the coefficients on each treatment dummy variable.

Table S12: Selection into surveys

	Ongoing survey			Endline anthropometrics		
	(1)	(2)	(3)	(4)	(5)	(6)
CORPs only	-.075**			.012		
	(.034)			(.022)		
Birth kits	-.005			.002		
	(.035)			(.023)		
Media	-.046			.016		
	(.040)			(.021)		
Married		.288***			-.114	
		(.096)			(.089)	
Has other wives		.013			.006	
		(.013)			(.012)	
Age at marriage		.005			.002	
		(.004)			(.004)	
Num marriages		-.012			-.017	
		(.017)			(.013)	
Age		-.001			-.005***	
		(.002)			(.002)	
Ever attended school		.065***			.0009	
		(.023)			(.016)	
Reads Hausa		-.026			-.005	
		(.029)			(.021)	
Muslim		-.186			-.241***	
		(.165)			(.021)	
Wealth index		.007			.004	
		(.005)			(.006)	
Birth parity		.011**			.007	
		(.005)			(.005)	
Any ANC visits			.040			.044
			(.062)			(.040)
Number of visits			-.007			-.008
			(.010)			(.006)
ANC quality index			-.029			.006
			(.089)			(.061)
Care comp.			-.009			.0006
			(.039)			(.024)
Facility birth			-.085			-.032
			(.087)			(.062)
Skilled birth			.129			.0006
			(.086)			(.054)
Postnatal care			.001			-.016
			(.031)			(.024)
Obs.	4420	4420	1312	4420	4420	1312

Notes: This table reports a series of regressions using the sample of all respondents reporting a pregnancy during the intervention period who were interviewed in at least one follow-up survey. The dependent variable is a dummy variable equal to one if the respondent was included in any ongoing survey (3-day or 28-day postnatal survey, or the audit) or the endline anthropometric data collection, and the independent variables are dummies for treatment assignment, demographic characteristics, and variables capturing care utilization as observed at baseline. All regressions include LGA fixed effects and standard errors clustered at the cluster level. Asterisks denote significance at the ten, five and one percent level.

Table S13: Baseline socioeconomic characteristics *for households observed at follow-up*

Variables	Control (N=1623)	CORPs only (N=1649)	CORPs + birth kits (N=1666)	CORPs + drama (N=1556)	Joint p-value	Obs.
Percentage of respondents observed at follow-up	.912	.912	.923	.929	.23	7069
Panel A: Respondent demographic characteristics						
Married	0.99	0.99	1.00	0.99	.43	6494
Number of other wives	0.34	0.38	0.35	0.35	.31	6494
Age at marriage	15.02	15.18	15.23	15.24	.45	6481
Number of marriages	1.23	1.24	1.18	1.23	.03	6489
Age	28.09	27.69	27.77	28.02	.17	6494
Ever attended school	0.16	0.16	0.19	0.18	.33	6494
Literate in Hausa	0.08	0.08	0.11	0.10	.78	6494
Muslim	1.00	1.00	1.00	1.00	.03	6494
Birth parity	4.25	4.19	4.24	4.32	.03	6494
Wealth index	-0.04	0.05	-0.05	0.06	.33	6494
Panel B: Household income and consumption						
Head attended school	0.27	0.31	0.32	0.33	.82	6094
Head's highest educ. (prim.)	0.13	0.14	0.13	0.14	.87	6094
Head's highest educ. (sec.)	0.10	0.12	0.11	0.12	.29	6094
Head's occupation (Own cultivation)	0.50	0.50	0.51	0.50	.96	6466
Head's occupation (Non-farm self-emp.)	0.29	0.29	0.29	0.30	.65	6466
Head's occupation (Outside employment)	0.11	0.13	0.13	0.12	.62	6466
Owns land	0.95	0.94	0.94	0.94	.49	6494
Number plots	3.75	3.64	3.60	3.50	.10	6130
Cultivates any millet	0.90	0.89	0.88	0.88	.27	6494
Cultivates any guinea corn	0.88	0.85	0.84	0.84	.99	6494
Cultivates any beans	0.78	0.75	0.75	0.75	.61	6494
Cultivates any ground nut	0.48	0.45	0.41	0.48	.14	6494

Notes: This table reports the mean values of household demographic characteristics as reported at baseline for households in each experimental arm; the sample is restricted to households observed at follow-up. We also estimate a regression in which each demographic characteristic is regressed separately on three dummy variables for assignment to each treatment arm, as well as LGA fixed effects; standard errors are clustered at the cluster level. The reported p-value is the p-value on the joint test $\beta_1 = \beta_2 = \beta_3$, where the three coefficients refer to the coefficients on each treatment dummy variable.

Table S14: Baseline health care utilization, health practices and health outcomes *for households observed at follow-up*

Variables	Control (N=1623)	CORPs only (N=1649)	CORPs + birth kits (N=1666)	CORPs + drama (N=1556)	Joint p-value	Obs.
Panel A: Utilization of maternal health care						
Any antenatal visit	0.47	0.53	0.56	0.57	.28	3697
Number of antenatal visits	1.97	2.22	2.43	2.40	.32	3697
Antenatal quality index	0.27	0.31	0.34	0.34	.17	3697
Care-seeking for complications	0.47	0.47	0.52	0.53	.12	2427
Facility delivery	0.07	0.10	0.09	0.10	.39	3454
Skilled attendant at birth	0.07	0.11	0.10	0.13	.25	3454
Post-natal check-up	0.25	0.24	0.29	0.28	.05	3454
Panel B: Maternal and newborn health practices						
Male involvement in pregnancy	0.58	0.56	0.59	0.59	.19	3542
Infant breastfed (first day)	0.83	0.82	0.83	0.83	.49	3365
Excl. breastfeeding (first three days)	0.32	0.30	0.33	0.27	.19	3365
Panel C: Maternal morbidity						
Index of complications (pregnancy)	0.55	0.58	0.57	0.54	.27	3697
Index of complications (delivery)	0.05	0.06	0.07	0.08	.07	3454
Index of complications (postpartum)	0.21	0.24	0.21	0.21	.67	3454
Panel D: Infant and child anthropometrics						
Weight-for-age (< 1 year)	0.13	-0.18	-0.33	0.24	.02	1892
Height-for-age (< 1 year)	0.48	0.62	0.36	0.92	.41	1861
MUAC-for-age (< 1 year)	- 0.73	-0.76	-0.90	-0.93	.16	1478
Weight-for-age (< 2 year)	-0.44	-0.65	-0.77	-0.27	.00	3144
Height-for-age (< 2 year)	0.25	0.26	0.16	0.63	.05	3078
MUAC-for-age (< 2 year)	-0.88	-0.83	-0.91	-0.94	.20	2718
Joint F-test (All baseline characteristics in Table S13 and S14)					.47	

Notes: This table reports the mean values of household characteristics as reported at baseline for households in each experimental arm; the variables reported are constructed to be identical to the outcome variables of interest analyzed in Tables 2 through 4, and the sample is restricted to households observed at follow-up. We also estimate a series of regressions in which each demographic characteristic is regressed separately on three dummy variables for assignment to each treatment arm, as well as LGA fixed effects; standard errors are clustered at the cluster level. The reported p-value is the p-value on the joint test $\beta_1 = \beta_2 = \beta_3$, where the three coefficients refer to the coefficients on each treatment dummy variable.

Table S15: Analysis of differential attrition predictors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Respondent demographic characteristics						
Treated	.163	.163	-.011	.073	.019	-.041	-.005
	(.106)	(.106)	(.010)	(.077)	(.018)	(.027)	(.010)
Covariate	-.036	-.036	-.007	-.0002	.025*	-.001*	.003
	(.061)	(.061)	(.012)	(.005)	(.013)	(.0008)	(.017)
Treated x covariate	-.173	-.173	.006	-.005	-.022	.001	-.021
	(.107)	(.107)	(.014)	(.005)	(.015)	(.0009)	(.020)
Covariate	Married	Number other wives	Age at marriage	Number of marriages	Age	Attended school	Literate in Hausa
Obs.	7069	7069	7069	7052	7063	7069	7069
	Panel B: Respondent demographic characteristics, cont.						
Treated	.134	-.025*	-.009				
	(.131)	(.015)	(.008)				
Covariate	.089***	-.006***	-.00002				
	(.020)	(.002)	(.009)				
Treated x covariate	-.143	.004	-.005				
	(.130)	(.002)	(.009)				
Covariate	Muslim	Birth parity	Wealth index				
Obs.	7069	7069	7069				
	Panel C: Utilization of maternal health care						
Treated	-.012	-.006	-.005	-.022	-.006	.006	.008
	(.014)	(.014)	(.014)	(.017)	(.012)	(.011)	(.011)
Covariate	-.024	-.003	-.022	-.008	-.016	.008	.054**
	(.017)	(.003)	(.023)	(.021)	(.017)	(.030)	(.028)
Treated x covariate	.024	.003	.017	.015	.040*	-.014	-.050
	(.020)	(.004)	(.027)	(.024)	(.022)	(.034)	(.034)
Covariate	Any ANC visit	Number of visits	ANC quality index	Care comp.	Postnatal care	Facility birth	Skilled attendance
Obs.	4007	4007	4007	2625	3742	3742	3742

Notes: This table reports the results of a series of regressions in which a dummy for attrition is regressed on baseline covariates, a dummy for assignment to a treatment arm, and the interaction between the two; the covariates included are household demographic characteristics as previously reported in the balance tests. The attrition dummy is equal to one if a baseline respondent is not observed in any survey post-baseline. All regressions include LGA fixed effects and standard errors clustered at the cluster level. Asterisks denote significance at the ten, five and one percent level.

Table S16: Average standard treatment effects: Attrition bounds

	Utilization (Reduced)	Utilization (Full)	Health practices	Morbidity	Anthropometrics	Knowledge and attitudes
Panel A: Negative selection into attrition						
CORPs only	.017	0	.046**	-.015	.046	.023
	(.050)	(.035)	(.02)	(.037)	(.047)	(.030)
CORPs + Birth kits	.117***	.072**	.059***	.019	.112**	.060***
	(047)	(.035)	(.023)	(.035)	(.046)	(.024)
CORPs + Dramas	.154***	.072**	.071***	-.017	.078	.066***
	(.048)	(.034)	(.025)	(.039)	(.054)	(.026)
Panel B: Positive selection into attrition						
CORPs only	.007	-.008	.042*	-.015	-.003	.011
	(.056)	(.041)	(.022)	(.037)	(.053)	(.033)
CORPs + Birth kits	.118**	.073*	.059***	.019	.103**	.049**
	(052)	(.041)	(.024)	(.035)	(.049)	(.023)
CORPs + Dramas	.128**	.059	.061***	-.017	.049	.030
	(053)	(.039)	(.026)	(.039)	(.054)	(.027)

Notes: This table reports the average standardized treatment effects for specifications estimated to construct bounds assuming positive and negative selection for attrited respondents. First, we assume positive selection into attrition, and assign to all attrited respondents the 75th percentile for continuous outcome measures of interest, or one for binary outcome variables; second, we assume negative selection into attrition, assign to all attrited respondents the 25th percentile for continuous outcome variables, or zero for binary outcome variables. For binary variables that have a negative welfare interpretation – e.g., delivering alone — the opposite assignment strategy is employed.
The average standardized treatment effects correspond to the following ASTEs estimated in the main tables: Column (1) corresponds to Column (5) of Table 2. Column (2) corresponds to Column (9) of Table 2. Column (3) corresponds to Column (7) of Table 3. Column (4) corresponds to Column (3) of Table S5. Column (5) corresponds to Column (4) of Panel B of Table 4. Column (6) corresponds to Column (6) of Panel B of Table 5. Asterisks denote significance at the ten, five, and one percent level.