HHS Public Access

Author manuscript

*J Dev Stud*. Author manuscript; available in PMC 2024 January 01.

Published in final edited form as:

*J Dev Stud*. 2023 ; 59(1): 114–132. doi:10.1080/00220388.2022.2120805.

# Demand-Side Factors in Maternal Health Outcomes: Evidence from a Community Health Worker Programme in Uganda

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## Abstract

While community health workers (CHWs) are a core feature of many low-resource healthcare systems, evidence on both their health impacts and the mechanisms behind these impacts remains limited. Using a difference-in-differences design with a control and treatment group, this study evaluated a CHW programme in southwestern Uganda aimed at improving maternal health outcomes. We found relatively little evidence of an overall programme effect on health behaviours, including antenatal care attendance and delivery under skilled supervision. Analysis of heterogeneity by gestational age at first antenatal visit — which should have modulated exposure to the intervention — provided suggestive evidence that treatment effects varied predictably with gestational age. Altogether, the absence of strong programme effects may have been due to suboptimal performance by CHWs, thus highlighting the importance of studying and instituting appropriate monitoring and incentive schemes for such programmes. Additionally, in contrast to the weak treatment effect findings, analysis of the entire study sample between the pre- and post-intervention periods showed large improvements in healthcare-seeking behaviour across both the treatment and control groups. These changes may have arisen from concurrent supply-side health facility improvements affecting the entire study population, spillover effects from the CHWs, or background health trends.

### Keywords

maternal health; community health workers; health behaviour; evaluation; demand-side; Uganda

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Disclosures

Alongside his academic position, JG serves as the Chief Executive Officer and as a board member of Progressive Health Partnership (PHP), the organisation that carried out the intervention and data collection activities for this article. The author's relationship with PHP is unpaid. KD has also served as the Chief Research Officer of PHP, again in an unpaid capacity. JB is an employee of PHP.

Ethics Statement

Research reported in this publication was approved by the Duke University Institutional Review Board (A0457), the Harvard University Institutional Review Board (CR-24282-01), the Mbarara University of Science and Technology Research Ethics Committee (MUREC 1/7), and the Uganda National Council for Science and Technology (SS 2535).

# 1 Introduction

## 1.1 Background

Community health workers (CHWs) are a core feature of many low-resource healthcare systems and have been lauded for their ability to target vulnerable populations, better connect patients to local health facilities, and provide community-level care. Along these lines, CHW programmes may have a role to play in a range of primary healthcare functions, including maternity care. This study evaluates one such CHW intervention focused on improving maternal health in Uganda, where the central government runs an ongoing CHW programme called the Village Health Team. As in many low-income countries, maternal mortality in Uganda persists at an alarming level, with an estimated 375 women dying per 100,000 live births (WHO et al., 2019). Additionally, the perinatal mortality rate stands at 38 per 1,000 pregnancies of seven or more months' duration. These figures, along with substantial maternal and infant morbidity, are often thought to be driven by factors such as suboptimal rates of health facility deliveries and inadequate levels of antenatal and postnatal care attendance. For example, about 50 percent of Ugandan women do not have their first antenatal care (ANC) visit until roughly the fifth month of pregnancy, while about one-third of women complete only 2-3 such visits (UBOS & ICF, 2018).

Based on low attendance levels for recommended care, scholars have emphasised the importance of healthcare utilisation in improving health outcomes in lower- and middle-income countries. In this light, researchers often attribute suboptimal utilisation levels to demand-side factors that influence healthcare consumer choices (Hunter et al., 2014). However, while consumers do make healthcare utilisation decisions, their choices depend on multiple factors, including the availability of quality care. In economic terms, the decision to utilise health services pivots around the benefits of those services relative to their costs. So long as the benefits outweigh the costs, says the neoclassical economic prediction, consumers will utilise health services (Dupas, 2011). Within this basic framework, there are many possible explanations for low utilisation. For example, consumers may perceive less than the actual benefits of care because they are uninformed about the availability or purpose of recommended services; consequently, costs of care may exceed perceived benefits. Alternatively, the benefits of care may be objectively low if the healthcare system — that is, the supply side — does not reliably provide high-quality services (Chou, Walker, & Kanyangarara, 2019). Similarly, disrespectful care may discourage patients from seeking health services (Hulsbergen & van der Kwaak, 2020). Apart from low benefits, financial constraints may also inhibit consumers (Ensor & Cooper, 2004). On the cost side of the equation, consumers face direct costs associated with the care itself and any necessary travel, as well as an opportunity cost for the time foregone in seeking care. If, for instance, a consumer resides far from a health facility or has a highly rewarding alternative use of time, the benefits of care may fall short of the costs (Ensor & Cooper, 2004; Hunter & Murray, 2017).

Notwithstanding the many complexities in the foregoing cost-benefit framework, some scholars have argued that the objective benefits of maternity care are significant enough to reliably outweigh the costs. Moreover, there is widespread agreement on the importance of

maternal health to both child and social development (Borghi et al., 2006; Jowett, 2000). These observations suggest a potential role for interventions that "push" women to consume more maternal healthcare — for example, by delivering information on the benefits or by subsidising the costs (Gopalan et al., 2014). Such an approach accords with a substantial body of literature in development economics that has focused on why demand for health services by the poor remains low, given that investing in these services seems to have high private welfare returns (Dupas, 2011).

In a similar vein, if the benefits of maternity care outweigh the costs, low levels of maternal healthcare in low- and middle-income countries may suggest departures from the predictions of neoclassical economic behaviour. Alternatively, suboptimal maternal health service utilisation may reflect a range of other constraints, such as substandard supply-side quality. In this study, we explore these possibilities by evaluating a CHW programme in rural Uganda to shed light on the incentives and constraints that influence health choices during pregnancy. The remainder of the article is organised as follows. First, the rest of this section describes a theoretical framework for conceptualising the effect of CHWs. Section 2 provides an overview of the CHW programme of interest, along with the data source, while Section 3 outlines the empirical specification that we use to identify the programme effect. Section 4 presents the results of the main statistical model and several other analyses, and Section 5 concludes with implications.

### 1.2 Theoretical Framework

We envision two primary pathways through which the CHW programme may operate: information provision and bargaining power. First, because of educational deficiencies, as well as an overall lack of public awareness programmes, women and their husbands may experience imperfect information regarding the private returns to investment in maternal health services (Elo, 1992; Mersha, 2018; Zamawe, Banda, & Dube, 2016). Several studies have demonstrated changes in household decision-making in response to both information on health risks and information on specific prevention techniques (Cohen, Dupas, & Schaner, 2015; Dupas, 2011; Jalan & Somanathan, 2008). At the same time, the effectiveness of information provision may depend on its credibility level and the intended recipient's characteristics (Dupas, 2011). Thus, by offering personalised health education and advice to women and their husbands in their homes, CHWs may incentivise greater take-up of maternal health services (Ensor & Cooper, 2004).

On one hand, CHWs, as fellow community members, may occupy a unique social position as trusted neighbours, in contrast to the formal healthcare system, which tends to be an object of mistrust in Uganda (Agarwal et al., 2019; Druetz et al., 2015; Kane et al., 2010). Additionally, unlike local government health workers, CHWs operate at the village and household levels, making health advice more accessible (Agarwal et al., 2019; Haines et al., 2007). Therefore, CHWs may serve as a more optimal mechanism for information delivery. On the other hand, women and families may view CHWs less credibly, perhaps due to their lack of formal health training or to observations that the CHWs themselves do not follow their health advice in their own lives. If so, CHWs may have little impact on maternal health behaviours.

Second, low intra-household bargaining power of women may inhibit their ability to make optimal health choices (Ensor & Cooper, 2004). Women may experience low bargaining power because of imperfect information on the part of husbands regarding the benefits their support can achieve or because of different male preferences regarding maternal health choices. For example, Ashraf, Field, and Lee (2014) find important effects of gender-specific preferences and information on contraception adoption. Several studies also show the important role of women's bargaining power in determining maternal health behaviours (Beegle, Frankenberg, & Thomas, 2001; Maitra, 2004; Peters, 2011). Low bargaining power may create financial constraints that hamper a woman's ability to overcome barriers to care, such as transportation to a health facility when in labour. Additionally, because women often bear responsibility for most household chores — even during pregnancy — the opportunity cost of visiting a health centre for antenatal or postnatal care may exceed the perceived benefits (Ensor & Cooper, 2004). By speaking with women and their husbands about each party's role during pregnancy and encouraging men to become more supportive of their wives' care, CHWs may serve to increase women's bargaining power (Mullany, Becker, & Hindin, 2007). In contrast to many suggested strategies to increase women's power within the household, such as increasing women's economic status, the CHW intervention addresses norms as constraints on women's bargaining power — that is, the common, traditional practices of couples in relation to maternity care (Mabsout & Van Staveren, 2010; Onarheim et al., 2020).

## 2 Programme Background and Data Source

### 2.1 Study Location and Population

This evaluation is situated within the Ugandan public healthcare system, which is organised in a hierarchical structure that aligns with the geographic hierarchy of the country's administrative units. In brief, below the district hospital level, the upper-level health facility is called a Health Centre IV, which is based at the county level. Then, mid-level Health Centre III's operate at the level of subcounties/town councils/divisions, which are equivalent administrative units for rural, periurban, and urban areas, respectively. Such administrative units are comprised of parishes/wards, which are served by lower-level Health Centre II's. Finally, the lowest administrative unit, a village, is served by the Village Health Team. The Village Health Team generally consists of multiple individuals in each village whom the government has identified and recruited to voluntarily serve as CHWs. Within the rural healthcare system, Health Centre III's tend to be the core provider, with services that are to include preventive, promotive, outpatient curative, maternity, basic inpatient, and laboratory services.

This study uses data from an evaluation of a CHW intervention run by Progressive Health Partnership (PHP), a non-profit organisation that collaborates with the government in southwestern Uganda to improve healthcare delivery. PHP operates in two neighbouring rural subcounties, Kashongi and Kitura, which together encompass thirteen parishes, 106 villages, and approximately 41,000 people in Kiruhura District. The catchment area spans several square miles. Residents are predominantly poor, and most earn a living from subsistence farming.

## 2.2 Programme Description and Study Arms

In 2012, PHP launched a new maternal and neonatal health initiative, the Omukazi Namagara Program, in its 106 partner villages. The programme consisted of two components: (1) quality of care improvements at public health facilities (including more reliable medication and supply stocks, provision of new equipment, increased staffing, and improved patient education) and (2) home visits to women during and after pregnancy by PHP-trained CHWs.

**2.2.1 Treatment and Control Groups—**PHP implemented the programme's first component at the Kashongi and Kitura Health Centre III's, the two main government health centres that serve the subcounties. As a result, all members of the 106 villages had access to the improved facility-level services. However, the organisation implemented the programme's second component in only 52 of the 106 villages, meaning that only women in these villages received CHW home visits. We hereafter refer to these 52 villages as the treatment villages and the remaining 54 villages as the controls. It is the programme's second component — the CHW home visits — that is the main subject of this study. In addition to the two aforementioned programme components, at the programme's launch PHP conducted an information campaign throughout all 106 villages to raise awareness of various maternal and neonatal health topics.

**2.2.2 Community Health Worker Background and Selection—**PHP selected the programme's CHWs through the Ugandan government's Village Health Team. In each subcounty, the government has designated one member of the Village Health Team as the subcounty coordinator. Similarly, in each parish within each subcounty, the government has designated parish coordinators. Subcounty coordinators dual-serve as parish coordinators in their home parishes.

During the study period, however, the Village Health Team members were largely inactive, absent third-party activity by an organisation such as PHP. For the Omukazi Namagara Program, PHP identified a subset of Village Health Team members with whom to work. Specifically, the organisation selected two Village Health Team members for each of the thirteen parishes in Kashongi and Kitura, making for a total of twenty-six CHWs on its team. In each parish, PHP selected one male and one female CHW.

The organisation's selection of CHWs was driven by several considerations. First, although it would only be working with a subset of the Village Health Team members, PHP sought to maintain the core structure that the government had established for the Village Health Team. Consequently, the organisation chose both of the subcounty coordinators and all of the parish coordinators for its team. Second, beyond these thirteen CHWs, PHP had existing relationships from previous work with five Village Health Team members whom it added to the team. Third, the organisation randomly selected the remaining eight CHWs.

**2.2.3 Community Health Worker Village Assignments—**PHP assigned each CHW to work in two villages: the CHW's home village and the nearest neighbouring village, leading to 52 treatment villages total. As described above, both the CHW and village selection processes meant that not all of the treatment villages were randomly selected.

However, several observations suggest that the treatment assignment process was not subject to significant selection bias. First, the distribution of the treatment villages across the entire catchment area was relatively dispersed, with no evident geographic biases associated with treatment. Second, the selection of CHWs by both the government and PHP was predominantly driven by idiosyncratic qualities of the individual CHWs, including their history of leadership in the two subcounties, rather than by any defining features of their home villages. Thus, the villages that were included in the treatment group by virtue of PHP's selection of CHWs did not appear to exhibit any unique characteristics relative to the control villages. Similarly, although the organisation maintained the Village Health Team's hierarchical leadership structure, those CHWs with leadership positions in the Village Health Team did not operate differently in their assigned villages in comparison to the ordinary CHWs. In practice, the CHWs performed activities with relatively little hierarchy.

#### 2.2.4 Community Health Worker Training, Compensation, and Oversight—

Using materials that the organisation designed, PHP trained the CHWs on a range of topics related to maternal health, such as the importance of facility-based care, antenatal and postnatal hygiene, sanitation, nutrition, malaria prevention, basic recognition of danger signs, referral of women to local health centres, antenatal and postnatal medications and vitamins, and breastfeeding and immunisation practices. The CHWs also received training on interpersonal skills and on social issues that may complicate maternity care. PHP provided specific training on involving husbands in maternity care and instructed the CHWs to talk with both a woman and her husband (if applicable) during home visits. All CHWs received tools to guide their home visits and recordkeeping. Following the training, PHP administered a competency exam to the CHWs.

For each woman, CHWs were expected to make approximately three home visits during the antenatal period and two home visits during the postnatal period. PHP paid the CHWs 5,000 UGX for each home visit made. After the CHWs began the programme activities, PHP oversaw their work mainly through periodic in-person assessments and quarterly meetings held with the entire team.

### 2.3 Pre-Intervention Survey

At the beginning of the programme in July 2012 and lasting through July 2013, PHP conducted a baseline survey of pregnant women at the Kashongi and Kitura Health Centre III's. The survey sampling frame consisted of all pregnant women above the age of 18 who (1) originated from any of the treatment or control villages and (2) were attending their first ANC visit at one of the health facilities. While women needed to reside in one of the treatment or control villages to be eligible for the survey, this requirement could only be enforced based on self-report. Therefore, women who originated from outside villages but who may have misreported their home village to the survey team could not be screened from the sample at the point of data collection. The final sample included 1,131 women in total. Although the health facility-based sampling technique implies that women who never access ANC would be excluded from the sample, such population-level selection bias is likely minor, since data from prior to the study's start indicated that 95.4 percent of women in rural Uganda attend at least one ANC visit over the course of pregnancy (UBOS & ICF,

2012). In the study area, those women who obtain ANC are likely to do so via one of the government health centres where PHP performed the pre-intervention survey.

As each health centre had designated days of the week for ANC, the data collection team aimed to distribute itself optimally across the facilities to maximise the number of women interviewed. On many of the days during the baseline interview period, the data collection team surveyed all of the women present for ANC. However, on some days it was not feasible to interview all of the women, as the number of women who came for antenatal services may have been large relative to the number of interviewers. No woman who was asked to participate in the survey refused.

The survey lasted 1-1.5 hours, covering topics such as individual and household demographics; health practices and outcomes during previous pregnancies; care sought and plans for the current pregnancy; and attitudes, knowledge, and practices related to HIV/AIDS, malaria, and family planning. In the treatment villages, each woman interviewed for the survey only began receiving CHW home visits after her interview.

### 2.4 Post-Intervention Survey

Between March 2014 and March 2015, PHP conducted a follow-up survey of the same sample of women, this time interviewing them at their homes. While organising the follow-up survey logistics as efficiently as possible, the data collection team aimed to re-interview the women sequentially according to the timing of their interviews during the baseline survey. This procedure ensured an approximately equal duration of time between each woman's pre- and post-intervention interview. The follow-up survey content adhered to a similar structure as the baseline survey.[1] However, despite considerable follow-up efforts, there was significant sample attrition, with only 725 women re-interviewed.

## 3 Empirical Approach

### 3.1 Outcomes of Interest

In evaluating the effect of the CHW home visits, we focus on the following outcome variables: (1) attendance of four or more ANC visits[2]; (2) delivery with skilled attendance[3]; (3) breastfeeding within thirty minutes of birth; and (4) proper umbilical cord care, which is defined as not putting anything on the newborn's umbilical cord beside clean soap and water. The first two outcomes capture key targets used in measuring access to the formal healthcare system, while the latter two outcomes measure practices known to have direct impacts on health.

1. See the Supplementary Materials for additional information on outcome measurement, including quality measures to minimise recall bias.
2. Although the Uganda Ministry of Health recently increased the recommended number of ANC visits from four to eight, many women in Uganda continue to have four or less visits.
3. Skilled attendance almost certainly implies that a delivery occurs in a professional health facility. However, a home delivery with the attendance of a licensed, specially trained health professional would also qualify as a skilled delivery. In the study sample, only one woman reported such a case.

### 3.2 Difference-in-Differences Specification

To uncover the programme's intention-to-treat (ITT) effects, we employ a difference-in-differences framework, given by the following specification:

$$Y_{ijt} = \beta_0 + \beta_1 T_j + \beta_2 POST_t + \beta_3 (T_j * POST_t) + u_{ijt}, \quad (1)$$

where $Y_{ijt}$ denotes the level of the outcome variable of interest for woman $i$ in village $j$ at time period $t$, $T_j$ is an indicator variable equal to 1 if village $j$ is a member of the treatment group, $POST_t$ is an indicator variable equal to 1 for the time period after the intervention, and $u_{ijt}$ is an error term. The coefficient of interest is $\beta_3$, which represents the programme's ITT. $\beta_1$ represents the group fixed effect of treatment arm membership, while $\beta_2$ represents the time fixed effect of the period following the introduction of the programme. For all of the analysis in this article, we use heteroskedasticity-robust standard errors that account for clustering at the village level.

In the baseline survey, all women were asked about their practices during their most recent previous pregnancy within the past five years (if applicable) apart from the current one. The reported practices for the most recent previous pregnancy constitute the baseline levels that we use for the outcome variables in the difference-in-differences specification. As this procedure excludes primigravida (first-time pregnant) women, we employ additional specifications, shown in the Supplementary Materials, to account for the effects on this sub-population.

The coefficient $\beta_3$ represents the differential pre-post change in the outcome variable in the treatment group compared to the pre-post change in the outcome variable in the control group. For $\beta_3$ to have a causal interpretation, the model contains the parallel trends assumption. Letting $Y_{ijt}(T_j)$ denote an individual's potential outcome, the assumption requires that $\mathbb{E}[Y_{ij1}(0) - Y_{ij0}(0) \mid T_j = 0] = \mathbb{E}[Y_{ij1}(0) - Y_{ij0}(0) \mid T_j = 1]$. In words, the average pre-post change in potential outcomes for the control group must equal the average pre-post change in potential outcomes that would have occurred in the treatment group had the programme not been implemented.

### 3.3 Attrition

Reports from the data collection team indicated that the high sample attrition arose primarily because a large number of women in the pre-intervention survey originated from outside of Kashongi and Kitura but misrepresented their residential location, incorrectly believing that they needed to be from Kashongi or Kitura to be eligible for PHP's services at the health centres. Consequently, it was exceedingly difficult to relocate such women at follow-up.

For difference-in-differences estimation with a balanced panel of respondents, attrition does not introduce any bias into the identification result. Even if attrition is systematically related to treatment or control group membership, it has no effect on the parallel trends assumption, which is the only condition needed for the estimation strategy's internal validity. Naturally, however, the group of attrited respondents differs from the non-attrited portion of the sample along a number of dimensions, which may call into question the external validity of the results. Table 1 documents these differences. Among other findings, attrited respondents

have lower gravidity, are more likely to have received antenatal services from another source, are more likely to use herbs, are less likely to have husbands who make their health decisions, are more likely to have had a recent skilled delivery, and are less likely to have breastfed within 30 minutes of birth at their most recent delivery. While the parallel trends assumption is unaffected by sample attrition, note that the last test in the table verifies that the attrition is not systematically related to treatment or control group membership. All further analysis uses only the balanced panel of non-attrited respondents, which we refer to as the 'full' sample.

### 3.4 Analysis of Pre-Programme Trends

Although the parallel trends assumption itself is not testable (since the potential outcomes under control conditions are not observed for the treatment group), we examine pre-programme outcome variable trends across the treatment and control groups to help justify the assumption. This analysis uses two primary approaches: (1) we perform placebo tests for each outcome variable in the pre-programme period to verify that a false treatment would not have been associated with a statistically significant divergence in trends between treatment and control; (2) we estimate the pre-programme control and treatment linear time trends for each outcome variable and test for equality of trends across the study arms.

To construct the pre-programme trends, we use the data on each woman's most recent previous pregnancy within the past five years. Using either the approximate reported age of the child from this pregnancy or the approximate number of years elapsed since the pregnancy if the child did not survive[4], we back out the approximate number of years prior to baseline when this pregnancy occurred. Thus, in each year $t \in \{-5, -4, -3, -2, -1\}$, we observe the outcomes for the subset of women in the sample who gave birth in that year. The Supplementary Materials contain graphs of the trends across these years for each outcome variable.

Using the difference-in-differences specification in equation (1), we perform falsification tests between each consecutive pair of years $t$ and $t + 1$ (for $t + 1 \leq -1$), imagining year $t + 1$ as the post-programme period. Because of a particularly small sample size in the period five years preceding the baseline, we omit the interval between five and four years prior to baseline from testing. Table 2 shows the falsification test results, with each panel displaying the estimates of the difference-in-differences coefficient, $\beta_3$, in the corresponding regressions. A statistically significant estimate of $\beta_3$ for any of the year pairs would indicate non-parallel trends. This coefficient estimate is not statistically significant at conventional levels in any of the tests.

Reinforcing these findings, the Supplementary Materials contain results from a pooled version of the falsification test along with tests of equality in linear time trends between the treatment and control groups. Both sets of results corroborate the findings in Table 2. Thus, we maintain each outcome variable of interest in the analysis that follows.

[4]For cases in which the child did not survive, women were asked about any pregnancies that lasted at least six months from which the child was no longer alive at the time of the baseline interview.

### 3.5 Assessment of Concurrent Programmes or Policy Changes

We are also unaware of any other major programmes or policy changes related to maternal health that occurred in Kashongi and/or Kitura during the study period and that would have differentially affected the treatment and control groups. The government made no significant changes to healthcare delivery activities, and no other non-governmental organisations apart from PHP carried out maternal health activities in the local communities. This landscape suggests that the treatment and control groups likely would have continued to evolve in parallel had PHP's programme not been implemented.

## 4 Results

### 4.1 Intention-to-Treat Effects

Using equation (1), we estimate the ITT effect — the average effect of being assigned to treatment — of the CHW home visits on each of the outcome variables. Table 3 displays the results. The third row of the table, for the interaction term, gives the estimate of the coefficient of interest, $\beta_3$. The table's bottom two rows show the mean counterfactual level of the dependent variable and the post-programme mean of the dependent variable in the control group. The mean counterfactual level, equal to the estimate of $\beta_0 + \beta_1 + \beta_2$, gives the average level of the outcome variable that the treatment group would have experienced had the parallel time trend continued and the programme not been introduced. The control group post-programme mean is given by the estimate of $\beta_0 + \beta_2$.

None of the outcome variables show statistically significant treatment effects. The estimated effect on completing four ANC visits is unexpectedly negative, with the treatment group experiencing a differential change of −5.6 percentage points relative to the control group. All of the other treatment effect estimates move in the expected direction — that is, all positive, in accordance with the hypothesis that the programme would increase the likelihood of each outcome. The treatment group experienced a 4.0 percentage point differential increase in the likelihood of giving birth with skilled attendance. Also, proper cord care exhibited a 9.0 percentage point differential pre-post increase in the treatment group relative to the control, an effect that approaches statistical significance and is sizeable compared to the mean counterfactual level of 27.1 percent. The $\beta_3$ estimate for breastfeeding within 30 minutes is minimal.

For additional context, note that the interpretation of the parameter of interest as an ITT in these estimations arises not because of limited intervention adoption by pregnant women (e.g., refusal to allow a CHW to conduct home visits) but because of limited intervention delivery by CHWs. Although PHP aimed for the CHWs to visit all pregnant women in their assigned villages, only 59.2 percent of women in the treatment group reported receiving a home visit.

### 4.2 Heterogeneity by Respondent Characteristics

While the treatment group does not appear to experience an effect due to the intervention, it is possible that the treatment effect depends on respondent characteristics. To examine the influence of such characteristics, we estimate the following equation:

$$Y_{ijt} = \beta_0 + \beta_1 T_j + \beta_2 POST_t + \beta_3 H_i + \beta_4 (T_j * H_i) + \beta_5 (POST_t * H_i) + \beta_6 (POST_t * T_j) + \beta_7 (POST_t * T_j * H_i) + u_{ijt}, \quad (2)$$

where $H_i$ denotes the characteristic of interest for women $i$. Specifically, we consider time to health facility and gestational age.[5]

**4.2.1 Time to Health Facility—**The unexpected negative, though statistically insignificant, programme effect for ANC visits may suggest that pregnant women viewed CHWs as a substitute for ANC. Although the programme primarily aimed for the CHWs to address demand-side barriers to maternal health through information provision, pregnant women may have viewed the CHWs through a supply-side lens as an extension of health services closer to the household level. If so, CHWs may have crowded out health-seeking behaviour in the formal healthcare system.

Time to the local health facility may help to elucidate whether such substitution affects ANC visits and possibly other outcomes. Given direct and indirect costs of travel, women who reside further from the health facility may be more likely to substitute CHW visits for ANC, implying that $\beta_7$ in equation (2) is negative. Additionally, following from Section 1.2's theoretical discussion, the behaviour of women who face longer travel to the local health facility may be more constrained by limited bargaining power, again implying a negative sign for $\beta_7$. Table 4 shows the estimation results. Here, the time to health facility variable is the reported time to the facility where the woman seeks ANC, centred at the mean of 1.08 hours.[6] The $\beta_7$ estimate for the ANC regression has the expected sign but, along with all of the other $\beta_6$ and $\beta_7$ estimates, is not statistically significant. A clear pattern does not emerge among the estimates from the other regressions.

**4.2.2 Gestational Age—**Women with lower gestational age (recorded in weeks) at the time of their first ANC visit may experience greater intervention exposure, since CHWs will in principle have more opportunities to visit these women. If such a pathway is active, the sign of $\beta_7$ should oppose the sign of $\beta_6$; that is, the further in pregnancy a woman is at her first ANC visit, the lesser the treatment effect. Table 5 shows the results of the estimation, in which gestational age has been mean-centred at 18.99 weeks of pregnancy. While none of the programme effect estimates are statistically significant, they all conform to the hypothesis that $\beta_7$ should oppose $\beta_6$. For the skilled delivery, proper cord care, and breastfeeding regressions, the $\beta_7$ estimates are relatively small. In the ANC visits regression, the effect of gestational age alone, absent treatment, is statistically significant and logically negative; almost mechanically, greater gestational age makes it more difficult to complete all four ANC visits. As in the main specification, the primary difference-in-differences coefficient estimate $(\hat{\beta}_6)$ for ANC visits remains unexpectedly negative. By a similar token, the estimate of $\beta_7$ is surprisingly positive. The estimate implies that, for women who attend their first ANC visit at eight weeks beyond the mean gestational age, treatment increases

5. See the Supplementary Materials for additional examination of heterogeneity based on two CHW characteristics: location and gender.

6. Because some women may give birth at different locations than their ANC facility, the time to the delivery facility may differ. Nevertheless, time to health facility may reflect more generalised levels of access to healthcare.

the likelihood of ANC visit completion by 11.2 percentage points. If this effect is true, and if the ANC-CHW substitution hypothesis described above holds, the positive coefficient may reflect reduced substitution among women with lower exposure to the intervention. Alternatively, if no ANC-CHW substitution is present, then the effect could suggest that CHW visits help to 'catch up' women who face more difficult pregnancy situations due to late presentation for ANC. Alongside these possibilities, the failure to detect a statistically significant effect, across both the primary and heterogeneity estimates, may arise from insufficient power.

### 4.3 Programme Feedback

Given the absence of any striking results, programme feedback solicited from respondents who received one or more CHW visits may shed light on the intervention. Table 6 displays a list of feedback questions or responses, with the proportion of subjects answering 'yes' reported in the right-hand column. Overall, respondents gave positive feedback on the CHWs. Forty-five percent of women report that the CHWs influenced their number of ANC visits, while 37.7 percent report that the CHWs influenced their delivery location. Ninety-eight percent of women report that they liked when the CHW visited. However, only 29.8 percent of respondents reported that the CHW talked to their husbands. Despite the feedback's positive nature, we must take respondent feedback with a grain of salt, since respondent perceptions may diverge from tangible, recorded outcomes. Nevertheless, such feedback may be an important signal of welfare benefits not captured by the analysis above. At the same time, with only 59.2 percent of treatment group women reporting one or more CHW visits, and then only a fraction of these women reporting that their health behaviours were influenced by the CHW, the treatment quality may have been too diluted to produce an observable programme effect.

### 4.4 Pre-Post Changes

Despite the absence of clear programme effects, the coefficient on $POST_t$ in Table 3 points to significant changes over time for ANC visit completion and skilled delivery for both treatment and control. This result suggests that the single difference across pre- and post-programme periods for the entire sample, rather than the double difference distinguishing treatment effects, may be important to examine. Table 7 compares the pre- and post-programme means for the four outcomes in the entire sample. The four ANC visits and skilled delivery outcomes show highly statistically significant 27.3 percent and 24.2 percent increases over baseline levels, respectively. As further detailed in Section 5, several different explanations may underlie these changes. Regardless of the specific explanation, significant background changes over time likely render the estimation of the causal effect of the CHW home visits more challenging.

## 5 Discussion

### 5.1 Treatment Effects

This article provides evidence on the maternal health outcomes of a CHW home visit programme. Despite the excitement around such interventions, we observe mixed results. We find no statistically significant programme effects on completing four ANC visits, giving

birth with skilled attendance, breastfeeding within thirty minutes of birth, and engaging in proper umbilical cord care, either through the primary ITT estimation or through the heterogeneity analysis. Although not statistically significant, the estimated ITT effects for skilled delivery, breastfeeding within thirty minutes, and proper umbilical cord care have positive signs, aligning with the expectation that the programme would improve these outcomes. However, the estimated ITT effect for ANC visit completion is negative, moving in the unexpected direction.

If we believe that the ANC effect signals a meaningful change, one interpretation may be that pregnant women view CHWs as a substitute for ANC. Based on theory, we might expect that women who reside further from the health facility — and therefore experience a greater cost of seeking care — exhibit stronger substitution between CHWs and ANC. However, the analysis of heterogeneous effects based on time to health facility does not conclusively confirm this hypothesis; although the treatment effect becomes increasingly negative with greater time to the health facility, the estimate is small and is not statistically significant. At the same time, the analysis of heterogeneity by gestational age at first ANC visit provides suggestive evidence that treatment effects decrease with increasing gestational age, which should indicate reduced exposure to the intervention. This analysis also shows a pattern of enhanced substitution between CHWs and ANC for women with greater intervention exposure. Finally, across all of the results, it is important to keep in mind that the failure to reject some of the null hypotheses tested herein may arise from insufficient statistical power.

In relation to existing literature, our study offers important evidence for sub-Saharan Africa, as many CHW evaluations to-date have been conducted in South Asian countries.[7] This study complements a handful of others that have employed rigorous research methods to credibly measure the causal effect of CHW home visits on maternal health outcomes in sub-Saharan Africa. Using a randomised controlled trial, Kirkwood et al. (2013) show that a CHW home visit programme in Ghana leads to improvements in a range of newborn care behaviours but does not affect the important indicators of ANC visit completion and delivery with skilled attendance. Waiswa et al. (2015) produce analogous results with a similar study design in Uganda. In contrast, Geldsetzer et al. (2019), also using an experimental approach, find an increase in skilled deliveries but no change in the completion of four ANC visits under a CHW intervention in Tanzania. Okeibunor et al. (2011), who employ a difference-in-differences design similar to our own, find that a CHW programme in Nigeria improves malaria prevention behaviours during pregnancy, including use of insecticide-treated nets and uptake of intermittent preventative treatment distributed by the CHWs. However, they find no effect on ANC.

Overall, our findings on ANC and skilled attendance at delivery are consistent with the findings of the foregoing studies, with the exception of Geldsetzer et al.'s (2019) positive finding for skilled delivery. Taken together, the collection of literature, including this study, might suggest that it is more difficult to affect ANC visits and skilled delivery because

[7]·Review articles by Gogia and Sachdev (2010) and Gogia and Sachdev (2016) consist entirely of studies in South Asian settings. For a broader review of the literature that includes a few rigorous studies in sub-Saharan Africa, see Gilmore and McAuliffe (2013).

these outcomes necessitate more costly behaviour — that is, travelling to a health facility, often with no guarantee of receiving quality services. In contrast, Okeibunor et al. (2011) demonstrate effects for malaria prevention behaviours, which are confined to the household level and are relatively costless for women to adopt. By a similar token, Tylleskär et al. (2011) and Cooper et al. (2009) show that CHWs successfully promote exclusive breastfeeding and parenting techniques, respectively, both of which constitute interventions that are readily accessible to women and only depend on household-level behaviour. At the same time, our evaluation also includes umbilical cord care and breastfeed within 30 minutes outcomes — both of which seem relatively accessible for adoption — but identifies no effect. None of the other studies include these outcomes, so we are unable to draw any direct comparisons. The present study is also distinctive for its use of a quasi-experimental design, which offers a rigorous alternative to randomised experiments that is shared only by Okeibunor et al. (2011). In addition, our study is specifically designed around the dimensions of economic behaviour that may influence the success of CHW interventions, whereas the other literature originates primarily from a public health perspective.

### 5.2 Pre-Post Changes

While the ITT effects appear to be weak, the pre-post changes in ANC visit completion and skilled delivery for the entire sample are striking. The following possibilities may account for these trends: (1) concurrent health facility improvements, (2) CHW spillover effects, and/or (3) background health trends. First, the changes may have been driven by the first component of PHP's Omukazi Namagara Program, which included quality of care improvements at the local government health centres that are accessible to all women in the study area. Women and households may have been far more responsive to the quality of care improvements than to the CHW home visits. Such a finding would shine light on the influence of supply-side factors as determinants of health decision-making, in contrast to the view of demand-side frictions that impede neoclassical behaviour.

Attributing the pre-post change to the health service strengthening alone is complicated by the aforementioned information campaign that PHP carried out in all 106 villages at the intervention launch. However, the facility improvements likely play a prominent role. As described above, the substantial sample attrition appears to be largely due to women from localities outside the study area who sought services at the health facilities. Field reports indicate that these women opted to visit these health facilities because they had learned of the improved quality of care. As it may be unlikely for news about the facility improvements to spread very quickly from the experiences of individual patients, it is entirely possible that the information campaign and the facility improvements together produced an interactive effect on healthcare-seeking behaviour. Ultimately, such a result would still point to the responsiveness of women to supply-side factors.

An alternative explanation for the large pre-post changes is that the programme's CHW component produced treatment-to-control spillover effects, possibly in interaction with the health facility improvements. In this case, the CHW visits may have substantially impacted health, but the effect would be difficult to causally detect. As the CHWs are unlikely to have made home visits in the control villages[8], such spillover effects would have operated

through the spread of information disseminated via the CHW visits. It is also possible that the information spread through spillovers included information regarding the health facility improvements and impacted healthcare-seeking behaviour through a mechanism similar to the interactive effect described above.

A third possibility is that the pre-post changes reflect health trends already underway in Uganda and/or the particular study localities. According to data from the Demographics and Health Survey, the completion of four ANC visits in rural Uganda increased from 45.8 to 58.3 percent between 2011 and 2016, and deliveries in a health facility in southwestern Uganda increased from 40.3 to 70.6 percent over the same period (UBOS & ICF, 2012; UBOS & ICF, 2018). Additionally, the pre-programme trend for ANC completion appears to show a clear upwards path; the trend for skilled delivery is less evident.

### 5.3 CHW Management and Performance

If the intervention truly produced no impact, it is important to explore why. The 59.2 percent CHW coverage rate reported by treatment group women stands out as a key explanation. Moreover, the impact of CHWs depends jointly on performance along both the extensive and intensive margins. On the extensive margin, the CHW decides whether to visit a woman; on the intensive margin, the CHW performs according to dimensions such as those featured in Table 6. Given this joint determination of performance, the ‘effective coverage’ may have been substantially below 59.2 percent, reducing the treatment quality.

CHW performance is a matter of its own that warrants attention. While we do not have any quantitative data from the CHWs themselves to determine the reasons for partial coverage, there are several possible explanations based on field observations. First, because many of the CHWs tend to be more educated community members, sometimes with additional income sources, PHP’s payments to the CHWs may not sufficiently compensate for the opportunity cost of their time. Second, as described above, each CHW was assigned to work in his or her home village and the nearest neighbouring village. However, the workload of two villages may have been overly demanding. Additionally, the CHWs reported difficulties to PHP about working in neighbouring villages, due to lesser familiarity with all of the households.[9] Third, because PHP compensated CHWs based on the home visit records that they submitted, some CHWs may have been incentivised to fabricate the records without actually visiting all of the women. While PHP instituted monitoring mechanisms to help mitigate this possibility, such monitoring was relatively lax during the study period. Lastly, it is possible that CHWs selectively chose to visit some women but not others, perhaps based on social networks or perceived need.

Overall, interventions of this nature may require substantial managerial resources and oversight to achieve a high rate of coverage. The success of CHW programmes at a policy level may hinge on the monitoring mechanisms and performance incentives that

8. With regard to the team of CHWs working on PHP’s programme, the organisation only compensated them for home visits within their two assigned villages. Given that the CHWs did not even fully cover the women in the treatment villages, they would have had little incentive to conduct extraneous home visits in outside villages, which almost certainly would have been more geographically distant.

9. See the Supplementary Materials for further analysis of treatment effects based on the CHW home and neighbouring villages.

programmes implement. In this regard, future research should focus on the supply side by evaluating different monitoring and incentive schemes to achieve optimal CHW effort. For example, for monitoring schemes, researchers can investigate how both top-down and bottom-up approaches affect CHW effort. Top-down approaches to monitoring may include supervisory audits of CHW activities, while bottom-up approaches may entail mechanisms for programme recipients themselves to report feedback on CHW performance. In terms of performance incentives — some of which may be linked to monitoring outcomes — researchers can compare different schemes, such as voluntary service, non-pecuniary incentives (e.g., community recognition), piece rate pay (e.g., based on visits made), pay-for-performance compensation (e.g., based on patient healthcare utilisation or health outcomes), and unconditional lump-sum compensation. Notably, however, the systems needed to implement monitoring and incentive schemes are not free. Given the managerial inputs required to execute some of these schemes, CHW programmes may need to expend more resources on supervisory training, alongside their standard CHW training activities. Ultimately, to determine the social contribution of such approaches, researchers along with policymakers must weigh the benefits of higher programme coverage against the additional input costs.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

## Acknowledgements

We thank Hoyt Bleakley, John Bound, Fred Muhumuza, Edward Norton, Dean Yang, and Cheryl Moyer for their feedback on this paper. We also thank Joram Amanya, Diana Asiimwe, Ephratah Nahabwe, and Alex Niwagaba for the instrumental roles they played in the data collection and program implementation. In addition, we are indebted to all of the leaders and healthcare professionals in Kiruhura District who helped to facilitate the activities of this study. Equally important, we are grateful to Christine Convery for her financial management of project activities.

### Funding

Research reported in this publication was supported by the Nancy Allison Perkins Foundation, the Eunice Kennedy Shriver National Institute of Child Health and Human Development of the National Institutes of Health (JG, award number T32HD007339), and the National Institute of General Medical Sciences of the National Institutes of Health (JG, award number T32GM007863). The content of the article is solely the responsibility of the authors and does not necessarily represent the official views of any of the funders.

## Data Availability Statement

Alongside his academic position, JG serves as the Chief Executive Officer and as a board member of Progressive Health Partnership (PHP), the organisation that carried out the intervention and data collection activities for this article. The author's relationship with PHP is unpaid. KD has also served as the Chief Research Officer of PHP, again in an unpaid capacity. JB is an employee of PHP.

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**TABLE 1:**

ANALYSIS OF SAMPLE ATTRITION

| | (1) Overall | (2) Missing | (3) Non-Missing | (4) *p*-value |
|---|---|---|---|---|
| Respondent Education Level | 5.863 | 5.987 | 5.793 | 0.293 |
| Household Head Education Level | 6.320 | 6.468 | 6.240 | 0.329 |
| Respondent is Household Head | 0.030 | 0.037 | 0.026 | 0.308 |
| Asset Index | −0.000 | −0.045 | 0.025 | 0.584 |
| Frequency of Listening to Radio | 1.915 | 1.995 | 1.871 | 0.096 |
| Household Owns Mosquito Bednet | 0.750 | 0.686 | 0.786 | 0.000 |
| Family Member Maternal/Neonatal Death | 0.451 | 0.480 | 0.435 | 0.118 |
| Number of Previous Pregnancies | 2.752 | 2.015 | 3.165 | 0.000 |
| Gestational Age (weeks) | 18.988 | 19.938 | 18.452 | 0.006 |
| Walked to Health Center | 0.596 | 0.601 | 0.594 | 0.813 |
| Accompanied by Husband to Health Center | 0.134 | 0.155 | 0.123 | 0.131 |
| Time to Health Facility | 67.720 | 66.997 | 68.126 | 0.706 |
| Burden to Come to ANC | 0.509 | 0.519 | 0.503 | 0.654 |
| Received ANC from Other Source | 0.222 | 0.303 | 0.176 | 0.000 |
| Has Taken Herbs | 0.539 | 0.594 | 0.509 | 0.004 |
| Number of HIV Prevention Methods Known | 1.681 | 1.712 | 1.664 | 0.365 |
| Husband Makes Health Decisions | 0.398 | 0.345 | 0.427 | 0.009 |
| Four ANC Visits | 0.564 | 0.569 | 0.562 | 0.872 |
| Skilled Delivery | 0.490 | 0.591 | 0.451 | 0.003 |
| Husb. Decides Location | 0.267 | 0.273 | 0.264 | 0.834 |
| Breastfeed 30 Minutes | 0.642 | 0.570 | 0.670 | 0.005 |
| Proper Cord Care | 0.354 | 0.326 | 0.364 | 0.372 |
| Treatment | 0.607 | 0.641 | 0.588 | 0.160 |
| Joint ($\chi^2$) test *p*-value | | | | 0.000 |

*Notes.* This table displays baseline covariate means for the overall sample and for the sub-samples that were missing and non-missing at endline, along with *p*-values for difference-in-means tests between these sub-samples. All tests use heteroskedasticity-robust standard errors adjusted for clustering at the village level.

**TABLE 2:**

PRE-PROGRAMME FALSIFICATION TESTS

| | −4 to −3 Years | −3 to −2 Years | −2 to −1 Years |
|---|---|---|---|
| *Panel A: Four ANC Visits* | | | |
| Post * Treatment | 0.166 | 0.056 | −0.036 |
| | (0.171) | (0.120) | (0.121) |
| *Panel B: Skilled Delivery* | | | |
| Post * Treatment | 0.039 | 0.042 | −0.152 |
| | (0.167) | (0.123) | (0.123) |
| *Panel C: Breastfeed 30 Minutes* | | | |
| Post * Treatment | −0.095 | 0.070 | 0.102 |
| | (0.176) | (0.118) | (0.112) |
| *Panel D: Proper Cord Care* | | | |
| Post * Treatment | −0.018 | 0.172 | −0.038 |
| | (0.162) | (0.124) | (0.125) |

*Notes:* This table displays the results of pre-programme falsification tests between successive years prior to the baseline survey. Heteroskedasticity-robust standard errors, adjusted for clustering at the village level, are shown in parentheses.

*Significant at 10% level,

**Significant at 5% level,

***Significant at 1% level.

**TABLE 3:**

INTENTION-TO-TREAT EFFECT ESTIMATION

| | (1) Four ANC Visits | (2) Skilled Delivery | (3) Proper Cord Care | (4) Breastfeed 30 Minutes |
|---|---|---|---|---|
| Treatment | 0.117** | 0.012 | −0.086* | −0.005 |
| | (0.047) | (0.050) | (0.050) | (0.043) |
| Post | 0.187*** | 0.085** | −0.060 | 0.021 |
| | (0.038) | (0.035) | (0.047) | (0.048) |
| Post * Treatment | −0.055 | 0.040 | 0.090 | 0.017 |
| | (0.056) | (0.048) | (0.061) | (0.060) |
| Constant | 0.493*** | 0.443*** | 0.417*** | 0.686*** |
| | (0.037) | (0.039) | (0.039) | (0.032) |
| Mean Counterfactual | 0.796 | 0.540 | 0.271 | 0.701 |
| Mean Control Post | 0.679 | 0.528 | 0.643 | 0.707 |

*Notes:* This table displays the results of the main intention-to-treat estimation, with the coefficient of interest corresponding to the *Post*Treatment* variable. Heteroskedasticity-robust standard errors, adjusted for clustering at the village level, are shown in parentheses.

* Significant at 10% level,

** Significant at 5% level,

*** Significant at 1% level.

**TABLE 4:**

INTENTION-TO-TREAT EFFECT HETEROGENEITY BASED ON HOURS TO FACILITY

| | (1) Four ANC Visits | (2) Skilled Delivery | (3) Proper Cord Care | (4) Breastfeed 30 Minutes |
|---|---|---|---|---|
| Treatment | 0.114** | −0.003 | −0.087* | −0.012 |
| | (0.047) | (0.048) | (0.049) | (0.043) |
| Post | 0.191*** | 0.092** | −0.046 | 0.030 |
| | (0.037) | (0.036) | (0.048) | (0.047) |
| Hours to Facility | −0.010 | −0.072* | −0.037 | −0.028 |
| | (0.035) | (0.041) | (0.038) | (0.040) |
| Treatment * Hours to Facility | −0.032 | −0.052 | −0.001 | −0.046 |
| | (0.053) | (0.064) | (0.053) | (0.055) |
| Post * Hours to Facility | −0.026 | −0.028 | −0.034 | −0.051 |
| | (0.053) | (0.040) | (0.048) | (0.050) |
| Post * Treatment | −0.054 | 0.039 | 0.075 | 0.014 |
| | (0.057) | (0.049) | (0.063) | (0.060) |
| Post * Treatment * Hours to Facility | −0.010 | 0.030 | −0.007 | 0.079 |
| | (0.073) | (0.067) | (0.066) | (0.068) |
| Constant | 0.492*** | 0.454*** | 0.410*** | 0.686*** |
| | (0.036) | (0.037) | (0.039) | (0.032) |

*Notes:* This table displays the results of the intention-to-treat estimation for the heterogeneity analysis based on time to health facility. The coefficient of interest corresponds to the *Post * Treatment * HourstoFacility* variable. Heteroskedasticity-robust standard errors, adjusted for clustering at the village level, are shown in parentheses.

* Significant at 10% level,

** Significant at 5% level,

*** Significant at 1% level.

**TABLE 5:**

INTENTION-TO-TREAT EFFECT HETEROGENEITY BASED ON GESTATIONAL AGE

| | (1) Four ANC Visits | (2) Skilled Delivery | (3) Proper Cord Care | (4) Breastfeed 30 Minutes |
|---|---|---|---|---|
| Treatment | 0.115** | 0.010 | −0.079 | −0.012 |
| | (0.046) | (0.049) | (0.051) | (0.042) |
| Post | 0.189*** | 0.077** | −0.060 | 0.021 |
| | (0.035) | (0.035) | (0.046) | (0.049) |
| Gestational Age | −0.012** | −0.007 | 0.004 | 0.004 |
| | (0.005) | (0.004) | (0.005) | (0.005) |
| Treatment * Gestational Age | −0.001 | 0.008 | 0.001 | −0.004 |
| | (0.007) | (0.006) | (0.006) | (0.006) |
| Post * Gestational Age | −0.011* | 0.001 | −0.005 | 0.003 |
| | (0.006) | (0.006) | (0.007) | (0.006) |
| Post * Treatment | −0.062 | 0.043 | 0.086 | 0.025 |
| | (0.054) | (0.047) | (0.060) | (0.060) |
| Post * Treatment * Gestational Age | 0.014 | −0.004 | −0.006 | −0.007 |
| | (0.008) | (0.008) | (0.009) | (0.008) |
| Constant | 0.493*** | 0.450*** | 0.412*** | 0.687*** |
| | (0.037) | (0.037) | (0.040) | (0.031) |

*Notes:* This table displays the results of the intention-to-treat estimation for the heterogeneity analysis based on gestational age. The coefficient of interest corresponds to the *Post * Treatment * Gestational Age* variable. Heteroskedasticity-robust standard errors, adjusted for clustering at the village level, are shown in parentheses.

* Significant at 10% level,

** Significant at 5% level,

*** Significant at 1% level.

**TABLE 6:**

RECIPIENT ASSESSMENT OF CHW VISITS

| | Mean |
|---|---|
| Talked about something you did not know? | 0.353 |
| Influenced # ANC visits? | 0.450 |
| Influenced ANC location? | 0.448 |
| Influenced delivery location? | 0.377 |
| Influenced # postnatal visits? | 0.120 |
| Influenced postnatal care location? | 0.135 |
| Liked when CHW came? | 0.980 |
| Talked to husband? | 0.298 |
| Want more frequent visits? | 0.536 |
| Want distribution of more information? | 0.595 |
| Overall performance good or very good | 0.825 |

*Notes*. This table displays binary variable averages of responses to feedback questions asked of programme recipients.

**TABLE 7:**

PRE- TO POST-INTERVENTION CHANGES FOR ENTIRE SAMPLE

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | **Mean Pre** | **Mean Post** | **Difference** | **Percent Change** | ***p*-value** |
| Four ANC Visits | 0.562 | 0.716 | 0.154 | 27.3 | 0.000 |
| Skilled Delivery | 0.450 | 0.559 | 0.109 | 24.2 | 0.000 |
| Proper Cord Care | 0.365 | 0.359 | −0.006 | −1.6 | 0.845 |
| Breastfeed 30 Minutes | 0.683 | 0.714 | 0.031 | 4.6 | 0.282 |

*Notes.* This table displays pre-intervention and post-intervention outcome means for the overall sample, along with *p*-values for difference-in-means tests across these periods. All tests use heteroskedasticity-robust standard errors adjusted for clustering at the village level.