Breaking with Tradition: Integrating Traditional Birth Attendants with Formal Clinics in Western Kenya

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Abstract:

The majority of women in rural Kenya fail to meet recommendations for antenatal care (ANC) timing and use. This paper examines the extent to which locally informed intermediaries, with potentially opposed motivations, can be exploited and provided with incentives to change the health-seeking behavior of pregnant women in rural Kenya. I use a randomized controlled trial to evaluate an incentive program and its effect on ANC visit attendance. Village-level randomization was performed, so that a Traditional Birth Attendant (TBA) in a treatment village would receive monetary incentives for encouraging pregnant women to seek prenatal care at a health facility. Intent-to-treat effects, using administrative visit data, suggest that living in a TBA treatment village increases the likelihood of a woman attending the recommended number of ANC visits by 21 percent. Additionally, I found that women, who were in villages where the TBAs were more educated, less likely to be trained as a TBA, and less experienced, appeared to respond to the TBA program to a greater degree. These heterogeneous results suggest that if program placement could be targeted, the results would be better if targeted to villages whose TBAs have options besides working as a birth attendants.

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1 Introduction

This paper examines the extent to which locally informed intermediaries can be exploited and provided with incentives to change the health-seeking behavior of pregnant women in rural Kenya. Despite Kenya being the largest and most advanced economy of East Africa, maternal and infant health outcomes are typical for those of other sub-Saharan countries, which lag significantly behind the developed world. There is evidence that antenatal care (ANC) is associated with improved maternal health outcomes, yet the majority of women in rural Kenya fail to meet recommendations for ANC timing and use, despite the availability of government subsidized healthcare. I examine whether a local intermediary, whose own incentives might oppose those of the government, can be co-opted to assist the government's objective of increasing women's ANC utilization.

Traditional Birth Attendants (TBAs) are informal providers of pregnancy care who work locally in villages. While women may see both a TBA and a formal provider for pre-delivery services, she will see either the TBA *or* a formal provider for her baby's birth. So although the services offered may differ, TBAs and formal providers do compete for pregnancy services.

In this paper, I use a randomized controlled trial (RCT) to evaluate a program, which provides financial incentives for TBAs to encourage pregnant women to seek ANC at a formal medical facility. Competition between the TBAs and the formal clinics makes the effect of the program an empirical question, as there is no guarantee that the TBAs will respond to the incentive. With some probability, clients encouraged to attend ANC at the government facilities might choose to deliver at those facilities instead of with the TBA. So despite having a financial incentive to encourage women to attend the formal clinic for ANC, the TBA may withhold encouragement if by doing so she increases her chances of keeping the woman as a client for delivery services. Furthermore, the repeated nature of the relationship between the TBA and the women may cause her to fail to provide encouragement; the TBA may be concerned that the women will choose the clinic for future pregnancies.

This intervention gains access to the TBAs' social capital and exploits their preexisting relationships with their clients in order to achieve increased ANC utilization. In this way, my study contributes to the literature on the demand for health services. Existing literature has

found that a household's decision to seek formal general health care depended upon factors such as distance, quality, price, type of illness, as well as individual characteristics (Akin et al 1995; Collier et al 202; Litvack et al 1993; Mwabu 1986). Other studies have shown increased healthcare utilization from short message service (SMS) campaigns, which remind the patient of the schedule for health care visits (Guy 2012) or encourage adherence to treatments (Lester 2010). Research focused on pregnancy-specific services in Kenya similarly found that utilization is determined in part by the following: distance from a modern facility, gender of household head, household's insurance status (Hodgkin 1996), perceived lack of need or effectiveness, transport expenses, service costs (Van Eijk et al 2006), as well as prevailing cultural norms (Naanyu et al 2011).

Most of the research examining demand for health care takes as given the quality. However, there is evidence that the quality of medical care provided and service delivery in low-income countries is low. Das et al (2008) found that medical providers often know very little and exert low effort in their diagnoses and treatments. Chaudhury's 2006 research documented high rates of absenteeism as well as poor service provision among present workers in primary schools and health clinics in six countries.

Literature on incentives examines behavioral considerations related to service delivery. The standard economics prediction is that incentives increase effort and performance, however the empirical findings are mixed. Gneezy et al 2011 find undesired effects resulting from small incentives, arguing that if incentives weaken intrinsic motivation, then positive incentive effects may only exist in the short run (Gneezy 2011 *see also* Gneezy 2000). Another negative consequence of incentives is the crowding out of public goods; for example, in 1970 Titmuss argued that incentives for blood donation could decrease the amount donated. In a 2010 survey paper, Goette et al find that incentives for blood donation work in anonymous donor settings but turn counterproductive when the public setting creates image concerns for donors. I add to the literature on incentives, with the unique contribution that the agent to whom we provide incentives has a strong motive not to comply.

Other means of improving service delivery include pay-for-performance or results-based financing (Oxman 2008). In an examination of a pay for performance program in Rwanda, Basinga 2010 find that paying health facilities for set indicators of performance improved the

quality of prenatal care. The Janani Suraksha Yojana program in India paid women to deliver in health facilities and paid Community Health Workers (CHWs) to accompany them and has been associated with an increase in institutional deliveries (USAID 2010). This program builds in conditionality of the TBA's payments, to be discussed in more detail below, and thus corresponds with existing literature on results-based-financing and pay-for-performance programs.

By paying TBAs for encouraging women to the ANC clinic, this program "breaks with tradition" by creating a partnership between the formal clinics and the TBAs, the clinics' informal competitors. Integrating TBAs with formal facilities redefines the TBAs as intermediaries of the health clinics. Understanding the characteristics of successful intermediaries has applications beyond health economics as intermediaries exist in a variety of economic sectors; for example through the use of agricultural extension agents (Anderson 2004), loan officers for microfinance (Siwale 2011), and referral incentive programs – like that used by the British colonial army in Ghana (Fafchamps 2013). However in contrast with most typical intermediary arrangements, this program has a unique nontrivial incentive problem resulting from the potential for TBA client loss.

This RCT was implemented in Vihiga, a district in the Western Province of Kenya near Lake Victoria. The project team randomized all of the TBAs in Vihiga District into the treatment or control groups. A separate, but related, voucher program for pregnancy services, hereinafter referred to as the Voucher Program, was conducted concurrently. Targeted to 1,600 pregnant women, the Voucher Program analyzes the effect of transportation and maternity vouchers on utilization of clinic services. The Voucher and TBA programs are cross-randomized, with the Voucher Program randomized at the individual woman level and the TBA program randomized at the village level.

The initial design of the TBA program specified that treatment TBAs would be paid when women provided their names at the clinics. Due to implementation challenges in the first four months, we changed the TBA incentive program. The adjustment specified that when a woman from a treatment village came in to the clinic, all of the TBAs from that village were paid, no matter which TBA provided encouragement. Each TBA received the same amount of money as

before, making the program change a more expensive way to approximate the effect of the incentive.

I find that living in a TBA treatment village increases the likelihood of attending the recommended number of visits by 20.7%. Women living in TBA treatment villages are 4.4 percentage points more likely to attend the recommended number of visits than women living in control villages, who attend the recommended number of visits 21.3% of the time. The results of this experiment, the first to study the extent to which TBAs can be motivated to encourage women to attend the prenatal clinic, could have important policy implications. The program's success suggests that despite having a risk of losing clients, TBAs can be utilized as intermediaries of health facilities. Furthermore, finding that TBAs can induce pregnant women to attend ANC visits indicates that cultural norms, which discourage women going to ANC visits, can be overcome with relatively small financial incentives. By increasing the demand for formal maternal healthcare, TBAs' encouragement of ANC attendance by women may help achieve improved maternal and child health outcomes.

2 Context

2.1 Maternal Health in Kenya

The World Bank estimates the number of maternal deaths per 100,000 live births to be 360 in Kenya, similar to the 500 for Sub-Saharan African, and as compared to 16 for countries classified as high income (Trends 2010). Problems concerning child health are magnified in Western Kenya where the infant mortality rate is 25% above the national average, and the child mortality rate is nearly double the national rate (Kenya DHS 2010).

As is typical for the region, women in Vihiga attend antenatal care (ANC) visits late in their pregnancy; Figure **1** indicates that 85% of women in Western Kenya attend their first ANC after their first trimester of pregnancy.[1] Additionally, they rarely attend the World Health Organization (WHO) recommended number of at least four ANC visits, represented in Figure 2 by the dashed line (Lawn 2006). In fact, the most recent Demographic Health Survey (DHS)

[1] Often this is too late to be useful for successfully diagnosing and treating complications, such as testing of HIV status and prevention of mother to child transmission care. Early treatment is also crucial for the prevention of congenital syphilis, control of anaemia, and prevention of malaria complications (Lawn 2006).

data for women in Western Kenya reveals that while most pregnant women attend at least one ANC visit, 57% fail to attend at least four ANC visits (Figure 2).

Figure 1: Month of First ANC by Women in Western Kenya (DHS 2008-2009)

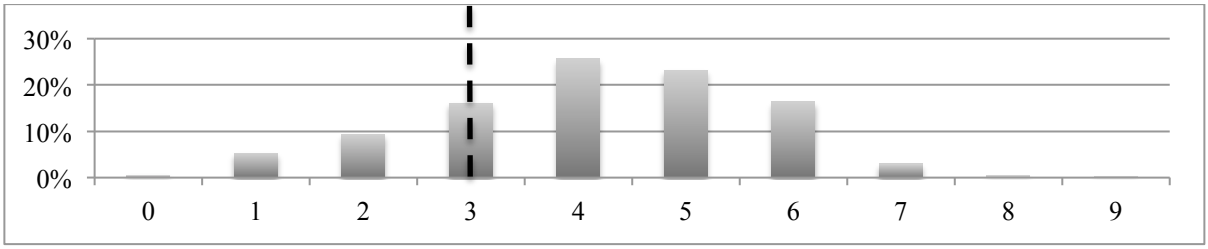
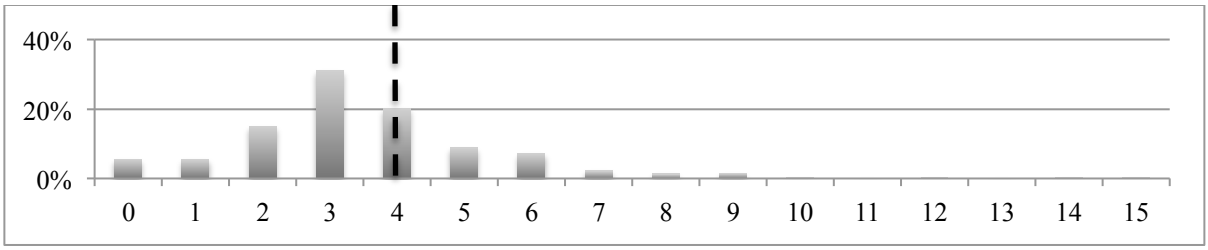


Figure 2: Number of ANC Visits by Women in Western Kenya (DHS 2008-2009)



While skilled delivery in a health center is believed to be the best strategy to reduce maternal mortality, many benefits from ANC exist, including improved growth and survival as well as reduced risk of infection of the baby (Campbell 2006).[2] Additionally, the WHO estimates that between one third to one half of all maternal deaths are from causes related to inadequate care during pregnancy.[3] Furthermore, studies have documented a positive association between ANC visits and a woman's decision to deliver with a skilled practitioner (Brown 2008).

While evidence suggests that ANC is associated with improved maternal health outcomes, the lack of consistency in the quality of services provided could diminish the benefits. The 2010 Kenya Service Provision Assessment reported that quality of services is a concern for maternal health care; only 25% of facilities that offered ANC had all essential supplies.[4] Self-reported endline survey responses from women in our study confirm that challenges in provision of

[2] Other studies have shown that antenatal care is associated with reductions in pre-term labour, low birthweight, and perinatal death (Brown 2008).
[3] The life-threatening conditions ANC can help treat include hypertension, antepartum haemorrhage, malaria, HIV/AIDS, and anemia (Lawn 2006).
[4] The components considered essential for ANC include the tetanus toxoid vaccine, iron and folic acid tablets, blood pressure apparatus, and foetoscope (KSPA 2010).

maternal health services do exist in Vihiga District.[5]

2.2 Traditional Birth Attendants

TBAs are community-level agents similar to but less skilled than midwives; TBAs typically work in villages either out of their own homes or the homes of their clients to provide both pre-delivery and delivery services to pregnant women. 96.2% of TBAs have clients in their own village, facilitating the TBA's provision of care within the village. The average value and quantity of TBA services are reported in Figure **3**. This average value includes the value from in-kind payments. TBAs most frequently report in-kind payments of a Chicken/Hen, but also report payment in the form of Buckets, Baskets, Beans, Dresses, Maize, Soap, and Sugar.

Figure 3: Traditional Birth Attendants' Average Service Values and Quantities

	Value per Service	**Number of Services**
Pre-delivery Visits[6]	48 KSH (~ $0.6 USD)	3.50 in past month
Delivery[7]	365 KSH (~ $4.4 USD)	1.60 in past 3 months

The Kenyan government trained TBAs in the 1970s and 1980s when the shortage of skilled healthcare practitioners was even more severe than it is today; however with the expansion of health facilities in Kenya[8], the government now promotes facility-level pregnancy care. The minimal training provided to TBAs has now stopped, so knowledge is mainly passed down from one TBA to another. Today, less than half (48.7%) of TBAs have been trained by any method. Of those trained, 59.9% were trained by a government organization; 9.1% by a Nongovernment organization or Private Organization; 10.3% by a Family member; 19.4% by an Existing Traditional Birth Attendant; and 1.3% by an another community member. Even considering the quality challenges at the government facilities, the services offered by the TBAs are deficient

[5] Only 27% of women in our study had their height measured at least once; only 66% had been counseled on pregnancy complications at least once; only 69% had been provided folic acid supplements at least once; and only 42% had been provided a deworming pill at least once. However, other ANC visit indicators proved encouraging, with over 85% of women reporting to have given blood and urine samples at least once, between 81% and 98% of women reporting to have been counseled on various topics related to pregnancy care, 94% of women reporting to have received an insecticide treatment mosquito net, and 90% of women reporting to have received malaria prophylaxis and tetanus toxoid vaccine at least once.

[6] Average based on value of services in past 30 days and number of services provided in last 30 days.

[7] Based on what the TBA charges generally for a delivery.

[8] One source estimates that the number of health facilities in Kenya quadrupled between Kenya's independence in 1963 and the late 1980s. Kenya: The Big Picture on Health available at http://www.smartglobalhealth.org/pages/kenya-mission/kenya-health.

compared to those facilities in a number of ways.[9]

Despite being previously trained by the government, TBAs are not affiliated with the facilities and operate as their own private providers of maternity care. The private provision of TBA services suggests that competition could exist between TBAs and the formal clinics. Competition is also likely between TBAs since the average number of TBAs in a village is 2.9.

While TBAs provide delivery assistance throughout Kenya, they are utilized heavily in the Western region. In the Western region, the TBA-assisted delivery rate is 45.0%, the second highest rate out of all regions in Kenya and substantially larger than the national average of 27.6% (Kenya DHS 2010). The use of TBAs is not limited to Kenya or even to Africa; Byrne 2011 provides a review of TBA interventions in 20 countries.

Part of the reason various programs have attempted to work with TBAs is the prime position the TBAs hold as healthcare practitioners within the villages. Other local agents, for instance CHWs, could similarly be utilized; however, CHWs' broad focus on general health programs, such as hand-washing, vaccinations, and malaria-prevention campaigns, etc., diminishes their potential to encourage utilization of pregnancy services. The TBAs' specialization in pregnancy combined with their local base of information makes them ideally suited to encourage prenatal care at government health facilities.

3 Theory

With 37% of women in the sample reporting to have a TBA, encouraging clinic attendance is an easy way for a TBA to earn extra income, assuming mothers' payments to TBAs for their services remain the same. TBAs already identify women in need of delivery and prenatal services as part of their primary activities. This extra payment for encouragement alters the decision for situations where the TBA was previously indifferent or only weakly motivated for or against recommending ANC.

However, the TBAs' beliefs about the risk of client loss for delivery will in part determine her

[9] An ANC visit at a government facility will provide the following services that a TBA cannot: blood tests and profiling, mosquito nets to prevent malaria, intermittent preventive therapy for malaria, vaccines and vitamin supplements, deworming treatment, and information related to warning signs during the pregnancy and the delivery process.

participation in the program. Even though the incentive payment, described in more detail below, was a nontrivial amount (it represented approximately 25% of what a TBA would receive for a delivery and double what the TBA would receive for conducting her own pre-delivery visit), there was no guarantee that the TBAs would respond to it. A treatment TBA had to weigh the benefit from the incentive payment against the potential loss of delivery revenue from the woman's current and future pregnancies.

The timing of encouragement may influence the TBAs' beliefs about the risk of client loss. If the TBAs are averse to the potential loss of clients and perceive the threat of losing their client to a clinic delivery as being greater when the delivery date is near than at the beginning of pregnancy they might choose to encourage less frequently the closer the woman is to her expected date of delivery. On the other hand, if the TBA has cultivated a strong relationship with the woman by the time the woman is in the advanced stages of her pregnancy, the risk of client loss could be greater in the earlier stages and the TBA would encourage women later in their pregnancies.

The cross-randomization between the Voucher program and the TBA program provides an opportunity for analysis of complementarities between these programs. I will explore whether the TBA program works for all women in the Voucher program or only a subset. Specifically, if the voucher program only works for women who have a voucher, evidence exists that TBAs concentrated their encouragement on women who received vouchers or that voucher-control group women were not receptive to encouragement by the TBAs.

There are various reasons why the TBA might focus on encouraging women with a voucher. First, if the TBA knows that women with a voucher are more likely to go to the clinic for maternity care (including delivery) and the TBA is worried about losing encouraged clients to the clinic, the TBA might fail to encourage voucher-control group women so as to keep these women as her clients. Second, the TBA might focus on encouraging voucher recipients since the possession of a voucher (and resulting increased likelihood of attending the clinic) makes for a better return on the TBA's encouragement. Third, the fee structure of the incentives and service payments combined with potential indifference amongst voucher-control group women could make the encouragement of these women not profitable for the TBA. For example, if the voucher-control group women are indifferent between going to the clinic and not, the TBA might

have to offer to split the incentive payment with the woman to induce her to go. Since the incentive payment is roughly double what the TBA would receive for conducting her own pre-delivery visit, by splitting the payment with the woman, she would take home approximately the amount she would get by providing a pre-delivery service herself.

4 Experimental Design

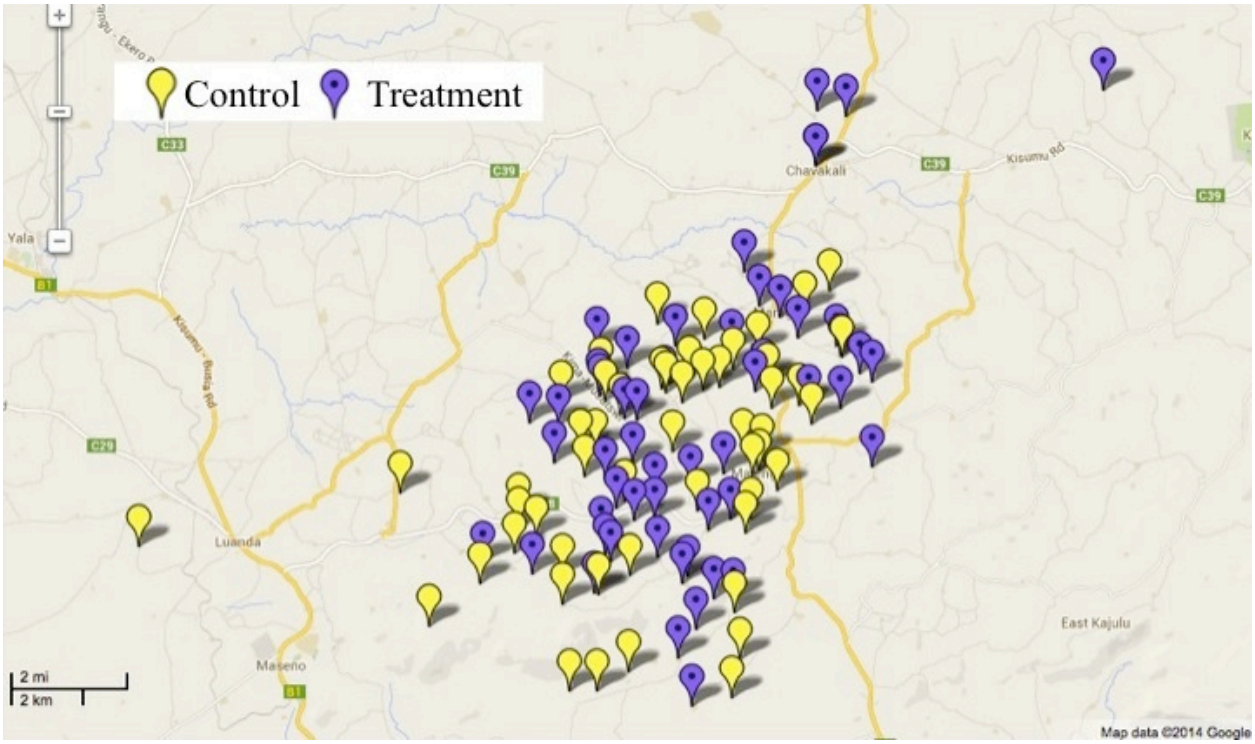
The ideal experimental design would have allowed treatment TBAs to be paid for encouraging their specific clients to the clinic. However, identifying the match between each TBA and her clients was not feasible. At baseline, 12.3% of women knew of a TBA. Due to the low response rate, it was not possible to know which women were associated with specific TBAs. Individual level randomization of the TBAs also presented challenges since there were multiple TBAs in a village, making spillovers likely. I thus employed village-level randomization.

The study was undertaken in all 172 villages of Vihiga District. Village level randomization assigned the TBAs into two groups of equal size: the treatment group and the control group, see Figure **4** below. This village level randomization was orthogonal to the separate Voucher program treatments, which were randomized at the individual woman level. Because the Voucher program began before the start of the TBA program, 457 pregnant women delivered or were estimated to have delivered[10] by the start of the TBA program, leaving 1,141 pregnant women in the TBA study.

A TBA assigned to the treatment group received 101 KSh[11] (approximately $1.20) for each ANC visit made by her clients. The maximum number of payments was four per pregnant woman since four ANC visits is the WHO recommended number. Two visits by the same women must have been at least four weeks apart for the TBA to receive payment. This was to avoid situations where the TBA encouraged a woman to go to the ANC four times in the same week, just to receive payments for four visits.

[10] A woman was estimated to have delivered if her gestation at the start of the TBA program was 42 weeks or more.
[11] 101 KSh was chosen as the incentive payment as this amount was the minimum that could be sent to a phone not registered with the mobile money provider. This amount ensured that all TBAs could receive their incentives.

Figure 4: Map of Treatment and Control Villages



The payments for encouraging the women to attend the clinics were only made if the woman actually attended the clinic and was from the same village as the TBA. This restriction helped to prevent spillovers of the TBA treatment reducing the size of the control group. If treatment TBAs could encourage women in all villages, the control group would be contaminated since any woman could have been encouraged by a treatment TBA. Additionally, this design reduces the risk of collusion between the treatment TBAs and the control group TBAs. Without the same-village client restriction, a TBA in the control group could instruct her clients to provide a treatment group TBA's identity during the check-in process at the facilities and the two TBAs could split the payment. By safeguarding against this program manipulation, the study design maintains the integrity of the treatment group and control group.

The restriction that the encouraged client must be from the TBA's same village may induce TBAs to focus their efforts on some clients at the expense of others. 88% of TBAs work in villages besides their own; TBAs work in 1.9 other villages on average. If it proves easy to encourage women to the clinic, the TBA might spend a minimal amount of time with women in her own village, choosing to send these women to the clinic, so that she can concentrate her time

on providing services to clients outside of her village. By doing so the TBA would be able to earn money from encouragement while still keeping clients in other villages for delivery. Or, if encouragement is difficult and requires the TBA to concentrate her time on women from her village, she might spend less time with clients in other villages. Either way, the random assignment ensures that the measured effects are unbiased.

From June to August 2013, we employed CHWs to recruit all of the Traditional Birth Attendants in Vihiga District into the TBA encouragement program, 474 in total. CHWs' experiences as government employees working on health campaigns have made them aware of which individuals are birth attendants. 86 villages were assigned to each of the treatment and control groups. 11 of the 172 villages had no TBA, and of the 474 TBAs recruited 231 were in treatment villages. The TBAs were informed of their assignment into the treatment or control groups at the end of the recruitment survey. While all of the TBAs living in a treatment village were assigned to the treatment group, the CHWs and the TBAs did not know that the assignment was on the village level.[12]

The TBAs were informed that they would only be paid if the women were from the TBAs' own villages and if the women provided the TBAs' names to staff at one of the public health facilities. Additionally, the TBAs would only be paid for attendance by women who were participating in the Voucher Program (and had unique patient numbers assigned from that program). This restriction ensured that payments would be provided to the TBAs only for actual women, preventing program manipulation whereby the TBA claims to have encouraged women when in fact the encouragement was fake. Since virtually all pregnant women (95.3%) in Vihiga participated in the Voucher Program, this restriction does not introduce any significant biases.

In the first months of the program, few pregnant women reported names of TBAs at the time of the clinic visits, which made it difficult to make the incentive payments. Various explanations exist for this behavior. First, since they know that health facility staff discourage the use of TBAs, pregnant women might feel stigmatized about naming a TBA when they check in to the health facilities. Second, the TBAs might not have responded to the program so there was no TBA name for the women to provide. Third, discrepancies between two sources of data might

[12] This procedure minimized the risk that CHWs would recruit friends, who were not actually TBAs, to the treatment group.

explain the infrequent reporting of TBA names.[13] Finally, there may have been data limitations with capturing the names of the birth attendants, which would enhance the minimal TBA-naming.[14]

To strengthen the TBA incentives, in October 2013 the intervention was changed so that when a woman from a treatment village made an ANC visit, all of the TBAs from that village were paid, no matter which TBA name, if any, the woman provided. The intervention still maintained the cap at four visits and the four-week spacing requirements. All of the treatment TBAs were notified of this change both by SMS message and individual phone calls. Each TBA still received 101 KSh, the same amount as before, making the program change a more expensive way to approximate the effect of the incentive.

5 Data and Implementation

5.1 Data

The study utilizes data collected from three different survey instruments. First, as part of the Voucher Program, recruited pregnant women answered baseline questions related to basic demographics, socioeconomic status, housing, health care information related to current and previous pregnancies, health insurance coverage, and financial characteristics. Second, an endline survey of pregnant women was conducted after their delivery and postnatal visits. Part of the data captured in this endline survey is information related to their ANC, delivery, and postnatal utilization, both at the clinic and with a TBA. Third, TBAs were surveyed directly upon recruitment into the encouragement program, capturing information on background, socioeconomic status, financial experience, as well as more professional information on the person's role as a TBA.

[13] The data indicating which TBA provided encouragement was captured in a clinic survey of pregnant women and was separate from the visit data. The survey data appear to be missing in some instances. 5.5% of the women captured in the visit data do not appear in the TBA encouragement data from the survey. Furthermore, of the women who are captured in both datasets, the TBA encouragement data captured fewer observations per woman 24.4% of the time.

[14] Although every recruited TBA was added to the survey that the pregnant women answer at the clinic, the names were entered by the first name/last name order given during the baseline survey of TBAs. In Kenya, the first names and last names are sometimes reversed. If the woman provided a different TBA name order when surveyed, the facility staff might not record the right TBA; in this case the woman would have appeared in both the survey and visit data but the TBA encouragement would not have been appropriately matched.

Additionally, we use clinic utilization data collected from three sources. Hired staff at the facilities captured data on ANC, delivery, and postnatal visits made by Voucher Program participants using a computerized healthcare information management system. The hired staff also administered a brief survey to all women at the clinic. Finally, the clinics maintained records of visits through handwritten entries in their ANC and delivery register books.

One concern with the data captured on the computerized system is the potential for missing women who were in the Voucher Program control group. If a woman received a voucher, she had to be registered in the system in order to use the voucher. Women in the control group of the Voucher Program had no incentive to register. The resulting potential bias should not affect the evaluation of the TBA program due to the village level randomization performed for the TBA program. There is, however, a chance that the requirement that women provide the name of the TBA could have increased the incentive for Voucher Program control group women in TBA treatment villages to register with the hired staff. This creates a risk that Voucher Program control group women in TBA treatment villages are overrepresented compared to Voucher Program control group women in TBA control villages. If this was the case, the TBA program should yield larger effect sizes for women in the Voucher Program control group. As will be discussed below, there is no effect of the TBA program for the Voucher Program control group women, mitigating concerns that Voucher Program control group women in TBA treatment villages are overrepresented.

Tables 1-3 present basic summary statistics from the baseline surveys of pregnant women and TBAs. Table 1 reveals that distribution of both the number of women per village and the number of TBAs per village was similar between treatment and control groups. With *p*-values based on standard errors that were clustered at the village level, Tables 2 and 3 reveal differences between the groups at levels that can be expected with randomization. For example, women in treatment villages were more likely to consider themselves the head of the household; and TBAs in control villages appear to be more experienced than TBAs in treatment villages, where the former have more years of experience and have conducted more deliveries.[15] Overall, these tables indicate that the randomization was balanced across groups.

[15] One control group TBA reported having conducted 30,000 deliveries, 30 times more than the next most experienced TBA. The finding that control group TBAs have conducted on average 79.88 deliveries and treatment TBAs have conducted 48.03, is exclusive of the 30,000 outlier.

5.2 Implementation

With support from officials within Kenya's Ministry of Public Health and Sanitation (MPHS), the program was administered at all eight public health facilities in Vihiga District: the three dispensaries and four health centres overseen by MPHS as well as the Vihiga District Hospital, which is overseen by the Ministry of Medical Services. While a woman could receive ANC at any of the health facilities, limited nursing and other medical staff often resulted in a significant wait for the women to be seen by the doctor or nurse. Surveys of women at the facilities indicate that across all facilities, the average time a woman spent from arrival to the clinic until departure was just over 3 hours, with some variation across facility type.[16]

Pregnant women were recruited for the Voucher Program between January and July of 2013, and implementation began in January 2013. TBAs were recruited between June and August of 2013, and implementation began in June 2013. Both programs ended in March 2014. On June 1, three days before the start of the TBA program, a new government policy regarding maternity care was introduced. Kenya's president Uhuru Kenyatta announced that day that free maternity services would be available immediately in all public health facilities (Daily Nation). However, since this policy affects women living in both treatment and control villages, the control village women remain a valid comparison group.

During the baseline survey, each TBA gave a mobile phone number on which to receive M-PESA[17] mobile payments. Afterwards, their phone numbers, villages, and group assignment were uploaded to a server. As pregnant women visited health facilities, staff stationed at the facilities uploaded the TBA's name and pregnant woman's unique identifier to the server.[18] The server performed numerous checks and if an eligible visit occurred, it immediately sent the TBA an SMS indicating that payment would be made. On the Tuesday of every week, the payments from the previous week were made by M-PESA to the TBAs.

[16] Women who visited dispensaries had the shortest time spent at the clinic at approximately 1.25 hours. With just over 3.5 hours spent at the clinic, the patients at health centres spent the longest amount of time. Women visiting Vihiga District Hospital spent about 2 hours at the clinic.
[17] M-PESA is a mobile money service from Safaricom. It enables people to send money with their mobile phone. You can deposit money, withdraw money, send money to any mobile phone, and pay bills (Jack 2010).
[18] At recruitment into the program, the women were assigned unique identification numbers. Should a woman forget her number, staff at the facilities can look up the woman in a secure web portal using her National ID.

6 Empirical Framework

The empirical framework for this study exploits the randomized assignment of Traditional Birth Attendants to treatment and control groups. I use their random assignment to examine the average intent-to-treat effect stemming from the TBAs simply being assigned to the treatment group.

Estimating a treatment-on-the-treated effect is not feasible in this setting due to the unobservability of the TBA's actions. After randomly assigning TBAs to the program, I cannot guarantee that they will actually encourage women to visit an ANC clinic. Although we survey women who attend the clinics and ask if a TBA encouraged them, this does not represent the TBA's participation in the program. It could be the case that a TBA exerts effort encouraging women to go to the clinic and the women either do not go or do not report the TBA's name at the clinic. All that was observed was if the woman went to the clinic and gave the name of a TBA, but this data constitutes part of my *outcome* data on clinic utilization.[19]

In my analyses, I focus on two dependent variables related to clinic utilization. The first is an indicator that a woman attended at least the recommended number of ANC visits.[20] Comparison of the recommended number of visits calculated for women in the treatment and control groups reveals no difference between the two groups. That is, women in the treatment group were just as likely to have a recommended number of visits of 0, 1, 2, 3, or 4 (depending on her stage of pregnancy at recruitment), as women in the control group. For analyzing this binary variable, I report estimates from both ordinary least squares (OLS) and probit regressions. I focus on the linear probability estimates from the OLS regressions since these makes no distributional assumptions.

The second dependent variable I considered is the number of ANC visits a woman attends, which I analyzed using OLS and negative binomial regression methods. The preferred method to

[19] One possible indicator of "take-up" is ownership of a mobile phone and registration with M-PESA. However since virtually all TBAs had a phone and were registered, this variable provides little explanatory power.

[20] The recommended visit calculation is based on MPHS's recommendations that a woman attend her 1st ANC visit before week 16, 2nd ANC visit between the 16th and 26th week, 3rd ANC visit between the 26th and 34th week, and her 4th ANC visit after her 34th week. Her gestation is calculated using her expected date of delivery, the date of her last menstrual period, or her estimate of her gestation, which were captured at baseline.

account for the fact that the dependent variable is an overdispersed[21] count variable is the negative binomial regression method. For both the binary and count variables, I perform a difference-in-means analysis using the clinic utilization data from the period after the start of the TBA program until the end of the woman's pregnancy. To complement my difference-in-means findings, I perform a difference-in-differences analysis where I use the clinic utilization data from recruitment until the start of the TBA program as well as the data after the start of the TBA program.

All analyses unless otherwise specified are based on data from the computerized system. I estimate versions of the following regression equations with no controls and with two different combinations of controls. The first set of controls include the Voucher Program assignment, and the second set combines these with characteristics such as age, years of schooling, transport time to the clinic, head of the household, number of household members, a wealth index measure, and gestation at the start of the TBA program.

6.1 Difference-in-Means

My analysis starts with a simple comparison of sample means captured in the following regression equation.

$$Y_{iv} = \alpha + \beta T_v + \varepsilon_{iv} \quad (1)$$

Y_{iv} represents the two dependent variables: 1) an indicator that equals one if woman i in village v attends her recommended number of ANC visits from the start of the TBA program until the end of the woman's pregnancy[22] and 2) the count variable of the number of ANC visits woman i in village v attends after the start of the TBA program; T_v, the random assignment of TBAs, is an indicator that equals one if the woman is in a village where the TBAs were in the treatment group. β is the coefficient of interest representing the intent-to-treat effect of the TBA encouragement program.

[21] A test of overdispersion eliminates Poisson regression as a viable means of analysis for the count data.
[22] I calculated the recommended number of ANC visits each woman should make after the start of the TBA program based on either 1) her gestation at the start of the TBA program if she was recruited prior to the TBA program start or 2) her gestation at recruitment if she was recruited after the start of the TBA program. I calculated recommended visits equal to 4 ANC visits if she was less than 16 weeks pregnant; 3 visits if she was between 16 and 26 weeks pregnant; 2 visits if she was between 26 and 34 weeks pregnant; 1 visit if she was between 34 and 38 weeks pregnant; and 0 visits if she was more than 38 weeks pregnant.

6.2 Heterogeneous Effects

Delving into sources of heterogeneity in response to the TBA treatment assignment helps to clarify which characteristics are conducive to the TBA program. By informing us of the characteristics of the places in which the TBA program is most effective, a heterogeneous effects analysis would help determine program placement if it would be possible to initiate the TBA program only in areas where it would be most effective. Utilizing the difference-in-means specification, I interacted various village-level aggregates of baseline characteristics of the TBAs with the TBA treatment assignment. The regression below specifies the heterogeneous effects set up for generic characteristic X_v.

$$Y_{iv} = \alpha + \beta T_v + \theta T_v * X_v + \delta X_v + \varepsilon_{iv} \quad (2)$$

6.3 Effect of the Program Change

To account for the TBA encouragement program change that occurred in October 2013, the empirical strategy makes use of an overlapping indicator structure. Starting in October, all TBAs in a treatment village were paid if any eligible woman from that village visited the clinic, no matter which TBA name, if any, the woman provided. The overlapping indicator structure is captured below.

$$Y_{iv} = \alpha + \beta T_v + \gamma T_v * Prog\_Change_i + \delta Prog\_Change_i + \varepsilon_{itv} \quad (3)$$

As before β represents the intent-to-treat effect of the program. $Prog\_Change_t$ is an indicator that equals one if the woman is still pregnant after the TBA program change.[23] Including the interaction between the TBA treatment assignment and the program change indicator gives γ the interpretation of the marginal effect of the treatment after the TBA program change.

6.4 Difference-in-Differences

Clinic utilization data collection began with the start of the Voucher Program, which was six months before the start of the TBA Program. Utilizing this data, I employ a difference-in-differences strategy to account for any time-invariant but unobserved difference between treatment and control groups that could bias the difference-in-means estimates. The difference-

[23] A woman is classified as being still pregnant if she has not yet delivered, or if her gestation at the time of the TBA program change is less than 42 weeks.

in-differences estimator is the difference between the value of the treatment villages and the value of the counterfactual after the TBA program. Abstracting from the program change, the regression version of the difference-in-differences estimation is represented below.

$$Y_{itv} = \alpha + \beta T_v + \gamma T_v * Post_t + \delta Post_t + \varepsilon_{itv} \quad (4)$$

Y_{itv} represents both dependent variables: 1) an indicator that woman i in village v attends her gestation-based recommended number of ANC visits in period t and 2) the number of ANC visits woman i in village v attends in period t. t is either before the TBA program start or after. $Post_t$ is an indicator that equals one if the time period is after the TBA program start. The coefficient of interest is γ, which represents the joint effect of being in a treatment village after the treatment started.

Figure 5: Sample Division by TBA Program Start

June 2013

	Before TBA Program		**After TBA Program**
A	Women Delivered Before TBA Start (Later in Pregnancy)		
B	Women Who Were in Both Periods (Earlier in Pregnancy)	C	Women Who Were in Both Periods (Later in Pregnancy)
		D	Women Recruited After TBA Start (Earlier in Pregnancy)

There are various samples of women that I could use for this purpose, depicted in Figure **5**. For instance, I could use only the panel of women who were in both the period before the start of the TBA program and the period after the start of the TBA program (Groups B and C). However, although they are the same women, women in Group B might not serve as a valid comparison group for women in Group C due to the different pregnancy stages experienced. If anything, it might make more sense to compare Group D women to Group B women, since women in both of these groups were in the earlier stages of their pregnancies. However, similarly I might want to include Group A women because those women are later in their pregnancies and might serve as a more valid comparison group for Group C women. For the difference-in-difference results that I show, I use all of the available data from both before and after the start of the TBA program (that

is I use Groups A, B, C, and D).

The focus is again on two dependent variables, with two values for each woman, one before the start of the TBA program and one after. The first dependent variable is the indicator that the woman had attended at least the recommended number of ANC visits either before or after the start of the TBA program.[24] The recommended number of ANC visits each woman should make before the start of the TBA program was based on the amount of time from her recruitment until the start of the TBA program. As an example, if before the start of the TBA program a woman was 17 weeks pregnant when recruited, she would be expected to make 3 visits before her delivery. How many of these 3 visits are expected before and after the beginning of the TBA program depends on the timing of her recruitment. For example, she might be expected to make 1 visit before the start of the TBA program and 2 after. If she attended 1 visit before the start of the TBA program and 1 visit after, the value of the indicator would be 1 before and 0 after. The second dependent variable is the number of ANC visits a woman attended before the TBA program and the number of ANC visits attended after the TBA program.

7 Results

7.1 Difference-in-Means

Table 4 displays the results of the estimates of equation (1) for the binary dependent variable that indicates the woman went to the recommended number of visits in the period after the start of the TBA program until the end of her pregnancy. Living in a village assigned to the TBA treatment results in a substantial increase in the probability of attending the recommended number of visits. At the 10 percent significance level, I find that the probability of a woman attending the recommended number of visits is approximately 4.4 percentage points higher in treatment villages; this represents a 20.7% increase[25] over the control group's probability of attending the recommended number of visits. While columns (3), (5), and (6) in Table 4 have similar coefficients, they have *p*-values of 0.13, 0.11, and 0.13, respectively, indicating that those specifications are on the threshold for significance. Finding a substantial intent-to-treat effect of

[24] To calculate the recommended number of visits a woman should make before the start of the program, I utilized her gestation at recruitment and her gestation at the start of the TBA program. The recommended number of visits a woman should make after the start of the TBA program was calculated as described above.
[25] Calculated using the specification without controls.

the TBA treatment on the binary outcome variable suggests that incentives for TBAs to encourage antenatal care could be an important policy tool for achieving accordance with the Ministry of Health's visit guidelines.

Interestingly, the effect of the TBA program appears to work only for pregnant women who received a voucher as part of the existing Voucher Program. Table 5 includes the results for regressions that interact the TBA treatment with an indicator variable that equals one if a woman received either a voucher for her maternity care or a voucher for her transportation. The coefficient on TBA Treatment, which represents the effect of the treatment on women in the voucher control group, is not statistically distinguishable from zero. On the other hand, the joint effect of being in the TBA Treatment and having a voucher is positive and significant in the OLS regressions. This finding is evidence either that TBAs concentrated their encouragement on women who received vouchers, that voucher-control group women were not receptive to encouragement by the TBAs, or that TBAs did not find it profitable to encourage voucher-control group women due to the potential need for side payments to these women.

Another explanation for the lack of effect on the voucher-control group women relates to potential bias associated with incomplete data. By not receiving a voucher, the voucher-control group lacks an incentive to register with the computerized system and is not fully captured in the data. If there is a continuum of women with different proclivities to register and receptiveness to TBA treatment is related to the probability of registering, then downward bias in the treatment effect estimate could exist. For instance, suppose across treatment and control villages there are two types of women, those who always attend ANC (no matter whether the TBA encourages) and those who attend as a response to the TBA encouragement. Furthermore, suppose that women who always attend ANC always register whereas women who are responders to the TBA treatment, women easily influenced by the TBA, are *less likely* to register. In this situation, the computerized system would have data on voucher-recipients who always attend (across both treatment and control villages) as well as voucher-recipients from treatment villages who respond to the treatment. Despite being less likely to register, the voucher provides an incentive for the responders to register, so we find an effect of the TBA program. On the other hand, regarding the voucher-control group participants, the computerized system has only the data from both treatment and control villages on those who always attend, and whose type specifies

they also always register. Absent a voucher, that is absent the incentive to register, responders from the TBA treatment villages, whose type makes them less likely to register, may attend the clinic without registering, resulting in incomplete data and effect estimates biased downwards. Nevertheless, because of the cross-randomization between the voucher and TBA programs, my results hold on just the women with vouchers.

Estimates of equation (1) for the number of ANC visits made by the woman after the start of the TBA program are presented in Table 6. The OLS estimates indicate that women in the control villages went to on average 1.49 visits in the period after the start of the TBA program. However, the change in the number of visits with respect to the TBA treatment is both numerically small and statistically not distinguishable from zero. The negative binomial regression models the log of the expected number of visits as a function of explanatory variables. In this specification, the coefficients indicate that the difference in the logs of expected number of visits between women in treatment villages and control villages is expected to change by about 0.008, which is not statistically distinguishable from zero. The lack of significance of the estimates in Table 6 reveals that as modeled there is no effect of the TBA program when comparing the mean number of ANC visits made by the TBA treatment versus control groups.

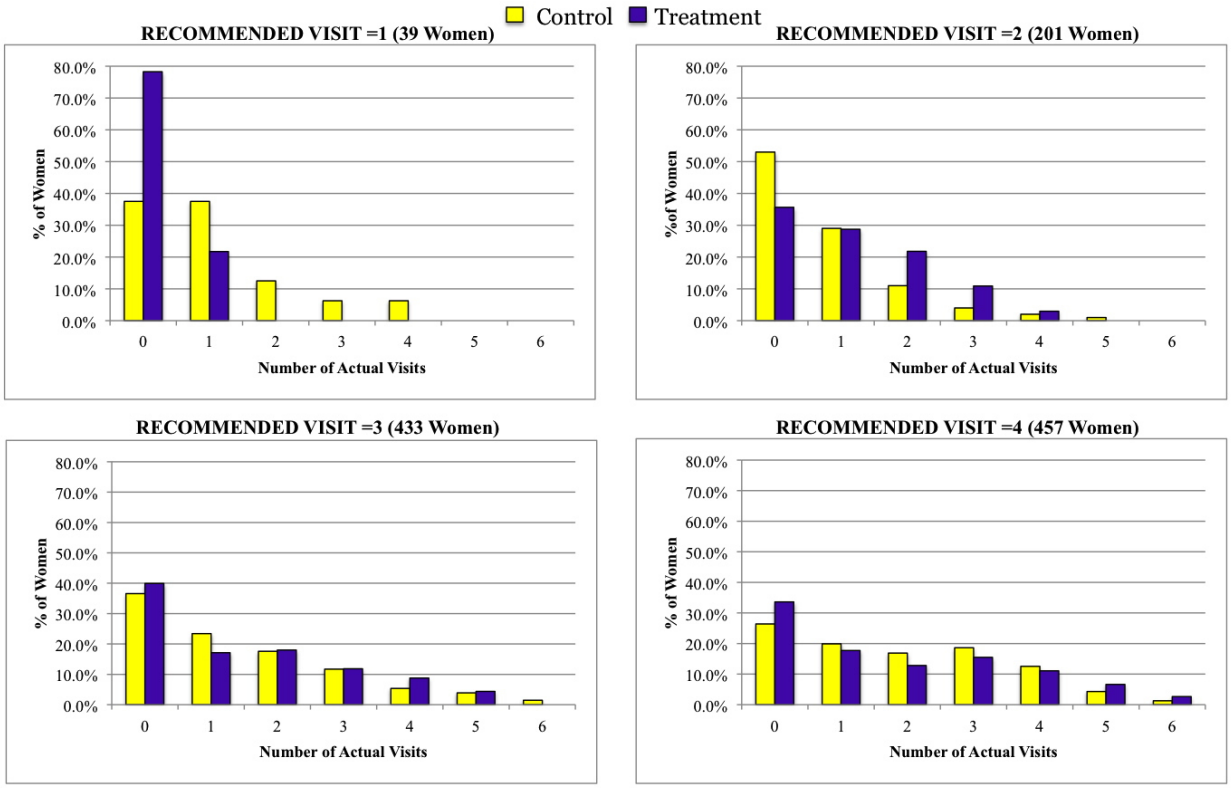
As a first step in exploring why there is an effect for the binary variable but not count variable, I ruled out bias in the calculation of the women's recommended visits.[26] Women in the treatment group were as likely as women in the control group to have recommended visit numbers of 0, 1, 2, 3 or 4. Additionally, women in the treatment group were as likely as women in the control group to have delivered prior to the start of the TBA program and were as likely to have been recruited after the start of the TBA program.

Next since the count variable results revealed that women in the treatment and control groups went to a similar number of ANC visits on average, I considered the distribution of visits by recommended visit number. Figure **6** reveals that the positive treatment effects are driven by women who had two recommended visits, women who were between 26 and 34 weeks pregnant at the time the TBA program started. Furthermore, Figure **6** reveals that negative treatment effects exist for women who had one recommended visit, women who were between 34 and 38

[26] Analyses available upon request.

weeks pregnant at the start of the TBA program. Finally, Figure **6** suggests that there was no treatment effect for women who had 3 or 4 recommended visits.

Figure 6: Visit Distributions by Recommended Visits Number



Tables 7 and 8 correspond to the graphical representations in Figure **6**. Table 7 shows results for the interaction between TBA treatment assignment and having a recommended visit of 2. The positive joint effect suggests that the TBA treatment increases the number of ANC visits for women living in treatment villages who had a pregnancy gestation between 26 and 34 weeks at the start of the TBA program. Contrastingly, Table 8 reveals that the TBA treatment decreases the number of ANC visits for women living in treatment villages who had a pregnancy gestation between 34 and 38 weeks at the start of the TBA program.

With only 39 women making up the group of those with a recommended visit value of 1, I first examine whether differences between the treatment and control groups are responsible for the negative treatment effect amongst these women. Specifically, for the subset of women with 1 recommended visit, I consider whether TBA control group women are more likely to have a

voucher than TBA treatment women; if that was the case, then the biased sample might explain the negative treatment effect for this group. Although statistically there was no difference between the 16 control women and 23 treatment women in terms of their voucher compositions, a higher fraction of control group women had maternity vouchers and transportation vouchers.[27]

As a possible explanation for the positive effect of treatment for those with 2 recommended visits and negative effect of treatment for those with 1 recommended visit, I make use of the fact that all TBAs who provide pre-delivery services report encouraging their last client to the health facility for ANC. If we assume TBAs, even those in the control group, do some encouraging and that there is a fixed capacity to encourage, then when treatment TBAs begin to be paid for encouraging, they might choose to encourage women who have more potential visits available (ie. a higher recommended number), since they can earn money for each visit the woman makes. Additionally, the TBA may be concerned about encouraging a woman with only 1 recommended visit if by doing so the woman subsequently delivers in a clinic instead of with the TBA. This client loss concern might cause the TBA to focus encouragement on women with 2 recommended visits as opposed to women with only 1. However, these explanations do not shed light on why I do not find an effect of the treatment on women with 3 or 4 recommended visits.

7.2 Heterogeneous Effects

Heterogeneity existed for the regressions of whether the woman went to the recommended number of visits. The results from equation (2) are displayed in Table 9, which list the interacted variable underneath the regression results. Each of the characteristics X_v are indicators, which equal one if the village average is greater than the average of the value across all villages. Each column in Table 9 represents a regression with a different single interaction term.[28] Interactions based on these various village averages of TBA characteristics revealed that the TBA program worked more successfully in places where the TBAs had an outside option besides working as a

[27] Of those women who had a recommended visit value of 1, 62.5% of TBA control group had a maternity voucher, compared to 47.8% for the treatment group; similarly 62.5% of TBA control group had a transportation voucher, compared to 52.1% for the treatment group.

[28] Columns (6)-(10) are indicators based on TBAs' subjective poverty ratings of their consumption as just or more than adequate along the dimensions of food, housing, clothing, health, and children's schooling. The qualitative assessments of perceived consumption adequacy were based on survey questions designed by Menno Pradhan and Martin Ravallion (Pradhan 2000).

TBA. I found that women appeared to respond more to the TBA program if the women were in villages where the TBAs were more educated, less likely to be trained as a TBA, and less experienced. Overall these results suggest that if program placement could be targeted, the results would be better if targeted to villages whose TBAs have options besides working as a birth attendants.

7.3 Effect of the Program Change

Table 10 contains the OLS and probit regressions for equation (3), which interacts the treatment assignment with an indicator that the woman was still pregnant after the TBA program change. The dependent variable in Table 10 is the indicator variable that the woman attended the recommended number of visits after the start of the program. The coefficient on the TBA treatment assignment is positive and significant in all specifications. However the coefficient on the interaction of the TBA treatment and the indicator for the program change is statistically indistinguishable from zero.

One explanation for the lack of effect of the encouragement after the program change is that the TBAs were already exerting effort before the program change. The change was a more expensive means of delivering the incentive. As such, the lack of a marginal effect is not surprising given that there was an effect of the TBA program; TBAs were already trying and women were already responding before the change, despite the minimal reporting of TBA names. The discovery that the alteration of the program had no marginal effect could simply be evidence that the change did not pollute the program.

Furthermore, the change suggests segmentation in the market for TBA services. Had the program change induced a TBA to encourage all women in her village, not just her own clients, we might expect a positive marginal effect since women would have received multiple sources of encouragement. However, if the TBA market were segmented, each TBA would only encourage her specific clients, and as we find, there would be no marginal effect of the program change.

7.4 Difference-in-Differences

Table 11 includes the results of the OLS and probit difference-in-differences regressions from equation (4) for the indicator variable. The coefficient on the interaction of TBA treatment and Post (the indicator for the start of the TBA treatment) represents the difference-in-differences

estimator, and it is significant at the 10 percent level. This difference-in-differences estimator indicates that being in a TBA Treatment village after the start of the TBA program increases the probability of attending the recommended number of ANC visits by approximately 6 percentage points. The reason for the high significance and positive coefficient of the constant and the high significance and negative coefficient of Post is related to the distribution of the recommended visits. Before the start of the TBA program the recommended number of visits a woman should go to was on average .72; afterwards the recommended number was on average 3.28. So before the TBA program, the control group was more likely to attend the recommended number, and afterwards was less likely to attend the recommended number. The results are consistent no matter which combination of women, as shown in Figure **5**, I use,[29] with the exception that the effect is not significant when I use only the panel of women who were in both the period before and after the start of the TBA program; the lack of significance when using the pure panel of women is likely due to the fact that that sample excludes 288 women who were recruited after the start of the TBA program and thus had a chance to be affected by the treatment.

Table 12 illustrates the effect of the TBA program on the number of visits a woman attends. The coefficient on TBA Treatment interacted with Post was insignificant in all but one of the regressions; thus as with the difference-in-means, there appears to be no effect of the TBA program on the number of visits attended. Furthermore, the significance in column (4) is driven by the difference between TBA treatment and control women before the start of the TBA program and is not robust to the sample of women, based on Figure **5**, used. In fact, the finding of a negative and significant coefficient on TBA Treatment similarly lacks robustness. It appears that before the program only women in treatment villages who delivered before the start of the TBA program went to fewer visits than women in control groups. Excluding the women who delivered before the TBA program start (Group A in Figure **5**) in the difference-in-differences specification results in no difference between treatment and control groups before the TBA program. The high significance of post, an indicator that the TBA treatment began, represents a time effect; after the start of the TBA program, even women in the TBA control went to more ANC visits. Two explanations exist for the time effect. First, if over time, women developed

[29] The possible combinations include using all the women (A, B, C, and D), using only the panel of women who were in both periods (B and C), or using all women except for the ones that delivered before the start of the TBA program (B, C, and D).

more trust in the separate Voucher Program, this time effect might be explained by the simultaneous implementation of that program. Second, the announcement of free maternity care by Kenya's president could be responsible for the increased utilization by women in the control group.

7.5 Effect on Delivery Outcomes

Although the financial incentives to the TBA were only for encouraging ANC visits, the program had the potential to modify pregnant women's utilization of formal delivery services. Women, who attended the ANC clinic due to the TBA's encouragement, may have been more likely to attend delivery services as well. Understanding the effect of the TBA program assignment on delivery outcomes is of interest both to see changes in women's clinic utilization for delivery as well as the TBA's composition of clients for delivery. If the program has an effect on delivery outcomes, there is evidence that there are spillover benefits from financial incentives to encourage ANC visits. However, if more women attend the clinic for delivery, the TBA will likely lose clients for delivery services. This potential delivery client-loss may make the TBA program not sustainable in the long run, if TBAs stop participating for fear they will lose clients. To analyze the effect of the TBA program assignment on delivery outcomes, I utilized the difference-in-means set up where the dependent variable was an indicator that equaled one if the woman delivered in a health facility (even one outside of Vihiga District). Employing women's endline survey responses for data on the delivery locations, regression analysis, displayed in Table 13, did not indicate any effect of the TBA treatment assignment on delivery in a health facility. The absence of an effect on delivery outcomes indicates that the ANC encouragement incentive does not spill over to delivery clinic utilization and that the risk of client-loss due to the TBA encouragement program is minimal.

8 Conclusion

The results suggest that locally informed intermediaries can be exploited to change the health-seeking behavior of pregnant women in rural Kenya. The program generates intent-to-treat effects on the margin of attending at least the recommended number of visits, but only for women who have received a voucher as part of the existing Voucher Program. Furthermore the effect is driven by women who have 2 recommended visits, women whose gestation at the start

of the TBA program is between 26 and 34 weeks. Examination of the heterogeneous effects indicates that the TBA program was more successful in places were the TBAs on average were more likely to have outside options; the program worked better in places where the TBAs' average years of schooling was higher, where the TBAs were less likely to be trained as a TBA, and where they had fewer years of experiences as a TBA. Analysis of the program change reveals no marginal impact of the modification. The success of the TBAs in inducing some pregnant women to attend ANC visits is an indication that cultural norms regarding ANC care can be overcome with relatively modest financial incentives.

These results apply to the subpopulation of Vihiga District. The randomization ensures that we can examine the causal impact of the intent-to-treat in this region, but since the program was only implemented there it will be unclear if the results could extend to Kenya as a whole or even just the western region of Kenya, which encompasses Vihiga District. Nevertheless these findings are informative for helping to determine if any potential treatment effect does exist.

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Table 1: Village Characteristics by TBA Group

	Control		Treatment		
	Mean	SD	Mean	SD	P-Value
Number of Villages with Pregnant Women	83		86		
Villages with 5 or less women (1st quartile)	0.27	0.44	0.29	0.46	0.71
Villages with 6-9 women (2nd quartile)	0.30	0.46	0.23	0.42	0.32
Villages with 10-13 women (3rd quartile)	0.27	0.44	0.24	0.43	0.76
Villages with 14 or more women (4th quartile)	0.17	0.38	0.23	0.42	0.30
Number of Villages with TBAs	82		81		
Villages with less than 2 TBAs (1st quartile)	0.22	0.42	0.20	0.40	0.73
Villages with 2 TBAs (2nd quartile)	0.32	0.47	0.31	0.46	0.91
Villages with 3-4 TBAs (3rd quartile)	0.33	0.47	0.36	0.48	0.70
Villages with 5 or more TBAs (4th quartile)	0.13	0.34	0.14	0.34	0.98
Number of Villages with Both Pregnant Women and TBAs	80		81		

Note: p-values are for tests of equality of the means across groups.
Based on standard errors that have been clustered at the village level.

Table 2: Baseline Characteristics of Pregnant Women by TBA Group

	Control		Treatment		
	Mean	SD	Mean	SD	P-Value
Age	26.30	6.32	26.42	5.93	0.72
Married	0.78	0.41	0.78	0.41	0.88
Head of Household	0.17	0.38	0.26	0.44	0.03
Main Income Earner	0.15	0.35	0.19	0.39	0.28
Number of Household Members	3.98	1.99	3.96	2.02	0.88
Farming as Main Occupation	0.47	0.50	0.55	0.50	0.11
More than Primary Education	0.36	0.48	0.33	0.47	0.40
Own a Mobile Phone	0.64	0.48	0.64	0.48	0.93
Have been Pregnant Before	0.74	0.44	0.76	0.42	0.42
Number of Previous Pregnancies	2.46	1.42	2.53	1.49	0.46
Had 4 or more prior pregnancies	0.16	0.37	0.18	0.39	0.29
Last delivery in hospital	0.53	0.50	0.54	0.50	0.80
Delivered Before TBA Start	0.29	0.45	0.28	0.45	0.72
Recruited After TBA Start	0.18	0.38	0.19	0.39	0.75
Weeks Pregnant at Recruitment	25.50	9.52	25.24	9.51	0.64
Weeks Pregnant at TBA Start	27.53	9.18	26.84	8.91	0.23
Third Trimester at TBA Start	0.58	0.49	0.56	0.50	0.48
Seen Anyone for ANC by Recruitment	0.53	0.50	0.48	0.50	0.30
Saw Doctor, Nurse or Midwife for ANC by Recruitment	0.52	0.50	0.46	0.50	0.21
Saw TBA for ANC by Recruitment	0.03	0.18	0.03	0.17	0.77
Number of ANC visits made at Recruitment	0.96	1.19	0.98	1.30	0.88
Mins from the Nearest Clinic	85.01	56.93	84.68	44.35	0.96
Surveyed at Home	0.94	0.24	0.94	0.24	0.83
Surveyed at Work	0.00	0.04	0.00	0.06	0.34
Surveyed at Friend's or Relative's Home	0.04	0.20	0.04	0.19	0.83
Surveyed at Market	0.00	0.04	0.01	0.08	0.19
Surveyed at Clinic	0.02	0.12	0.01	0.12	0.94
Has Electricity, Gas, or Solar	0.03	0.18	0.04	0.19	0.81
Has Clean Source of Water	0.55	0.50	0.52	0.50	0.65
Has Thatch/Makuti/Grass Roof	0.08	0.28	0.08	0.27	0.73
Blood Pressure Measured	0.76	0.43	0.81	0.39	0.23
Urine Sample Taken	0.67	0.47	0.74	0.44	0.20
Blood Sample Taken	0.79	0.41	0.83	0.37	0.28
Blood Sample Tested for Malaria	0.49	0.50	0.59	0.49	0.19
Blood Sample Tested for Anaemia	0.17	0.38	0.18	0.38	0.87
Blood Sample Tested for HIV	0.63	0.48	0.73	0.44	0.16
Know a TBA	0.13	0.34	0.12	0.32	0.75
Had Health Insurance in Past	0.07	0.26	0.06	0.23	0.64
Cannot Afford Health Insurance	0.76	0.43	0.73	0.44	0.56
Can Read Simple Sentence in Full	0.82	0.38	0.78	0.41	0.10
Full Maternity/Unconditional Transport Vouchers	0.10	0.30	0.11	0.31	0.54
Full Maternity/Conditional Transport Vouchers	0.12	0.33	0.10	0.30	0.11
Full Maternity Voucher/Transport Control	0.08	0.27	0.11	0.31	0.16
Copay Maternity/Unconditional Transport Vouchers	0.10	0.31	0.09	0.29	0.33
Copay Maternity/Conditional Transport Vouchers	0.10	0.30	0.12	0.32	0.24
Copay Maternity Voucher/Transport Control	0.09	0.29	0.12	0.33	0.06
Maternity Control/Unconditional Transport Voucher	0.10	0.30	0.10	0.31	0.72
Maternity Control/Conditional Transport Voucher	0.11	0.32	0.08	0.27	0.02
Maternity/Transport Control	0.19	0.39	0.18	0.38	0.45

Note: p-values are for tests of equality of the means across groups.
Based on standard errors that have been clustered at the village level.

Table 3: Baseline Characteristics of Traditional Birth Attendants by TBA Group

	Control		Treatment		
	Mean	SD	Mean	SD	P-Value
Age	58.85	12.39	57.39	13.12	0.30
Married	0.95	0.21	0.90	0.29	0.13
Head of Household	0.54	0.50	0.55	0.50	0.83
Main Income Earner	0.72	0.45	0.68	0.47	0.51
Number of Household Members	4.26	2.39	4.45	2.30	0.49
Surveyed at Home	0.93	0.26	0.92	0.28	0.71
Surveyed at Work	0.03	0.18	0.04	0.20	0.61
Surveyed at Friend's or Relative's Home	0.02	0.13	0.02	0.13	0.96
Surveyed at Market	0.02	0.13	0.01	0.11	0.84
Surveyed at Clinic	0.00	0.06	0.01	0.09	0.68
Has Electricity, Gas, or Solar	0.08	0.28	0.11	0.31	0.41
Has Clean Source of Water	0.76	0.43	0.69	0.46	0.25
Has Thatch/Makuti/Grass Roof	0.03	0.18	0.02	0.15	0.58
Own a Mobile Phone	0.85	0.36	0.88	0.33	0.39
Rates HH's Food as Less than Adequate	0.34	0.47	0.41	0.49	0.33
Rates HH's Housing as Less than Adequate	0.28	0.45	0.36	0.48	0.22
Rates HH's Clothing as Less than Adequate	0.32	0.47	0.41	0.49	0.17
Rates HH's Health as Less than Adequate	0.25	0.43	0.28	0.45	0.61
Rates Children's Schooling as Less than Adequate	0.35	0.48	0.44	0.50	0.17
Trained as TBA	0.51	0.50	0.46	0.50	0.50
Years of Experience	17.67	11.67	13.17	9.83	0.00
Score on TBA Skill Assessment	0.47	0.08	0.46	0.09	0.55
More than Primary Education	0.16	0.37	0.20	0.40	0.29
Farming as Main Occupation	0.64	0.48	0.71	0.45	0.25
TBA as Main Occupation	0.34	0.48	0.26	0.44	0.18
TBA as Main or Secondary Occupation	0.70	0.46	0.61	0.49	0.16
Provides ANC Services	0.87	0.34	0.81	0.39	0.22
Provides Delivery Services	0.55	0.50	0.58	0.49	0.65
Number of Deliveries Conducted	79.88	144.15	48.03	126.28	0.10
Number of Clients in Own Villages	3.05	2.06	2.85	1.80	0.31
Number of Other Villages Worked In	1.87	1.22	1.87	1.36	0.97
ANC Price Based on What Client can Afford	0.80	0.40	0.77	0.42	0.69
Delivery Price Based on What Client can Afford	0.77	0.43	0.72	0.45	0.57
Main Motivation to be TBA: Pay	0.14	0.35	0.19	0.40	0.29
Main Motivation to be TBA: To Serve	0.64	0.48	0.70	0.46	0.37
Main Motivation to be TBA: To Earn Status	0.17	0.38	0.07	0.26	0.03
Main Motivation to be TBA: Interesting Job	0.02	0.14	0.01	0.11	0.52
Main Motivation to be TBA: To Acquire Skills	0.02	0.14	0.02	0.13	0.80
Main Motivation to be TBA: Personal Reasons	0.01	0.09	0.00	0.07	0.59
Interested in Work Other than TBA	0.50	0.50	0.54	0.50	0.64
Encouraged Facility Delivery Before Delivery	0.83	0.38	0.81	0.39	0.78

Note: p-values are for tests of equality of the means across groups.
Based on standard errors that have been clustered at the village level.

Table 4: Binary Variable Difference in Means: Went to at least the recommended number of visits

	OLS Regressions			Probit Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	0.044*	0.042*	0.040	0.044*	0.041	0.041
	(0.025)	(0.025)	(0.027)	(0.025)	(0.025)	(0.027)
Constant	0.213***	0.033	0.190**			
	(0.020)	(0.020)	(0.080)			
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1138	1138	1098	1138	1138	1098
r2	0.003	0.050	0.100			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Probit regression values represent marginal effects calculated at the means.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent probit regressions.

Table 5: Interaction Effects with Voucher Program: Binary Variable of Attending Recommended Number

	OLS Regressions			Probit Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	-0.026	-0.026	-0.030	-0.072	-0.072	-0.072
	(0.030)	(0.030)	(0.034)	(0.084)	(0.083)	(0.087)
TBA Treatment * Voucher	0.080*	0.083*	0.086*	0.122	0.125	0.126
	(0.043)	(0.043)	(0.047)	(0.091)	(0.091)	(0.095)
Voucher	0.182***	0.168***	0.125**	0.191***	0.181***	0.182***
	(0.034)	(0.054)	(0.055)	(0.034)	(0.044)	(0.044)
Constant	0.066***	0.066***	0.225***			
	(0.024)	(0.024)	(0.081)			
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1138	1138	1098	1138	1138	1098
r2	0.044	0.051	0.101			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Probit regression values represent marginal effects calculated at the means.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent probit regressions.

Table 6: Count Variable Difference in Means: Number of ANC Visits

	OLS Regressions			Negative Binomial Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	0.012	-0.001	-0.015	0.008	-0.006	-0.002
	(0.091)	(0.088)	(0.085)	(0.061)	(0.061)	(0.056)
Constant	1.489***	0.515***	1.855***	0.398***	-0.662***	0.182
	(0.069)	(0.101)	(0.317)	(0.047)	(0.172)	(0.250)
lnalpha						
Constant				-0.589***	-0.999***	-1.932***
				(0.096)	(0.126)	(0.305)
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1139	1139	1098	1139	1139	1098
r2	0.000	0.104	0.258			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent Negative Binomial regressions.

Table 7: Count Variable Difference in Means: Number of ANC Visits; Interaction with Gestation Indicator for 26-34 weeks

	OLS Regressions			Negative Binomial Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	-0.080	-0.072	-0.067	-0.049	-0.050	-0.041
	(0.109)	(0.108)	(0.102)	(0.067)	(0.068)	(0.063)
TBA Treatment * Gest at Start 26-34 weeks	0.488**	0.374*	0.297	0.479***	0.411**	0.337**
	(0.191)	(0.198)	(0.185)	(0.172)	(0.170)	(0.154)
Gest at Start 26-34 weeks	-0.890***	-0.884***	-0.312***	-0.775***	-0.766***	-0.331***
	(0.121)	(0.126)	(0.119)	(0.127)	(0.123)	(0.116)
Constant	1.650***	0.663***	1.834***	0.501***	-0.569***	0.167
	(0.081)	(0.108)	(0.323)	(0.049)	(0.171)	(0.253)
lnalpha						
Constant				-0.687***	-1.160***	-1.983***
				(0.105)	(0.148)	(0.324)
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1139	1139	1098	1139	1139	1098
r2	0.029	0.135	0.261			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent Negative Binomial regressions.

Table 8: Count Variable Difference in Means: Number of ANC Visits; Interaction with Gestation Indicator 26-34 weeks

	OLS Regressions			Negative Binomial Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	0.052	0.036	0.019	0.034	0.018	0.021
	(0.091)	(0.089)	(0.087)	(0.060)	(0.060)	(0.056)
TBA Treatment * Gest at Start 34-38 weeks	-0.897***	-0.817**	-0.913***	-1.620***	-1.535***	-1.589***
	(0.317)	(0.334)	(0.324)	(0.473)	(0.498)	(0.486)
Gest at Start 34-38 weeks	-0.439	-0.395	0.322	-0.346	-0.333	0.207
	(0.299)	(0.286)	(0.290)	(0.280)	(0.265)	(0.268)
Constant	1.502***	0.538***	1.842***	0.407***	-0.643***	0.171
	(0.069)	(0.102)	(0.320)	(0.046)	(0.172)	(0.249)
lnalpha						
Constant				-0.651***	-1.075***	-1.998***
				(0.100)	(0.131)	(0.318)
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1139	1139	1098	1139	1139	1098
r2	0.016	0.117	0.261			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent Negative Binomial regressions.

Table 9: Heterogeneous Binary Variable Effects of the TBA Encouragement Program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TBA Treatment	0.062	-0.034	0.104***	0.117***	0.069*	0.038	0.042	0.066**	0.036	0.068*
	(0.040)	(0.041)	(0.034)	(0.032)	(0.039)	(0.037)	(0.034)	(0.033)	(0.034)	(0.035)
TBA Treatment*X	-0.042	0.126**	-0.098**	-0.151***	-0.049	0.004	-0.010	-0.071	0.004	-0.067
	(0.053)	(0.053)	(0.049)	(0.047)	(0.051)	(0.052)	(0.054)	(0.054)	(0.053)	(0.052)
X	0.028	-0.044	0.117***	0.112***	-0.025	0.019	0.006	0.050	-0.012	0.074*
	(0.043)	(0.044)	(0.038)	(0.037)	(0.041)	(0.041)	(0.041)	(0.042)	(0.039)	(0.042)
Constant	0.196***	0.241***	0.137***	0.149***	0.231***	0.205***	0.211***	0.193***	0.218***	0.180***
	(0.033)	(0.035)	(0.026)	(0.024)	(0.031)	(0.032)	(0.028)	(0.028)	(0.029)	(0.029)
X Attribute (TBA)	Age	Yrs Scl	Trained	Yrs exp	Occup	Food	Housing	Clothing	Health	School
N	1091	1091	1091	1091	1091	1091	1091	1091	1091	1091
r2	0.003	0.008	0.011	0.011	0.006	0.003	0.002	0.004	0.002	0.006

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

All regressions utilize OLS and include controls for other voucher treatments and basic characteristics of the women.

Table 10: Binary Variable: Effect of TBA Encouragement Program After Program Change

	OLS Regressions			Probit Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	0.054**	0.045*	0.045	0.061**	0.051*	0.049
	(0.026)	(0.027)	(0.028)	(0.030)	(0.030)	(0.031)
TBA Treatment*Program Change	-0.044	-0.019	-0.015	-0.051	-0.029	-0.022
	(0.062)	(0.060)	(0.059)	(0.049)	(0.049)	(0.051)
Program Change	0.210***	0.203***	0.110**	0.220***	0.215***	0.097*
	(0.048)	(0.047)	(0.049)	(0.049)	(0.049)	(0.050)
Constant	0.157***	-0.027	0.062			
	(0.019)	(0.022)	(0.088)			
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1138	1138	1098	1138	1138	1098
r2	0.042	0.091	0.106			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Probit regression values represent marginal effects calculated at the means.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent probit regressions.

Table 11: Binary Variable Difference-in-Differences: Went to at least the recommended number of visits (All women)

	OLS Regressions			Probit Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	-0.021	-0.020	-0.025	-0.026	-0.025	-0.033
	(0.025)	(0.024)	(0.025)	(0.030)	(0.030)	(0.035)
TBA Treatment*Post	0.065*	0.061*	0.063*	0.082**	0.080*	0.088*
	(0.033)	(0.033)	(0.036)	(0.042)	(0.043)	(0.050)
Post	-0.524***	-0.523***	-0.568***	-0.525***	-0.547***	-0.619***
	(0.024)	(0.024)	(0.023)	(0.024)	(0.025)	(0.024)
Constant	0.737***	0.539***	0.827***			
	(0.017)	(0.025)	(0.065)			
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	2446	2446	2065	2446	2446	2065
r2	0.241	0.282	0.331			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Probit regression values represent marginal effects calculated at the means.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent probit regressions.

Includes all women.

Table 12: Count Variable Difference-in-Differences: Number of ANC Visits (All women)

	OLS Regressions			Negative Binomial Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	-0.132*	-0.125*	-0.137*	-0.137*	-0.136**	-0.145*
	(0.068)	(0.066)	(0.079)	(0.071)	(0.069)	(0.081)
TBA Treatment*Post	0.143	0.125	0.140	0.145*	0.130	0.145
	(0.106)	(0.103)	(0.111)	(0.087)	(0.086)	(0.094)
Post	0.461***	0.469***	0.435***	0.371***	0.375***	0.330***
	(0.077)	(0.075)	(0.081)	(0.060)	(0.059)	(0.065)
Constant	1.028***	0.246***	0.592***	0.028	-1.012***	-0.760***
	(0.048)	(0.066)	(0.221)	(0.047)	(0.124)	(0.225)
lnalpha						
Constant				-0.937***	-1.435***	-1.496***
				(0.098)	(0.129)	(0.155)
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	2447	2447	2065	2447	2447	2065
r2	0.041	0.128	0.150			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent Negative Binomial regressions.

Includes all women.

Table 13: Facility Delivery Difference in Means: Delivered in Public Health Facility or Private Sector/NGO Facility (endline data)

	OLS Regressions			Probit Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
TBA Treatment	0.025	0.026	0.014	0.025	0.025	0.014
	(0.022)	(0.022)	(0.024)	(0.022)	(0.022)	(0.023)
Constant	0.836***	0.762***	0.726***			
	(0.017)	(0.034)	(0.084)			
Vouchers	No	Yes	Yes	No	Yes	Yes
Women's Characteristics	No	No	Yes	No	No	Yes
N	1392	1392	1074	1392	1392	1074
r2	0.001	0.014	0.035			

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parenthesis are robust to heteroskedasticity and are clustered at the village level.

Probit regression values represent marginal effects calculated at the means.

Columns (1)-(3) represent ordinary least squares (OLS) regressions.

Columns (4)-(6) represent probit regressions.

Based on endline survey data.