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Cash on delivery: Results of a randomized experiment to promote maternal health care in Kenya☆

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ABSTRACT

We conducted a randomized controlled experiment to test whether vouchers, cash transfers, and SMS messages were effective in boosting facility delivery rates among poor, pregnant women in rural Kenya. We find a strong effect of the full vouchers and the conditional cash transfers: 48% of women with access to both interventions delivered in a health facility, while only 36% of those with neither did. Amongst women who did not receive a cash transfer, we find that a small copayment dramatically reduced voucher effectiveness, suggesting a discontinuous impact of cost-sharing on the demand for health services. Both the unconditional cash transfer and the text messages had limited effect on the use of health services. Finally, we also find no evidence that a government policy to eliminate user fees increased demand for maternal health services.

1. Introduction

Despite recent progress, rates of maternal and newborn mortality remain persistently high in the developing world, due in part to a lack of timely access to quality maternal health services (Chou et al., 2015; Alkema et al., 2016; You et al., 2015). Millennium Development Goal (MDG) 5, a target to reduce maternal mortality rates by three quarters globally between 1990–2015, was not met and the situation is particularly dire in Sub-Saharan Africa where more than half of all maternal deaths occur (Kassebaum et al., 2014). Ensuring access to quality maternal health services at the time of delivery is seen as critical to improving both maternal and newborn health (Campbell and Graham, 2006; Chou et al., 2015).

There is growing empirical evidence that delivering in a health facility rather than at home leads to improved health outcomes. Data from observational studies suggest that women who either give birth in a health care facility or in the presence of a skilled birth attendant have better pregnancy outcomes, as do their newborns (Ronsmans et al., 2009; Darmstadt et al., 2009; Scott and Ronsmans, 2009). These studies, however, could be biased by selection issues (Chinkhumba et al., 2014). Two more rigorous studies have suggested that there are important health returns to facility deliveries. In the Netherlands, where facility delivery and newborn survival rates are much higher than in the developing world, exploiting variation in distance to health facilities, one study has shown that newborns have lower mortality rates when delivered in a health facility rather than at home (Trandafir and Daysal, 2015). Exploiting variation in the timing of the onset of labor, another study found that children born at night in areas of rural Nigeria without 24-hour

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health facilities were almost twice as likely to die as children born in areas with those facilities (Okeke and Chari, 2018).

Given the priority given to facility-based deliveries, many countries have adopted strategies to increase rates of institutional deliveries. However, there is also limited rigorous evidence on the effectiveness of these approaches at increasing facility delivery rates and growing concerns that even if they succeed in boosting demand, quality of care can suffer as a result. For example, quasi-experimental evaluations of a national maternal conditional cash transfer program in India found that the program increased rates of institutional deliveries but was not consistently associated with health improvements (Lim et al., 2010; Powell-Jackson et al., 2015b) and suggested quality of care in Indian facilities likely limited the impact of the program.

One popular approach adopted by a number of countries to increase facility delivery rates has been to introduce user-fee exemption policies to remove financial barriers. Reviews of evaluations of these experiences have concluded that while most published studies have shown increases in rates of facility deliveries, the overall quality of the evidence is weak and that many of these policies reduced the quality of care delivered (Ridde et al., 2012; Hatt et al., 2013). Other countries have implemented targeted maternal vouchers, which have been found to be associated with higher facility delivery rates and to be better than free-care policies in maintaining quality, but again few voucher programs have been rigorously evaluated in any international context (Bellows et al., 2013).

Another strategy is to provide conditional or unconditional cash transfers (CCTs or UCTs) which can both subsidize the demand for health services and potentially also affect health seeking behavior through behavioral mechanisms. While a CCT is formally similar to a voucher-mediated price subsidy (i.e. we could expect a CCT to have an equivalent price effect as a voucher of the same magnitude), in addition to the financial incentive, CCTs may make the subsidies more salient to consumers and could focus attention on complementary goods and services, such as transportation, which are also believed to be important barriers to the use of maternal health services in developing countries (Sacks et al., 2015). Experimental evidence of the impact of CCTs on maternal health outcomes has been demonstrated in Mexico (Barber and Gertler, 2010) and quasi-experimental studies have also shown positive impacts in other contexts (Glassman et al., 2013), including in Indonesia (Kusuma et al., 2016), Nepal (Powell-Jackson and Hanson, 2012), and India (Lim et al., 2010; Powell-Jackson et al., 2015a).

If pregnant women have challenges planning ahead or face liquidity constraints, a labeled but otherwise unconditional cash transfer (UCT) delivered at the appropriate time could address these constraints. To date, the evidence on the impact of UCTs is mixed, and there is limited evidence of their impact on health service utilization. A UCT in Morocco, in which beneficiaries were provided money unconditionally but were informed that it was to be used to finance education expenses found that such a "nudge" was almost as effective as a CCT at improving educational outcomes (Benhassine et al., 2015). Similar results were observed for a subset of the health outcomes in the UCT arm of a study in Malawi (Baird et al., 2011). However, an evaluation of the short-term impact of a UCT program in Kenya found little short-term impact on any health measure (Haushofer and Shapiro, 2016). A unlabeled UCT in Zambia only had a small effect on use of skilled attendants at birth among a subset of better-off women (Handa et al., 2015). And a labeled UCT was less effective at incentivizing Kenyan women to use higher quality health facilities than a CCT, although the evaluation did not examine impact on facility delivery rates (Cohen et al., 2017).

Over the past decade, there has been proliferation of mobile phone enabled health interventions to improve health outcomes in developing countries (Labrique et al., 2013), which have targeted information to health care providers, or have sent text reminders to patients to maintain adherence to medicines or to increase the use of routine health services (Sondaal et al., 2016; Lester et al., 2010; Pop-Eleches et al., 2011; Raifman et al., 2014). Numerous countries have introduced SMS reminder programs targeted at pregnant women in order to encourage the use of maternal health services, however, there is limited evidence on the effectiveness of these programs (Lee et al., 2016). In addition, there has been almost no attention in the literature on how such messages should be designed to be most effective.

Studies have shown that mobile phones can be leveraged to deliver a range of benefits beyond text messages and have the potential to do so less expensively than traditional targeting methods. In Niger, the use of mobile phones to deliver a cash transfer program provided more benefits to households than resources transferred via more manual methods (Aker et al., 2016). A perceived limitation of many targeted health programs is that such programs may be costly to administer and to target (Schmidt et al., 2010). This may be especially relevant for poor and geographically remote populations. However, the reach of the mobile phone, and the explosion of mobile money in Kenya has provided a platform over which both targeted financial subsidies and information can be delivered at minimal cost (Suri and Jack, 2016; Suri et al., 2012; Jack and Suri, 2014). Additionally, the mobile platform allows multiple interventions to be deployed in tandem.

Building off this literature and in an attempt to generate rigorous evidence on the effectiveness of strategies to encourage facility-based deliveries, in early 2013 in collaboration with a Kenyan micro-health insurance company, we evaluated the effectiveness of 3 types of interventions in increasing facility delivery rates among pregnant women in Western Kenya: maternal vouchers, cash transfers labeled as transport subsidies, and text reminders. For each type of intervention, we fielded two versions to test the effectiveness of key design considerations. For the vouchers, we developed both a full voucher as well as one that included a small cost-sharing component based on evidence from previous studies that have demonstrated discontinuous impact of any cost-sharing on the demand for health products (Cohen and Dupas, 2010; Ashraf et al., 2010; Kremer and Miguel, 2007). For the transportation subsidies, we implemented a version in which payments were transferred conditional upon presenting at a health facility and an unconditional variant to examine the role of conditionality. Finally, we developed two types of text messages, both of which provided information on the importance of a healthy pregnancy and maternal health care. The "plain" version provided reminders and encouragement to seek care, while a "contextualized" version modified the "plain" messages to make the health of the baby more salient to the prospective mothers.

Unexpectedly in June 2013, mid-way through our experiment, the government of Kenya introduced a free-care policy to exempt pregnant women from user fees at all public clinics across Kenya. While this secular policy shift was unanticipated, it provided an opportunity to also investigate the impact of making care free on the use of health services.

Our study makes a number of contributions to the literature on the design of policies to subsidize demand for health services in low-income settings. First, we find a strong effect of the full voucher and the CCT on facility deliveries, especially when they were delivered in combination. Forty-eight percent of women with access to both the CCT and the full voucher delivered in a clinic or hospital, while only 36 percent of those with neither did so, a 12 percentage point, or 33 percent, increase in institutional delivery rates. Second, we find that the UCT had little impact, suggesting women in this context were not simply liquidity constrained. Third, amongst women who did not receive a cash transfer, and whose only financial incentive at the margin to deliver in a clinic was a maternity

care voucher, our evidence suggests the small copayment reduced demand to levels close to the control group. Specifically, our preferred point estimate of the effect the copay voucher is negative and is not statistically different from zero. On the other hand, we reject the null of equal average treatment effects of the full and copay voucher, but only at the ten percent level among non-recipients of a cash transfer. Our tentative conclusion is that a small cost-sharing contribution for maternal health services has a large, if not discontinuous, impact on demand. Fourth, we find no evidence that either type of text message affected utilization of maternal care services across the full sample. Fifth, we explore impact heterogeneity and find that the interventions worked better among women who did not own a phone at baseline and suggestive evidence that they were more effective among women living closer to major clinics or hospitals. Finally, we find little impact of the free care policy. Even after the policy change, women with vouchers consistently had higher rates of facility delivery than women without vouchers. We find some evidence that the voucher-holding women paid less for maternity care, even after the free maternity care policy was implemented and evidence that women may have had concerns about the impact of the policy on quality. We also investigate the impact of the policy on other maternal outcomes on the perceived quality of care. Using an instrumental variable approach, we find that prior to the free care policy, voucher recipients perceived somewhat higher quality care in some dimensions, particularly those that could be varied on a patient-by-patient basis. During the new policy regime however, this quality differential appears to be eliminated, although it is difficult to tell if this convergence reflects real quality changes.

The remainder of this article is organized as follows: Section 2 describes the context and our interventions in more detail, Section 3 describes our recruitment activities and the data sources used, Section 4 outlines our empirical strategy, Section 5 presents our results, Section 6 develops a simple model to rationalize our results on demand side interventions, while Section 7 concludes.

2. Context and description of interventions

Improvements in the rates of maternal and newborn mortality in Kenya from 1990 to 2015 were relatively modest and overall insufficient to meet the MDGs (Keats et al., 2017). The maternal mortality ratio was estimated to be 362 per 100,000 live births during 2007–2014, only slightly lower than some estimates for 1990. There was no measurable decline in MMR during the early 2000s and rates may have even increased during the 1990s. There has also been limited progress in reducing newborn mortality in the country (Keats et al., 2017).

In the 2014 Kenya Demographic and Health Survey (DHS), only 61% of births in the preceding 5 years were reported to have taken place in a health facility (Kenya National Bureau of Statistics and The DHS Program, 2015). In Western Province,[1] where this study was carried out, the corresponding figure was 47%, although that was nearly double the rate reported in the 2008-9 DHS (Kenya National Bureau of Statistics and ICF Macro, 2010). In the earlier DHS, the most commonly cited reasons for not delivering in a health facility were geographic and financial barriers. While nearly all pregnant women in Kenya (96%) received some antenatal care (ANC) services, only 58% made the recommended four or more visits during their pregnancies, and they often sought care late. Finally, more than 60% of pregnant women in Western Province did not receive any postnatal care (PNC) after their most recent live birth.

In this context, we collaborated with Changamka, a microhealth insurance organization, whose "m-Kadi" program provided the platform over which the experimental interventions were delivered to pregnant women in Vihiga County, a relatively rural area located in Western Province. Below we describe in more detail the three types of interventions: maternity vouchers, cash transfers labeled as transport subsidies, and reminder text messages. The three intervention types, which each included two treatments and a control, were crossed, yielding a 3×3x3 design.

2.1. Maternal vouchers

All vouchers covered the costs of ANC visits, delivery, and PNC visits, plus a small premium to compensate facilities for the administrative burden of adopting the system and recording utilization. The vouchers are described in Appendix Table 1.

Women assigned to the "full voucher" group made no payments to the facility, and in principle were not required to purchase any inputs or consumables at the point of service. As the majority of the health facilities in our catchment area only offered basic emergency obstetric care, if a woman in the full voucher group presented at a health clinic in labor, but required a cesarean section or additional obstetric procedures, she would be transported to the Vihiga District Hospital, the only health facility in the catchment area that provided comprehensive emergency obstetric services. Under this circumstance, the voucher program reimbursed both the health clinic and the hospital. The cost of transport from the clinic to the hospital was also covered if the woman had a voucher, but women without a voucher had to pay for those services, as they would under normal circumstances.

A second group of women received a "copay voucher," under which they received free care for all ANC and PNC services, but were required to pay a 100 KSh (about $1.20 at the time) copayment for a facility delivery, which represented about 10 percent of the median reported price for a normal delivery paid by the control group. The copayment, which had to be made at the time of check-in via one of Kenya's mobile money platforms, was the same across facilities, and did not depend on whether a cesarean section was performed. The facility would not be reimbursed for deliveries of women in the copay voucher group unless the copayment had been made.[2]

2.2. Cash transfers to subsidize transport costs

We administered two types of cash transfers – conditional (CCT) and unconditional (UCT) – designed to help overcome transport cost barriers. These subsidies, also delivered by mobile phone, were not tied directly to the purchase of transport services, primarily because of the challenges of keeping track of receipts and limiting fraud. Instead, those women assigned to the CCT arm were sent transfers if and when they presented at a health facility for one of the prescribed maternal health services, and if their visit was recorded in the online accounting system. The CCT was paid for up to four ANC visits, a facility delivery, and up to three PNC visits. As shown in appendix Table 1, an eligible woman received a transfer of 250 KSh (about $3 USD) for each eligible ANC and PNC visit and a 500 KSh ($6 USD) transfer for her delivery.

[1] Under the 2010 Kenyan Constitution, new administrative structures were drawn up and the 8 provinces were replaced by 47 counties. Vihiga County, the location of this study, was part of Western Province in the 2008 DHS, and thus could not be directly identified in those survey data but could be identified in the 2014 DHS.

[2] After the free care policy came into effect, if the facility was to be reimbursed for serving copay voucher recipients, the 100 KSh payment still had to be made. It is possible that the facility itself could have executed this payment on behalf of the woman, in order to earn the face value of the voucher, if the woman was unwilling to do so but our programmatic data, which includes the mobile phone number from which payments were made, indicates that this practice was not prevalent.

Another group of women were randomized into an UCT arm. Transfers in the same amounts as the CCT were remitted to these women on the mobile money platform a few days before each of their predicted ANC and PNC visits, according to the schedule recommended by the Kenyan Ministry of Health, and a week before their expected delivery date.

We reiterate that the cash transfers were not formally linked to the purchase of transport services. However, because we told women that they were meant to help with transport costs, and they were large enough to cover reasonable fares, there could have been a labeling effect associated with these two arms.[3]

2.3. Reminder SMS messages

Finally, some women were selected to receive weekly mobile text messages via SMS, intended to promote a healthy pregnancy and to remind them of upcoming ANC and PNC visits and to encourage facility deliveries. We used messages developed by the Mobile Alliance for Maternal Action (MAMA),[4] a partnership established to develop vital health messages to new and expectant mothers in developing countries based on clinical data via their mobile phones. Women in one SMS treatment arm received the "plain" version of these messages, while women in a second arm received a more "contextualized" version, which we modified to make the health of their baby a more salient motivation for seeking care. Again, a control group of women received no weekly SMS messages from the program.

Appendix Table 2 provides a comparison of the plain reminder messages and those that were contextualized to strengthen the incentive to use the service. Both types of messages were sent on the same schedule, and assignment of the SMS treatments was orthogonal to that of the financial interventions.

2.4. Free-care policy

In March 2013, Uhuru Kenyatta, was elected president of Kenya. In his inauguration speech in April 2013, Kenyatta announced a free maternity care policy, which stated that as of June 1, 2013 all pregnant women would be exempt from paying user fees for all maternity services, including ANC, delivery services, and PNC in all public facilities across the country (Gitobu et al., 2018). Health centers and dispensaries were to be reimbursed 2500 Ksh (28 USD) per normal delivery, hospitals 5000 (57 USD) Ksh, and 17,500 (200 USD) at referral hospitals in the country (Pyone et al., 2017). Cesarean sections were reimbursed at the same rate, however, the prices paid were meant to reflect an 80/20 ratio in normal to cesarean section deliveries. The reimbursed rates were also meant to cover ANC and PNC.

3. Recruitment and data collection

Community Health Workers (CHWs), whose regular job includes identifying, monitoring, and counseling pregnant women in their home villages, were trained and engaged by our research team in collaboration with Changamka to implement the recruitment activities for this project. Village meetings were conducted in which women and other community members were informed about the new "m-Kadi" program. Women were told that by enrolling in the program they could receive financial support to help them cover the costs of their pregnancy, although they were not told which benefits, if any, they might receive. After the meetings, the CHWs approached pregnant women in their villages to see if they were interested in enrolling in the program. Women were informed of the potential benefits of the program and were asked to give informed consent to participate in the study. Between February and July 2013, a total of 1595 eligible pregnant women consented and were enrolled into the program.[5] Our sample was drawn from the former Vihiga Division, now called Vihiga District, which was estimated to have an approximate population of 120,000 in 2009, when the last census was conducted. According to the latest KDHS, the crude rural birth rate in Kenya in 2014 was 30.3. Based on these parameters, we estimated that there could be as many 3000–4000 births in our district per year, or roughly 300 new pregnancies per month. Assuming a pregnancy is 40 weeks (i.e. 10 months), we should expect a stock of no more than 3000 pregnant women during our first day of recruitment, and given that we conducted 5 months of recruitment, we estimate that we recruited approximately one third of the 4500 potential pregnancies in all of Vihiga District (assuming no pregnancy loss). It therefore appears that we managed to recruit a considerable share of all eligible women in our catchment area.

At recruitment, CHWs administered a face-to-face survey collecting data on demographics, current pregnancy status, prior pregnancies, and other information using a mobile phone for data collection. Participating women were given a unique ID number, which they were told to present at health facilities for all maternal health visits. Random assignment to one of each of the 3 treatment arms was done remotely on the server and communicated at the end of the baseline survey. All treatment arms were orthogonal to the other treatment arms. Although cash transfers, copayments, and text messages were sent over the mobile phone platform, women were not required to possess a mobile phone to take part in the study. Instead, they could specify the number of a phone that could be used for these purposes, often belonging to a family member, friend, or sometimes the community health worker herself.

At the time of our experiment, there were 8 public facilities in Vihiga District, all of which participated in our study, and no private facilities providing deliveries in the area (some private providers provided additional services, such as ultrasounds, but these were not covered under the voucher program). When a woman visited one of the 8 participating facilities, her program ID number was recorded, and basic information collected in an online database, which was linked to the payment system through which vouchers were redeemed by the facility. Data clerks at the facilities were in charge of these operations and worked in shifts to provide 24-hour coverage. However, as we report below, compliance with the data collection protocol at the facilities was imperfect, particularly, and not surprisingly, for women who did not receive a maternity care voucher or CCT. As part of routine administrative procedures at public facilities, and unrelated to our experiment, nurses and other staff also recorded information on all patients visiting a facility in register. As we describe below, we use both baseline and survey data as well as register book data in our analysis.

The endline survey was administered in person by the CHWs on a rolling basis in four phases between August 2013 and March 2014 to women whose expected delivery date (as estimated at recruitment) had recently passed. We attempted to reach women who could not be interviewed in person with follow-up phone surveys.[6] Data on the number and timing of ANC visits, when and where

[3] Of course, we are unable to independently identify such a labeling effect, as no women were offered the transfers without the reference to transport costs.

[4] http://www.mobilemamaalliance.org/

[5] Eligibility for the m-Kadi program was restricted to poor women without a formal occupation.

[6] We attempted to reach by phone 36 recruited women who had relocated outside of Vihiga district before the endline survey.

Table 1
Summary of treatment assignments.

Panel A: Endline eligible sample at baseline

	Full	Copay	None	Total
UCT	160	144	154	458
CCT	172	166	143	481
None	139	161	275	575
Total	471	471	572	1514

Panel B: Endline eligible sample that completed endline survey

	Full	Copay	None	Total
UCT	149	139	140	428
CCT	168	160	141	469
None	130	150	224	504
Total	447	449	505	1401

Panel C: SMS treatment assignment

	Baseline	Endline
Plain	606	556
Contextual	334	311
None	574	534
Total	1514	1401

Notes: Panel A summarizes the endline-eligible sample at baseline, which excludes women who died (n = 2), miscarried (n = 72), or turned out to not be pregnant (n = 7) among the 1595 women originally recruited. Treatment status did not predict any of these exclusions (not shown, but available upon request). Panel B summarizes the sample who were found, consented, and completed the endline among those eligible to complete the survey. Panel C reports on the experimental sample for our SMS interention and is orthologal to the other interventions.

their delivery took place, the number of PNC visits she made, and measures of her perceived quality of care were all collected in the endline survey.

Of the 1595 women recruited, only 1514 were eligible for our endline survey: 2 women turned out not to be pregnant, 7 died during the course of the study, and 72 experienced a miscarriage.[7] Panel A of Table 1 reports the numbers of women in our study at baseline, Panel B reports the number of eligible sample assigned to each combination of health care voucher and cash transfer treatments, Panel C reports the distribution of our sample that actually completed the endline survey, while Panel D reports the assignment to SMS treatments, which was orthogonal to the other two treatment assignments. A full description of the attrition in our sample is also available in Appendix Table 3.

Table 2 reports balance tests using baseline data for the maternal health care vouchers (Panel A) and cash transfer arms (Panel B). We also report balance tests for a saturated design of voucher and cash transfer treatments (Appendix Table 4) and pre- and post-free care policy (Appendix Table 5).[8] With the exception of a few baseline characteristics, treatment and control groups for all of the interventions were well balanced.

We face two potential biases in using the endline survey data to measure pregnancy outcomes: measurement error issues could arise if women gave false or incorrect answers or if there was significant non-random attrition from the baseline sample.

Table 3 reports the correlation of attrition at endline with assignment to the voucher and cash transfer arms, and their interactions. Of the 1514 women who were eligible to take the endline survey, 1401 (92.5% of the eligible sample) completed either the in-person endline survey or the phone survey: we were unable to find 43 of the non-respondents (2.8%), while 70 women (4.6%) refused to consent to the survey. Column (2) shows that other than a small negative effect of being assigned to the CCT only, there was no correlation between being found at endline and any of the other treatment assignments, while Column (3) confirms that assignment to either a health care voucher or cash transfer treatment arm, or their interactions, reduced consent-related attrition.[9]

To account for possible measurement error, and also to correct for non-random attrition, we constructed a complementary dataset on the place of delivery, using data from the register books from all eight health facilities in our catchment area. First, we first took photographs of the pages in the register books in the labor and delivery units of each facility over the entire study period. Entries from these books were then transcribed into a database by two independent transcribers, if the transcriptions were not exact, then the transcripts were manually reconciled by a third person. We then developed an automated matching algorithm to find the closest match for each woman in our baseline survey to entries in the transcribed database. Matching register data to our baseline and endline datasets was non-trivial since, for example, women's names are often spelled differently in the register book and delivery dates are remembered or recorded with error. The matching algorithm calculated a match score that was based on each woman's expected date of delivery, the age of the woman, her first and last name, and her home village. Two research assistants working independently were then asked to determine if there was a match among the closest matches obtained from the matching algorithm. If both research assistants agreed on the same match for the woman in our baseline survey, then this was coded as a "full match" for a facility delivery or 0 otherwise. We also allowed for a "partial match" definition of facility delivery, equal to 1 if either one or both research assistants indicate such. According to these definitions, between 590 (full match) and 772 (partial match) of the 1514 eligible women were recorded as having delivered in a health care facility. Using this approach, the rate of facility deliveries in the control group ranged between 33 and 46%, which is more consistent with estimates we would have expected based on data from the 2014 Kenyan DHS (which was 47% in the preceding 5 years).

Although the register book data could be incomplete, there is no reason to believe it is biased with respect to the research design. For this reason, while we present the results from both the endline survey and the register book dataset, our preferred specification is based on the full match algorithm. As we were only able to generate a comparable dataset for delivery outcomes (but not for ANC or PNC visits), we used the endline survey data for other health care utilization outcomes.

4. Empirical strategy

4.1. Maternal health care utilization

Our primary outcome variable of interest, y_i is an indicator for in-facility or institutional delivery, but we also estimate treatment effects on hospital deliveries, referrals to hospitals, as well as the number of ANC and PNC visits. We first present results from unsaturated regressions of the form:

$$y_i = \alpha + \sum_{j=1}^{2} \beta^j V_i^j + \sum_{k=1}^{2} \gamma^k T_i^k + \sum_{l=1}^{2} \delta^l S_i^l + \sum_{m=1}^{M} \tau^m X_i^m + \varepsilon_i$$

where:

[7] We acknowledge that the second and third outcomes could have been affected by the treatments themselves, but we lack power to speak definitively to this possibility.

[8] We do no present the balance test for the SMS subsidies, but they are available upon request.

[9] Attrition was uncorrelated with assignment to the SMS treatment arms and whether or not women in the initial sample were eligible for the endline survey (results not shown).

Table 2
Balance Test - Unsaturated Design.

Panel A: Maternity Voucher Arms	(1) Full	(2) Copay	(3) Control	(4) Full Sample	(5) (1) vs. (2)	(6) (1) vs. (3)	(7) (2) vs. (3)	(8) p-value
Age in years	26.246	26.255	26.455	26.328	−0.008	−0.208	−0.200	0.818
	(0.272)	(0.278)	(0.262)	(0.156)	(0.389)	(0.380)	(0.383)	
Married	0.752	0.790	0.808	0.785	−0.038	−0.056**	−0.018	0.086
	(0.020)	(0.019)	(0.016)	(0.011)	(0.027)	(0.026)	(0.025)	
Head of household	0.227	0.202	0.217	0.215	0.025	0.010	−0.015	0.633
	(0.019)	(0.019)	(0.017)	(0.011)	(0.027)	(0.026)	(0.025)	
Completed more than primary education	0.369	0.361	0.313	0.345	0.008	0.056*	0.048	0.113
	(0.022)	(0.022)	(0.019)	(0.012)	(0.031)	(0.029)	(0.029)	
Owns a phone	0.658	0.614	0.656	0.643	0.045	0.003	−0.042	0.267
	(0.022)	(0.022)	(0.020)	(0.012)	(0.031)	(0.030)	(0.030)	
Number of household members	4.032	4.045	3.913	3.991	−0.013	0.119	0.132	0.496
	(0.094)	(0.095)	(0.081)	(0.052)	(0.134)	(0.123)	(0.124)	
Has lights	0.051	0.021	0.033	0.035	0.030**	0.018	−0.012	0.044
	(0.010)	(0.007)	(0.007)	(0.005)	(0.012)	(0.012)	(0.010)	
Has clean water	0.527	0.544	0.510	0.526	−0.017	0.016	0.033	0.568
	(0.023)	(0.023)	(0.021)	(0.013)	(0.033)	(0.031)	(0.031)	
Has a traditional roof	0.068	0.079	0.094	0.081	−0.010	−0.026	−0.016	0.292
	(0.012)	(0.012)	(0.012)	(0.007)	(0.017)	(0.017)	(0.018)	
Weeks pregnant at recruitment	26.974	26.579	26.703	26.747	0.395	0.271	−0.124	0.820
	(0.472)	(0.434)	(0.413)	(0.253)	(0.640)	(0.626)	(0.602)	
Has had prior ANC for this pregnancy	0.564	0.506	0.495	0.520	0.057*	0.069**	0.012	0.066
	(0.023)	(0.023)	(0.021)	(0.013)	(0.033)	(0.031)	(0.031)	
Has seen a doctor for this pregnancy	0.357	0.287	0.295	0.312	0.070**	0.062**	−0.008	0.037
	(0.022)	(0.021)	(0.019)	(0.012)	(0.030)	(0.029)	(0.028)	
Has seen a TBA for this pregnancy	0.017	0.028	0.045	0.031	−0.011	−0.028**	−0.018	0.027
	(0.006)	(0.008)	(0.009)	(0.004)	(0.010)	(0.011)	(0.012)	
Number of AnC visits for this pregnancy	1.883	1.891	2.000	1.928	−0.008	−0.117	−0.109	0.391
	(0.069)	(0.071)	(0.067)	(0.040)	(0.099)	(0.096)	(0.098)	
Wealth Index	−0.012	0.031	−0.015	0.000	−0.043	0.003	0.046	0.549
	(0.039)	(0.028)	(0.031)	(0.019)	(0.048)	(0.049)	(0.043)	
Distance to nearest clinic, meters	1683.611	1704.570	1663.702	1682.588	−20.959	19.910	40.869	0.772
	(43.281)	(40.547)	(38.438)	(23.471)	(59.316)	(57.763)	(56.121)	
Distance to nearest major clinic/hospital, meters	3800.114	4088.847	4079.918	3995.494	−288.733**	−279.805**	8.929	0.051
	(96.175)	(99.169)	(86.255)	(54.002)	(138.137)	(129.036)	(130.906)	
N	471	471	572	1514	942	1043	1043	
Panel B: Cash Transfer Arms								
	UCT	CCT	Control	Full Sample	(1) vs. (2)	(1) vs. (3)	(2) vs. (3)	p-value
Age in years	26.299	26.291	26.381	26.328	0.008	−0.082	−0.090	0.965
	(0.276)	(0.275)	(0.260)	(0.156)	(0.390)	(0.382)	(0.380)	
Married	0.779	0.796	0.779	0.785	−0.017	0.000	0.017	0.756
	(0.019)	(0.018)	(0.017)	(0.011)	(0.027)	(0.026)	(0.025)	
Head of household	0.231	0.195	0.219	0.215	0.036	0.012	−0.024	0.391
	(0.020)	(0.018)	(0.017)	(0.011)	(0.027)	(0.026)	(0.025)	
Completed more than primary education	0.343	0.349	0.344	0.345	−0.006	−0.002	0.005	0.976
	(0.022)	(0.022)	(0.020)	(0.012)	(0.031)	(0.030)	(0.029)	
Owns a phone	0.646	0.651	0.635	0.643	−0.004	0.012	0.016	0.854
	(0.022)	(0.022)	(0.020)	(0.012)	(0.031)	(0.030)	(0.030)	
Number of household members	3.950	4.046	3.977	3.991	−0.096	−0.028	0.068	0.750
	(0.094)	(0.090)	(0.085)	(0.052)	(0.130)	(0.127)	(0.124)	
Has lights	0.041	0.023	0.040	0.035	0.019	0.001	−0.017	0.214
	(0.009)	(0.007)	(0.008)	(0.005)	(0.011)	(0.012)	(0.011)	
Has clean water	0.528	0.522	0.527	0.526	0.007	0.001	−0.005	0.977
	(0.023)	(0.023)	(0.021)	(0.013)	(0.033)	(0.031)	(0.031)	
Has a traditional roof	0.063	0.090	0.089	0.081	−0.026	−0.025	0.001	0.241
	(0.011)	(0.013)	(0.012)	(0.007)	(0.017)	(0.017)	(0.018)	
Weeks pregnant at recruitment	26.927	26.637	26.705	26.747	0.290	0.222	−0.068	0.897
	(0.446)	(0.442)	(0.424)	(0.253)	(0.630)	(0.624)	(0.615)	
Has had prior ANC for this pregnancy	0.507	0.518	0.532	0.520	−0.011	−0.026	−0.014	0.711
	(0.023)	(0.023)	(0.021)	(0.013)	(0.033)	(0.031)	(0.031)	
Has seen a doctor for this pregnancy	0.260	0.351	0.322	0.312	−0.091***	−0.062**	0.029	0.009
	(0.021)	(0.022)	(0.019)	(0.012)	(0.030)	(0.029)	(0.029)	
Has seen a TBA for this pregnancy	0.024	0.035	0.033	0.031	−0.011	−0.009	0.002	0.566
	(0.007)	(0.008)	(0.007)	(0.004)	(0.011)	(0.011)	(0.011)	
Number of AnC visits for this pregnancy	1.858	1.891	2.010	1.928	−0.033	−0.151	−0.119	0.243
	(0.077)	(0.067)	(0.064)	(0.040)	(0.101)	(0.099)	(0.093)	
Wealth Index	−0.007	0.021	−0.012	0.000	−0.029	0.004	0.033	0.752
	(0.034)	(0.035)	(0.030)	(0.019)	(0.049)	(0.046)	(0.046)	
Distance to nearest clinic, meters	1702.354	1686.430	1663.666	1682.588	15.925	38.689	22.764	0.790
	(43.772)	(41.843)	(37.182)	(23.471)	(60.524)	(57.113)	(55.845)	
Distance to nearest major clinic/hospital, meters	4192.475	3857.542	3953.333	3995.494	334.933**	239.141*	−95.792	0.042
	(93.547)	(93.868)	(91.854)	(54.002)	(132.594)	(132.538)	(132.198)	
N	458	481	575	1514	939	1033	1056	

Notes: Panel A shows selected means and standard errors in parentheses across the Full, Copay, and control maternity voucher arms in columns (1)-(3) while Panel B shows the same values for the Conditional, Unconditional, and control arms for the cash transfer arms. In both panels, column (4) shows the means for the full endline eligible sample. Columns (5)-(7) show pairwise comparisons of means between the various study arms. Column (8) reports the p-value for the test that all study arms have the same mean. All variables are measured at baseline and are binary unless otherwise stated. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

Table 3
Attrition analysis.

Panel A: Unsaturated Model	(1) All attrition	(2) Not found	(3) No consent
Full Voucher	−0.052***	−0.0012	−0.050***
	(0.016)	(0.011)	(0.013)
Copay Voucher	−0.057***	−0.0025	−0.055***
	(0.016)	(0.011)	(0.013)
UCT	−0.049***	0.0070	−0.056***
	(0.018)	(0.012)	(0.013)
CCT	−0.089***	−0.026***	−0.062***
	(0.015)	(0.0091)	(0.012)
SMS: Plain	0.0080	−0.0059	0.014
	(0.015)	(0.0097)	(0.012)
SMS: Contextual	−0.0028	−0.0036	0.00084
	(0.017)	(0.011)	(0.013)
Constant	0.18***	0.041*	0.14***
	(0.032)	(0.021)	(0.027)
Panel B: Saturated Model	All attrition	Not found	No consent
Full Voucher * UCT	−0.11***	0.0097	−0.12***
	(0.030)	(0.018)	(0.026)
Full Voucher * CCT	−0.16***	−0.017	−0.14***
	(0.026)	(0.013)	(0.023)
Full Voucher * No Transfer	−0.12***	0.010	−0.13***
	(0.032)	(0.019)	(0.026)
Copay Voucher * UCT	−0.15***	−0.0078	−0.14***
	(0.028)	(0.016)	(0.024)
Copay Voucher * CCT	−0.15***	−0.014	−0.13***
	(0.027)	(0.013)	(0.025)
Copay Voucher * No Transfer	−0.11***	0.018	−0.13***
	(0.031)	(0.019)	(0.025)
UCT Only	−0.090***	0.039*	−0.13***
	(0.033)	(0.022)	(0.025)
CCT Only	−0.17***	−0.028***	−0.14***
	(0.025)	(0.010)	(0.024)
SMS: Plain	0.0076	−0.0067	0.014
	(0.015)	(0.0096)	(0.012)
SMS: Contextual	−0.0017	−0.0041	0.0023
	(0.017)	(0.011)	(0.013)
Constant	0.22***	0.034	0.18***
	(0.036)	(0.022)	(0.031)
Controls	X	X	X
Observations	1514	1514	1514
Dependent Variable Mean	0.075	0.028	0.046

Notes: The dependent variables in each column correspond to three forms of attrition. Column (1) takes the value 1 if the woman cannot be found or does not consent to the endline survey and 0 otherwise. Column (2) takes the value 1 if woman cannot be found at endline and 0 otherwise. Column (3) takes the value of 1 if the woman is found but does not provide consent for the endline and 0 otherwise. All estimates include as controls indicators for marital status, completed more than primary school, uses electricity or gas for lighting, has a traditional roof, had received antenatal care prior to recruitment, saw a doctor or TBA before recruitment and distance to nearest major clinic/hospital in thousands of meters. To preserve observations we impute missing values and include a missing indicator for each imputed variable.Standard errors in parentheses clustered at the village level. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

- $V_i^1 = 1$ if individual i is assigned to the full voucher group and zero otherwise;
- $V_i^2 = 1$ if she is assigned to the copay voucher group and zero otherwise;
- $T_i^1 = 1$ if she is assigned to the conditional cash transfer group and zero otherwise;
- $T_i^2 = 1$ if she is assigned to the unconditional cash transfer group and zero otherwise;
- $S_i^1 = 1$ if individual i is assigned to the contextual SMS group and zero otherwise; and
- $S_i^2 = 1$ if she is assigned to the plain SMS group and zero otherwise.
- X_i represents a set of M control variables to adjust for potential bias due to baseline imbalance.

Alternatively, we present results for the partially saturated model including interactions between the voucher and cash transfer interventions as follows:[10]

$$y_i = \alpha + \sum_{j=1}^{2} \beta^j V_i^j + \sum_{k=1}^{2} \gamma^k T_i^k + \sum_{j,k=1}^{2} \phi^{j,k} V_i^j \times T_i^k + \sum_{l=1}^{2} \delta^l S_i^l + \sum_{m=1}^{M} \tau^m X_i^m + \varepsilon_i$$

We estimate the impact of the free-care policy including potentially differential effects of the experimental treatments before and after the policy, by running an additive as well as a partially interacted model of the specifications above with an indicator equal to 1 if the expected delivery date is after the policy change.[11] We conduct a Chow test to determine the likelihood that the process determining facility delivery is the same before and after the free care policy.

4.2. Quality

To assess possible quality impacts, we used self-reported measures of quality that we collected in the endline survey, which we only have for all women who completed the survey and who reported having delivered in a health facility. A simple treatment-control comparison presents us with the obvious selection issue: quality is only observed for those who attend a facility and attendance rates differ by treatment assignment. For example, women who respond to the vouchers might be relatively poor, relatively healthy, or different in other unobserved ways. In turn, reported quality could respond in various ways to these underlying characteristics, say because health care workers treat poor people badly, because healthier women don't need such high quality, or because the perceptions of what constitutes "good" quality are different for the marginal client.

To address this problem, we exploit the orthogonal cash transfer arms of our experiment as instruments in a Heckman selection model (Heckman, 1976).[12] In particular, we argue that on a person-by-person basis, the cash transfers aimed at subsidizing transport costs should not directly affect quality, since providers do not have any claims on the funds made available to recipients, and they are unlikely to be aware of the subsidy status of individual women.[13] On the other hand, we know that the subsidy was instrumental in inducing women to deliver at a clinic.[14]

[10] We do not present results of the fully saturated model, in which the SMS treatments are also interacted with the other treatments.

[11] In additional results available from the authors, we also estimate a model with month dummies to capture potential time varying effects of the free care policy to reflect the gradual adoption of the change.

[12] We also estimated a propensity score matching model, in which we estimated the probability of clinic delivery on the control group, and matched women in the treatment group on the basis of their predicted probabilities. This method would also have allowed us to match women in the two groups who were more likely to answer the endline survey. However, the sample of close matches was too small to yield statistically meaningful results (available on request).

[13] Figure 3 suggests that the CCT could reduce quality if it increases demand sufficiently. We argue however that at the individual level, receipt of the CCT should not directly affect the quality of services offered to a particular woman, while possession of the voucher could induce the provider to supply higher quality care.

[14] It is possible that the process that drove selection to deliver at a clinic was different before and after the June 1st policy change. If so, we would need a second instrument to properly identify the impact of the vouchers on quality received, using data from both before and after the change. However, we observe little impact of the policy change on utilization rates, and suggest that it is difficult to see how the policy change would have altered the mix of women delivering in clinics, while leaving

Table 4
Impact of Vouchers and Cash Transfers on Facility Delivery.

Panel A: Unsaturated Model	(1) Endline Data	(2) Lower Manski Bound	(3) Upper Manski Bound	(4) Partial Match	(5) Full Match
Full Voucher	0.054**	0.019	0.12***	0.038	0.066**
	(0.024)	(0.025)	(0.023)	(0.032)	(0.031)
Copay Voucher	0.034	0.00093	0.099***	0.036	0.038
	(0.024)	(0.025)	(0.024)	(0.031)	(0.030)
UCT	0.045*	0.00023	0.11***	−0.0086	−0.024
	(0.024)	(0.026)	(0.024)	(0.032)	(0.030)
CCT	0.057**	0.051**	0.11***	0.080**	0.084***
	(0.023)	(0.024)	(0.023)	(0.031)	(0.031)
SMS: Plain	−0.0042	0.0018	−0.013	−0.0081	−0.016
	(0.021)	(0.023)	(0.021)	(0.029)	(0.028)
SMS: Contextual	−0.022	−0.0098	−0.027	0.0040	0.017
	(0.026)	(0.027)	(0.026)	(0.034)	(0.034)
Constant	0.79***	0.80***	0.67***	0.48***	0.35***
	(0.041)	(0.041)	(0.041)	(0.053)	(0.050)
Panel B: Saturated Model					
	Endline Data	Lower Manski Bound	Upper Manski Bound	Partial Match	Full Match
Full Voucher * UCT	0.11***	0.0062	0.26***	0.027	0.029
	(0.039)	(0.039)	(0.039)	(0.050)	(0.048)
Full Voucher * CCT	0.11***	0.046	0.25***	0.087*	0.12**
	(0.038)	(0.036)	(0.038)	(0.049)	(0.048)
Full Voucher * No Transfer	0.088**	−0.012	0.24***	0.068	0.096*
	(0.042)	(0.041)	(0.041)	(0.052)	(0.052)
Copay Voucher * UCT	0.11***	0.035	0.26***	0.040	0.026
	(0.039)	(0.038)	(0.039)	(0.051)	(0.049)
Copay Voucher * CCT	0.087**	0.011	0.23***	0.13***	0.14***
	(0.040)	(0.038)	(0.039)	(0.049)	(0.048)
Copay Voucher * No Transfer	0.048	−0.052	0.20***	−0.0055	−0.0043
	(0.042)	(0.041)	(0.041)	(0.050)	(0.047)
UCT Only	0.038	−0.084*	0.19***	−0.040	−0.037
	(0.044)	(0.043)	(0.042)	(0.051)	(0.047)
CCT Only	0.11***	0.054	0.25***	0.090*	0.079
	(0.040)	(0.037)	(0.040)	(0.052)	(0.050)
SMS: Plain	−0.0038	0.0040	−0.013	−0.0091	−0.018
	(0.022)	(0.023)	(0.021)	(0.029)	(0.028)
SMS: Contextual	−0.022	−0.0086	−0.029	0.0038	0.016
	(0.026)	(0.027)	(0.026)	(0.034)	(0.034)
Constant	0.94***	1.08***	0.79***	0.48***	0.36***
	(0.060)	(0.061)	(0.058)	(0.055)	(0.053)
Controls	X	X	X	X	X
F tests (p-values)					
CCT = UCT: Including voucher interaction parameters	0.37			0.048	0.01
CCT = UCT: Excluding voucher interaction parameters	0.098			0.035	0.039
Copay = Full: Including cash transfer interaction parameters	0.76			0.55	0.37
Copay = Full: Excluding cash transfer interaction parameters	0.36			0.24	0.081
Observations	1401	1514	1514	1514	1514
Dependent Variable Mean	0.85	0.85	0.85	0.51	0.39

Notes: This table provides linear probability estimates of the impact of treatments on facility delivery, which is defined in different ways. In column (1), the dependent variable takes on the value of 1 if the woman reports delivering in a facility in endline survey or 0 otherwise. In Column (4) the dependent variable takes on the value of 1 if the woman's delivery is partially matched to the register books using methodology described in the text while column (5) uses the full match algorithm instead. Columns (2) and (3) represent delivery outcomes that account for attrition following Manski (1990). The Lower Manski Bound assumes facility delivery for attrited controls and home delivery for attriting treated observations. The Upper Manski Bound assumes institutional deliveries for those who attrited from the treatment group. All estimates include as controls indicators for marital status, completed more than primary school, uses electricity or gas for lighting, has a traditional roof, had received antenatal care prior to recruitment, saw a doctor or TBA before recruitment and distance to nearest major clinic/hospital in thousands of meters. To preserve observations we impute missing values and include a missing indicator for each imputed variable. Standard errors in parentheses clustered at the village level. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

Following this strategy, we estimate a first stage equation of facility delivery instrumenting with assignment to the cash transfer arms, and estimate the second stage estimating the impact of vouchers on quality indicators, including the inverse Mills ratio.[15]

5. Results

5.1. Effects of experimental interventions on facility delivery rates

Table 4 presents our main experimental results on the impacts of the financial interventions including a set of baseline controls. The results of the unsaturated model, which effectively constrains treatment interactions to be zero, are shown in Panel A, while the saturated model results are reported in Panel B. The coefficients in each row reflect the mean impact of assignment to the respective experimental cell or cells. Column (1) uses self-reported data from the endline survey, which could suffer from both attrition and potential misreporting. To account for the former, in Columns (2)

utilization rates themselves unaffected. In light of this, we assume the selection process before and after June 1st was fixed.

[15] In results not shown, we also estimate specifications including month dummies, to account for the fact that quality changes might have emerged gradually over time, both because the policy change was announced in advance, and because some providers might have become aware of the difference in marginal revenues only slowly after the change was implemented.

and (3) we report lower and upper bounds on the estimated effects, using Manski's non-parametric method (Manski, 1990). Finally, in light of our concerns over selection and misreporting, Columns (4) and (5) use data on endline eligible sample of women, using data collected from the register books and the match algorithms discussed above, using either the partial or full match algorithms respectively. Our discussion will focus on Columns (1) and (5).

In both specifications, the first striking observation is that the mean of the dependent variable, reflecting the rate of facility deliveries amongst women, differ widely between Columns (1) and (5).[16] The fact that the facility delivery rate from the register data books is more in line with data from the DHS, provides further justification for our preference for the register book data over the data from the endline data, which we suspect suffered from over-reporting of facility deliveries. In addition, as previously discussed, we have no reason to believe that the register book data would be biased according to treatment status.

A first summary of treatment effects from the upper panel is that the full voucher and the CCT each have large impacts on delivery rates, increasing them by 6.6 and 8.4 percentage points off a base of 35.0 percent. The copay voucher and the UCT have little effect on average, as do the SMS treatments.

Turning to the saturated model in the lower panel, the impact of receiving just the full maternity voucher is 8.8–9.6 percentage points, using both endline and register book data. Similarly, the impact of receiving both the full maternity voucher and a CCT is 11.0 and 12.0 points. However, when coupled with an UCT, the measured impact of the full voucher with endline data (11.0 points) disappears in the register book data.

Using the full match protocol, women who were assigned the copay voucher, which required them to pay a small fraction (approximately 10 percent) of the regular delivery price, exhibited virtually no change in facility deliveries, unless the voucher was coupled with a CCT. This stands in contrast to the 10.0-point impact of the full voucher when received in isolation, significant at the 10-percent level. We report the p-value for F-test of the hypothesis of equal treatment effects of the full and copay voucher both when they are interacted with the cash transfers (Copay = Full: Including cash transfer interaction parameters), and when they are not (Copay = Full: Excluding cash transfer interaction parameters) at the bottom of the table. We cannot reject the hypothesis of pairwise equality of voucher effects in all three cash transfer groups (Copay = Full: Including cash transfer interaction parameters) – that is, a test of the hypothesis that (UCT x Full = UCT x Copay) *and* (CCT x Full = CCT x Copay) *and* (Full only = Copay only), however it is significant at the only the 10 percent level (p-value = 0.081) when the effects of the cash transfers are excluded.

Coupling the copay voucher with the CCT, however, increases the measured effect relative to the control group to 14.0 percentage points (8.7 points using endline data). As we observed with the full voucher, the impact of the copay voucher combined with the UCT is large when measured using the endline survey, but small and insignificant when using the register book data.

The effect of receiving the CCT alone is between 7.9 and 11.0 percentage points, while the impact of the UCT is statistically insignificant, using both data sources. In this case, as reported in the bottom panel, the effects of the CCT and UCT are statistically different from each other using the register book data, both across all groups ($p < 0.01$), and between the two cash transfer cells in which there was no maternity voucher ($p < 0.05$). In neither panel in any of the specifications, do we measure any significant association between assignment to either SMS treatment group and facility delivery rates.

Table 5
Impact of Free Delivery Policy on Facility Delivery.

	Dependent Variable: Full Match Facility Delivery		
	(1)	(2)	(3)
Panel A: Unsaturated Model	Full Sample	Before June 1	After June 1
Full Voucher	0.065**	0.069	0.061*
	(0.031)	(0.061)	(0.037)
Copay Voucher	0.039	0.0054	0.051
	(0.030)	(0.057)	(0.036)
UCT	−0.025	−0.029	−0.024
	(0.030)	(0.057)	(0.036)
CCT	0.083***	0.056	0.097***
	(0.031)	(0.058)	(0.037)
SMS: Plain	−0.016	−0.0092	−0.016
	(0.028)	(0.053)	(0.033)
SMS: Contextual	0.017	−0.082	0.061
	(0.034)	(0.061)	(0.041)
Post-Jun1	0.033		
	(0.028)		
Controls	X	X	X
Constant	0.32***	0.38***	0.32***
	(0.057)	(0.10)	(0.059)
Observations	1514	436	1078
Chow Test (p-value)		0.34	
Panel B: Saturated Model	Full Sample	Before June 1	After June 1
Full Voucher * UCT	0.026	−0.015	0.042
	(0.048)	(0.094)	(0.056)
Full Voucher * CCT	0.12**	0.14	0.11*
	(0.048)	(0.088)	(0.058)
Full Voucher * No Transfer	0.093*	0.052	0.11*
	(0.052)	(0.099)	(0.061)
Copay Voucher * UCT	0.026	0.013	0.028
	(0.049)	(0.089)	(0.058)
Copay Voucher * CCT	0.14***	0.027	0.19***
	(0.048)	(0.088)	(0.057)
Copay Voucher * No Transfer	−0.0047	−0.036	0.0066
	(0.047)	(0.088)	(0.057)
UCT Only	−0.038	−0.064	−0.034
	(0.047)	(0.089)	(0.056)
CCT Only	0.076	0.021	0.11*
	(0.050)	(0.10)	(0.059)
SMS: Plain	−0.018	−0.0045	−0.021
	(0.028)	(0.054)	(0.034)
SMS: Contextual	0.017	−0.080	0.057
	(0.034)	(0.061)	(0.041)
Post-Jun1	0.033		
	(0.029)		
Controls	X	X	X
Constant	0.33***	0.39***	0.33***
	(0.059)	(0.11)	(0.063)
Observations	1514	436	1078
Chow Test (p-value)		0.51	

Notes: This table presents two tests for changes in the delivery specification before and after June 1. The dependent variable takes on the value of 1 if the woman's delivery is a full match with the register books. Column (1) is the estimate for all endline eligible women, while column (2) estimates the model for women with expected delivery dates before June 1, 2013 and column (3) estimates it for women with expected due dates after that date. All estimates include as controls indicators for marital status, completed more than primary school, uses electricity or gas for lighting, has a traditional roof, had received antenatal care prior to recruitment, saw a doctor or TBA before recruitment and distance to the nearest major clinic or hospital in thousands of meters. To preserve observations we impute missing values and include a missing indicator for each imputed variable. Standard errors in parentheses clustered at the village level. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

Table 5 incorporates temporal effects to measure any changes to the data generating process that might have occurred after the introduction of the free care policy. Again, we present the unsaturated model in the top panel, and the saturated version in the lower

[16] When asked at endline, 79 percent of respondents in our control group reported delivering in a facility, while the validated data from the facilities' record books using the full match suggest only 33 percent of the pure control women were validated to have delivered in a clinic. The partial match yields a rate in the control group of 46 percent.

panel. Using the full match validated register book data, Column (1) first includes a dummy variable equal to 1 for women whose expected delivery data was after June 1, 2013, but this is found to have no detectible impact on delivery rates. Columns (2) and (3) in Table 5 estimate the basic models respectively before and after the policy change. Although the parameter values are similar in the two periods, we lack power in the pre-policy change regime to generate the same statistical significance levels. The *p*-values from a Chow test are presented at the bottom of each panel and suggest no change in the data generating process before and after the policy.

Our findings suggest that the government's policy of free care had little impact, if any, on the demand for maternal services in the time period we observe after the policy came into force.[17] A number of reasons could lie behind this finding. First, women may not have known about the free care policy, although as part of the new government's platform, it was widely publicized and discussed in the media. In our endline survey we asked women if they were aware of the free maternity care policy, and almost all women (93%) reported knowing about the policy and finding out about the policy while they were pregnant. Lack of knowledge of the policy was therefore not widespread.

Second, the free care policy might not have reduced all costs associated with delivery to zero. Our endline survey provides some support to this argument, with women who did not receive a voucher and who gave birth after June 1, 2013 reporting average delivery-related expenditures of 782 KSh (about $US10 at the time). Voucher recipients on the other hand reported costs of 375 KSh on average. This roughly 50 percent cost difference was however much smaller than for women who gave birth before the free care policy came into effect: women without a voucher reported costs of 1760 KSh, while voucher recipients paid 337 KSh on average, a difference of 81 percent. The free care policy appears to have lowered the cost to voucher non-recipients, but only by about half, and not to zero. This could account for the continued efficacy of the vouchers in inducing some women to deliver at a facility.

A third reason for the limited impact on observed demand could have been due to concerns about quality of care. In our endline survey, we also asked women who were aware of the policy whether or not they believed the free maternity care policy would improve the quality of services in Kenya and slightly more than 70% of them disagreed that it would.

Taken together our findings are consistent with the following observations on the effectiveness of demand side interventions at increasing facility delivery rates. First, we find evidence that both the full voucher and the CCT were very effective at increasing demand for institutional deliveries in our sample of women. The full voucher alone increased the utilization of services by 9.6 percentage points while the CCT alone increased it by 7.9 percentage points. When given together, women increased their propensity to deliver in a facility by 12.0 percentage points, or a roughly 33% increase compared to the control, a very large proportional increase in delivery rates.

Second, we find that while both the co-pay voucher and the UCT had some impact according to the endline survey, neither demonstrated any significant effects using the register book data. Women who received a voucher or an UCT appear to have over-reported their rates of facility deliveries in the endline survey. What is more challenging to understand is why there is an impact on facility deliveries for women with a full maternity voucher only, while (according to the register book data) this effect is offset by receipt of an UCT. One possible explanation is that women with both a full voucher and the UCT misunderstood the combination, and interpreted the cash received as being the voucher itself, intended to finance maternity services, but realized they could spend it on something else. An alternative reason could be that women prefer to deliver at a private facility or with a traditional birth attendant, or at a facility outside Vihiga, and that receipt of the UCT leads them to pay for such alternatives, even when care at the local facilities is free.[18] We further investigated this hypothesis by first observing that the number of deliveries at private facilities in Vihiga, or at any facility outside the county, was very low – only 1–2 percent as reported in the endline survey across all treatment arms. Notwithstanding the possible bias of the endline data, it seems unlikely that a large number of women use the UCT to purchase care from either of these alternative sources.

In further interrogating the data, we also explore heterogeneous impacts of the interventions by baseline characteristics using a two-step process. First, we applied machine-learning techniques to identify baseline characteristics and cut-offs that were associated with larger or smaller treatment effects using the Causal Tree algorithm (Athey and Imbens, 2015). Second, we run OLS regressions that include interactions (and main effects) of treatment indicators with each of baseline characteristics identified by the Causal Tree algorithm. In Table 6, we present the results for two of the identified baseline variables – indicator variables for phone ownership and whether the woman lived less than 4.26 km to the nearest major clinic/hospital. We do not present the results for the other identified variables – indicator variables for gestation age < 18 weeks and mothers age < 24 years – but the results are available on request. Our main outcome variable is the full match definition of facility delivery. Columns (1)-(2) present one estimation, with the main effects reported in column (1) and the interaction with *mobile phone ownership* coefficients in column (2); Columns (3) and (4) present corresponding results when we control for the main and (saturated treatment) interaction effects of the wealth index. Columns (5)-(8) present results for *distance to major clinic/hospital <4.26 km*; and columns (9)-(11) present estimates of a specification with both sources of heterogeneity included and controls for wealth (main effect and treatment interactions).

Our preferred results from columns (3) and (4), which suggest that phone ownership mediates the size of treatment impacts, including for the UCT. In particular, across all of the main treatments, impacts are *larger* for women who don't own a phone. Amongst women who do not own phones at baseline, facility delivery rates increase by 17, 13, and 8 percentage points for the Voucher and UCT, Copay and UCT, and UCT only arms. Other treatment arms also show large effects for women without a mobile phone. In particular, women without a phone assigned to receive the contextual reminders are nearly 10 percentage points more likely to have a facility delivery (significant at the 10 percent level).

Our results in columns (7) and (8) suggest that distance plays a weaker role in mediating the size of treatment effects. The sign of the interaction terms varies, even though on balance it appears that women who live closer to major facilities respond more to the interventions.

In columns (9)-(11) we run a specification that includes interactions with both sources of heterogeneity in addition to controls for interactions with a wealth index. The results confirm the patterns we observed above – impacts are considerably larger for women

[17] Data from the 2014 Kenyan DHS (not shown) also shows facility delivery rates increased almost linearly between 2009-2014 with no measurable jump in deliveries in 2013 when the policy was first introduced.

[18] We test the hypothesis that women with UCT act on strong preferences to deliver in private/NGO facilities or outside the district. The *p*-value of our statistical test of no differences in this behavior between the CCT and UCT is 0.56. Similarly, we find no evidence that these women were more likely to travel further for their deliveries, or use more expensive modes of transport.

Table 6
Treatment Effect Heterogeneity - cell phone ownership and distance to clinic.

Interaction Variable	Mother has personal phone				Distance to major clinic/hospital <4.26 km					Personal Phone	Distance <4.26 km
	Treatment Main Effect	Treatment* Interaction Variable	Treatment Main Effect	Treatment* Interaction Variable	Treatment Main Effect	Treatment* Interaction Variable	Treatment Main Effect	Treatment* Interaction Variable	Treatment Main Effect	Treatment* Interaction Variable	Treatment* Interaction Variable
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Full Voucher * UCT	0.16**	−0.20**	0.17**	−0.21**	−0.021	0.082	−0.015	0.076	0.12	−0.22**	0.083
	(0.078)	(0.098)	(0.078)	(0.099)	(0.075)	(0.098)	(0.076)	(0.098)	(0.097)	(0.099)	(0.098)
Full Voucher * CCT	0.21**	−0.14	0.23***	−0.15	0.20**	−0.10	0.21**	−0.11	0.30***	−0.15	−0.11
	(0.084)	(0.10)	(0.084)	(0.10)	(0.079)	(0.099)	(0.080)	(0.099)	(0.10)	(0.10)	(0.099)
Full Voucher * No Transfer	0.13	−0.042	0.13	−0.042	0.14	−0.054	0.14	−0.049	0.17	−0.040	−0.055
	(0.082)	(0.10)	(0.082)	(0.10)	(0.089)	(0.11)	(0.089)	(0.11)	(0.11)	(0.10)	(0.11)
Copay Voucher * UCT	0.13*	−0.16	0.13*	−0.14	−0.062	0.15	−0.062	0.16	0.047	−0.17*	0.17*
	(0.077)	(0.099)	(0.077)	(0.10)	(0.072)	(0.099)	(0.073)	(0.100)	(0.093)	(0.10)	(0.100)
Copay Voucher * CCT	0.26***	−0.18*	0.25***	−0.17*	0.11	0.064	0.11	0.051	0.21**	−0.17*	0.061
	(0.078)	(0.099)	(0.077)	(0.099)	(0.078)	(0.10)	(0.078)	(0.10)	(0.095)	(0.10)	(0.10)
Copay Voucher * No Transfer	0.14*	−0.22**	0.14*	−0.22**	−0.056	0.082	−0.051	0.078	0.082	−0.22**	0.085
	(0.073)	(0.095)	(0.074)	(0.096)	(0.073)	(0.096)	(0.073)	(0.096)	(0.092)	(0.097)	(0.096)
UCT Only	0.076	−0.17*	0.082	−0.18*	−0.037	−0.013	−0.035	−0.018	0.086	−0.19*	−0.0052
	(0.080)	(0.099)	(0.082)	(0.10)	(0.074)	(0.096)	(0.073)	(0.097)	(0.100)	(0.10)	(0.097)
CCT Only	0.14*	−0.081	0.13*	−0.069	−0.0065	0.14	0.0011	0.13	0.064	−0.078	0.12
	(0.079)	(0.10)	(0.079)	(0.10)	(0.080)	(0.10)	(0.081)	(0.11)	(0.099)	(0.10)	(0.11)
SMS: Plain	−0.0066	−0.021	0.0057	−0.038	−0.022	0.018	−0.022	0.019	−0.0010	−0.039	0.023
	(0.046)	(0.058)	(0.046)	(0.059)	(0.045)	(0.058)	(0.044)	(0.058)	(0.056)	(0.060)	(0.059)
SMS: Contextual	0.091	−0.12*	0.094*	−0.12*	0.027	−0.019	0.032	−0.026	0.10	−0.12*	−0.013
	(0.056)	(0.070)	(0.056)	(0.071)	(0.053)	(0.069)	(0.053)	(0.070)	(0.068)	(0.071)	(0.070)
Interacting Variable: Main Effect	0.17***		0.17***		−0.055		−0.053			0.18***	−0.062
	(0.065)		(0.065)		(0.067)		(0.067)			(0.065)	(0.067)
Constant	0.25***		0.24***		0.34***		0.34***		0.23***		
	(0.063)		(0.063)		(0.060)		(0.061)		(0.071)		
Controls	X		X		X		X		X		
Main and Interaction Effect with Wealth Index			X				X		X		
Observations	1514		1514		1514		1514		1514		

Notes: This table presents linear probability estimates of the impact of voucher, cash transfer and SMS treatments interacted with baseline characteristics. Columns (1) and (2) presents the main effects and interactions with an indicator for mother owns her own cell phone. Columns (3) and (4) represents the corresponding estimates for the interaction with an indicator for household lives less than 4.26km away from the nearest program clinic. The dependent variable takes on the value of 1 if there is a matched (verified using delivery registers) delivery at one of the 8 program facilities and 0 otherwise. All estimates include as controls indicators for marital status, completed more than primary school, uses electricity or gas for lighting, has a traditional roof, had received antenatal care prior to recruitment, saw a doctor or TBA before recruitment and distance to the nearest major clinic or hospital in thousands of meters. To preserve observations we impute missing values for control variables and include a missing indicator for each imputed variable. In columns (3) and (4), we also include the main effect and interactions of treatments with the indicator for missing data for distance to clinic. Standard errors in parentheses clustered at the village level. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

Table 7
Impact of Voucher and Transfer treatments on other maternal services.

Panel A: Hospital Deliveries and Referrals	(1) Inconsistent: Full Match	(2) Hospital: Full Sample	(3) Hospital: Consistent Only	(4) Referral: Full Sample	(5) Referral: Consistent Only
Full Voucher * UCT	0.067	0.056	0.090	0.053	0.085
	(0.053)	(0.044)	(0.061)	(0.040)	(0.063)
Full Voucher * CCT	−0.030	0.12***	0.093	0.10**	0.11*
	(0.052)	(0.045)	(0.057)	(0.040)	(0.056)
Full Voucher * No Transfer	0.013	0.025	0.063	0.065	0.026
	(0.056)	(0.046)	(0.062)	(0.043)	(0.054)
Copay Voucher * UCT	0.069	0.080*	0.13**	0.030	0.061
	(0.054)	(0.045)	(0.065)	(0.039)	(0.062)
Copay Voucher * CCT	−0.059	0.083*	0.13**	0.065*	0.087
	(0.051)	(0.044)	(0.056)	(0.039)	(0.054)
Copay Voucher * No Transfer	0.078	−0.0054	−0.021	−0.0084	0.023
	(0.053)	(0.042)	(0.055)	(0.036)	(0.055)
UCT Only	0.12**	0.023	−0.00062	0.015	0.019
	(0.054)	(0.044)	(0.056)	(0.038)	(0.060)
CCT Only	0.066	0.017	0.039	0.0096	0.032
	(0.054)	(0.043)	(0.059)	(0.037)	(0.057)
SMS: Plain	0.0044	0.022	−0.038	−0.0045	−0.051
	(0.030)	(0.026)	(0.036)	(0.023)	(0.037)
SMS: Contextual	−0.042	−0.0097	−0.033	−0.025	−0.046
	(0.036)	(0.030)	(0.040)	(0.026)	(0.040)
Constant	0.43***	0.27***	0.22***	0.14***	0.12*
	(0.060)	(0.049)	(0.065)	(0.044)	(0.064)
Controls	X	X	X	X	X
Inconsistent self reports included		Y	N	Y	N
Observations	1401	1401	724	1189	530
F Tests (p-value)					
CCT = UCT: All women		0.71	0.95	0.64	0.96
CCT = UCT: No voucher women only		0.91	0.55	0.90	0.85
Copay = Full: All women		0.81	0.55	0.32	0.98
Copay = Full: No cash transfer women only		0.55	0.22	0.10	0.95

Panel B: ANC and PNC visits	4+ANC	4+ANC	3+PNC	3+PNC
Any Voucher * No Transfer	0.032	0.064	−0.012	−0.015
	(0.040)	(0.056)	(0.034)	(0.047)
UCT Only	0.030	−0.021	0.0011	0.0078
	(0.048)	(0.075)	(0.041)	(0.061)
CCT Only	0.026	0.045	−0.015	−0.015
	(0.049)	(0.068)	(0.042)	(0.059)
Any Voucher * UCT	−0.025	−0.010	0.026	−0.0079
	(0.060)	(0.091)	(0.053)	(0.077)
Any Voucher * CCT	−0.039	−0.085	−0.028	−0.020
	(0.060)	(0.083)	(0.053)	(0.074)
SMS: Plain	0.022	−0.0041	−0.014	0.019
	(0.027)	(0.038)	(0.025)	(0.035)
SMS: Contextual	−0.012	−0.034	0.035	0.056
	(0.032)	(0.044)	(0.027)	(0.037)
Constant	0.65***	0.65***	0.79***	0.73***
	(0.054)	(0.077)	(0.049)	(0.065)
Controls	X	X	X	X
Inconsistent self reports included	Y	N	Y	N
Observations	1371	711	1309	686

Notes: This table presents linear probability estimates of the impact of experimental treatments on other delivery outcomes. In Panel A Columns (1) and (2) start by indicating the impact of treatments on misreporting delivery based on the partial (1) and full (2) matches. Columns (4) and (6) exclude all observations that are not identified as having delivered in a facility using the partial match. In Panel B, we examine the impact of treatments on recommended utilization of ANC (at least four visits) and PNC (3 visits). Note that since there was no co-pay for these services, the voucher arms are collapsed into one treatment. All estimates include as controls indicators for marital status, completed more than primary school, uses electricity or gas for lighting, has a traditional roof, had received antenatal care prior to recruitment, saw a doctor or TBA before recruitment and distance to the nearest major clinic or hospital in meters. To preserve observations we impute missing values for control variables and include a missing indicator for each imputed variable. Standard errors in parentheses clustered at the village level. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

who don't own a phone at baseline. We speculate that commitment to deliver in facilities may be enhanced by interactions with another household member who, being the owner of the phone number with which women first registered, mediates the delivery of information and/or financial incentives.

5.2. Effects on demand for other services

Table 7 reports treatment effects on other outcomes, including hospital (as opposed to clinic) deliveries, referrals, and the number of reported ANC and PNC visits. Information on these outcomes is only available from the endline survey, so we report results using both the full endline sample, and a sub-sample restricted to those interviewed at endline whose answers to the delivery question were consistent with the full match delivery outcome. Column (1) reports a regression in which the dependent variable is equal to one if her endline delivery response is inconsistent with the record book data. Assignment to the UCT only cell is associated with more inconsistent responses. There are, however, no systematic patterns across the other cells.

Table 8
Impact of treatments and free-maternity care on reported quality measures, Heckman corrections.

	(1) Index of Counseling services	(2) Index of Goods and Services	(3) Respect: Excellent and very good	(4) Overall Satisfaction with Delivery
Full Voucher	0.47**	0.017	0.28	0.15
	(0.20)	(0.22)	(0.22)	(0.11)
Copay Voucher	0.27	−0.12	0.33	0.076
	(0.19)	(0.20)	(0.21)	(0.099)
Full voucher*Post June 1	−0.27	0.070	−0.47*	−0.20*
	(0.22)	(0.23)	(0.24)	(0.11)
Copay voucher*Post June 1	−0.043	0.31	−0.44*	−0.0011
	(0.22)	(0.23)	(0.25)	(0.12)
Post-Jun1	0.19	−0.044	0.47***	0.10
	(0.16)	(0.17)	(0.17)	(0.082)
SMS: Reminder	0.038	−0.076	−0.12	−0.017
	(0.092)	(0.098)	(0.10)	(0.049)
SMS: Contextual	0.085	−0.027	−0.095	−0.0081
	(0.11)	(0.11)	(0.12)	(0.057)
Controls	X	X	X	X
Constant	−0.30	−0.0064	0.19	0.90***
	(0.51)	(0.56)	(0.56)	(0.27)
Inverse mills ratio	0.37	−0.056	−0.46	−0.36
	(0.40)	(0.43)	(0.44)	(0.22)
Observations	1461	1440	1461	1479
F tests (p-values)				
Free Regime Not Different	0.46	0.43	0.061	0.22
Voucher Recipients No different in Free Regime¨	0.40	0.37	0.097	0.14

Notes: This table presents Heckman selection estimates of the impact of voucher assignment on the quality of services reported by women. The first stage probit is not reported (available on request). Column (1) & (2) represent indices of counseling and services inputs. Column 3 is an index of how respectful facility staff were and column 4 is an indicator for overall satisfaction with the delivery. We use an indicator for assignment to the conditional cash transfer as our instrument for the first stage. All estimates include as controls indicators for marital status, completed more than primary school, uses electricity or gas for lighting, has a traditional roof, had received antenatal care prior to recruitment and saw a doctor or TBA before recruitment. To preserve observations we impute missing values for control variables and include a missing indicator for each imputed variable. Standard errors in parentheses clustered at the village level. ***, ** and * indicate statistical significance at the 1, 5 and 10 percent level respectively.

Columns (2) and (3) of Panel A report effects of random assignment on hospital deliveries, for the full endline sample and amongst all those whose delivery answers were consistent with the record books. Similarly, columns (4) and (5) correspondingly report results for referrals. Having the full voucher and the CCT had a large effect on hospital delivery, raising it by 12 and 9 points, respectively, off a base of about 20 percent (using both samples). The copay voucher, when coupled with either cash transfer, does have an effect on hospital deliveries, but not in the absence of a cash transfer. Similarly, CCT or UCT by itself has no effect. The impacts on referrals follow those for hospital deliveries closely, suggesting the increase in hospital deliveries might be due to supply-side decisions rather than demand-side price effects. This in turn might mitigate concerns that a voucher program could lead to excessive demand at higher-level facilities, if referring health workers make decisions based primarily on perceived need.

In Panel B of the same table, we report results for regression of ANC and PNC visits on treatment assignment. In this table, we combine the full voucher and copay voucher arms, because under both non-delivery services were free (that is, there was *no* copayment for ANC or PNC services under the copay voucher arm). Across the board, we find no noticeable effect of any of the treatments on demand for ANC or PNC services – none of reported coefficients is statistically significant. These results are likely due to the fact that ANC and PNC attendance rates are relatively high in our sample – unconditional means (not shown) indicate that more than 70 percent of the control group made at least 4 ANC visits, while more than 80 percent made at least 3 PNC visits.

5.3. Effects on self-reported quality

Table 8 explores whether assignment to treatment affected measures of self-reported quality using the cash transfers and SMS reminders as instruments in a Heckman selection model, to account for endogenous facility delivery and participation in the endline survey.[19] We do not report the first stage, but include the inverse Mills ratio as an independent regressor. The quality measures include indices constructed for each of the following: (i) whether the woman received various counseling services during her delivery; (ii) the availability and use of various supplies and materials such as gloves, gauze, medications, etc.; (iii) a measure of overall respect shown by facility staff to clients; and (iv) a measure of overall satisfaction. The data used are reports by those women who delivered in a facility as determined from the register books. The coefficient on the Mills ratio is not significant across all regressions, suggesting selection is not consistently biasing the quality results.

Based on this analysis, there is no evidence of any decline in perceived quality following the introduction of the free maternity care policy on June 1. Although the evidence is relatively weak, we do

[19] We collected information on the quality of care the women received during their pregnancy, including information on whether the woman was counseled on such topics as breast-feeding, umbilical cord care and other perinatal care, immunization, and hand-washing, all considered practices that are advantageous the child (Larson et al., 2014). From these questions, using principal component analysis we construct an index of counseling coverage. Women were also asked whether they received potentially important health commodities during their delivery, including antibiotics (or another drug) by intravenous drip, a pill to stop the contraction of the uterus post birth (alluding to the use of misoprostol which is recommended postpartum for most women), manual removal of the placenta, or a blood transfusion. While higher levels of these services are generally understood to be a positive measure of quality, their appropriate use depends on the woman's needs. From these indicators we constructed a service availability index, also using principal component analysis. We also asked all women who reported having delivered in a health facility to rate the quality of care they received as excellent, very good, good, fair or poor. As measures of perceived quality, we believe the responses to this question are more subjective than the indicators described above. Some questions related to the quality of care delivered at an individual level: how would you rate your experience of being greeted and talked to respectfully?; how would you rate the privacy you were given (e.g. having a curtain)?; and how would you rate the clarity with which health providers explained things to you? From these questions we calculated an index of respect.

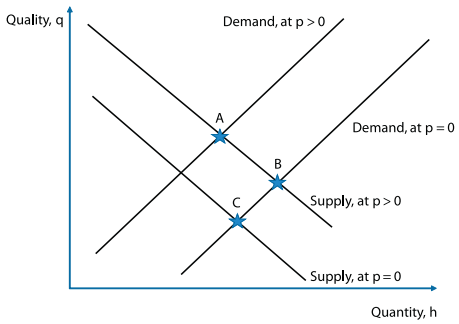

Fig. 1. Demand and supply in quantity-quality space. Equilibrium under fee-for-service is at A; with fully reimbursed free care the equilibrium is at B; with free care and no marginal reimbursement, the equilibrium is at C.

find some support that before the policy change, provision of counseling services were perceived to be higher for voucher recipients (Column (1)). Respect shown to clients appears to have increased after the June 1 st policy change for voucher non-recipients (Column (3)), essentially converging to that of voucher holders. Other measures of quality did not change noticeably at that time. One interpretation of the data is that quality levels might have differed prior to the free-care policy, but this differential was eliminated afterwards. One possible rationalization of this result is that women without vouchers who received free care might have revised their quality expectations downward, and reported greater satisfaction than they otherwise would have, however, we are limited by the availability of data on quality to be able to draw firm conclusions.

6. Discussion: demand-side subsidies and quality

In this section we present a framework in which demand-side subsidies in the form of vouchers, and cash transfers, both conditional and otherwise, could be integrated with the free maternity care policy. Given the important concerns raised in the introduction with regards to how these policies maintain quality, we allow the quality of services to adjust endogenously to different subsidy regimes. The types of quality of adjustments that this could entail, but are not limited to, include the amount of crowding in health facilities, wait times at clinics, provider effort, and other measures of interpersonal care, such as those we attempted to measure with the self-reported measures of quality. The resulting model provides a simple way to compare the likely effects of the policy interventions discussed in this paper and how they might interact in combination. While the link to our empirical results is incomplete, primarily due to the limitations we face in measuring quality of care, the framework helps rationalize some of our findings.

Theoretically, prices for deliveries and other services are fixed in Kenya, although as is common in many low-income countries some informal payments and additional charges may also occur. Under the assumption that prices are not flexible enough to clear the market, we present a model in which quality adjustments play an equilibrating role.

First, suppose the price of a delivery paid by consumers is equal to that received by clinics, and is fixed at price p. It is natural to assume consumers are sensitive to quality, and the clinics face a trade-off between providing a greater volume of services or of providing higher quality services. As a result, supply and demand curves in (volume, quality)-space are downward- and upward-sloping respectively. For a given price, equilibrium quality equates demand and supply. See Fig. 1.

For a given level of quality, supply is increasing in price, and demand is decreasing. This observation allows us to track the impact of price changes effected by different policies on the equilibrium quality by considering shifts in the supply and demand curves.

Vouchers: A voucher reduces the price paid by the consumer without affecting the price received by the clinic. Thus, while the supply curve is unaffected, the demand curve shifts to the right, so equilibrium quality falls and the volume of services increases.

Conditional Cash Transfer: A CCT has the same effect on the price faced by the consumer, but does not impact the clinic price, so has the same effect as a voucher when the prevailing price is greater than the transfer.

Free care: The impact of a free care policy depends crucially on how it is implemented. For example, Gitobu et al. (2018) and Pyone et al. (2017) point to ambiguities in Kenya's free care policy around which services were meant to be free, and delays in reimbursement, while there were few reliable verification mechanisms for claims submitted by facilities to the government. Conceptually, three scenarios can be distinguished:

(i) If the effect is to reduce the consumer price to zero while leaving the clinic price unaffected, supply is unaffected and demand shifts out, with the same impact on volume and quality as observed with a voucher or a CCT.

However, the clinic price is unaffected only inasmuch as it is reimbursed promptly for each delivery performed in an amount equal to the previously prevailing price. There are two ways in which this assumption might not be sustained:

- Delays in payment, explicit underpayment, and the administrative costs of tracking and following up on payments could all mean the effective price received by the clinic per delivery is less than the previously prevailing price. In this case, the free care policy shifts supply down, and could result in (a) an offsetting fall in equilibrium volume, and (b) a further contraction of quality. Depending on the elasticities, volume under a free care policy could even be lower than under a fee for service model, and at much lower quality.
- Alternatively, facilities could submit false invoices for deliveries. In the extreme case, this would break the link between revenue and services, effectively yielding a zero marginal per-delivery price, with an outcome similar to that in (ii) above. On the other hand, a clinic might guard against audit by overstating, but nonetheless linking, invoices to actual deliveries. Such a strategy would result in an effective price per actual delivery that was *higher* than the official reimbursement rate. The supply curve would then shift out, offsetting the quality fall identified in (i), and further increasing the equilibrium volume of services.

Therefore, the impact of both the vouchers and CCTs during a free care regime will also depend on the implementation of the free care policy itself: in the short run, a poor understanding by providers and/or households could yield treatment effects similar to those obtained before the free care policy was implemented, consistent with the results presented in Table 5. And in the long run steady state, when the free care policy is fully implemented, utilization could potentially increase.

7. Conclusions

In this paper we report results of a randomized evaluation of two types of financial incentives delivered over the mobile network aimed at boosting the use of maternal health care services amongst poor rural women in western Kenya: vouchers and cash transfers. We also tested the efficacy of text messages delivered over the mobile phone network in increasing demand for care in

this population. Finally, we comment on evidence of the impact of a free care policy introduced by the government of Kenya during the course of our experiment.

Our main findings suggest that two of the interventions tested – full maternity vouchers and CCTs – were highly effective at driving important improvements in facility delivery rates. The combination of the two incentives shows correspondingly larger, although sub-additive, impacts. On the other hand, vouchers with a small copayment only had a very small positive but statistically insignificant effect and among women who don't receive any cash transfers, we are able to draw a moderately strong conclusion about the discontinuity of the demand function at zero as our point estimate of the impact of the 10 percent copayment is statistically distinguishable from the (significant) estimate of the effect of the full voucher at only the 10% level (p-value = 0.083). We take this as weak evidence that small cost-sharing may discontinuously decrease the demand for health services, similar to previous studies of preventative health products, but acknowledge that more research is needed to better understand this relationship.

Our secondary findings relate to the lack of evidence of effectiveness of some of the interventions investigated. We find no evidence that the UCT or either of the text message interventions increased facility delivery rates. And while this study was not specifically designed to evaluate the impact of the free maternity care policy, we also do not observe any significant impact of that policy on demand over the time period of the study. Indeed, it is notable that the full voucher, which should not have directly affected demand after the government's free care policy went into effect, nonetheless continued to boost delivery services. We developed a model that integrates the various demand side incentives to rationalize some of these findings.

While we believe that our study adds to the current literature in many ways, we also acknowledge it has a number of limitations which could limit its findings or its generalizability. First, while we had access to register book data women who delivered in a clinic, which allowed us to validate the self-reported data on where births took place, we were unable to generate a similar matched database for other outcomes (e.g. ANC) to allow us to provide unbiased measures of those outcomes. We also rely upon self-reported measures of quality, which may be a function of expectations and is likely a more subjective measure of quality than other more directly observed measures of quality, which could also limit our ability to measure changes in quality in our outcomes. In our discussion of the various interventions, we describe the vouchers and the cash transfers as primarily financial interventions and the text messages as an informational intervention but we acknowledge that all of these interventions could have an effect at increasing information and salience for the importance of facility-based deliveries above and beyond just through the impact of the financial mechanism on demand. As we do not have a control in which women receive vouchers or cash transfers of zero dollars, we cannot disentangle these effects in our results. In addition, while we managed to enroll a large number of women into our study in one geographic area, it is unclear if we would expect similar effects sizes if these interventions had been implemented at scale or in geographies. Finally, as we only have data on 14 months of delivery data, it was not possible to control for any seasonal effects that could affect our results.

Our work also confirms the feasibility of delivering financial and informational incentives over the mobile network to poor women in remote areas. The conditional cash transfers, which potentially require little in the way of registration and documentation, could be especially easy to administer at scale. However, our research tentatively also provided some subtle insights into the potential mechanisms behind the effectiveness of mHealth strategies. For example, our investigation of impact heterogeneity actually suggests that women did not own their own mobile phone were more likely to respond to some of the experimental interventions than those who did. One interpretation of the observed heterogeneous treatment effect is engagement by a third party (the owner of the phone) may have encouraged to woman to deliver in a clinic and that such personal interaction was instrumental in inducing behavior change.[20] Future research could further investigate the impact of intermediaries in influencing the effectiveness of mHealth initiatives.

Increasing coverage access to maternal health services was a key priority for the global community as part of the Millennium Development Goals. As part of the Sustainable Development Goals, the international community has committed to achieve Universal Health Coverage, which entails all citizens having access to quality health services without undue financial hardship. The findings of our study suggest that reducing financial barriers to maternal health services could lead to important increases in the proportion of women delivering in a health clinic but our findings also suggest challenges associated with maintaining quality of services. Future research should aim to better understand both the impact of policies on utilization as well as its impact on quality of care delivered.

Appendix B. Supplementary data

Supplementary material related to this article can be found, in the online version, at doi:https://doi.org/10.1016/j.jhealeco.2018.12.001.

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