Why don't the poor receive antenatal care?: Evidence from incentive and information experiments in rural Nigeria

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Abstract

This paper examines why promoting antenatal care among the poor is difficult based on randomized experiments in rural Nigeria. Treatment effects are strongly differentiated by individual history. First, cash incentives conditional on fast uptake are effective only among women who received care before. Others who are less likely to receive care but could learn most from their first visit are irresponsive; we fail to promote their follow-up visits through learning. Second, the combination of lack of experience and low cognitive ability (illiteracy) can adversely affect information intervention (negative effects); though information combined with incentives does not alter the incentive effects.

Keywords: Antenatal care; Randomized experiment; Conditional cash transfer; Information; Experience; Cognition; Nigeria.

JEL classification: O15.

1. Introduction

Why do the poor underinvest in preventive health care that could tremendously improve their health? This is a critical question for effective policy interventions (Dupas, 2011b). Antenatal care is critically important for reducing maternal and infant mortality and morbidity risks (Lavender et al., 2007). Antenatal care package often consists of micronutrient supplementation, screening for infections and anemia, counseling/testing on HIV, counseling on family planning/infant feeding, preventative anti-malaria medication, and tetanus toxoid vaccination (two does and injections, respectively, are needed for the last two).[1] As more than one uptake with a suggested interval are required for full benefits of antenatal care, early and repeated uptakes are needed. African mothers' uptake, however, is considerably low and slow. In rural Nigeria where we conducted our study, the proportion of pregnant women who received at least one antenatal care is 46.4%, and among them only 27% made their first visit in their less-than-fourth months pregnant, though 77% made four or more visits suggested by World Health Organization (WHO) (National Population Commission, 2009). Compared to other preventive health technologies and behaviors, such as bed nets, water treatment, deworming, contraceptives, and immunization, antenatal care in Africa has been less studied by economists.[2] Based on randomized experiments in rural Nigeria, this paper examines why promoting antenatal care among the poor is difficult.

Potential roles of conditional cash transfers (CCT) in improving health behaviors and outcomes have been witnessed in the developing world (Largarde et al., 2007). Barber and Gertler (2010) show that Mexico's CCT program, PROGRESA, improves birth-weight outcomes through better quality of antenatal care. Morris et al. (2004) find

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[1] Adverse effects on mother and fetus of malaria infection during pregnancy include maternal anemia, fetal loss, premature delivery, intrauterine growth retardation, and delivery of low birth-weight babies (Steketee et al., 2001). Presumptive intermittent treatment of malaria in pregnancy has been shown to be highly effective in reducing the risk of placental infection and delivery of low birth-weight babies (Schultz et al., 1994). Tetanus toxoid injections are given during pregnancy for the prevention of neonatal tetanus, which is a major cause of infant mortality in many developing countries (Blencowe et al., 2010).

[2] Systematic evaluations of the effectiveness of antenatal care are limited in developing countries (Carroli et al., 2001). Examining impacts of antenatal care is a topic of another paper.

that Honduras's CCT program augments the use of antenatal care and postnatal care. More broadly, extant studies show that small conditional inkind or cash transfers can significantly increase the use of preventive health care in various locales – e.g., child immunization through lentils in India (Banerjee et al., 2010), antenatal care through free bed nets in Kenya (Dupas, 2005), and HIV test through cash in Malawi (Thornton, 2008). Since CCT programs are expensive policy options, whether or not program effects sustain after the program ended is a critical question. Macours, et al. (2012) find sustainable CCT impacts on early childhood cognitive development in Nicaragua (which stands in contrast with the results from evaluations of preschool programs in the United States, e.g., Currie and Thomas, 2000; Garces et al., 2002; Heckman et al., 2010). By using facility-level data in Kenya, Dupas (2005) finds that inkind subsidies (free bed nets) conditional on the first antenatal care visit significantly increase follow-up visits, suggesting that women learn the benefits of antenatal care through experimentation. In her review on health behavior among the poor, Dupas (2011b) emphasizes that the poor need enough experimentation to become convinced of the benefits of a given preventive health care (Dupas, 2011b). Learning through experimentation is particularly relevant for antenatal care to be repeated by each woman across her pregnancies: Behavioral change in the current pregnancy caused by one-time intervention may sustain in subsequent pregnancies (see Kremer and Miguel, 2007 for a related discussion in social learning). To test learning from early visit, we make cash transfers conditional on early uptake only, but not suggested repeated uptakes (even though the latter conditionality is better to promote repeated uptakes, as found in PROGRESA, Barber and Gertler, 2010). If CCT promotes not only early uptake, but also subsequent uptakes after CCT ended, this provides evidence for its sustainable effects through learning from experimentation, as found in Dupas (2011b).

Compared to CCT programs, information interventions are often much cheaper and their effects, if any, can be more sustainable if they change people's behaviors (e.g., Cairncross et al., 2005; Utzinger et al., 2003); they may also complement learning from experimentation. Empirical findings of information interventions are mixed: Although some studies show their positive impacts on preventive health care and behaviors (e.g., Cohen et al., 2012; Dupas, 2011a; Jalan and Somanathan, 2008; Madajewicz et al., 2007;

Rhee et al., 2005), others show no such effects. Dupas (2011a) shows that Kenyan teenagers' sexual behaviors are not responsive to the HIV curriculum with its abstinence-until-marriage message, although they respond to information on the relative riskiness of potential partners; Duflo et al. (2011) also find no effects of the HIV curriculum on sexually transmitted infection and early pregnancy. Kremer and Miguel (2007) show that an intensive school health education intervention has no impact on worm prevention behaviors. As a second treatment, we design basic information interventions to promote repeated uptakes. Do they have expected sustainable effects?

We also combine CCT and information as a third treatment. Does the combination of these two mutually augment their effects (i.e., complementarity)? Experimental studies capturing multiple interventions – which are relatively rare in economics – often find the strongest impacts in such a combined treatment, though clear evidence for complementarity is limited: e.g., services (peer advising/study groups) and incentives (merit-scholarships) for college students in Canada (Angrist et al., 2007); services (peer tutoring) and information (parental communication) (plus universal cash incentives for grades) for primary school students in China (Li et al., 2010); and, information (nutrition information for mothers) and incentives (performance pay) among childcare workers in India (Singh, 2011). On the other hand, in Kenya, Duflo et al. (2011) reveal that although incentives (school uniform) alone significantly reduce early pregnancy, when they are bundled with information (teacher training for HIV curriculum) which has no individual effect, their impacts become weaker, because girl's sexual behaviors (casual vs. committed relationships) and schooling decisions are altered by additional information. As such, whether or not CCT bundled with information is more effective than individual intervention is an empirical question.

Our randomized experiments – the random allocation of 100 villages to one of the three treatments discussed above plus the control group – cover over 900 pregnant women in Nigeria. Two key outcomes are take-up within a month after the baseline, on which CCT is made conditional (henceforth *fast take-up*), and take-ups both within a month and more than a month after the baseline, i.e., at least two take-ups after the intervention, among women who had sufficient time after their fast take-up (*repeated take-ups*). The average treatment effects on these two capture temporary and sustainable effects, respectively. We find that regardless of information combined, CCT increases fast, but not repeated, take-up, and information has no impact. Thus, information neither has expected positive impacts nor complementarity with CCT, and CCT effects are not sustainable. We fail to promote learning from early visit. We then examine heterogeneous treatment effects across subpopulations. By doing so, we seek to shed light on key constraints underlying persistent low and slow uptake of antenatal care among the poor.

First, we pay attention to the timing of interventions received by pregnant women. In each pregnancy women need to start their first uptake early enough for full benefits of antenatal care. An important question for policy is whether intervention effects are strong at the early stage of pregnancy when many women have not yet made their first visit. If this is so, early intervention, including pre-pregnancy one, is recommended; otherwise, policy makers need to tradeoff low responses and high benefits. Our finding is encouraging: CCT effects are strong among women in the first trimester of pregnancy and become weak in the third trimester.

Second, we deepen our examination of roles of learning through experimentation by paying attention to individual history. Although the previous use of preventive health care is not relevant (e.g., one-time immunization) or has received limited attention as a potential factor differentiating program effects in the literature, whether or not women received antenatal care in their past pregnancy determines their learning experience in the past. In our control sample, compared to women with past take-up experience (henceforth *past takers*), both non-first-time pregnant women (*non-first timers*) with no such experience (*past nontakers*) and first-time pregnant women (*first timers*) are less likely to get antenatal care, especially at the early stage. This is an encouraging finding. On one hand, past takers tend to become repeaters without interventions. This indicates the potential role of learning from past experimentation, though this may only reflect individual heterogeneity. On the other hand, if learning works and past nontakers/first timers start to get antenatal care now, they can also become repeaters in their future pregnancies. Regardless of the significance of learning, promoting first take-up of those with no past experience is a primary policy goal. Potential learning is crucial among them: They could learn much more from their first experimentation in their life than others with past experience.

Third, we consider people's cognitive ability which can differentiate the effectiveness of information interventions (Cutler and Lleras-Muney, 2010, 2012), though evidence for cognition gradient is limited in developing countries (e.g., Cohen et al., 2012; Thomas et al., 1991). Distinct from developed countries' public health whose main concern is low health literacy (e.g., Berkman et al., 2011 review health literacy interventions in developed countries), general literacy is a first concern among the poor with very limited education. Indeed, in our control sample, uptake is much less common among illiterate women than literate.

We find that (1) regardless of information combined, CCT increases fast take-up only among past takers; (2) information alone does not strongly increase fast take-up even among the literate and rather decreases uptake among past nontakers/first timers, especially among the illiterate. That is, treatment effects are strongly differentiated by individual past experience: Without it, women do not respond to cash incentives and ill-processed information can have negative effects. Cash incentives have no sustainable effects, because those who could learn most from their first visit are irresponsive; cash incentives only encourage past takers to become repeaters, even though they tend to do so anyway without incentives. Linked with low cognitive ability, lack of experience can also adversely affect information interventions. We argue that these roles of past experience are consistent with both individual heterogeneity and learning from past experimentation.

The rest of the paper is organized as follows. The next section offers a description of the experimental design. Section 3 provides a description of antenatal care in the past and current pregnancies. Section 4 estimates average treatment effects on fast and repeated take-ups. Section 5 estimates heterogeneous treatment effects on fast take-up. The last section summarizes main findings and discusses policy implications.

2. Experimental design

According to the 2008 Demographic and Health Survey, Nigeria's neonatal, infant, and under-five mortality rates are 46, 87, and 171 per 1,000 live births; among six zones in the country, the North East Zone where our study site is located attain the highest rates in the country – 53, 109, and 222, respectively (National Population Commission, 2009). Although the proportion of pregnant women who received antenatal care in the country is 57.7%, that in the North East is only 43%.

We conducted a randomized experiment in Adamawa State in 2009. Out of 21 local government areas (LGAs) in the state, we intentionally selected 5 with distinct ethnic groups and political power – 2 from 6 poor LGAs and 3 from 15 non-poor LGAs (based on household income with the mean as a threshold). In each selected LGA, villages (excluding small and large villages with population below 130 and over 1,000, respectively) were stratified by the presence of health facility in the village, and in each stratum, villages were randomly sampled – 100 villages in total. In each village, all pregnant women were stratified by pregnancy trimester, and in each stratum, women were randomly sampled – 927 women in total.

We randomly allocated 100 villages to one of the following:

Information: the provision of basic information about antenatal care (24 villages)

CCT: cash transfer conditional on fast take-up (within a month) (23 villages)

Combined: both (25 villages)

Control: neither (28 villages).

Women in the information and combined treatments received information about the recommended number of antenatal care visits (at least four) and basic explanations about the purpose of and benefits from antenatal care and about risks associated with not taking antenatal care (e.g., delivery of low birth-weight baby and infant mortality/morbidity, Lavender et al., 2007) in their local language (mostly Hausa); information about specific services available in antenatal care was not included and no elaborated visual aid (e.g., pictures) was used. We also distributed a handout with written descriptions of the information and explanations same as the oral ones (in Hausa only) to facilitate recall of information for repeated take-ups. Any additional impacts through this handout should depend on women's literacy.[3] Women in the CCT and combined treatment received an oral explanation about the CCT, the uniform amount of which is 400 naira, or equivalently US$4, which is close to women's median daily wage in the study region.[4]

[3] Since written information was given to all women in information and combined treatments, we cannot tell whether the literate could better process oral explanation or the written information, or both.

[4] In her experiment in Adamawa State, Sato (2009) uses 1,000 naira in the same CCT design as ours, finding similar treatment effects. This suggests that the use of antenatal care is price inelastic.

Right before these interventions (June 2009), we conducted a baseline survey to collect information about women's reproductive health behaviors (antenatal care and immunization) and outcomes (delivery, mortality, morbidity) in their past pregnancies, if any, and their health behaviors in their current pregnancy, as well as basic individual, household, and village characteristics. Three follow-up surveys – 1, 4, and 11 months after the baseline – collected women's reproductive health behaviors (antenatal care, immunization, postnatal care, breastfeeding) and their and newborns' health outcomes (delivery, mortality, morbidity, weight, height);[5] cash transfers were made during the first follow-up survey for women, but not their husbands, with proof of uptake in the past one month.

3. Antenatal care in rural Nigeria

3.1. Take-up rates

Our analysis is based on 842 pregnant women in 99 villages for which complete baseline and first follow-up data are available.[6] In this panel sample, 21% of women are first timers (no past pregnancy) and 79% are non-first-timers (with 3 past pregnancies on average); among non-first-timers, 81% are past takers (at least one antenatal care in their past pregnancy) and 21% are past nontakers (see Table 1). The proportion of pregnant women who take at least one antenatal care in the sample is much higher than the average in the North East zone (43%). Overall take-up rates per each pregnancy event gradually increased over time, from 80% in the past pregnancies among non-first timers to 83% and 86% in the control and whole samples, respectively, in the current pregnancy (see Table 2). At the time of baseline, according to respondents' subjective assessment, 20%, 51%, and 29% of women, respectively, are in the first, second, and third trimesters (1st-3rd

[5] We conducted three more follow-up surveys up to 30 months after the baseline to further capture maternal health behaviors and infant/child health outcomes.

[6] We exclude women at the 9th month of pregnancy at the baseline who have completed pregnancy at the time of first follow-up interviews, because it is unlikely that they had enough time to receive antenatal care between the baseline interview and their delivery. As a result, one village in the information group is dropped. We include women at the less-than-9th month of pregnancy at the baseline who have completed pregnancy at the time of first follow-up interviews, assuming that they had enough time to receive antenatal care; excluding those does not alter the main results reported below.

month, 4th-6th month, and 7th-8th month) (Table 1),[7] and 55% of women have already received at least one antenatal care in the current pregnancy at the baseline (henceforth *baseline takers*) and the reaming 45% have not (*baseline nontakers*); the rate of fast take-up (at least one within a month after the baseline) increased to 67% (Table 2).

We examine take-up more than a month after the baseline only among 340 women who have not yet completed pregnancy (mostly delivery; miscarriage is uncommon) at the time of the second follow-up interviews (4 months after the baseline) (henceforth *follow-up sample*).[8] Women in the follow-up sample had enough time to receive a subsequent antenatal care following the fast take-up with a sufficient interval (at least 3 months). In the follow-up sample, 37% and 63% of women are in the first and second trimesters, respectively, at the baseline (all women in the third trimester have already completed their pregnancy) (Table 1). The rates of baseline and fast take-ups (40% and 59%) in the follow-up sample are smaller than those in the whole sample simply because the former are at the earlier pregnancy stage than the latter on average; 68% of women received at least one antenatal care more than a month after the baseline until they complete their pregnancy.

3.2. Services

In the 99 villages in the sample, the nearest health facilities are mostly small public ones – public health clinic, primary health care, or health post – with community health extension workers, midwives, or nurses as service providers; 70% of the facilities provide free antenatal care services. On average, facilities are located in a 21-minutes distance (range: 4-90) mostly on foot or by motorcycle; the median transportation cost to the nearest health facility among villages involving any transportation cost is 90 naira (range: 20-1600); and the median fee for antenatal care among villages without free services is 250 naira. Thus, the amount of CCT (400 naira) well covers these costs in many locales. Although tetanus toxoid vaccination, iron/folic acid supplementation, and preventative anti-malaria medication are available in most facilities, HIV test is available

[7] Although comparing self-reported pregnancy trimesters and the timing of delivery suggests the existence of measurement errors in the former, such errors should not cause significant bias in our analyses on experimental data.
[8] A small number of women with lack of complete data on take-up more than a month after the baseline are also dropped.

only in about one third of facilities (Table 2). According to the subjective assessment of respondents (village leaders), the quality of antenatal care is considered to be at least good (according to a standard five-scale measure) in most facilities (85%).

Among women who got antenatal care in their past pregnancy (past takers), over 70% actually received tetanus toxoid vaccination, iron/folic acid supplementation, and preventative anti-malaria medication though only about 20% received HIV test. Antenatal care services improved over time: Over 90% of women who got antenatal care in the current pregnancy received tetanus toxoid vaccination and iron/folic acid supplementation (information about other antenatal care services is lacking).[9] Over 80% of women who received antenatal care in their past and current pregnancies considered the quality of antenatal care at least good, and these patterns changed little over time.

Among women who did not receive antenatal care in their past pregnancy (past nontakers), four most common reasons are high cost (41%), non-necessity (they felt it was unneeded) (26%), lack of service (15%),[10] and long distance (13%); at the baseline in the current pregnancy, although high cost and non-necessity are also two most common reasons, lack of service and long distance are uncommon reasons, suggesting recent improvements in health supply.[11] Overall, these results suggest that the supply of antenatal care services is not terribly bad in the study area especially in the current pregnancy.

3.3. Sequence of antenatal care take-ups

The sequence of uptakes of antenatal care across periods – baseline, within a month, and more than a month after the baseline – is depicted in Figure 1. Among four possible sequences in the first two periods, 1-1 is the most common (45%) and 1-0 is the least common (10%) in the whole sample (panel A) and three sequences but 1-0 are as

[9] Interestingly, these services were more commonly received within a month after the baseline than before and after that one-month period.
[10] Lack of service not only captures the permanent nonexistence of antenatal care services in the past, but also may mean temporal non-availability because of out-of-stock medical supply, absenteeism, or closure.
[11] High cost became a more common reason (64%) more than a month after the baseline. This suggests that liquidity constraints matter more at the later pregnancy stage with a potentially higher demand for medical expenses. After CCT ended, people may also have augmented their negative feeling about cost.

common as each other in the follow-up sample (panel B), where 0 and 1, respectively, mean no visit and at least one visit at each period. The three most common sequences across three periods are 1-1-1 (22%), 0-0-1 (21%), and 0-1-1 (19%) and the three least common sequences are 1-0-0 (2%), 1-0-1 (5%), and 0-1-0 (7%). In the follow-up sample, we define repeated take-ups by aggregating post-intervention sequences: The dummy for repeated take-ups takes 1 for (0/1)-1-1 and 0 otherwise (panels B and C). Thus, this variable measures the multiple take-ups across two post-intervention periods – within a month and more than a month after the baseline; it does not capture potential multiple take-ups within each of these two periods and it ignores baseline take-up which can be also multiple. The rate of repeated take-ups is 41%, which is much lower than the take-up rates at each period and less than a half of the overall take-up rate.

4. Average treatment effects

4.1. Randomness check

Means of various individual, household, and village factors are compared by four treatment statuses in Table 1. In the whole sample, the mean differences across four groups are not significantly different from zero at a 10% significance level for almost all variables but third trimester at the baseline and age of pregnant woman. Specifically, we regress each variable on three treatment dummies and test a null hypothesis that the estimated three coefficients are all zero, with standard errors clustered by village. None of the estimated coefficients for the three treatment dummies are significantly different from zero for the third trimester and only one for CCT is significant for age. These results offer strong evidence that the randomization in our experiment was well performed.

The mean differences of the dummy for the follow-up sample across treatment groups is not significantly different from zero, suggesting that attrition is not systematic related with treatments. This is further confirmed by the mean comparison of women in the follow-up sample with others: Although baseline take-up and pregnancy trimesters are significantly different between them as expected, the mean differences are not significantly different from zero at a 10% significance level for almost all variables but age and polygamy; the mean differences across treatment groups in the follow-up sample are not significantly different from zero at a 10% significance level for almost all variables but baseline take-up and age of pregnant woman, either. Thus, as far as fixed-effects baseline characteristics (e.g., past experience and literacy) are concerned, randomization sustained well in the follow-up sample. This is simply because attrition was determined by the timing of pregnancy completion uncorrelated with randomized treatments. On the other hand, systematic difference in baseline take-up happened to emerge in the follow-up sample: It is significantly less common among all three treatment groups than the control group (the difference is over .15 for each treatment). These accidental correlations with baseline take-up (time-variant factor) need to receive careful attention in estimating the treatment effects in the follow-up sample.

4.2. Econometric specification

Let us ignore multiple treatments for brevity. The initial estimand of interest is the Average Treatment Effect $\tau = E[Y_{ivt}(1) - Y_{ivt}(0)]$ in the population of experimental pregnant women, where $Y_{ivt}(1)$ and $Y_{ivt}(0)$ denote the “potential outcomes” for individual i’s outcome at time t if village v were treated and were not treated, respectively (Rubin, 1974). The first outcome is fast take-up (with a month after the baseline). The primary specification is a standard fixed-effects difference-in-differences (DID) estimator for 2 periods (t = 0 for baseline, 1 for post-intervention):

$$Y_{ivt} = \tau T_{vt} d_t + \gamma d_t + \theta_i + \varepsilon_{ivt}\,, \qquad (1)$$

where Y_{ivt} is a dummy for antenatal care take-up; T_v is a dummy for treatment; d_t is a dummy for post-intervention; θ_i is individual fixed effects; and ε_{ivt} is an error term. Taking the difference between 2 periods yields:

$$Y_{iv1} - Y_{iv0} = \gamma + \tau T_{v1} + \left(\varepsilon_{iv1} - \varepsilon_{iv0}\right), \qquad (2)$$

This is estimated in Ordinary Least Squares (OLS) using robust standard errors clustered by village. As discussed by Bertrand et al. (2004), these standard errors are consistent in the presence of any correlation pattern in the errors within individual over time.

So far, we have ignored the sequential nature of women’s uptake decisions: Fast take-up is a function of baseline take-up. In the control sample baseline takers are more likely to get fast take-up than baseline nontakers by 50% – both in the whole sample and follow-up sample (see Table 3); the qualitatively the same patterns hold for all four treatment groups combined (see Table 4). These results suggest that baseline take-up positively affects fast take-up. For example, baseline takers may know more about the required repeated take-ups for full benefits of antenatal care than baseline nontakers. In equation (1), baseline take-up (Y_{iv0}) is omitted at $t = 1$. The identifying assumption is that treatment is uncorrelated with this lagged outcome. This is supported by the descriptive statistics above: In the whole sample, baseline take-up is not correlated with three treatment dummies (p-value for the joint significance test is .80, Table 1). That is, omitted lagged outcome does not cause bias in estimating average treatment effects, because the randomized treatment is uncorrelated with the lagged outcome. This stands in contrast to non-experimental data (Imbens and Wooldridge, 2009).

Our second outcome is repeated take-ups (both within a month and more than a month after the baseline) in the follow-up sample.[12] As individuals with repeated take-ups are a subset of those with fast take-up, if the treatment effect on repeated take-ups is significant, that on fast take-up should be also significant, though the converse does not necessarily hold. In the control sample, although there is no significant difference in uptake more than a month after the baseline between baseline takers and nontakers, baseline takers are more likely to take repeated take-ups than baseline nontakers by 40% (Table 3); the qualitatively the same patterns hold for all four treatment groups combined (Table 4). These results suggest baseline take-up positively affects repeated take-ups. As found above, in the follow-up sample, all three treatment dummies are negatively correlated with baseline take-up, thus the identifying assumption for equation (1) does not hold anymore. The omitted lagged outcome makes the OLS estimate of equation (2) biased upward; in contrast, the OLS estimate of equation (1) for $t = 1$ is biased downward. Although the latter simple-difference estimates do not control for individual heterogeneity, the mean test results above suggest that unobserved heterogeneity is unlikely to be a major source of bias. Then, the simple-difference and fixed-effects DID estimates, respectively, provide the lower and upper bounds of average treatment effects. When treatment and baseline take-up are not significantly correlated with each other, these two estimates should be close to each other.

[12] We ignore the sequential decisions from one within a month after the baseline to another afterwards. Addressing them requires us to control for endogenous lagged outcomes in each of these two post-intervention periods, which is infeasible with our data for the reason given below.

An alternative specification is to control for baseline take-up as an exogenous covariate in equation (2) at $t = 1$ with an assumption that Y_{iv0} is uncorrelated with ($\varepsilon_{iv1} - \varepsilon_{iv0}$) (Imbens and Wooldridge, 2009):

$$Y_{iv1} - Y_{iv0} = \gamma + \tau T_{v1} + Y_{iv0} + \left(\varepsilon_{iv1} - \varepsilon_{iv0}\right), \tag{3}$$

Since whether or not this assumption holds is an empirical question, it is better to control for the potential endogeneity of Y_{iv0} in equation (3). We use pregnancy stage (trimesters) at the baseline interacted with the post-intervention dummy as an excluded instrumental variable (IV). The identifying assumption is that the pregnancy stage at $t = 0$ determines baseline take-up but does not directly affect repeated take-ups once the baseline take-up is controlled for. In the control group in the follow-up sample, although women at the second trimester at the baseline are more likely to take baseline, fast, and repeated take-ups than those at the first trimester, there is no significant difference in uptake more than a month after the baseline between them (Table 3). This provides evidence that pregnancy stage at the baseline influences repeated take-ups only through fast take-up, because among women in the follow-up sample who had sufficient time for subsequent uptakes until they complete their pregnancy, pregnancy stage does not matter anymore. Equation (3) is estimated in Two-stage Least Squares (2SLS). In contrast, pregnancy stage still matters a lot for fast take-up only within a month right after the baseline. Thus, the exclusion restriction of pregnancy stage does not hold for fast take-up in equation (3).

4.3. Estimation results

Estimated average treatment effects are reported in Table 5. According to the fixed-effects DID estimates on fast take-up, CCT and combined treatment have significant positive effects at almost the same magnitude (about .13); information has no impact (column 2 of panel A). The simple difference estimates are also similar though they are statistically weak (column 1). Adding baseline take-up as an exogenous covariate does not significantly alter the results (column 3). When pregnancy trimester dummies and LGA dummies interacted with the post-intervention dummy are used as controls, almost the same results are obtained (panel B). These results suggest that omitted lagged outcome in equation (1) does not cause bias, as expected.

The simple-difference and fixed-effects DID estimates of treatment effects on repeated take-ups in the follow-up sample are quite different from each other, although almost none of them are statistically significant (without and with controls) (columns 4 and 5). As expected, DID estimates (upper bound) are always greater than simple-difference estimates (lower bound). Adding baseline take-up as an exogenous covariate decreases all the estimated coefficients of three treatment dummies though they are still greater than the corresponding simple-difference estimates (column 6). Similar results are obtained for IV fixed-effects DID estimates using the second trimester dummy as an excluded IV for baseline take-up (column 7); the estimated coefficients of baseline take-up are also similar to each other, and the exogeneity of baseline take-up is not rejected (p-value for Wu-Hausman F test is almost .50 across specifications).[13] The largest estimated treatment effect in IV fixed-effects DID is .14 for CCT alone at a 20% significance level with controls; the estimated effects of information and combined treatments are close to zero. These results suggest that none of the three treatments significantly affect repeated take-ups. Hence, although cash incentives temporarily promote uptake, their effects are not sustainable; information has neither individual nor interaction effects.

5. Heterogeneous treatment effects

The next estimand of interest is the Conditional Average Treatment Effect $\tau_c = E[Y_{ivt}(1) - Y_{ivt}(0)|X_i]$ for the subpopulation with $X_i \in \boldsymbol{A}$, where $\boldsymbol{A}$ be a subset of the covariate space $\boldsymbol{X}$. We focus on fast take-up, because the number of observations in the follow-up sample is too small for such disaggregate analyses on repeated take-ups. As discussed above, distinct from repeated take-ups, we cannot control for the endogeneity of baseline take-up as a determinant of fast take-up. Thus, we focus on the comparison of simple-difference and fixed-effects DID estimates. How much we can narrow the bounds of the conditional average treatment effects depends on the correlation of each treatment and baseline take-up (lagged outcome). Since treatments were not randomized within the subsamples determined by factors examined here – the timing of interventions (in terms of pregnancy trimesters at the baseline), past pregnancy/take-up experience, and literacy –, unobserved heterogeneity is a potential source of additional bias in the simple-difference estimation which does not control for individual heterogeneity. This is especially so for past experience and literacy, because distinct from the timing of

[13] In the first stage, the second trimester dummy strongly positively affects baseline take-up (column 8).

interventions which is random among individuals, these two are correlated with individual heterogeneity.

5.1. Timing of interventions

As found in the follow-up sample above, baseline and fast take-ups increase as pregnancy stage progresses in the whole sample: In the control group, women at the second and third trimesters at the baseline are more likely to take baseline and fast take-ups by over 30% than those at the first trimester (Table 3); the qualitatively the same patterns hold for four treatment groups combined (Table 4). These results provide strong evidence for slow uptake in our sample; in particular, only about 30% of women in the first trimester have started antenatal care at the baseline, though 65% of women in the third trimester have already done so.[14] How does the pregnancy stage at which the treated receive interventions differentiate treatment effects? We conjecture that treatment effects become small at the late stage of pregnancy simply because the remaining time of pregnancy decreases. Although the earlier the intervention, the more benefits can those who responded to it obtain, the net outcome depends on whether women's responses are strong at the early pregnancy stage when many of them have not yet made their first visit.

Estimation results are reported in Table 6, where panel A reports the OLS estimates of three treatment dummies as determinants of baseline take-up to check their correlations and column (1) reports the estimation results for the whole sample. These results are based on specifications with controls (those of columns 1 and 2 of panel B of Table 5 are replicated in columns 1 of panels B and C here); all results without controls are very similar.

First, among women in the first trimester (column 2), none of the three treatments are correlated with baseline take-up (p-value for the joint significance test is .94) and the simple-difference and DID estimates are very similar to each other. According to the DID estimates, the estimated treatment effects are .27 for CCT and .19 for combined treatment and information effect is nonsignificant. Hence, regardless of information combined, cash incentives have significant positive effects in the first trimester.

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[14] Take-up patterns of women in the second and third trimesters are different across treatment groups: Although these two trimesters do not differentiate baseline and fast take-ups in the control group, they are relatively more common in the third trimester than the second in the whole sample.

Second, among women in the third trimester (column 4), although none of the three treatments is significantly correlated with baseline take-up, the positive correlation of combined treatment is considerable (almost .15). As a result, although the simple-difference and DID estimates are similar to each other for information and CCT and all their estimated coefficients are small with no statistical significance at a conventional level, the DID estimate is smaller than the simple-difference estimate for combined treatment (note that with the positive correlation of combined treatment and baseline take-up, the former is biased downward and the latter is biased upward). The upper-bound estimate is .14 with no statistical significance at a conventional level. Thus, consistent with our conjecture, treatment effects are nonsignificant in the third trimester.

Third, among women in the second trimester (column 3), all three treatments are negatively correlated with baseline take-up (p-value for the joint significance test is .14).[15] As a result, DID estimates (upper bound) are larger than the corresponding simple-difference estimates (lower bound), and although either upper or lower bound estimates are statistically significant, we cannot tell whether or not their true effects are significant. The treatment effect of CCT alone should be smaller than that in the first trimester, .27, because the upper-bound estimate is only .14, which is a little bit greater than that in the third trimester. Thus, the effects of CCT alone diminish as the pregnancy stage progresses. We cannot be sure about the patterns of combined treatment.

5.2. Past experience – descriptive evidence

In the control sample, the overall take-up rates of past takers and past nontakers in the current pregnancy are 87% and 69%, respectively (results are similar in the whole sample). This indicates that the significant proportion of past nontakers have started to receive antenatal care in the current pregnancy without interventions, probably because of improved supply factors, the evidence for which is found above (results are very similar in the whole sample with four treatment groups combined). Compared to past takers, past nontakers and first timers are less likely to take baseline, but not fast, take-up in the control sample (in the follow-up sample, the number of past nontakers and first timers is

[15] These correlation patterns are similar to those in the follow-up sample found above, suggesting that the latter correlations are mainly caused by the former ones among those in the second trimester included in the follow-up sample.

very small) (Table 3). These results provide evidence that past takers are likely to become repeaters in the current pregnancy and start their uptake earlier than others without interventions; although many past nontakers have changed their behaviors, they did so late. Since promoting first experimentation among women with no past experience is a primary policy goal, whether treatment effects are strong among them is a critical question. The descriptive evidence suggests the opposite: In the whole sample, past nontakers/first timers are less likely to take not only baseline take-up, but also fast take-up than past takers (Table 4).[16]

Although past uptake decisions among non-first timers are shaped by individual heterogeneity, heterogeneity can also evolve through experimentation. Evidence is found in the knowledge and perceptions about antenatal care at the baseline: Past takers know more about antenatal care than past nontakers/first timers, and past takers feel more positive about antenatal care than past nontakers, though the difference in perceptions between past takers and first timers is limited (see Table 7).[17] These distinct knowledge/perceptions among past takers – measured at the baseline – not only must have shaped their past take-up decisions (i.e., heterogeneity), but also could be developed through their past experimentation (i.e., learning).

As supply factors are more common reasons for not taking antenatal care in the past (Table 2), they could be major constraints on non-first timers' past decisions. Supportive evidence is found in reasons for not taking antenatal care at the baseline and current health facilities: Bad access is more common among past nontakers than past takers, and past nontakers have access to facilities with longer distance and lower subjective quality than past takers (see Table 8). Thus, supply constraints are still strong among past nontakers. In contrast, no significant difference exists in reasons for non-uptake and supply factors between past takers and first timers, suggesting that supply

[16] None of the take-ups across periods are significantly different between first timers and non-first timers (i.e., past takers and nontakers combined) in the control sample and the whole sample (results not shown).

[17] Among seven questions about knowledge, the second through fifth questions correspond to basic information given in the information intervention. Positive response diminishes as the questions become more specific/detailed from the first question through the sixth and seventh about specific antenatal care services.

factors are not a major constraint anymore among fist timers, who are younger than non-first timers.

5.3. Literacy – descriptive evidence

In our sample, 38% of women are literate – can read and write in Hausa or English.[18] In the control sample, overall take-up rates in the current pregnancy are 75% and 92%, respectively, and compared to illiterate women, literate women are more likely to take baseline, fast, and repeated take-ups, but not take-up more than a month after the baseline (results are similar in the whole sample); results of the comparison by women's education – no education vs. any education – is similar. This provides strong evidence for the cognition/education gradient, which is strong especially at the early stage of pregnancy.[19] Clearly, illiterate women should be given a higher priority in antenatal care promotion. This targeting is not inconsistent with experience-based targeting because literacy and past take-up experience are positively correlated with each other. Distinct from past experience, women's literacy does not strongly differentiate knowledge or perceptions about antenatal care (Table 7). This provides evidence that if the effects of information are greater among the literate than the illiterate, cognitive ability itself, but not individual heterogeneity correlated with literacy, is likely to be a main differentiating factor.

In contrast, whether and how the effects of cash incentives are differentiated by literacy is unknown. Literacy developed through education can be correlated with other factors determining uptake decisions. The comparison of reasons for non-uptake at the baseline and supply factors by literacy is also quite distinct from past experience: Compared to the illiterate, high cost is less common, facilities with free antenatal care are more common, and non-necessity is more common among the literate (Table 8) (the mean comparison by women's education yields qualitatively the same results). These findings provide evidence that literacy is positively correlated with household liquidity and literate (educated) women tend to make stronger self-assessment about antenatal care. These results suggest that compared to the literate, the illiterate with stronger liquidity

[18] English literacy is uncommon; most English-literate are literate in Hausa.
[19] In the control group, the gap of baseline take-up between the literate and illiterate in the follow-up sample (those at the earlier stage) is more than twice that in the whole sample (54% vs. 26%).

constraints and weaker self-assessment ability could be more responsive to cash incentives.

5.4. Past experience and literacy – treatment effects

Estimation results with controls are reported in Table 9, the format of which is the same as Table 6. Since treatment effects in the third trimester are nonsignificant, the analyses focus on women in the first/second trimesters at the baseline.[20] The results for this base sample reported in column (1) are the combination of those in the first and second trimesters found above.

First, consistent with the descriptive finding, cash incentives increase past takers' uptake (column 4). Results are stronger for CCT alone: The lower- and upper-bound estimates for CCT are close to each other and both of them are statistically significant; the lower-bound estimate for combined treatment is relatively small and nonsignificant at a conventional level. In contrast, among past nontakers/first timers and first timers alone, the upper-bound estimates for CCT and combined treatments are all small and not significantly different from zero (columns 2 and 3; similar analyses for past nontakers are infeasible with a small number of observations). Hence, cash incentives work to encourage those with past experience to become repeaters; they do not provide enough incentives for others – our primary target – to initiate their first experimentation, thereby resulting in unsustainable effects on repeated take-ups found above in the whole sample.

Second, among past nontakers/first timers and first timers, the information intervention decreases uptake by over 20% (columns 2 and 3). Among past nontakers/first timers, with no correlation of information and baseline take-up, the simple-difference and fixed-effects DID estimates are very close to each other; among first timers, these two results are similar to each other and the upper-bound estimate is -.23. In contrast, information has no impacts among past takers; both the lower and upper bound estimates are not significantly different from zero (column 4). We interpret these strong negative effects of information below.

Third, although there are no correlations between treatments and baseline take-up among the illiterate, strong negative correlations exist for all three treatments among the

[20] The mean comparisons of observable attributes across four treatment groups are similar to those in the whole sample.

literate (p-value for the joint significance test is .92 and .02, respectively, columns 5 and 6). Accordingly, clear estimates are obtained among the illiterate: Regardless of information combined, cash incentives increase uptake by about 20% and information has no impacts. Consistent with our conjecture, the literate's response to CCT is weaker than the illiterate's, because the upper-bound estimate for the former is no greater than the lower-bound estimate for the latter (.15 vs. .17); though such comparison for combined treatment is generally ambiguous. A very strong negative correlation of information and baseline take-up (-.25) among the literate means that its upper-bound estimate .14 is strongly biased upward; then, its true effect is unlikely to be considerable in magnitude.

5.5. Negative treatment effects of information

Although lack of past experimentation does not lead to negative effects of information, its combination with cognition can. This is because information about antenatal care can be more ill-processed by women who lack both literacy and past experience than those with either or both of them; that is, past experimentation and literacy can subtitle for each other in processing the information intervention. Supportive evidence is found in the comparison of illiterate and literate women among those with no past experience (columns 7 and 8 of Table 9). Information is strongly positively and negatively correlated with baseline take-up among the illiterate and literate, respectively. The upper-bound estimate for the information effect among the illiterate (-.22) is close to the lower-bound estimate in the latter (-.25) and neither of them is statistically significant at a conventional level (interpreting these simple-difference requires caution as discussed above); the lower-bound DID estimate among the illiterate (-.39) is statistically significant at a 5% significance level. The similar comparison of illiterate and literate women among past takers echoes earlier findings of distinct cash incentives between them (columns 9 and 10), which makes sense because past takers strongly respond to cash incentives.

The estimated effects of CCT alone and combined treatment are not significantly different from each other across all specifications examined so far. Thus, combined information does not alter CCT effects on fast take-up. This is explained by two distinct reasons. First, among past takers, information has no impact. Second, for past nontakers/first timers, combined CCT, which has no impact by itself, mutes the negative effects of information, probably because they considered information less seriously than CCT with specific conditionality.

6. Conclusion

This paper examined why promoting antenatal care among the poor is difficult based on randomized experiments in rural Nigeria. We found that treatment effects are strongly differentiated by individual history. First, cash incentives conditional on fast uptake are effective only among women who received care before. This is especially so among poor illiterate women, and the effects are stronger at the early stage of pregnancy than the late stage. In contrast, those without past experience – both first-time and non-first-time pregnant women – who are less likely to receive care but could learn most from their first visit are irresponsive. As a result, cash incentives fail to promote their follow-up visits through potential learning from their first experimentation. Second, basic information intervention has no significant positive effect even among literate women and the combination of lack of experience and low cognitive ability (illiteracy) can adversely affect information intervention (negative effects). These roles of past experience are consistent with both individual heterogeneity and learning from past experimentation, though supply factors also constrain women who never got care in their past pregnancy. Lastly, information combined with cash incentives does not alter the incentive effects.

These results suggest the following policy implications for promoting antenatal care. First, interventions should target pregnant women at the early stage of pregnancy and/or young women prior to their first pregnancy. Second, with effective design of conditionality cash incentives can promote uptake, though the effects are limited to potential repeaters with past uptake experience. Third, promoting others to start their first experimentation requires stronger or more innovative interventions than those in our experiment; information interventions need to address the cognition constraint which can bind in combination with lack of experience. Fourth, supply interventions are needed especially for non-first-time pregnant women whose decisions have been persistently constrained by supply factors.

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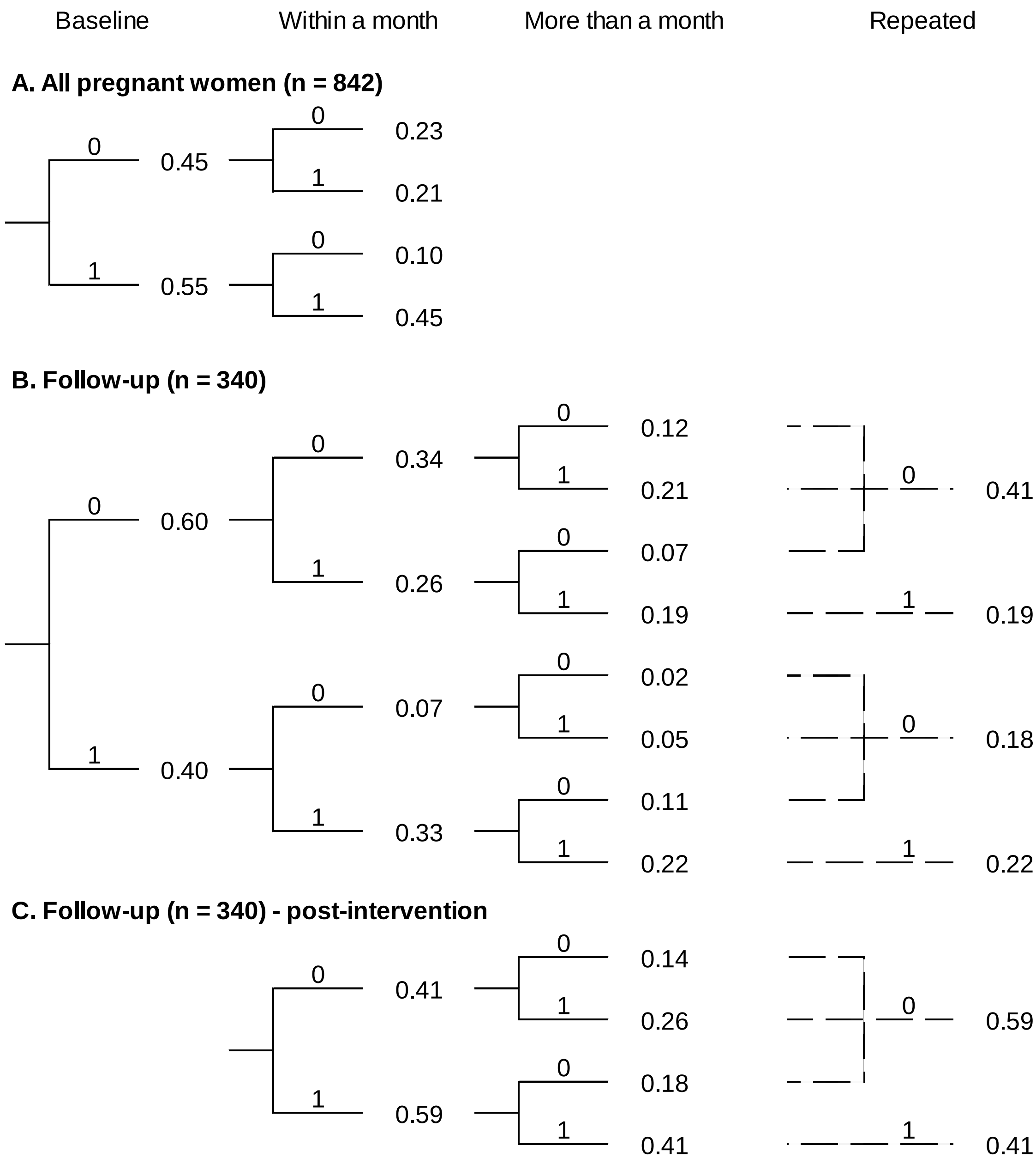
Figure 1. Sequence of antenatal care take-ups.



Notes: These are proportions for each squence. 0 = no visit, 1 = at least one visit.

Table 1. Means of baseline characterstics by treatment status and attrition.

	All									Follow-up				
	All	Control	Infor-mation	CCT	Com-bined	Diff.[a] (p-value)	Follow-up	Non follow-up	Diff. (p-value)	Control	Infor-mation	CCT	Com-bined	Diff.[a] (p-value)
No. observations	842	236	185	205	216		340	502		101	76	89	74	
Baseline take-up (0/1)	0.55	0.58	0.52	0.54	0.56	0.795	0.40	0.66	0.000	0.52	0.33	0.37	0.34	0.077
First trimester at baseline (0/1)	0.20	0.20	0.22	0.20	0.17	0.772	0.37	0.08	0.000	0.36	0.38	0.37	0.36	0.990
Second trimester at baseline (0/1)	0.51	0.53	0.54	0.51	0.46	0.391	0.63	0.42	0.000	0.64	0.62	0.63	0.64	0.990
Third trimester at baseline (0/1)	0.29	0.27	0.24	0.30	0.37	0.085	0.00	0.49	0.000					
Follow-up (0/1)	0.40	0.43	0.41	0.43	0.34	0.363								
Non first-time pregnancy (0/1)	0.79	0.81	0.77	0.77	0.79	0.717	0.77	0.80	0.227	0.80	0.78	0.79	0.69	0.326
Age of pregnant woman	26.1	27.0	26.4	25.0	26.0	0.013	25.7	26.5	0.078	26.9	26.3	24.5	24.8	0.037
Literate pregnant woman (0/1)	0.38	0.41	0.40	0.40	0.32	0.439	0.41	0.36	0.209	0.45	0.43	0.38	0.35	0.660
Any education of pregnant woman (0/1)	0.51	0.48	0.52	0.55	0.49	0.821	0.52	0.50	0.429	0.44	0.54	0.53	0.61	0.311
Literate husband (0/1)	0.71	0.74	0.74	0.67	0.71	0.611	0.71	0.72	0.670	0.73	0.75	0.63	0.72	0.528
Any education of husband (0/1)	0.70	0.70	0.74	0.72	0.66	0.517	0.71	0.70	0.700	0.69	0.76	0.74	0.66	0.483
Polygamy (0/1)	0.22	0.20	0.23	0.21	0.23	0.921	0.17	0.25	0.007	0.17	0.18	0.17	0.16	0.995
Moslem pregnant woman (0/1)	0.36	0.23	0.39	0.43	0.41	0.241	0.38	0.35	0.394	0.24	0.42	0.45	0.43	0.322
Floor construction (0/1)	0.30	0.24	0.27	0.41	0.29	0.146	0.30	0.30	0.992	0.21	0.29	0.36	0.36	0.198
Distance to health facility (minutes)	20.3	21.7	22.2	20.2	17.1	0.580	20.5	20.1	0.692	22.2	22.0	18.4	19.3	0.846
Free antenatal care (0/1)	0.70	0.80	0.64	0.68	0.69	0.603	0.70	0.71	0.803	0.78	0.64	0.74	0.59	0.581
Number of past pregnancy[b]	2.95	3.10	2.79	2.81	3.05	0.455	2.93	2.97	0.361	3.23	2.90	2.63	2.88	0.314
Take-up in the last pregnancy (0/1)[b]	0.76	0.73	0.81	0.76	0.76	0.780	0.73	0.78	0.105	0.72	0.79	0.72	0.68	0.744
Take-up in any past pregnancy (0/1)[b]	0.81	0.76	0.86	0.81	0.81	0.583	0.80	0.81	0.630	0.76	0.86	0.81	0.76	0.450

[a] A null hypothesis is that the estimated coefficients for three treatment dummies are all equal to zero (standard errors are clustered by village).

[b] Means among non-first time pregnant women.

Table 2. Antenatal care services received and reasons for not getting antenatal care.

	Past pregnancies[a]	Current pregnancy: Overall	Current pregnancy: Baseline	Current pregnancy: Within a month	Current pregnancy: More than a month	Village[b]
Antenatal care take-up (0/1)	0.80	0.86	0.55	0.67	0.68	
No. observations	2734	842	842	842	340	
Main antenatal care services received among those who got antenatal care (0/1):						
Tetanus toxoid vaccination	0.73	0.92	0.77	0.90	0.71	0.95
Iron/folic acid supplementation	0.78	0.95	0.81	0.96	0.83	0.88
Preventative anti-malaria medication	0.77					0.91
HIV test	0.21					0.34
Subjective quality of antenatal care received among those who got antenatal care (proportion):						
Very good	0.26		0.27	0.21	0.07	0.19
Good	0.55		0.55	0.58	0.74	0.66
Fair	0.16		0.15	0.18	0.19	0.15
Poor	0.03		0.03	0.03	0.00	0.00
Reasons for not getting antenatal care among those who did not get antenatal care (proportion):						
High cost	0.41		0.44	0.48	0.64	
Lack of service	0.15		0.06	0.06	0.01	
Long distance	0.13		0.08	0.06	0.00	
Non-necessity	0.26		0.30	0.16	0.19	
Other	0.04		0.13	0.23	0.16	

[a] 776 women, [b] These are about antenatal care services available at the baseline among 99 villages.

Table 3. Antenatal care take-up rates across periods in the control group.

	All			Follow-up				
	No. obs.	Baseline	Within a month	No. obs.	Baseline	Within a month	More than a month	Repeated
All	236	0.58	0.64	101	0.52	0.57	0.73	0.44
Baseline takers (a)	138	1.00	0.84	53	1.00	0.81	0.77	0.62
Baseline nontakers (b)	98	0.00	0.35	48	0.00	0.31	0.69	0.23
(a) - (b)			0.494 ***			0.499 ***	0.086	0.394 ***
			(0.055)			(0.086)	(0.089)	(0.092)
First trimester (c)	47	0.32	0.38	36	0.31	0.39	0.72	0.31
Second trimester (d)	125	0.66	0.70	65	0.65	0.68	0.74	0.51
Third trimester (e)	63	0.65	0.68					
(c) - (d)		0.337 ***	0.321 ***		0.341 ***	0.288 ***	0.016	0.202 *
		(0.082)	(0.080)		(0.099)	(0.100)	(0.093)	(0.102)
(c) - (e)		0.332 ***	0.300 ***					
		(0.092)	(0.090)					
Past takers (f)	143	0.69	0.65	60	0.57	0.52	0.78	0.43
Past nontakers (g)	45	0.22	0.56	19	0.21	0.58	0.68	0.47
First timers (h)	44	0.57	0.66	20	0.65	0.70	0.60	0.35
(f) - (g)		0.470 ***	0.095		0.356 ***	-0.062	0.099	-0.040
		(0.078)	(0.083)		(0.127)	(0.133)	(0.113)	(0.132)
(f) - (h)		0.124	-0.009		-0.083	-0.183	0.183	0.083
		(0.081)	(0.083)		(0.128)	(0.128)	(0.113)	(0.128)
Literate (i)	95	0.74	0.73	45	0.82	0.73	0.78	0.56
Illiterate (j)	138	0.48	0.57	56	0.29	0.45	0.70	0.34
(i) - (j)		0.259 ***	0.161 **		0.537 ***	0.287 ***	0.081	0.216 **
		(0.064)	(0.064)		(0.085)	(0.096)	(0.089)	(0.098)

*10% significance, **5% significance, ***1% significance.

Table 4. Antenatal care take-up rates across periods - all treatment groups combined.

	All			Follow-up				
	No. obs.	Baseline	Within a month	No. obs.	Baseline	Within a month	More than a month	Repeated
All	842	0.55	0.67	340	0.40	0.59	0.68	0.41
Baseline takers (a)	466	1.00	0.82	136	1.00	0.82	0.68	0.55
Baseline nontakers (b)	376	0.00	0.48	204	0.00	0.44	0.68	0.32
(a) - (b)			0.346 ***			0.382 ***	0.000	0.233 ***
			(0.030)			(0.050)	(0.052)	(0.053)
First trimester (c)	165	0.31	0.49	125	0.24	0.47	0.74	0.35
Second trimester (d)	427	0.56	0.68	215	0.49	0.67	0.64	0.45
Third trimester (e)	247	0.71	0.76					
(c) - (d)		0.251 ***	0.193 ***		0.253 ***	0.193 ***	-0.094 *	0.095 *
		(0.044)	(0.042)		(0.054)	(0.054)	(0.053)	(0.055)
(c) - (e)		0.399 ***	0.266 ***					
		(0.048)	(0.047)					
Past takers (f)	524	0.64	0.70	204	0.46	0.59	0.69	0.42
Past nontakers (g)	126	0.24	0.57	52	0.15	0.56	0.65	0.42
First timers (h)	178	0.52	0.63	79	0.41	0.61	0.65	0.34
(f) - (g)		0.401 ***	0.127 ***		0.307 ***	0.031	0.032	-0.002
		(0.047)	(0.046)		(0.074)	(0.077)	(0.073)	(0.077)
(f) - (h)		0.117 ***	0.069 *		0.056	-0.019	0.041	0.080
		(0.042)	(0.040)		(0.066)	(0.065)	(0.062)	(0.065)
Literate (i)	318	0.65	0.72	138	0.56	0.67	0.74	0.47
Illiterate (j)	518	0.49	0.63	202	0.29	0.54	0.63	0.37
(i) - (j)		0.154 ***	0.094 ***		0.266 ***	0.134 **	0.106 **	0.100 *
		(0.035)	(0.034)		(0.052)	(0.054)	(0.052)	(0.054)
Control	236	0.58	0.64	101	0.52	0.57	0.73	0.44
Information	185	0.52	0.56	76	0.33	0.50	0.62	0.32
CCT	205	0.54	0.73	89	0.37	0.66	0.73	0.49
Combined	216	0.56	0.74	74	0.34	0.64	0.59	0.38

*10% significance, **5% significance, ***1% significance.

Table 5. Average treatment effects.

	Fast take-up (n=842)			Repeated take-ups (n=340)				
	Simple difference	Fixed-effects DID	Fixed-effects DID	Simple difference	Fixed-effects DID	Fixed-effects DID	IV Fixed-effects DID	First stage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. No control								
Information	-0.073	-0.008	-0.051	-0.120	0.076	-0.075	-0.047	-0.190 **
	(0.066)	(0.066)	(0.058)	(0.085)	(0.103)	(0.082)	(0.092)	(0.087)
CCT	0.091	0.135 *	0.106 *	0.059	0.213	0.094	0.116	-0.150 *
	(0.066)	(0.079)	(0.063)	(0.104)	(0.129)	(0.104)	(0.112)	(0.087)
Combined	0.101	0.125 *	0.109 *	-0.057	0.130	-0.014	0.012	-0.185 **
	(0.070)	(0.067)	(0.060)	(0.091)	(0.109)	(0.090)	(0.106)	(0.085)
Baselein take-up[a]			-0.655 ***			-0.770 ***	-0.628 ***	
			(0.039)			(0.062)	(0.218)	
Second trimester								0.250 ***
								(0.051)
R-squared	0.022	0.015	0.370	0.018	0.018	0.391	0.378	0.089
B. With controls								
Information	-0.075	0.000	-0.052	-0.110	0.095	-0.062	-0.029	-0.189 **
	(0.062)	(0.063)	(0.057)	(0.088)	(0.099)	(0.086)	(0.093)	(0.077)
CCT	0.090	0.151 **	0.109 *	0.079	0.238 *	0.116	0.142	-0.142 *
	(0.063)	(0.073)	(0.061)	(0.106)	(0.122)	(0.105)	(0.108)	(0.082)
Combined	0.083	0.145 **	0.102 *	-0.054	0.124	-0.012	0.017	-0.176 **
	(0.068)	(0.062)	(0.060)	(0.090)	(0.107)	(0.088)	(0.100)	(0.087)
Second trimester	0.185 ***	-0.060	0.109 **					0.241 ***
	(0.049)	(0.051)	(0.045)					(0.052)
Third trimester	0.243 ***	-0.156 ***	0.119 **					
	(0.052)	(0.057)	(0.050)					
Baseline take-up[a]			-0.690 ***			-0.765 ***	-0.601 ***	
			(0.041)			(0.063)	(0.218)	
R-squared	0.079	0.041	0.384	0.027	0.045	0.397	0.381	0.125

[a] Endogenous variable in column (7). *10% significance, **5% significance, ***1% significance. Notes: Controls not shown here are constant in panel A, constant and LGA dummies in columns (1) and (4) of panel B, and post-intervention dummy and LGA-post-intervention dummies in columns (2), (3), and (5)-(8) of panel B. Standard errors clustered by village are in parentheses.

Table 6. Heterogeneous treatment effects - timing of interventions.

		Prgenancy trimester at baseline		
	All	First	Second	Third
	(1)	(2)	(3)	(4)
A. OLS - baseline take-up				
Information	-0.066	0.022	-0.131 *	0.016
	(0.068)	(0.114)	(0.073)	(0.105)
CCT	-0.043	-0.019	-0.103	0.033
	(0.067)	(0.117)	(0.084)	(0.105)
Combined	-0.025	-0.049	-0.176 **	0.147
	(0.072)	(0.106)	(0.083)	(0.100)
F (p-value)	0.80	0.94	0.14	0.40
No. observations	842	165	427	247
B. Simple difference - fast take-up				
Information	-0.075	0.088	-0.200 ***	0.056
	(0.062)	(0.116)	(0.072)	(0.105)
CCT	0.090	0.278 **	0.038	0.081
	(0.063)	(0.126)	(0.077)	(0.083)
Combined	0.083	0.114	0.042	0.140
	(0.068)	(0.124)	(0.081)	(0.094)
R-squared	0.079	0.076	0.066	0.037
C. Fixed-effects difference-in-differences - fast take-up				
Information	0.000	0.053	-0.047	0.071
	(0.063)	(0.120)	(0.080)	(0.112)
CCT	0.151 **	0.270 **	0.142 *	0.101
	(0.073)	(0.130)	(0.081)	(0.102)
Combined	0.145 **	0.193 *	0.216 ***	0.034
	(0.062)	(0.104)	(0.074)	(0.104)
R-squared	0.041	0.044	0.043	0.056

*10% significance, **5% significance, ***1% significance. Notes: Controls not shown here are constant in panel A, constant and LGA dummies in panel B, and post-intervention dummy and LGA-post-intervention dummies in panel C. Standard errors clustered by village are in parentheses.

Table 7. Baseline knowledge and perceptions about antenatal care by experience and literacy.

			Past pregnancy/take-up experience					Pregnant women's literacy		
		All	Past takers	Past non-takers	First timers			Literate	Illiterate	
		(1)	(2)	(3)	(4)	(2)-(3)	(2)-(4)	(7)	(8)	(7)-(8)
Knowledge (0/1) (n=686):										
1	Knew antenatal care	0.90	0.93	0.81	0.86	0.13 ***	0.07 **	0.91	0.89	0.02
2	Antenatal care could help keep you and your baby healthy	0.89	0.92	0.79	0.84	0.13 ***	0.08 ***	0.90	0.88	0.02
3	Antenatal care could help detect potential problems early, prevent them, and direct you to appropriate specialists, hospitals, etc., if needed	0.86	0.89	0.78	0.82	0.11 ***	0.07 **	0.86	0.86	0.00
4	Babies of mothers who did not get antenatal care are much more likely to die, have low birth weight, and be unhealthy than those born to mothers who got antenatal care	0.73	0.77	0.61	0.70	0.16 ***	0.07 *	0.74	0.73	0.01
5	In Nigeria many children die before they reach the age of 5 because mothers did not take antenatal care	0.61	0.65	0.49	0.57	0.17 ***	0.08 *	0.63	0.60	0.03
6	Antenatal care includes tetanus toxid vaccination	0.72	0.74	0.68	0.69	0.07	0.05	0.76	0.70	0.06 *
7	Antenatal care includes iron/folic acid supplementation	0.65	0.64	0.63	0.67	0.02	-0.03	0.69	0.62	0.06 *
Perceptions (n=664):										
8	Thought antenatal care was good for your and your baby's health (0/1)	0.92	0.92	0.87	0.95	0.06 *	-0.02	0.94	0.91	0.03
9	Importance of antenatal care (1: not important at all, 2: unimportant, 3: neither unimportnat nor important, 4: important, 5: very important)	4.33	4.40	3.90	4.46	0.50 ***	-0.06	4.37	4.31	0.06
10	Thought you could get good enough antental care (0/1)	0.78	0.79	0.75	0.76	0.05	0.03	0.78	0.78	0.01
11	Quality of antental care you thought you could get (1: very poor, 2: poor, 3: fair, 4: good, 5: very good)	4.04	4.13	3.79	3.90	0.34 ***	0.23 **	4.04	4.03	0.01

*10% significance, **5% significance, ***1% significance.

Table 8. Reasons for not getting antenatal care and antenatal care facilities by experience and literacy.

		Past pregnancy/take-up experience					Pregnant women's literacy		
	All	Past takers	Past non-takers	First timers			Literate	Illiterate	
	(1)	(2)	(3)	(4)	(2)-(3)	(2)-(4)	(7)	(8)	(7)-(8)
Reasons for not taking antenatal care at baseline (proportion) (n=339):									
High cost	0.44	0.47	0.42	0.38	0.05	0.09	0.33	0.48	-0.14 **
Lack of service	0.06	0.05	0.07	0.06	-0.01	-0.01	0.03	0.07	-0.03
Long distance	0.08	0.05	0.14	0.05	-0.09 **	0.01	0.09	0.08	0.01
Non-necessity	0.30	0.26	0.32	0.35	-0.05	-0.08	0.44	0.23	0.21 ***
Other	0.13	0.16	0.05	0.17	0.11 **	-0.01	0.10	0.15	-0.05
Nearest antenatal care facilities (n=842):									
Distance to health facility (minutes)	21.2	20.0	24.2	18.4	-4.2 ***	1.7	19.5	20.9	-1.4
Free antenatal care (0/1)	0.70	0.71	0.66	0.71	0.05	0.00	0.77	0.66	0.11 ***
Subjective quality of antenatal care (1: fair, 2: good, 3: very good)	2.03	2.09	1.88	2.11	0.21 ***	-0.03	2.09	2.04	0.04

*10% significance, **5% significance, ***1% significance.

Table 9. Heterogeneous treatment effects - experience and literacy.

		Experience			Literacy		Past nontakers/first timers		Past takers	
	All[a]	Past nontakers /first timers	First timers	Past takers	Illiterate	Literate	Illiterate	Literate	Illiterate	Literate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. OLS - baseline take-up										
Information	-0.093	0.007	-0.137	-0.122	0.035	-0.253 ***	0.204	-0.274 *	-0.024	-0.212 **
	(0.072)	(0.109)	(0.149)	(0.087)	(0.097)	(0.081)	(0.146)	(0.147)	(0.124)	(0.094)
CCT	-0.081	-0.121	-0.148	-0.050	0.002	-0.175 *	-0.106	-0.203	0.042	-0.065
	(0.068)	(0.101)	(0.132)	(0.083)	(0.082)	(0.099)	(0.122)	(0.166)	(0.100)	(0.105)
Combined	-0.141 *	-0.108	-0.142	-0.139	-0.040	-0.219 **	-0.079	-0.139	0.007	-0.242 **
	(0.074)	(0.101)	(0.126)	(0.093)	(0.082)	(0.097)	(0.112)	(0.185)	(0.103)	(0.119)
F (p-value)	0.25	0.50	0.59	0.38	0.92	0.02	0.17	0.32	0.96	0.08
No. observations	592	214	132	367	357	231	120	93	230	134
B. Simple difference - fast take-up										
Information	-0.120 *	-0.226 **	-0.284 **	-0.072	-0.116	-0.073	-0.219	-0.247	-0.074	-0.025
	(0.068)	(0.107)	(0.126)	(0.082)	(0.077)	(0.082)	(0.135)	(0.153)	(0.105)	(0.093)
CCT	0.095	-0.034	-0.118	0.164 **	0.173 **	0.024	0.027	-0.112	0.233 ***	0.070
	(0.072)	(0.121)	(0.134)	(0.076)	(0.074)	(0.104)	(0.151)	(0.183)	(0.084)	(0.109)
Combined	0.059	-0.012	-0.045	0.104	0.138 *	-0.015	0.053	-0.101	0.200 **	0.050
	(0.071)	(0.117)	(0.127)	(0.071)	(0.078)	(0.081)	(0.145)	(0.138)	(0.081)	(0.107)
R-squared	0.081	0.077	0.128	0.110	0.088	0.107	0.071	0.127	0.141	0.127
C. Fixed-effects difference-in-differences - fast take-up										
Information	-0.019	-0.252 **	-0.234	0.066	-0.098	0.137 *	-0.386 **	-0.081	0.018	0.196 *
	(0.072)	(0.121)	(0.163)	(0.070)	(0.102)	(0.080)	(0.156)	(0.157)	(0.105)	(0.099)
CCT	0.175 **	0.042	-0.064	0.218 **	0.214 **	0.145	0.134	-0.028	0.258 **	0.081
	(0.077)	(0.138)	(0.151)	(0.094)	(0.092)	(0.112)	(0.160)	(0.186)	(0.118)	(0.158)
Combined	0.208 ***	0.093	-0.006	0.232 ***	0.217 ***	0.186 **	0.135	0.010	0.236 **	0.301 **
	(0.059)	(0.104)	(0.131)	(0.076)	(0.079)	(0.084)	(0.142)	(0.131)	(0.094)	(0.137)
R-squared	0.043	0.085	0.106	0.045	0.083	0.032	0.176	0.085	0.078	0.101

*10% significance, **5% significance, ***1% significance. [a] Pregnant women at the first and second trimesters at baseline. Notes: Controls not shown here are constant in panel A, constant and LGA dummies in panel B, and post-intervention dummy and LGA-post-intervention dummies in panel C. Standard errors clustered by village are in parentheses.