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Short-run Incentive and Information in Sequential Adoptions: An Antenatal Care Experiment in Rural Nigeria

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Abstract

This paper experimentally studies sequential adoptions of antenatal care in rural Nigeria. We consider two policy targets: nonadopters, who make no adoptions, and late adopters, who make a first adoption late. Incentivizing first adoption can sustainably promote sequential adoptions if nonadopters positively update their belief about the product or shift their dynamic decision (even without learning) or if late adopters hasten their adoption sequence. We jointly examine sustainability and complementarity of interventions. Cash incentive promoted hastening, but not learning or shifting. Information intervention was ineffective. Bundled information, however, nullified the hastening, because the composition of compliers to the incentive changed.

Keywords: Sequential adoptions; Antenatal care; Incentive; Information; Sustainability; Complementarity; Nigeria
JEL classification: D83, I12, O15.

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1. Introduction

Underutilization of preventive health care in developing countries is a serious problem (Dupas 2011, Dupas and Miguel 2017, Kremer and Glennerster 2011). Many health products require sequential adoptions. They can be categorized into two: 1) adoptions made through clearly defined stages with an endpoint, such as weeks/months of pregnancy for antenatal care and age in weeks/months/years for child vaccination; and 2) adoptions made with no such stages or endpoint, such as water treatment products and bed nets. In the first type, adoption at the right time is key for the product to be fully effective, and once the right time is missed, it will never be possible; no adoption is feasible after the endpoint. In the second type, an individual who misses an adoption today can make an adoption with the same effectiveness in the future. Preventive maternal and child health care is mostly the first type.

Choosing an optimal adoption sequence for first-type products requires a dynamic decision that takes into account that current adoption alters the effectiveness of future adoptions. The significance of this dynamic link depends on specific products and whether decision makers incorporate it. To our knowledge, there are no experimental works on first-type health products considering the possibility of this dynamic decision. To fill this gap, we conducted a randomized experiment on antenatal care in rural Nigeria.[1]

Underutilization of first-type products concerns not only nonadoption, but also late first adoption and insufficient adoption. In rural Nigeria, 53.1% of pregnant women made at least one antenatal visit; 13.7% and 23.5% made a first visit in the first three months and fourth/fifth months, respectively; 34.4% made at least four visits (National Population Commission and ICF Macro 2009). Policy targets to mitigate the first two problems are different: *nonadopters* who make no adoptions (before the endpoint) vs. *late adopters* who make a first adoption late (missing the right time).[2] Nonadopters and late adopters cannot be identified ex ante. What is required to promote sequential adoptions is different between them: increasing both an early first adoption and subsequent adoptions vs. increasing an early first adoption without decreasing

[1] A growing literature documenting the links between long-term outcomes and health in fetal period (Almond and Currie 2011) suggests the critical importance of antenatal care not only for better maternal and infant health, but also for a newborn's better quality of life in the long run. At the same time, randomized control trials on the effectiveness of antenatal care in developing countries are very scarce (Carroli, et al. 2001). This paper does not address the effectiveness of antenatal care, which is a topic of our companion paper in progress.

[2] This paper does not address *insufficient adopters* who make fewer adoptions (but not zero) than required. Although late adopters are not always insufficient adopters, the later the first adoption made, the more likely they will become insufficient adopters. At the same time, those who made a first adoption early may not make sufficient adoptions.

subsequent ones (no substitution). Extant works on sequential adoptions have not given explicit attention to late adopters, probably because most of them study second-type products.

Since incentive and subsidy programs are expensive, whether the impacts of short-run interventions sustain after the program ends in the long run is a critical question (Kremer and Miguel 2007). We explore why incentivizing the first adoption can sustainably promote sequential adoptions. *Learning* from experience has been studied by Dupas (2014) on bed nets (and by many researchers on technology adoption, Foster and Rosenzweig 2010). If nonadopters underestimate the quality of experience goods, such as effectiveness and comfort, and positively update their belief through the first adoption, they can change behaviors, subsequently making a non-incentivized adoption.[3] Our model reveals two other potential channels. First, late adopters may hasten the timing of their first adoption without substitution (*hastening*). Learning and hastening are possible regardless whether the dynamic decision is made. Second, if nonadopters made an incentivized first adoption based on the dynamic decision, they alter their prior belief about a subsequent adoption from one based on no first adoption to another belief given the first adoption made; as a result, they may shift their long-run behaviors from the status quo (*shifting*). In contrast, shifting among late adopters means a substitution between the first and subsequent adoptions. Hence, the impacts of the temporary intervention need to persist for sustainable sequential adoptions through learning and shifting among nonadopters, but not through hastening among late adopters.

In our experiment, we offered a small amount of cash (about the amount of daily wage) to pregnant women, conditional on their making one antenatal visit within a month.[4] Compared to incentives and subsidies, information interventions are less costly. When the short-run incentive is bundled with information provision, these two may be complementary not only in the short run, but also in the long run. Our experiment captures this potential link of sustainability and complementarity. We employed a cluster randomized design at the village

[3] Similarly, short-run subsidies increasing experimentation may lead to more utilization without subsidies in the long run (Dupas 2014). At the same time, lower prices may serve as reference points (anchors) that work to decrease subsequent demand (Fischer, et al. 2014).

[4] Although the conditionality on one visit may be less effective in promoting multiple visits than the conditionality on multiple visits, which conditionality is more effective is an empirical question. When the incentive is conditional on multiple visits, nonadopters, who are unlikely to make required visits, may not even make a first visit (supporting evidence is found below). Such conditionality may be less effective in hastening the first visit of late adopters. Kohler and Thornton (2011) show that money given in the present is more effective to prevent HIV than rewards promised in the future. Banerjee et al. (2010) find similar results for child vaccination.

level, so that estimated treatment effects are robust to potential spillover within villages, such as social learning and information sharing not addressed in our study.

The temporary cash incentive sustainably increased first adoption without decreasing subsequent adoption, i.e., hastening among late adopters; it did not increase subsequent adoption through learning or shifting among nonadopters. When we conduct a conventional analysis of sequential adoptions that ignores late adopters, and thus hastening, it indicates unsustainable incentive effects instead. The earlier the stage of pregnancy, the stronger is the hastening. This was significant for women who had made antenatal visits in their past pregnancy. Although the information was ineffective in promoting first adoption, with the information bundled, the incentive effect on first adoption somewhat decreased and the hastening mostly vanished. This is not because the additional information weakened the persistent incentive effect, which was nonexistent, but because it altered the composition of compliers who responded to the incentive. Specifically, we find that early-stage women became less responsive and late-stage women with no time for second adoption instead became more responsive (we provide potential reasons below). In contrast, the small incentive did not increase first adoption among women who had not made antenatal visits before, including first-time pregnancy.

Our study mainly contributes to two strands of the literature on promoting preventive health care in developing countries. The first is the sustainability of program impacts. The learning channel has been supported by findings on subsidized new health products of the second type: Dupas (2014) shows positive learning from experimentation with new insecticide-treated bed nets whose quality was underestimated; Ashraf, Berry, and Shapiro's (2010) finding on water-treatment products whose quality was overestimated suggests negative learning. We study first-type products, considering potential dynamic decisions and late adopters. We develop a general framework to estimate the impacts of short-run interventions on sequential adoptions, test channels underlying sustainable adoptions, and assess sustainability. Shifting and hastening expand the scope of sustainability. Distinct from learning, shifting might be significant for familiar products. Our experiment finds strong hastening for familiar products: Temporary incentive sustainably promotes late adopters' early adoption.

The second strand of the literature is the complementarity of interventions. A growing literature examines interactions of information programs with subsidies, finding mixed results: Although information about an unfamiliar water-treatment product increases the effectiveness of

price subsidies (Ashraf, Jack, and Kamenica 2013), information interventions do not alter the effectiveness of price subsidies for familiar insecticide-treated bed nets (Dupas 2009).[5] Combined treatments might be less effective than individual treatments. Duflo, Dupas, and Kremer (2015) show that although education subsidies (school uniforms) alone reduce early pregnancy, when they are bundled with teacher training for HIV curriculum, which has no individual effect, their impacts weaken, because girls' sexual behaviors and schooling decisions are altered by the additional information. We jointly examine sustainability and complementarity, which have been studied separately in the literature. For familiar first-type products, we find a negative synergy over time: The additional information nullifies the hastening by altering the composition of compliers to the incentive according to individuals' adoption stage.

The rest of the paper is organized as follows. Section 2 describes the field experiment and verifies the randomness of treatments. Section 3 models sequential adoptions. Section 4 develops the empirical framework and reports results. Section 5 conducts a heterogeneity analysis. Section 6 examines complier heterogeneity. The last section concludes.

2. Field experiment

2.1. Sampling design

Our experiment was fielded in five local government areas (LGAs) of Adamawa State in northeastern Nigeria in 2009.[6] We considered 647 villages with a female population between 130 and 1,000 to be eligible for our study. In each of the five LGAs, we stratified all eligible villages by the availability of health facility within villages; villages with a facility were less common (120 vs. 527). Considering the village distributions across these 10 strata, we set a village sample size within each stratum (from 4 to 14) such that the numbers of villages with and without a facility are relatively balanced in each LGA; that is, we oversampled villages that had a health facility (see Online Table A-1 panel A for details). Thus, the sample does not represent average

[5] Empirical findings on information interventions alone to promote preventive health care are also mixed (Dupas 2011, Dupas and Miguel 2017, Kremer and Glennerster 2011).

[6] Neonatal, infant, and under-five mortality rates in the North East Zone were the highest in the country (53, 109, and 222, respectively, per 1,000 live births, National Population Commission and ICF Macro 2009). Out of 21 LGAs, we intentionally selected 5 with distinct income, ethnic groups, and political power: Maiha and Michika from 6 poor LGAs and Guyu, Numan, and Jada from 15 non-poor LGAs. Maiha and Jada are inhabited by major ethnic groups (Kilba and Chamba, respectively, with strong political power); Michika, Guyuk, and Numan are inhabited by minor groups.

villages in the study area. In each stratum, we randomly sampled the target number of villages using a computerized random number generator (100 villages in total).

The nearest health facilities providing antenatal care were mostly small public ones with skilled providers (community health extension workers, midwives, nurses). In 70 villages, antenatal care was free; the median fee in clinics without free services was 250 naira (US$1=150 naira, July 2009). Almost all antenatal clinics provided tetanus toxoid vaccination, iron/folic acid supplementation, and anti-malaria medication; about three quarters and one third provided family planning and HIV tests, respectively.[7]

With no administrative records of pregnant women, in each village, our survey team visited households, making a list of all pregnant women found. To ensure variation in the stages of pregnancy through which antenatal visits are made, in each village, we stratified women by their self-reported trimester of pregnancy (whose measurement errors are discussed later).[8] In each stratum, we randomly sampled women using a computerized random number generator (from 3 to 19 women per village). The baseline sample consists of 1,032 women.

2.2. Experimental design

We designed four treatment arms: 1) incentive, 2) information, 3) combined, and 4) control. Women in the incentive group were told that if they make one antenatal visit within a month, they would receive 400 naira (US$2.7), which was close to median daily wage. Women in the information group were read a script in their local language, Hausa, and were given the same script in Hausa. The script contained information about recommended visits (at least four, which is WHO guideline, WHO 2003), the purpose and benefits of antenatal care, and the risks of not receiving it (Online Appendix A provides the English translation of the script). The combined treatment group received both interventions. The control group received no interventions. All women were given an antenatal care card to be filled by health staff as a proof for their antenatal visits over the study period.

[7] These available services match those actually received. Almost all women who made antenatal visits received care from skilled providers at health facilities; over 90% were weighed, had blood pressure taken, and received tetanus toxoid vaccination and iron/folic acid supplementation, whereas about one half had urine and blood samples taken (these patterns are close to the average in Adamawa State in 2013, National Population Commission and ICF International 2014). Over 80% considered the quality of services received to be at least good.

[8] When possible, we excluded those in the first and ninth/tenth months of pregnancy, because the pregnancy status could be inaccurate and the remaining prenatal period was short, respectively.

In each stratum, we set the size of each treatment group: If the number of villages was in a multiple of four (4, 8, or 12), the group size was the same (1, 2, or 3); otherwise, it varied across groups (Online Table A-1 panel A). The assignment ratios thus varied across 4 treatment arms within and across strata (from .1 to .4). In each stratum, we randomly assigned villages to one of the four arms using a computerized random number generator (27 control, 24 incentive, 24 information, 25 combined). Thus, treatment assignments are random within strata.

The implementation did not perfectly follow the treatment protocol. Three villages with 31 women received treatments different from the original assignments: Two incentive villages were treated as a control village and one control village was treated as an incentive village. These implementation errors occurred only in one stratum – stratum 5, with no village health facility, consisting of 14 villages with 136 women (Online Table A-1 panels B and C).

In June 2009, we conducted a baseline survey, collecting information of current pregnancy, past pregnancies, if any, and various characteristics; at the end of the survey, the interventions were executed. About one month later, the first follow-up survey followed; the cash reward was given to women, not their husbands, with a proof of antenatal visit. Two more follow-up surveys followed about four and 11 months after the baseline. These follow-up surveys collected information about maternal health behaviors (antenatal visits with proof in particular) and outcomes (and newborns' health measures) over time.

2.3. Baseline balance

In the baseline sample, 19%, 52%, and 29% of women were in the first, second, and third trimesters, respectively, of pregnancy at the time of the baseline survey, 56% already had made at least one antenatal visit in the current pregnancy (*baseline uptake*), 22% were first-time pregnancy, and the mean parity was 2.3; 80% of non-first-time pregnant women received antenatal care at least once in their past pregnancies, 75% did so in the last pregnancy, and 49% made at least four antenatal visits in the last pregnancy. As such, women vary not only in the stage of pregnancy, but also in baseline uptake and past experience.

We check the balance of 20 baseline covariates across 100 villages.[9] Table 1 shows the difference in each covariate between each of the three treatment groups and the control group.[10]

[9] Women were 26.2 years old and belonged to households with 5.1 members, on average; 63% were illiterate and 67% belonged to households with a child at age 5 or younger. As a proxy for household welfare, we use a durable asset index, which is a z-score constructed by taking the first principal component of 11 measures of land holdings,

The equality of means across treatment groups is rejected at a 5% (and 10%) significance level for two variables, and two out of 60 mean differences are different from 0 at a 5% significance level, as expected by chance. These results suggest that the randomization performed well.

3. A model of sequential adoptions

To guide the empirical framework in the next section, this section constructs a simple dynamic model of sequential adoptions to show whether and why nonadopters and late adopters (policy targets) alter their non-incentivized adoption after making an incentivized adoption. We first model sequential decisions in the absence of learning, defining nonadopters and late adopters according to their prior belief about the product. We then consider a short-run incentive, identifying shifting and hastening channels. Next, we introduce learning. Lastly, we discuss an alternative static model. We assume time-consistent individuals; present bias does not alter our main results, as discussed below.

Figure 1 illustrates experimental and maternal timelines. The inception of conception varies from M1 through M2; the timing of delivery (endpoint) varies from O1 through O2. The prenatal period consists of period 0 before the baseline survey, period 1 between the baseline and first follow-up, and period 2 afterwards. The duration of period 0, i.e., the stage of pregnancy at the baseline, varies. Among women who delivered a baby after the first follow-up (O3O2), the duration of period 1 is about one month and that of period 2, i.e., the remaining prenatal period, varies. The shorter period 0, the longer is period 2. Women either made baseline uptake at period 0 (*baseline takers*) or did not (*baseline nontakers*). Only among the latter do nonadopters and late adopters exist. The model considers baseline nontakers who had not completed pregnancy at period 1.[11]

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housing quality (number of rooms, better wall, better floor, better roof), and household assets (possession of generator, television, phone, bicycle, motorcycle) (Filmer and Pritchett 2001). About 300 households resided, on average, 35% of villages had tapped water or a piped/tubed well, and 71% had any toilet. Among women, 50% had received no education, 21% were polygamous, and 36% were Muslim (the rest were Christian). In the regression analysis below, we do not control for these three covariates (which are balanced) and village characteristics with considerable missing values.

[10] When we regress each variable on the three treatment dummies with strata fixed effects controlled for, the results are very similar. When we make a comparison at the individual level (with standard errors clustered by village), the results are also very similar.

[11] Baseline takers included late adopters who already made a late first adoption and insufficient adopters this paper does not address. Baseline nontakers and takers made different decisions at periods 1 and 2: to make a first adoption and then continue adoptions afterwards vs. to continue the adoption sequence they had already started. The equality of the treatment effects between them is rejected below. Since baseline takers could update their belief from baseline uptake, their learning from adoption at period 1 should be much more limited than baseline nontakers.

A woman makes binary decisions through time: at least one antenatal visit at period 1 (*uptake 1*, d_1) and at least one visit at period 2 (*uptake 2*, d_2). Ignoring sequential decisions within period 2, this two-period model highlights the dynamic decision about a temporary incentive at period 1. Period 2 is long enough to make uptake 2, i.e., she is at a relatively early stage of pregnancy (woman *a*); we consider short period 2 (woman *b*) later. Then, uptake 1 captures *early adoption*, regardless whether she makes uptake 2; uptake 2 without making uptake 1 captures *late adoption*. The conventional analysis of sequential adoptions considers uptakes 1 and 2 as outcomes. Sustainable sequential adoptions mean at least one visit at both periods 1 and 2 (period 12) denoted by *uptake 12* (d_{12}). Our primary outcome is uptake 12 excluding late adoption.[12]

3.1. Long period 2

No learning

Suppose that the woman does not update her prior belief about antenatal care through uptake 1. Then, all decisions are made according to her prior belief. Whereas the net benefit at period 1 (better maternal health minus monetary and non-monetary costs) is determined solely by uptake 1, the net benefit at period 2, which includes that accrued afterwards (e.g., newborn's health), is determined by both uptakes 1 and 2; that is, early adoption persistently affects the effectiveness of the product. Let $B_1(d_1)$ and $B_2(d_1,d_2)$, respectively, be her prior belief about the net benefits at periods 1 and 2. $B_1(0) = B_2(0,0) = 0$ with no loss of generality. With a short time horizon of period 1, we ignore discounting. Her prior belief about the total net benefit at period 12 is $B_{12}(d_1,d_2) = B_1(d_1) + B_2(d_1,d_2)$. Let m be her perceived net benefit of late adoption: $m = B_2(0,1) = B_{12}(0,1)$. Let H be the difference between her perceived net benefits at period 2 with and without uptake 2 after making uptake 1: $H = B_2(1,1) - B_2(1,0)$. Then, $B_{12}(1,1) = G + H$, where $G = B_{12}(1,0) = B_1(1) + B_2(1,0)$, i.e., the net benefit from uptake 12 is decomposed into one from uptake 1 alone (G) and the additional benefit from uptake 2 (H).

In the control group, a woman's prior belief about G and H determines her sequential decisions, depending on the sign of m (Figure 2). When $m \leq 0$ (panel A), she never prefers late adoption to no adoption. Regardless of m, she makes uptake 1 if the net benefit from uptake 1

[12] Since the incentive effect on multiple adoptions at period 2 measured by a dummy for at least n (≥ 2) antenatal visits is smaller than or equal to at least one visit, only if the effect on uptake 2 is positive can that on multiple adoptions be positive. Only if the effect on uptake 12 is positive can that on at least $n + 1$ visits at period 12 (given one visit at period 1) be positive.

alone is positive ($G > 0$), regardless of the sign of H (areas C1 and C2), or if the net benefit from uptake 12 is positive ($G + H > 0$), even if $G < 0$ (area C3). If she made uptake 1, she makes uptake 2 if $H > 0$ (areas C1 and C3); otherwise, she does not (area C2). If she did not make uptake 1, she does not make uptake 2, because $m \leq 0$, i.e., she is a nonadopter. When $m > 0$ (panel B), the same decision rule applies, except that she compares G and $G + H$ with m, and if she did not make uptake 1, she always makes uptake 2, i.e., she is a late adopter.[13] Thus, nonadopters and late adopters are left of the solid bold line in panels A and B, respectively. Late adopters' stronger prior belief than nonadopters leads to their distinct decisions about uptake 2.

In the incentive group, a cash incentive of amount k (> 0) is given conditional on uptake 1. Suppose that the incentive has a positive impact on uptake 1. Compared to the control group, uptake 1 is additionally made among nonadopters in areas T1 and T2.[14] In the Local Average Treatment Effect (LATE) framework, those who changed their decision about uptake 1 in response to the incentive are compliers; those who make uptake 1 without an incentive (areas C1-C3) are always-takers. Among those compliers, one makes uptake 2 if $H > 0$ and otherwise does not. Although nonadopters would make no uptakes without the incentive, compliers in area T1 shift their decision about uptake 2 (*shifted nonadopters*) and those in area T2 do not (*non-shifted nonadopters*). The shift among the former is based on an alternative prior belief.[15] Distinct from learning discussed below, H is not updated. The same decision rule applies when $m > 0$ for late adopters (panel B). Compliers make uptake 2 if $H > 0$ (area T1), as in the control group (*non-shifted late adopters*), and otherwise do not (area T2) (*shifted late adopters*).[16] Although non-shifted late adopters hasten their first adoption without decreasing subsequent adoption, shifted late adopters shift their sole adoption from period 2 to period 1 (substitution).

As such, although the temporary incentive increases early adoption for all compliers, it sustainably promotes sequential adoptions only for shifted nonadopters and non-shifted late adopters. Nonadopters make uptake 2 if they change their behaviors through uptake 1; late adopters do so if they do not. Thus, the incentive can have a positive impact on nonadopters'

[13] Women with positive m do not necessarily make uptake 2: If the woman made uptake 1 and $H < 0$, she does not make uptake 2 (area C2).
[14] When $m \leq 0$ (panel A), a woman makes uptake 1 if $G + k > 0$ or ($G + k + H > 0$ and $G + k < 0$); otherwise, she does not (areas N1 and N2).
[15] For shifted nonadopters, $-k < G + H = B_{12}(1,1)$ (< 0), that is, their negative perceived net benefit from uptake 12 is smaller than k in magnitude.
[16] For shifted late adopters, $(0 <) m - G = B_{12}(0,1) - B_{12}(1,0) < k$, that is, the difference between their perceived net benefits from uptake 2 alone and from uptake 1 alone is smaller than k.

uptakes 2 and 12 through shifting and late adopters' uptake 12, but not uptake 2, through hastening. The magnitude of these effects depends on the composition of compliers, which is ambiguous, because women's distribution in the *G-H* space is generally heterogeneous (Online Appendix B discusses homogeneous distribution).

Learning

Now suppose that the woman can update her prior belief about H through uptake 1, regardless of the sign of m. We do not model this learning process itself, because our goal is to show how an updated belief potentially alters compliers' behaviors. Let H' be updated H. If $H < 0 < H'$, a non-shifted nonadopter changes her decision (i.e., positive learning increases uptake 2), and a shifted late adopter changes her decision, making uptake 2 the same as she would do without the incentive (i.e., positive learning cancels shifting). If $H' < 0 < H$, a non-shifted late adopter changes her decision (i.e., negative learning decreases uptake 2, counteracting hastening), and a shifted nonadopter changes her decision, not making uptake 2, as she would not do so without the incentive (i.e., negative learning cancels shifting). Learning can be opposite between nonadopters and late adopters, because their prior beliefs can be different. The incentive effects on uptakes 2 and 12 are determined by the composition of compliers based on H'.[17]

Complier behaviors

Table 2 summarizes how compliers to the incentive among nonadopters and adopters alter uptakes 2 and 12 depending on the channels in the potential outcome framework (Imbens and Rubin 2015).[18] Since a nonadopter changes to make uptake 2 through shifting or positive learning, the unit-level treatment effects of the incentive on uptakes 2 and 12 are both $1 - 0 = 1$ (panel A columns 2-5). Since a non-shifted late adopter makes uptake 2 regardless of the treatment (hastening), the treatment effect on uptake 2 is 0 and that on uptake 12 is $1 - 0 = 1$ (panel B columns 6 and 7). The effects through positive learning for a late adopter are the same as those through hastening; thus, positive learning does not qualitatively alter non-shifted late adopters' behaviors. In contrast, if shifting or negative learning occurs, the treatment effect on uptake 2 is $0 - 1 = -1$ (columns 2 and 8) and that on uptake 12 is 0 (columns 3 and 9).

[17] Women in areas C1-3 in both the incentive and control groups can also change uptake 2. In our randomized experiment, such updating should be the same between the groups, on average.
[18] The unit-level treatment effect of the incentive on uptake 1 is $1 - 0 = 1$ for both nonadopters and late adopters (column 1).

Two observations are noted. First, although the effects on uptakes 2 and 12 are always the same as each other among nonadopters (0 or 1), the former is always smaller than the latter among late adopters (-1/0 or 0/1). Thus, the estimated incentive effect on uptake 2 is no greater than that on uptake 12. Second, the treatment effects on uptake 2/uptake 12 are the same between shifting and positive learning for nonadopters and between shifting and negative learning for late adopters. Thus, it is infeasible to distinguish shifting and learning based on the estimated incentive effects.

If individuals are present biased, they can procrastinate adoptions, their response to the incentive can be different, and the composition of compliers can thus change. Present bias, however, should not alter the qualitative results summarized in Table 2, because they are based on the incentive increasing uptake 1, i.e., it deters procrastination.

Online Appendix C discusses the information intervention and combined treatment. It shows that the information can alter the incentive effects on uptakes 2 and 12 in either direction depending on the channels.

3.2. Short period 2

Consider the incentive group with no learning. As period 2 becomes shorter, the following relationships hold. First, nonadopters become more common than late adopters, simply because the shorter the remaining prenatal period, the more women with $m > 0$ have already made a first adoption at period 0 (they have become baseline takers, as confirmed below), while those with $m \leq 0$ have not. Second, non-shifted nonadopers/shifted late adopters become relatively more common than shifted nonadopters/non-shifted late adopters among compliers. This is because women are less likely to make uptake 2 soon after they made uptake 1, because the additional net benefit from doing so is small. Third, accordingly, non-shifted nonadopers become the majority among compliers.

As period 2 becomes close to 0, the two-period problem no longer applies. Nonadopters make a one-shot decision: Only if the cash incentive outweighs m ($k > -m$) do they make a sole adoption at period 1. It is too late to sustainably promote their sequential adoptions; the incentive can make only some of them late adopters. With learning and/or the information intervention, the same relationships hold based on updated H and m. In this way it is possible to roughly characterize unobserved compliers by observed stages of pregnancy.

3.3. Static model

Online Appendix D constructs a static model as a constrained version of the dynamic model. The only difference is that the net benefit at period 2 is independent of uptake 1. This can be attributed to the product or due to the decision maker's ignorance. Then, shifting is irrelevant, and shifted nonadopters/late adopters are nonexistent. Otherwise, all results in this section hold. Put differently, shifting is made possible solely by the dynamic decision.

4. Empirical framework and main analysis

This section develops an empirical framework based on the dynamic model. It can be directly applied to the static model by ignoring shifting and shifted non-adopters/late adopters. We then construct the analysis sample and conduct the main empirical analysis, offering interpretations based on the dynamic model.

4.1. Empirical framework

We employ the following Ordinary Least Squares (OLS) regression:

$$Y_{ivt} = \alpha_t Incentive_v + \beta_t Information_v + \gamma_t Combined_v + \phi_s + u_{ivt}, \quad (1)$$

where i, v, s, and t, respectively, denote individual, village, stratum, and period (t = 1, 2, 12); Y_{ivt} is uptakes 1, 2, and 12 (dummies); $Incentive_v$ is a dummy for the incentive alone; $Information_v$ is a dummy for the information alone; $Combined_v$ is a dummy for the combined treatment; ϕ_s is strata fixed effects; and u_{ivt} is an error term. Strata fixed effects control for LGA heterogeneity and the availability of a village health facility. The random assignment of the treatments within strata ensures that the estimated coefficient of each treatment dummy is an unbiased and consistent estimate of the treatment effect if the assignments are perfectly complied with and of the intention-to-treat effect with imperfect compliance. Conventional analysis of sequential adoptions is based on equation (1) for uptakes 1 and 2, but not 12.[19]

The cluster randomized design internalizes potential spillover within villages.[20] We cluster standard errors by village. In addition to the cluster robust variance estimator (CRVE), we

[19] If we assume that the incentive affects uptake 2 only through uptake 1 (exclusion restriction), we can estimate the effect of uptake 1 on uptake 2 using the incentive as an instrumental variable. Equation (1) for uptake 2 is a reduced-form equation. Positive α_2 indicates that uptake 1 increases uptake 2, and zero α_2 indicates that uptake 1 does not alter uptake 2. The former corresponds to learning/shifting, and the latter corresponds to hastening, as discussed below.

[20] Our data do not allow us to examine potential spillover across villages. If such spillover was significant, our estimates are biased toward zero and the significant positive effects found below are qualitatively robust.

employ a wild cluster bootstrap (Cameron, Gelbach, and Miller 2008), which is reliable for a small number of unbalanced clusters (MacKinnon and Webb 2017).[21] The results are similar in all analyses reported below.

Channels

Suppose that the incentive increases both uptakes 1 and 12 ($\alpha_1, \alpha_{12} > 0$). Recall that the incentive effect on uptake 2 is no greater than uptake 12, $\alpha_2 \leq \alpha_{12}$, where the equality holds if late adopters are nonexistent, and we cannot distinguish learning and shifting. Table 2 indicates that we can test channels driving the incentive effect on uptake 12 by comparing uptakes 2 and 12: 1) learning/shifting only: $0 < \alpha_2 = \alpha_{12}$; 2) hastening and learning/shifting: $0 < \alpha_2 < \alpha_{12}$; and 3) hastening only: $0 = \alpha_2 < \alpha_{12}$.[22] This test does not generally apply to the combined treatment, because the information can alter the incentive effects.

Sustainability

Suppose the treatment increases uptake 1 ($\tau_1 > 0$, where $\tau_t = \alpha_t, \beta_t, \gamma_t$). The sustainability of treatment effects is commonly measured by comparing uptakes 1 and 2: 1) full sustainability: $\tau_1 = \tau_2 > 0$; 2) partial sustainability: $\tau_1 > \tau_2 > 0$; and 3) no sustainability: $\tau_2 = 0$. This can be directly applied to uptake 12 (τ_{12}).[23]

When we employ this test for the incentive effects on uptakes 2 and 12 in our framework, how we can interpret the results depends on the channels. First, if only learning/shifting matters (i.e., $\alpha_2 = \alpha_{12}$), an increase in uptake 2 through nonadopters' positive learning/shifting determines how persistent the effect is in a standard way (this is captured by uptakes 2 and 12 in the same way). Second, if only hastening matters (i.e., $\alpha_2 = 0$), $\alpha_1 = \alpha_{12}$ among non-shifted late adopters and a decrease in uptake 2 among non-shifted nonadopters and shifted/negatively learning late adopters lowers α_{12} (the latter captures how common substitution is among late adopters). Thus, the test for uptake 12 measures the degree of hastening determined solely by the

[21] When the number of clusters is small, the CRVE over-rejects the null hypotheses (Cameron, Gelbach, and Miller 2008). Even if the number of clusters is not small, the CRVE can over-reject when cluster size is strongly unbalanced (MacKinnon and Webb 2017). Although the number of clusters of our data (baseline-nontakers) is not small (about 23 per treatment arm), cluster size is unbalanced (1 to 10). We use Rademacher weights and do 1,000 bootstrap replications.

[22] The third relationship indicates hastening only, because if positive learning or shifting also matters, α_2 should be positive as in the second. The reasoning for other relationships is analogous.

[23] If the treatment effect on uptake 1 is nonpositive ($\tau_1 \leq 0$), that on uptake 12 is the same as uptake 1, because individuals who did not make uptake 1 never make uptake 12.

proportion of non-shifted late adopters among compliers. The test for uptake 2 provides no additional information. Third, if both learning/shifting and hastening matter, the test for uptake 12 overestimates both the sustainability through learning/shifting and the degree of hastening. The test for uptake 2, which is independent of hastening, underestimates sustainability if late adopters' shifting/negative learning also matters. Thus, these two tests provide upper and lower bounds of sustainability through nonadopters' learning/shifting, and the test for uptake 2 serves as a conservative test. This ambiguity also exists in the static model.

These interpretations do not generally hold for the combined treatment. A notable exception is the case of $\tau_{12} = 0$. Table 2 indicates that in this case for incentive ($\alpha_{12} = 0$), $\alpha_2 = 0$ among non-shifted nonadopters (columns 6 and 8 in pane A) and $\alpha_2 < 0$ among shifted/negatively learning late adopters (columns 2 and 8 in panel B). This pattern also holds for the combined treatment, because the negative information effect does not alter uptake 2 for nonadopters and decreases it among late adopters without affecting uptake 12, as discussed in Online Appendix C.[24] Thus, in the case where $\gamma_{12} = 0$, $\gamma_2 = 0$ among non-shifted nonadopters, and if $\gamma_2 < 0$, we cannot distinguish shifting, negative learning, and negative information effect among late adopters.

Interaction and complementarity

In our experiment, if the estimated combined treatment effect is different from the incentive treatment alone, it should be due to the additional information. We can test this interaction effect by comparing the individual and combined effects (α_t vs. γ_t) for any outcomes.[25] A complementarity test compares the sum of the two individual effects with the combined effect: 1) complementarity: $\alpha_t + \beta_t < \gamma_t$; 2) no complementarity: $\alpha_t + \beta_t = \gamma_t$; and 3) negative synergy: $\alpha_t + \beta_t > \gamma_t$. These tests for uptake 1 assess contemporaneous interaction/complementarity in a standard way. Those for uptakes 2 and 12 capture the interaction/complementarity of short-run interventions in the long run.

Conventional analysis

The conventional analysis of sequential adoptions based on uptakes 1 and 2 implicitly focuses on nonadopters without considering late adopters. For example, suppose that the analysis

[24] If the information effect is positive, $\alpha_{12} > 0$.
[25] Testing the interaction effect of the incentive is analogous (β_t vs. γ_t).

shows a positive incentive effect on uptake 1 and no effect on uptake 2 ($\alpha_1 > \alpha_2 = 0$). In the conventional analysis, this indicates no sustainability of the temporary intervention (among nonadopters). Depending on whether and how much hastening matters, interpretations can be quite different. If only hastening matters, for example, this comparison indicates nothing about nonadopters or sustainability. As illustrated in this example, which is our empirical finding below, the conventional analysis ignoring late adopters could lead to wrong or incomplete interpretations.

4.2. Analysis sample

Our main analysis sample is baseline nontakers who had not completed pregnancy at period 1. Baseline uptake is not correlated with the treatments (Table 1), indicating that selection into baseline nontakers or takers is unrelated to the treatments. We exclude women who had already completed pregnancy (mostly delivery; stillbirths were very rare) at period 1 (O1O3 in Figure 1),[26] as well as those whom the follow-up surveys failed to track or for whom information of antenatal visits was incomplete. The analysis sample consists of the remaining 716 women in 99 villages who are either baseline nontakers (332 women in 93 villages) or takers (384 women in 92 villages).[27] Regressing the dummy for sample attrition on three treatment dummies with strata fixed effects controlled for shows that attrition is not correlated with the treatments in the whole, baseline-nontakers, and baseline-takers baseline samples (Online Table A-3).[28] This suggests that attrition bias in the analysis sample is not a major concern.

We did not design the experiment to account for heterogeneity between baseline nontakers and takers by doing randomization within each group.[29] Instead, in each village, we stratified women by the trimester of pregnancy, which is strongly correlated with baseline uptake.

[26] Among women who had completed pregnancy at period 1 (about 14% of the baseline sample), 24% were in the second trimester at the baseline (the rest were in the third trimester). This can be due to errors in the self-reported trimester, as well as premature birth.

[27] Baseline nontakers were at an earlier stage of pregnancy than takers: 34%, 51%, and 15% of nontakers and 13%, 58%, and 29% of takers were in the first, second, and third trimesters, respectively (Online Table A-2). Almost no characteristics reported in Table 1 are significantly different between them; as the only exception, baseline nontakers were less literate than takers.

[28] Attrition is positively correlated with third trimester, which corresponds to the completion of pregnancy at period 1; it is uncorrelated with baseline uptake and most covariates.

[29] To do so, we would have needed to sample two sets of villages for baseline nontakers and takers and randomly assign treatments across villages within each set. This alternative design, which would require a much larger number of villages covered, was logistically infeasible.

At which stage of pregnancy women were when they were listed should be random.[30] Despite not being explicitly stratified, the randomization has achieved covariate balance in each group: Among baseline nontakers (takers), the equality of means across treatment groups is rejected for no (one) variable and three (four) mean differences are different from zero at a 5% significance level (Online Table A-4).

The analysis sample includes women who completed pregnancy soon after the first follow-up survey with short period 2, especially those in the third trimester at the baseline. To identify the treatment effects on uptakes 2 and 12, a balanced distribution of the third trimester is crucial, because its positive (negative) correlation with the treatment causes downward (upward) bias, even if controlled for. Third trimester is balanced among baseline nontakers, but not in the baseline, whole analysis, or baseline-takers analysis samples (Table 3).[31] This imbalance could occur by chance, because we did not randomize women within the same trimester. It happened by chance only among baseline takers.

The trimester of pregnancy is a crude measure of the stage of pregnancy. Even if baseline trimesters are balanced, the unbalanced distribution of the fine stage of pregnancy in weeks within each trimester can cause bias. To see if this is a concern, we consider whether women completed pregnancy before the second follow-up survey (about three months after the first follow-up). This measure reflects the fine stage of pregnancy; distinct from self-reported trimesters, it is free from measurement errors, because enumerators visually confirmed the status.[32] The completion is uncorrelated with the treatments in the baseline-nontakers analysis sample; in the baseline, whole analysis, and baseline-takers analysis samples, it is so only with baseline trimesters controlled for (Online Table A-5).[33] This assures a balance of the stages of pregnancy among baseline nontakers.

The remainder of the paper focuses mostly on the baseline-nontakers analysis sample. Among them, treatment assignments happened to be almost perfect. Out of 31 women in stratum 5 with imperfect compliance, 20 remain in the analysis sample – three baseline notakers in two villages (one incentive and one control) and 17 takers in three villages (Online Table A-1 panel

[30] Baseline covariates are similar across women at different trimesters (not shown).
[31] First and second trimesters are also balanced among baseline nontakers; among baseline takers, first trimester is not so balanced and second trimester is balanced (Online Table A-4).
[32] Missing values are common in the self-reported month of pregnancy at the baseline and the self-reported date of delivery; these measures are also likely to be noisy.
[33] Specifically, we regress the dummy for the completion of pregnancy before the second follow-up survey on three treatment dummies, with strata fixed effects controlled for.

C). For a robustness check, we repeat all analyses excluding stratum 5. Unless treatment effects differ greatly between stratum 5 and the remaining nine strata, the results should be similar, which is what we find in all of the analyses below.

4.3. Main analysis

Table 4 shows the distributions of uptake sequence among baseline nontakers. In the control group, 38%, 49%, and 20% of women made uptakes 1, 2, and 12, respectively, and 67% made at least one antenatal visit;[34] thus, 33% were nonadopters and 29% (= 62% – 33%) were late adopters. For women at a relatively late stage of pregnancy, uptake 1 cannot be considered early adoption. In Section 5, we exclude them from the sample to better capture early adoption.

Table 5 reports the estimation results of equation (1) (columns 2, 5, and 9) (the results excluding stratum 5 are in Online Table A-6).[35] The incentive has positive effects on uptakes 1 and 12 (about .24 and .16 in probability, or 62% and 79% of the control means), but the effect on uptake 2 is small (.09) and not significantly different from 0. Although the channel test is statistically weak, these results are consistent with hastening, but not learning or shifting: The temporary incentive increased uptake 1 without decreasing uptake 2 among late adopters. Then, the sustainability test measures the degree of hastening. The incentive effects on uptake 2/uptake 12 are smaller than that on uptake 1, though the differences are not statistically significant. These results indicate that incentive compliers consisted mainly of both non-shifted late adopters and non-shifted nonadopters (about two thirds are the former).[36] Although both test results are weak, we show much stronger results consistent with these interpretations in the next section. As discussed above, the conventional analysis on uptakes 1 and 2 ignoring late adopters leads to very different interpretations.

The information alone has no significant impacts. The combined treatment has a positive effect on uptake 1 (about .18 in probability, or 47% of the control mean), but has no significant

[34] In the baseline sample 78% of women in the control group made at least one antenatal visit during the whole prenatal period. This rate is much higher than the average in the North East Zone, 43% in 2008 (National Population Commission and ICF Macro 2009). This is likely to be due to our oversampling of villages with a health facility, as well as heterogeneity across states.

[35] The results without strata fixed effects, i.e., treatment-control difference in means, are very similar (though some results are statistically weak, columns 1, 4, and 8), which indicates that treatment effects are not strongly heterogeneous across strata.

[36] Shifted/negatively learning late adopters should have been uncommon, because otherwise the estimated coefficient of the incentive for uptake 2 would be smaller than that for the combined treatment, the compliers to which consisted mainly of non-shifted nonadopters, as discussed shortly; the comparison is the opposite.

impacts on uptake 2 or 12 ($\gamma_{12} = \gamma_2 = 0$); the latter effects are significantly smaller than the former. As discussed above, these results suggest that combined-treatment compliers consisted mainly of non-shifted nonadopters. Although the additional information weakened the incentive effect on uptakes 1 and 12, only the latter result is statistically significant; the complementarity test indicates no complementarity for all outcomes. Thus, when the information with no individual effect was bundled, the incentive effect on uptake 1 decreased a little and that on uptake 12 mostly vanished. This is because the additional information altered compliers' response to the incentive: Non-shifted late adopters decreased and non-shifted nonadopters increased. This is not because the information weakened the persistent incentive effect, which was nonexistent.

To see whether these results are robust to the addition of covariates, we control for baseline trimesters (which determine the stage of pregnancy at period 1 and the duration of period 2), past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1 (columns 3, 6, and 10) (the estimation results for covariates are discussed in Online Appendix E).[37] To better capture the stage of pregnancy, we additionally control for the completion of pregnancy after the second follow-up survey for uptakes 2 and 12 (columns 7 and 11).[38] The results are very similar to those with no covariates (some test results become statistically stronger).[39]

5. Heterogeneity

This section conducts two heterogeneity analyses: one by the stage of pregnancy to characterize compliers and another by past experience.

5.1. Stage of pregnancy

Our sequential adoption model suggests that the later the stage of pregnancy at the baseline (shorter period 2), the more common are non-shifted nonadopters than non-shifted/shifted late adopters among compliers. To see whether this explains the composition of compliers found above, we repeat the analysis excluding women who had a short remaining

[37] Past adoption, which is relevant only among non-first-time pregnant women, is not controlled for. Access to a health facility is controlled for only through the availability of a village facility (strata fixed effects). Our data of self-reported distance to health facilities are incomplete and noisy.
[38] Although the timing of completion could potentially be affected by the treatments through antenatal care, the estimated effects are nonsignificant, as discussed above (Online Table A-5).
[39] The results for baseline takers are discussed in Online Appendix F. Among them, regardless of the information bundled, the cash incentive had no impacts on uptake 1. The estimated incentive effects on uptakes 1 and 12 are significantly different between baseline nontakers and takers.

prenatal period after the first follow-up survey in two ways: sample 1, which excludes those in the third trimester of pregnancy (15%), and sample 2, which excludes those who completed pregnancy before the second follow-up (36%) (the former excluded women are the latter's subset). Recall that the treatments are not correlated with these sample selections (Table 3 and Online Table A-5). Although uptake 1 in sample 1 mostly captures a first adoption made at the first or second trimester of pregnancy, thus better capturing early adoption, it contains women in the second trimester who completed pregnancy soon after the first follow-up. Although sample 2 excludes all with a short period 2, it also excludes women who had sufficient time for uptake 2 (those who completed a while later after the first follow-up).

Table 6 reports the results, which are similar without and with covariates.[40] Compared to the main results in Table 5, which are replicated as sample 0, the incentive effects on uptakes 1 and 12 increase, and the combined effects on uptake 1 decrease in sample 1. They are more so in sample 2; the incentive effects on uptake 12 are similar to those on uptake 1, and the combined effects on uptake 1 are not statistically significant and are less than half of the incentive effects.[41] Despite the small sample size, p-values for the channel and interaction tests are smaller than .15. Thus, although the earlier the stage of pregnancy, the stronger is the incentive effect on uptake 12 among late adopters as expected, when the information was bundled, early-stage women became less responsive. Instead, late-stage women with no time for uptake 2 became more responsive; that is, some nonadopters became late adopters by making a late adoption. The combined treatment effects on uptake 12 increase in samples 1 and 2, which is consistent with hastening among early-stage women who remained responsive to the incentive. Although these patterns explain the composition of compliers well, why the information had such heterogeneous interaction effects is a puzzle, to which we return in Section 6.

5.2. Past experience

Some women had received antenatal care in previous pregnancies (*past takers*), and others had not (*past nontakers*). Baseline nontakers consist of 177 past takers in 73 villages and

[40] The results with the completion of pregnancy controlled for not shown here are similar. Two women in stratum 5 are in samples 1 and 2. Online Table A-10 reports results excluding stratum 5.
[41] These results are not due to an unbalanced distribution of first and second trimesters at the baseline; they are balanced between the incentive and combined treatments in sample 0 (Online Table A-4) and samples 1 and 2 (not shown).

148 past nontakers, including 73 first-time pregnant women, in 73 villages.[42] We compare these two groups, because their scopes of hastening and learning are quite different; with the small number of observations, we cannot analyze first-time pregnant women separately, however. This inquiry is similar to that of Fischer et al. (2014), who compare health products with different scopes of learning and anchoring. We compare women with different experiences of the same product. First, past takers should have a stronger belief about antenatal care than non-first-time pregnant women with no past adoption (i.e., past nonadopters); in the first place, this is why the former made an adoption in the past.[43] Second, since antenatal care was a totally new product for past nontakers, they could potentially learn more from uptake 1 – the first adoption in their life – than past takers who were familiar with it. If our main analysis above masks learning by some, we expect to see it among past nontakers. In contrast, hastening should be stronger among past takers.[44]

The treatments are correlated with neither past pregnancy nor uptake (Online Table A-4), indicating that the selection into each group is not systematically related to the treatments. We augment equation (1) as follows:

$$\begin{aligned} Y_{ivt} = {} & \alpha_t^0 Incentive_v (1-q_{iv}) + \alpha_t^1 Incentive_v q_{iv} \\ & + \beta_t^0 Informatio n_v (1-q_{iv}) + \beta_t^1 Informatio n_v q_{iv} \\ & + \gamma_t^0 Combined_v (1-q_{iv}) + \gamma_t^1 Combined_v q_{iv} + \delta_t q_{iv} + \phi_s + u_{ivt} \end{aligned}, \qquad (2)$$

where q_{iv} is a dummy for past takers and other variables are the same as above.[45]

The results (without and with covariates) reveal a sharp contrast between the groups (Table 7).[46] Among past nontakers, the incentive and combined treatments had no significant impacts. Their estimated effects on uptake 1 are very close to zero with covariates. Estimation results for past takers are stronger than those in the original analysis: Incentive effects on uptakes 1 and 12 and the combined effect on uptake 1 are nearly twice the original ones (about .46, .29,

[42] Past takers were older and more commonly illiterate women in wealthy households than past nontakers (see Online Appendix G for details).
[43] This is confirmed by baseline knowledge and perceptions about antenatal care; past takers' belief was also stronger than first-time pregnant women (Online Table A-11).
[44] The comparison of the scope of shifting between the two groups is ambiguous.
[45] Antenatal visits in the current pregnancy are quite different from past actions: In the control group, 65% of past takers made at least one antenatal visit (the rest became nonadopters) and 71% of past nontakers did so (they were not nonadopters) (Online Table A-8 panels B and C). This comparison is not so surprising because it is among baseline nontakers and past nontakers include first-time pregnancy.
[46] The results with the completion of pregnancy additionally controlled for not shown here are similar. The wild cluster bootstrap results are similar (Online Table A-13).

and .30, respectively, in probability), and those on uptake 2 are small (.08) with no statistical significance.[47] The incentive effects on uptake 1/uptake 12 and the combined effect on uptake 1 are significantly different between the groups. Thus, the significant incentive and combined effects on uptake 1 found in the original analysis come mostly from past takers who consisted of about 54% of baseline nontakers. Distinct from past takers, the combined treatment did not increase nonadopters' late adoption among past nontakers.

This sharp contrast between the groups indicates much higher barriers to the adoption of the new product than the familiar one. This is bolstered by the fact that our study covered villages with a relatively good access to antenatal clinics. Although it is infeasible to pin down specific barriers, self-reported reasons for not making uptake 1 suggest that past non-takers had severer liquidity constraints than takers (see Online Appendix H).[48]

The test results among past takers are stronger than the original ones. First, the channel test is statistically significant, buttressing hastening as a unique channel. Second, the sustainability test for the incentive is statistically significant, bolstering the distinct composition of compliers. Third, the negative interaction effect of the information on uptake 1 is considerable (35% decrease at a 15% significance level). Fourth, the complementarity test indicates negative synergy for both uptakes 1 and 12. Thus, for the familiar product, although the temporary incentive strongly promoted hastening among late adopters, the additional information bundled nullified it; learning and shifting among nonadopters were limited. Since past nontakers did not respond to the incentive (i.e., no compliers), we cannot examine its potential sustainability through learning or shifting.[49]

6. Complier heterogeneity

This section examines why the information distinctly altered early- and late-stage women's responsiveness to the incentive. We conjecture potential reasons and provide

[47] Since the negative (positive) correlation of third trimester with the incentive among past nontakers (takers) is considerable compared to the control mean (though neither is statistically significant, Table 3), the estimated incentive effects on uptakes 2 and 12 could be biased upward (downward). Interpreting these results thus requires caution.

[48] This is consistent with the earlier finding that past nontakers were poorer than takers.

[49] Among past nontakers, the estimated effects of the information alone on uptake 1 are negative with considerable magnitude with no statistical significance; those on uptake 2 are small. These results suggest that the information may have delayed adoption sequence. This might be because the information about recommended visits gave women an *excuse* for delaying their first visit (for example, due to overconfidence). With the incentive bundled, this effect vanished maybe because money weakened their attention to the information. Similar patterns are found for baseline takers: This information might have served as an excuse for not making uptake 2 (Online Appendix F).

suggestive evidence. Following Angrist and Pischke (2009, pp166-172), we examine the mean characteristics of incentive and combined-treatment compliers among baseline nontakers and past takers (Online Appendix I details procedures). We assume the absence of defiers who are induced not to make uptake 1 by these two treatments. Stratum 5, with imperfect compliance in treatment assignments, is excluded. Recall that most compliers were past takers.

We first confirm that combined-treatment compliers were at a later stage of pregnancy at the baseline and completed pregnancy earlier than incentive compliers (the former were at a later stage and completed earlier than the sample mean; the converse holds true for the latter) (Table 8). In particular, combined-treatment compliers in the third trimester were much more common.[50]

We highlight the potential role of concrete information of recommended antenatal visits (at least four).[51] This information could have affected early-stage women in distinct ways: Although those who thought they could make sufficient visits were not affected, others stopped responding to the incentive because they negatively updated their belief about benefits from insufficient visits; they might have been simply overwhelmed by the recommended practice. Then, the information gave them an *excuse* not to respond to the incentive.[52] This excuse did not matter for late-stage women, because it was too late for them to make sufficient visits. The information about antenatal care might instead have served as a *reminder* for late-stage, but not early-stage, women precisely because their remaining prenatal period was limited (close to the endpoint), which is consistent with present bias.[53] Neither the excuse nor the reminder of the information alone was salient; only with the incentive bundled did both become salient. This is consistent with the findings of Karlan et al. (2016), who show that the effectiveness of reminders

[50] Among baseline nontakers, this difference is opposite to the comparison of third trimester between the incentive and combined treatment groups (Table 3). Among past takers, the huge difference (10 times) cannot be explained solely by the imbalance between the treatment groups (about two times).

[51] This very simple information appearing in boldface in the second sentence of the script (Online Appendix A) could have been more salient than other information given, especially for illiterate women. Evidence for the salience of this information is found for past nontakers and baseline takers: It might have served as an excuse for delaying and reducing adoptions, as discussed above. Although this information might not be new for past takers, many of them might had forgotten it.

[52] Then, an incentive conditional on multiple visits might less promote a first visit among nonadopters who are unlikely to make sufficient visits than the incentive conditional on one visit. At the same time, the incentive may not have increased sufficient visits because those who were unlikely to make sufficient visits also responded to the incentive.

[53] Ericson (forthcoming) theoretically shows that the optimal time to provide surprise one-shot reminders to present-biased individuals is much later than that for time-consistent individuals; in his simulation, it is in the very last period before the deadline.

for present-biased individuals depends on their salience. Without the incentive, an excuse not to respond to it is irrelevant.

As such, the incentive and information had negative synergy among early-stage women and complementarity among late-stage women. The former was stronger than the latter, because the former women were much more common than the latter.[54] As a result, the information weakened the incentive effect on uptake 1, especially among past takers. The excuse and reminder were strong among past takers, probably because their expectation about antenatal visits was based on past experience (learning), and it reminded them of their past visits. This explains why the complementary reminder did not increase late adoption among past nontakers, which suggests the limitation of reminders for the new product.

Both the excuse and reminder suggest that combined-treatment compliers had more experience of antenatal visits than incentive compliers; the excuse also suggests that the former would be more likely to make recommended visits than the latter. The comparison of past experience provides supportive evidence:[55] Combined-treatment compliers had greater parity and more commonly made four visits in the last pregnancy than incentive compliers (Table 8). Other characteristics are similar between them, especially among past takers. This indirectly supports the excuse and reminder as a driving force, because if other forces had mainly driven the contrast between the two compliers, they could have been related with some of those characteristics.

7. Conclusion

This paper studied sequential adoptions through stages with an endpoint, which are common in maternal and child health care. Incentivizing first adoption can sustainably promote sequential adoptions in three ways: 1) nonadopters positively update their belief about the product, 2) nonadopters shift their dynamic decision from the status quo, and 3) late adopters hasten their adoption sequence with no substitution. Conventional analysis ignoring late adopters,

[54] This is confirmed by complier size, measured in proportions in each treatment group and the control group (Online Table A-15; Online Appendix I details procedures). Among both baseline takers and past takers, compliers were more common in the incentive group than in the combined treatment group (.51 vs. .30 among past takers). This is so regardless whether compliers made uptake 1. In both the incentive and combined groups, compliers were more common among past takers than baseline takers, which include past nontakers. Although early-stage women in the combined treatment group who remained responsive to the incentive are likely to have made uptake 2, as found in sample 2 in Section 5.1, they were not common.

[55] Whether baseline nontakers experienced pregnancy/antenatal visits in the past is not useful for this comparison, because all compliers – both incentive and combined – were non-first-time pregnant women and past takers. This confirms that most compliers were past takers.

and thus hastening, could be wrong and misleading if hastening actually matters. Our simple framework can be widely applied to sequential adoptions in health and other domains, regardless whether a dynamic decision is made. It can also jointly examine the sustainability and complementarity of interventions.

We conducted an antenatal care experiment in villages with relatively good access to clinics in rural northeastern Nigeria. Small cash incentive promoted hastening, but not learning or shifting. The earlier the stage of pregnancy, the stronger was the hastening. It was significant only among women with past adoption experience. Information intervention with no individual effect nullified the hastening: Early-stage women became less responsive, and late-stage women became more responsive. This heterogeneity might be due to excuse and reminder. These findings suggest using temporary incentive schemes for familiar products according to individuals' adoption stage: 1) an early intervention without providing potential excuse, to sustainably promote late adopters' early adoption; and 2) a late intervention with a reminder for nonadopters' late adoption.

Small cash was not enough to incentivize a first adoption in life among women who had not made antenatal visits before, including first-time pregnancy. Whether their sequential adoptions can be sustainably promoted through learning or shifting is a remaining question. An empirical framework to distinguish learning and shifting is also needed.

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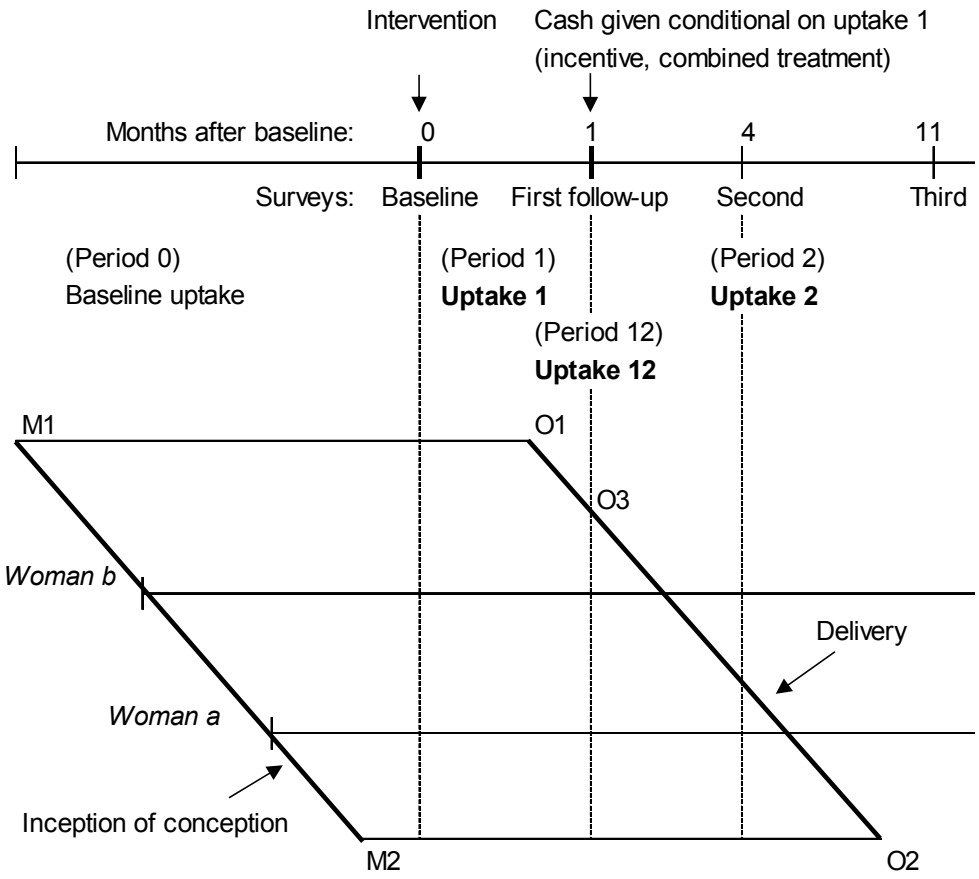
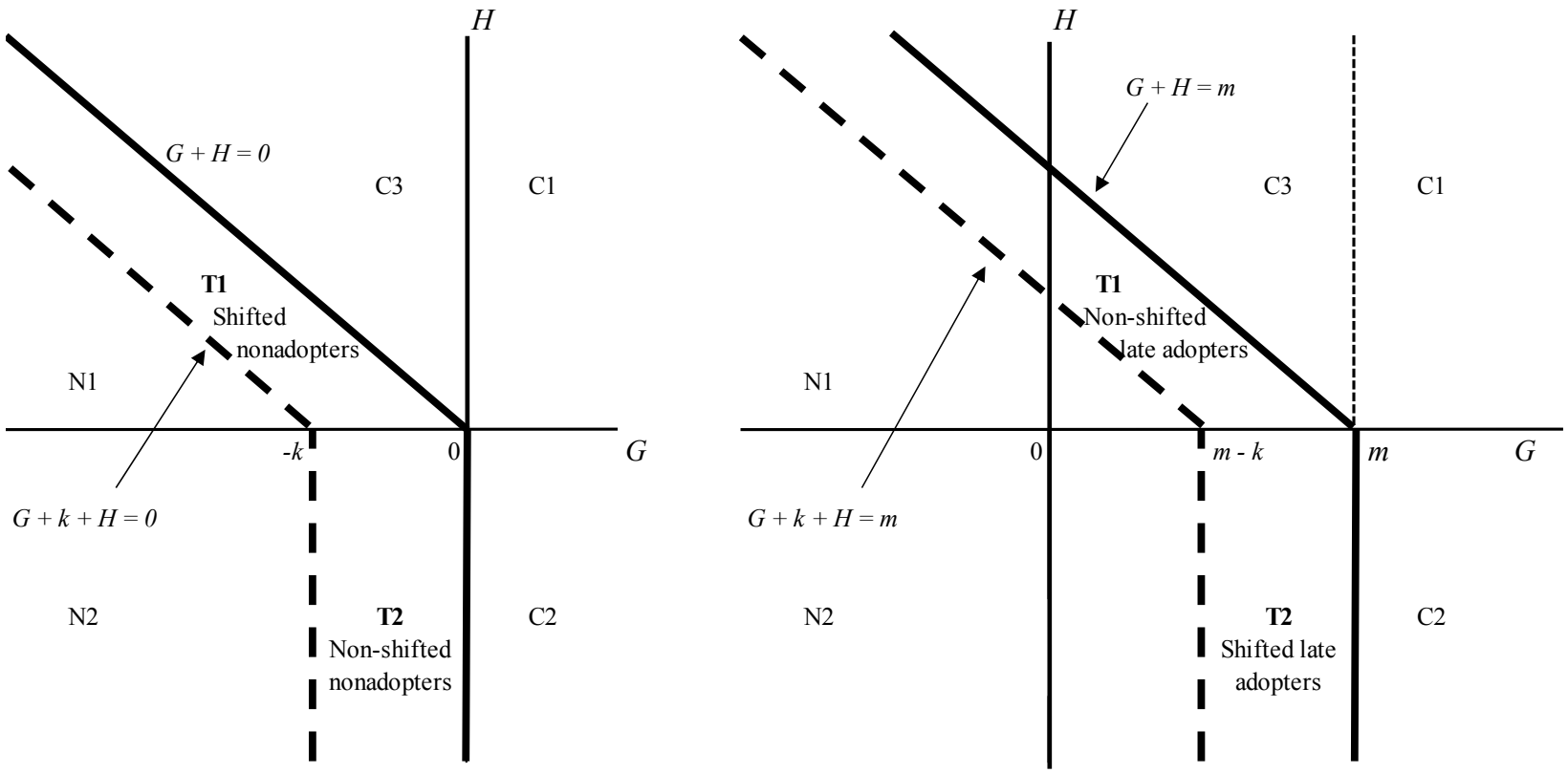
Figure 1. Experimental and maternal timelines



Note: Uptake 1 is at least one antenatal visit at period 1, uptake 2 is at least one antenatal visit at period 2, and uptake 12 is at least one antenatal visit at both periods 1 and 2.

Figure 2. A dynamic model of sequential adoptions



Notes: $G = B_1(1) + B_2(1,0)$, $H = B_2(1,1) - B_2(1,0)$, and $m = B_2(0,1)$, where $B_1(d_1)$ and $B_2(d_1, d_2)$, respectively, denote the perceived net benefits at periods 1 and 2, d_1 is a dummy for uptake 1, and d_2 is a dummy for uptake 2. G is the net benefit from uptake 1 alone, H is the additional net benefit from making an uptake 2, and m is the net benefit from making a late adoption. k is cash incentive.

Table 1. Baseline balance

	No. obs.	Control mean	Incentive - Control	Information - Control	Combined - Control	Joint sig. F (p-value)
Stage of pregnancy at baseline (means among women)						
First trimester	100	0.208	-0.0202	0.00642	-0.0478	0.575
Second trimester	100	0.528	-0.00146	0.0338	-0.0484	0.348
Third trimester	100	0.264	0.0217	-0.0402	0.0962*	0.019
Baseline outcomes (means among women)						
Baseline uptake	100	0.560	-0.0107	-0.0663	0.00787	0.713
Past pregnancy	100	0.809	-0.0507	-0.0335	-0.0214	0.668
Parity	100	2.504	-0.382	-0.180	-0.132	0.521
Any uptake in past pregnancies[a]	100	0.757	0.000218	0.0877	-0.000351	0.580
Any uptake in last pregnancy[a]	100	0.729	-0.0239	0.0417	-0.00857	0.886
At least 4 uptakes in last pregnancy[a]	100	0.462	-0.0319	0.0498	0.0569	0.717
Woman/household characteristics (means among women)						
Age of woman	100	27.03	-1.878*	-0.533	-0.873	0.028
Literate woman	100	0.377	0.0142	-0.0194	-0.0544	0.766
Household size	100	5.132	-0.389	0.165	0.0427	0.541
Children at age 5 or below	100	0.697	-0.0148	-0.0485	-0.0540	0.648
Asset index (z-score)	100	-0.0932	0.0886	0.158	0.0779	0.787
Health facility characteristics						
Free antenatal care	100	0.778	-0.111	-0.153	-0.0578	0.670
Family planning	100	0.815	-0.231	-0.0231	-0.0548	0.243
HIV test	100	0.444	-0.236	-0.153	-0.0844	0.333
Village characteristics						
No. households (log)	98	5.280	-0.280	-0.353	-0.171	0.674
Tapped water or piped/tubed well	97	0.308	0.0256	-0.00334	0.151	0.652
Any toilet	96	0.654	0.0128	0.0280	0.179	0.492

Notes: The table reports the difference in each variable between each of the three treatment groups and the control group. The joint signficance of the difference for the three treatment groups is tested. Among 100 villages, 27 are in the control group, 24 are in the incentive treatment, 24 are in the information treament, and 25 are in the combined treatment. [a]Among non-first-time pregnant women. *p<0.05

Table 2. Potential outcomes among compliers to incentive by channel

Channel:		Shifting		Positive learning		Hastening		Negative learning	
Uptake:	d_1	d_2	d_{12}	d_2	d_{12}	d_2	d_{12}	d_2	d_{12}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Nonadopters									
Control	0	0	0	0	0	0	0	0	0
Incentive	1	1	1	1	1	0	0	0	0
Incentive - Control	1	1	1	1	1	0	0	0	0
B. Late adopters									
Control	0	1	0	1	0	1	0	1	0
Incentive	1	0	0	1	1	1	1	0	0
Incentive - Control	1	-1	0	0	1	0	1	-1	0

d_1: uptake 1, d_2: uptake 2, d_{12}: uptake 12; 0: no uptake, 1: uptake

Table 3. Baseline balance of third trimester of pregnancy

Sample	No. obs.	Control mean	Incentive - Control	Information - Control	Combined - Control	Joint sig. F (p-value)
Baseline sample	100	0.264	0.0217	-0.0402	0.0962*	0.019
Analysis sample	99	0.165	0.0761	0.0167	0.118*	0.041
Baseline takers	92	0.186	0.104	0.0797	0.209*	0.068
Baseline nontakers	93	0.149	0.0405	-0.0277	-0.0269	0.674
Past takers	73	0.145	-0.0642	0.0526	0.0357	0.597
Past nontakers	73	0.133	0.0600	0.0382	0.00208	0.918

Notes: The table reports the difference in third trimester of pregnancy at the baseline (means among women) between each of the three treatment groups and the control group. The joint signficance of the difference for the three treatment groups is tested. In baseline sample, the results reported in Table 1 are replicated. See Online Table A-1 for the number of observations of each group in each sample. *p<0.05

Table 4. Antenatal care uptake sequence

	All		Control		Incentive		Information		Combined	
	Y_1	Y_2	Y_1	Y_2	Y_1	Y_2	Y_1	Y_2	Y_1	Y_2
$Y_1=Y_2=0$		0.28		0.33		0.18		0.37		0.25
$Y_1=0$	0.52		0.62		0.37		0.63		0.47	
$Y_1=0,\ Y_2=1$		0.24		0.29		0.19		0.26		0.22
$Y_1=1,\ Y_2=0$		0.24		0.18		0.27		0.20		0.32
$Y_1=1$	0.48		0.38		0.63		0.37		0.53	
$Y_1=Y_2=1$		0.23		0.20		0.36		0.17		0.21
No. observations	332		79		84		84		85	

Notes: The sample is baseline nontakers in the analysis sample. Proportions for each squence up to the corresponding period are shown. Y_1: uptake 1, Y_2: uptake 2 (0: no uptake, 1: uptake)

Table 5. Treatment effects on antenatal care uptake

	Uptake 1			Uptake 2				Uptake 12			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Incentive (α_t)	0.251*	0.235*	0.269*	0.0539	0.0897	0.110	0.0972	0.155	0.156*	0.184*	0.179*
	(0.101)	(0.0882)	(0.0923)	(0.0909)	(0.0842)	(0.0771)	(0.0635)	(0.0877)	(0.0738)	(0.0772)	(0.0748)
	0.02	*0.01*	*0.01*	*0.59*	*0.34*	*0.21*	*0.14*	*0.10*	*0.06*	*0.04*	*0.04*
Information (β_t)	-0.0107	-0.0103	0.00784	-0.0651	-0.0250	-0.0386	-0.0557	-0.0359	-0.0206	-0.00989	-0.0167
	(0.0842)	(0.0797)	(0.0878)	(0.0921)	(0.0910)	(0.0840)	(0.0768)	(0.0697)	(0.0734)	(0.0718)	(0.0716)
Combined (γ_t)	0.150	0.179*	0.208*	-0.0584	-0.0125	-0.0183	-0.00788	0.00923	0.0423	0.0596	0.0637
	(0.107)	(0.0887)	(0.0886)	(0.0959)	(0.0847)	(0.0837)	(0.0702)	(0.0693)	(0.0597)	(0.0612)	(0.0596)
	0.19	*0.08*	*0.04*	*0.55*	*0.89*	*0.82*	*0.92*	*0.91*	*0.51*	*0.34*	*0.29*
Strata fixed effects	No	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Pregnancy completion	No	No	No	No	No	No	Yes	No	No	No	Yes
No. observations	332	332	321	332	332	321	321	332	332	321	321
R squared	0.048	0.132	0.186	0.009	0.068	0.184	0.331	0.030	0.095	0.145	0.177
Control mean	0.380	0.380	0.372	0.494	0.494	0.500	0.500	0.203	0.203	0.205	0.205
Channel - Chi-squared (p-value):											
Incentive ($\alpha_2 = \alpha_{t2}$)								0.20	0.38	0.30	0.23
Combined ($\gamma_2 = \gamma_{12}$)								0.39	0.46	0.27	0.26
Sustainability - Chi-squared (p-value):											
Incentive ($\alpha_1 = \alpha_t$)				0.12	0.23	0.13	0.08	0.18	0.26	0.20	0.17
Combined ($\gamma_1 = \gamma_t$)				0.11	0.08	0.03	0.02	0.10	0.07	0.04	0.04
Interaction/complementarity - F (p-value):											
$\alpha_t = \gamma_t$	0.36	0.53	0.49	0.27	0.25	0.11	0.12	0.09	0.08	0.05	0.06
$\alpha_t + \beta_t = \gamma_t$	0.52	0.71	0.59	0.73	0.54	0.43	0.62	0.32	0.34	0.23	0.29

Notes: The sample is baseline nontakers in the analysis sample. Robust standard errors clustered by village are shown in parentheses, below which wild cluster bootstrap p-vaues are shown in italics for incentive and combined treatment. The number of clusters is 93. Covariates are second and third trimesters of pregnancy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. A dummy for completion of pregnancy after second follow-up survey is also controlled for in columns (7) and (11). *p<0.05

Table 6. Heterogeneity by the stage of pregnancy

	Uptake 1		Uptake 2		Uptake 12	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive (α_t)						
Sample 0	0.235*	0.269*	0.0897	0.110	0.156*	0.184*
	(0.0882)	(0.0923)	(0.0842)	(0.0771)	(0.0738)	(0.0772)
Sample 1	0.270*	0.306*	0.100	0.156	0.203*	0.255*
	(0.0942)	(0.103)	(0.0887)	(0.0815)	(0.0833)	(0.0870)
Sample 2	0.283*	0.314*	0.103	0.135	0.239*	0.288*
	(0.104)	(0.116)	(0.0947)	(0.0847)	(0.111)	(0.113)
Combined (γ_t)						
Sample 0	0.179*	0.208*	-0.0125	-0.0183	0.0423	0.0596
	(0.0887)	(0.0886)	(0.0847)	(0.0837)	(0.0597)	(0.0612)
Sample 1	0.165	0.193*	-0.0256	-0.0213	0.0593	0.0800
	(0.0858)	(0.0895)	(0.0914)	(0.0930)	(0.0641)	(0.0674)
Sample 2	0.127	0.147	0.00535	-0.00560	0.0787	0.0996
	(0.0949)	(0.0991)	(0.0998)	(0.100)	(0.0835)	(0.0863)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes
Test (p-value)	*Interaction*		*Channel*		*Sustainability*	
	F: $\alpha_1 = \gamma_1$		*Chi-sq.:* $\alpha_2 = \alpha_{12}$		*Chi-sq.:* $\alpha_1 = \alpha_{12}$	
Sample 0	0.53	0.49	0.38	0.30	0.26	0.20
Sample 1	0.26	0.23	0.23	0.25	0.35	0.47
Sample 2	0.13	0.12	0.21	0.14	0.52	0.71

Notes: Sample 0 is baseline nontakers in the analysis sample (n=332 with no covariates, n=321 with covariates; 93 clusters). Sample 1 excludes women in the third trimester of pregnancy at the baseline (n=281 with no covariates, n=272 with covariates; 92 clusters). Sample 2 excludes women who completed pregnancy before the second follow-up survey (n=214 with no covariates, n=210 with covariates; 88 clusters). Robust standard errors clustered by village are shown in parentheses. Covariates are second and third trimesters of pregnancy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. The results for the information alone are not reported and only selected test results are shown for brevity. *p<0.05

Table 7. Heterogeneity by past experience

	Uptake 1		Uptake 2		Uptake 12	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive * Past nontakers (α^0_t)	-0.0320	-0.00479	0.108	0.0756	0.0217	0.0139
	(0.123)	(0.123)	(0.108)	(0.0973)	(0.0856)	(0.0859)
Incentive * Past takers (α^1_t)	0.464*	0.513*	0.0782	0.141	0.287*	0.356*
	(0.110)	(0.109)	(0.111)	(0.111)	(0.102)	(0.100)
	0.00	*0.00*	*0.52*	*0.27*	*0.02*	*0.00*
Information * Past nontakers (β^0_t)	-0.227	-0.201	-0.0110	-0.0827	-0.0646	-0.0612
	(0.122)	(0.126)	(0.133)	(0.118)	(0.101)	(0.105)
Information * Past takers (β^1_t)	0.106	0.116	-0.0676	-0.0427	-0.0173	0.0124
	(0.0959)	(0.107)	(0.121)	(0.117)	(0.0888)	(0.0898)
Combined * Past nontakers (γ^0_t)	0.0000500	0.0197	0.0751	0.0533	0.0271	0.0339
	(0.132)	(0.128)	(0.118)	(0.113)	(0.0842)	(0.0865)
Combined * Past takers (γ^1_t)	0.303*	0.349*	-0.140	-0.131	0.0187	0.0594
	(0.105)	(0.107)	(0.105)	(0.116)	(0.0695)	(0.0790)
	0.01	*0.01*	*0.20*	*0.29*	*0.78*	*0.48*
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Past takers	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes
No. observations	325	314	325	314	325	314
R squared	0.170	0.219	0.085	0.197	0.122	0.172
Control mean - past nontakers	0.500	0.486	0.421	0.432	0.211	0.216
Control mean - past takers	0.275	0.275	0.575	0.575	0.200	0.200
Channel - Chi-squared (p-value):						
Incentive * Past takers ($\alpha^1_2 = \alpha^1_{12}$)					0.03	0.02
Combined * Past takers ($\gamma^1_2 = \gamma^1_{12}$)					0.12	0.06
Sustainability - Chi-squared (p-value):						
Incentive * Past takers ($\alpha^1_1 = \alpha^1_t$)			0.01	0.01	0.08	0.09
Combined * Past takers ($\gamma^1_1 = \gamma^1_t$)			0.00	0.00	0.00	0.00
Interaction/complementarity - F (p-value)						
Past takers ($\alpha^1_t = \gamma^1_t$)	0.15	0.12	0.06	0.02	0.01	0.00
Past takers ($\alpha^1_t + \beta^1_t = \gamma^1_t$)	0.08	0.07	0.35	0.14	0.05	0.02
Past nontakers = Past takers - F (p-value):						
Incentive ($\alpha^0_t = \alpha^1_t$)	0.00	0.00	0.83	0.64	0.03	0.00
Combined ($\gamma^0_t = \gamma^1_t$)	0.07	0.04	0.13	0.23	0.94	0.83

Notes: The sample is baseline nontakers in the analysis sample. Robust standard errors clustered by village are shown in parentheses, below which wild cluster bootstrap p-vaues are shown in italics for incentive and combined treatment among past takers. The number of clusters is 92 (73 both among past nontakers and takers). A dummy for past takers is always controlled for. Other covariates are second and third trimesters of pregnancy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. *p<0.05

Table 8. Mean characteristics of compliers

	Baseline nontakers						Past takers among baseline nontakers					
	All	Incentive compliers		Combined-treatment compliers			All	Incentive compliers		Combined-treatment compliers		
			(2)/(1)		(4)/(1)	(4)/(2)			(8)/(7)		(10)/(7)	(10)/(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Pregnancy stage at baseline and completion of pregnancy												
First trimester	0.35	0.48	*1.38*	0.21	*0.61*	*0.44*	0.32	0.43	*1.32*	0.24	*0.74*	*0.56*
Second trimester	0.49	0.45	*0.92*	0.49	*1.01*	*1.10*	0.53	0.51	*0.96*	0.39	*0.73*	*0.76*
Third trimester	0.16	0.08	*0.49*	0.27	*1.65*	*3.39*	0.15	0.03	*0.20*	0.29	*1.99*	*10.06*
Completion before second follow-up	0.36	0.21	*0.58*	0.56	*1.56*	*2.66*	0.37	0.27	*0.72*	0.51	*1.38*	*1.91*
Past experience												
Parity	2.48	3.59	*1.45*	4.65	*1.88*	*1.30*	3.42	3.46	*1.01*	4.71	*1.38*	*1.36*
At least four uptakes[a]	0.42	0.44	*1.03*	0.69	*1.63*	*1.58*	0.67	0.36	*0.54*	0.65	*0.97*	*1.80*
Woman/household/health facility characteristics												
Age of woman	26.3	26.5	*1.01*	32.0	*1.22*	*1.20*	28.2	26.1	*0.93*	32.6	*1.16*	*1.25*
Literate woman	0.32	0.26	*0.82*	0.13	*0.41*	*0.50*	0.24	0.27	*1.15*	0.26	*1.11*	*0.96*
Household size	5.34	6.47	*1.21*	7.70	*1.44*	*1.19*	6.48	6.20	*0.96*	7.06	*1.09*	*1.14*
Children at age 5 or below	0.65	0.84	*1.29*	0.90	*1.38*	*1.07*	0.84	0.97	*1.15*	0.83	*0.99*	*0.86*
Asset index (z-score)	-0.01	-0.05		0.07			0.10	-0.16		0.33		
Free antenatal care	0.65	0.72	*1.11*	0.38	*0.59*	*0.53*	0.68	0.76	*1.12*	0.67	*0.98*	*0.87*
Family planning	0.78	0.71	*0.91*	0.93	*1.19*	*1.31*	0.73	0.69	*0.94*	0.66	*0.90*	*0.96*
HIV test	0.36	0.49	*1.36*	0.65	*1.82*	*1.34*	0.34	0.41	*1.22*	0.36	*1.05*	*0.86*
Maximum no. observations	307						165					

Notes: The sample is the analysis sample excluding stratum 5 (defined in Online Table A-1). See Online Appendix I for procedures.

[a]Among non-first-time pregnant women with non-missing information in last pregnancy: n=180 in column (1) and 114 in column (7).

For Online Publication

Short-run incentive and information in sequential adoptions: An antenatal care experiment in rural Nigeria

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September 26, 2017

Contents

Appendix A. Information script

Antenatal care refers to the medical care recommended for women during pregnancy. **At least four antenatal care visits are required**. The aim of antenatal care is to detect any potential problems early, to prevent them if possible, and to direct the pregnant woman to appropriate specialists, hospitals, etc. if necessary. **Why do you need antenatal care?** It is because antenatal care can help keep you and your baby healthy. The routine antenatal care can reduce maternal mortality and miscarriage as well as birth defects and low birth weight. A survey has shown that babies of mothers who did not receive antenatal care are three times more likely to have low birth weight and five times more likely to die than those born to mothers who did receive care. In Nigeria, one in every five children never reaches age 5, but die before that. One of the main reasons is that women do not receive antenatal care.

Appendix B. Comparison of channels in sequential adoptions

This appendix compares the magnitude of shifting and hastening in the dynamic sequential adoption model in Section 3. We assume women's homogeneous distributions in the *G-H* space between panels A and B in Figure 2. Then, we can make the following comparisons of shifted nonadopters and non-shifted late adopters in the incentive group in the absence of learning. First, non-shifted late adopters are more common than shifted nonadopters (area T1 is larger in panel B than panel A), and this is more so with greater *m* (area T1 in panel B increases). Thus, the incentive effect on uptake 12 is more strongly driven by hastening than shifting, and this is more so with the stronger belief about antenatal care among late adopters. Second, augmenting the amount of cash incentive *k* increases non-shifted late adopters more than it increases shifted nonadopters (area T1 in panel B increases more than area T1 in panel A). Thus, augmenting the incentive strengthens hastening more than shifting. Similar comparisons do not hold between non-shifted nonadopters and shifted late adopters (area T2).

Appendix C. Information and combined treatments in sequential adoptions

This appendix discusses information and combined treatments in the dynamic sequential adoption model in Section 3. With the information received, a woman can update her prior belief about the net benefits at periods 1 and 2, including m. If the sign of m does not change, the positive (negative) information effect on uptake 1 means that the solid bold/broken line shifts leftward (rightward) in the information/combined treatment group in both panels A and B of Figure 2; if the value of positive m decreases (increases), the same change occurs in panel B. If the sign of m changes, a woman shifts between panel A and panel B. In these ways, the composition of compliers can change.

As such, regardless whether the information alone alters uptake 1, it can affect uptake 2. The information may directly affect uptake 2 without altering uptake 1 if it takes more than one month for the information effect to materialize. Regardless whether the information alters the incentive effect on uptake 1 in the combined treatment group, it can affect the incentive effect on uptake 2. In both the information and combined treatment groups, the positive (negative) information effect on uptake 2 means that $H = 0$ shifts downward (upward), which is the same as positive (negative) learning through uptake 1 discussed in the text.

Table 2 indicates how the additional information can alter the incentive effects on uptakes 2 and 12 depending on channels. Suppose that m does not change. First, if only shifting or positive learning matters, the positive (negative) information effect strengthens (weakens) the incentive effects on uptakes 2 and 12 among nonadopters. In addition, the information may also affect late adopters: The positive information effect increases uptake 12 without altering uptake 2, and the negative information effect decreases uptake 2 without altering uptake 12. Next, if only hastening matters, while the positive (negative) information effect strengthens (weakens) the incentive effect on uptake 12 among late adopters, the positive information effect does not alter the incentive effect on uptake 2 and the negative information effect additionally decreases uptake 2. In addition, the information may also affect nonadopters: The positive (negative) information effect increases (decreases) both uptakes 2 and 12. If the value of positive m and/or the sign of m change, these changes occur based on updated m.

Appendix D. A static model of sequential adoptions

This appendix constructs a static model of sequential adoptions as a constrained version of the dynamic model developed in Section 3. The net benefit at period 2 is independent of uptake 1 and determined solely by uptake 2, i.e., $B_2(d_2)$, where $B_2(0) = 0$. Then, $B_{12}(d_1,d_2) = B_1(d_1) + B_2(d_2)$, $G = B_{12}(1,0) = B_1(1) + B_2(0) = B_1(1)$, and $H = B_2(1) - B_2(0) = B_2(1) = B_{12}(0,1) = m$. That is, G and H are redefined as the net benefits from uptake 1 at period 1 and uptake 2 at period 2, respectively, which are equal to the net benefits from early adoption alone and late adoption alone.

Consider a woman with a long period 2 and no learning from uptake 1. Her prior belief about G and D determines her decisions, as depicted in Figure A-1. She makes an uptake decision at each period independently. In the control group, she makes uptake 1 if $G > 0$ (areas C1 and C2) and uptake 2 if $H > 0$ (areas C1 and C3). Women in area C1 make uptake 12. Those in areas C3 and N are late adopters and nonadopters, respectively.

Compared to the control group, uptake 1 is additionally made in response to the incentive among those in areas T1 and T2 (compliers). Those in area T1 make uptake 2, and those in area T2 do not as in the control group. Uptake 12 is additionally made by those in area T1. Thus, no women shift their decision about uptake 2 after making uptake 1. In the dynamic model (Figure 1), women in area T2 are the same as non-shifted nonadopters, and those in area T1 are the same as non-shifted late adopters.

Thus, all discussions regarding the dynamic model in the text can apply to the static model, except that the shifting channel is irrelevant and shifted nonadopters and shifted late adopters are nonexistent. In particular, with no learning from uptake 1, the incentive effects on uptake 12 are driven solely by hastening among late adopters (area T1).

Appendix E. Estimation results for covariates

This appendix discusses the estimation results for covariates in the main analysis in Table 5. The results are reported in Table A-7. Although women in the second and third trimesters at the baseline more commonly made uptake 1 than those in the first trimester (i.e., slow adoption is still evident at period 1), they less commonly made uptake 2 with the shorter period 2; in particular, women in the third trimester were less likely to have made uptake 2 by .46 in probability. As these two forces on uptakes 1 and 2 counteract each other, uptake 12 is neutral to baseline trimesters. When the completion of pregnancy is additionally controlled for, women who completed after the second follow-up survey more commonly made uptakes 2 and 12 than those who completed beforehand by over .5 and .2, respectively, in probability, and baseline trimesters no longer significantly affect uptake 2 (all women in the third trimester completed pregnancy before the second follow-up). Almost no other covariates are significant for any outcomes.

Appendix F. Baseline takers

This appendix analyses baseline takers in the analysis sample. The distribution of the uptake sequence is shown in panel A of Table A-8. In the control group, 81%, 46%, and 36% of women made uptakes 1, 2, and 12, respectively. The estimation results of equation (1) are reported in Table A-9. Since no treatments significantly increase uptake 1, the channel and sustainability tests do not have the interpretations discussed in Section 4.1.

Regardless of the information bundled, the cash incentive has no significant impacts on uptakes 1 and 12. The significant combined treatment effect without baseline trimesters controlled for is likely due to the imbalance of the third trimester (Table 2). For women who made an adoption in the current pregnancy just before the baseline survey, the small incentive conditional on another adoption within a month was ineffective. The result for uptake 12 is a mechanical one, as discussed in Section 4.1 (note 23). Although the incentive alone has no significant impacts on uptake 2, after the incentive vanished, the combined treatment has a negative effect on uptake 2, even with baseline trimesters controlled for (.14 in probability in magnitude, or about 31% of the control mean), which is due to the information effect discussed next. Since the positive correlation of third trimester and the combined treatment (Table 2) leads to downward bias, interpreting this result requires caution.

Although the information alone has no significant impact on uptake 1, it decreases uptakes 2 and 12 with a greater magnitude for the former (about .23 and .17 in probability in magnitude, or about 50% and 47% of the control means, with no covariates). This might be because the information about recommended antenatal visits (at least four) gave women an *excuse* not to make further visits. This excuse is not significant for uptake 1, because many women had not yet made sufficient visits. With the incentive bundled, the negative information effects were weakened (the difference is not significantly different from 0), though the effect on uptake 2 is still significant (with baseline trimesters controlled for). This interaction effect of the incentive might be because money weakened women's attention to the information. This is supported by the complementarity of the two interventions for uptake 1 indicated by the complementarity test.

Appendix G. Past experience

This appendix discusses the correlates of past experience. We regress past pregnancy, at least one antenatal visit in past pregnancies (*past uptake*) among non-first-time pregnant women, and past taker status on the woman/household/health facility characteristics reported in Table 1 with strata fixed effects controlled for (standard errors are clustered by village). We exclude household size and the presence of a young child, which can be an outcome of past pregnancy, as well as parity, which is mechanically determined by past pregnancy. Estimation results in the baseline, whole analysis, and baseline-nontakers analysis samples are reported in Table A-12. First, fertility is correlated with demographics and human capital: Past pregnancy was more common among older and illiterate women (columns 1, 5, and 9). Second, past uptake is correlated with wealth: Past uptake was more common among women belonging to households with greater assets (columns 2, 6, and 10); it is positively correlated with parity when it is added as an additional control (columns 3, 7, and 11). Distinct from baseline uptake (Table A-2), literacy is not significantly positively correlated with past uptake except for the baseline sample. These findings are largely consistent with extant works in the literature (see Simkhada, et al. 2008 for a review). Third, combining these two sets of results, compared to past nontakers, past takers were more common among old and illiterate women in wealthy households (the literacy result in the baseline sample is statistically weak) (columns 4, 8, and 12). All heath facility characteristics are nonsignificant across outcomes. None of the strata fixed effects are significant across outcomes, though they are jointly significant in some cases in the baseline and whole analysis samples.

Appendix H. Reasons for non-uptake

This appendix discusses self-reported reasons for not making baseline uptake and uptake 1 (Table A-14). Among baseline nontakers, reasons are similar between baseline uptake and uptake 1: High cost was the most common reason, and nonnecessity (they thought antenatal care was unnecessary) was the second most common (about 50% and 20% for uptake 1, respectively), followed by other unspecified reason. Long distance and lack of services were uncommon reasons, suggesting that supply-side factors are not a major barrier to antenatal visits. This should be partly because of our sampling design, which oversampled villages with health facilities. Religious reasons were nonexistent. Similar patterns are found for past nontakers and takers.

Similar patterns are also observed across treatment groups with the following exceptions only for uptake 1 (with the small number of observations in each treatment arm, interpreting this comparison requires caution). High cost was a less common reason and nonnecessity was a more common reason in all three treatment groups than the control group among past takers, but not nontakers. These results suggest that although a small amount of cash relaxed the liquidity constraint among some past takers and those who still did not make uptake 1 were ones with weak beliefs about antenatal care, the constraint was not relaxed among past nontakers.

Appendix I. Counting and characterizing compliers

This appendix discusses how we can obtain the size and the distribution of compliers' characteristics, following Angrist and Pischke (2009).

Notations and assumptions

For clarity, we use the same notations as those used in the LATE framework by Angrist and Pischke (2009, pp166-172). Let Z_i denote the offer of treatment for individual i, the assignment of which is random. Let D_i denote individual i's treatment status: treated ($D_i = 1$) or controlled ($D_i = 0$). Let D_{1i} and D_{0i} denote individual i's treatment status when $Z_i = 1$ and 0, respectively. We assume monotonicity: $D_{1i} \geq D_{0i}$ for all i (i.e., no defiers). In our empirical model, D_i is uptake 1 (Y_{iv1}) – the treated are individuals who made uptake 1 and the controlled are those who did not – and Z_i is a dummy for one of the randomized interventions (e.g., Incentive). Using a cluster randomized design does not alter the subsequent discussions, and we drop the subscript i for simplicity.

Complier size

Given monotonicity, the population is partitioned into the following three subgroups: 1) Compliers ($D_1 = 1, D_0 = 0$); 2) Always-takers ($D_1 = D_0 = 1$); and 3) Never-takers: ($D_1 = D_0 = 0$). The size (proportion) of these subgroups is given as follows:

$$\text{Compliers:} \quad \begin{aligned} &P(D_1 > D_0) = E[D_1 - D_0] = E[D_1] - E[D_0] \\ &= E[D \mid Z = 1] - E[D \mid Z = 0] \end{aligned} \tag{I1}$$

$$\text{Always-takers:} \quad P(D_1 = D_0 = 1) = E[D \mid Z = 0] \tag{I2}$$

$$\text{Never-takers:} \quad P(D_1 = D_0 = 0) = 1 - E[D \mid Z = 1] \tag{I3}$$

The third equality of equation (I1) follows by randomization. Equation (I1) is the difference in the mean treatment between individuals with and without the offer of the treatment, which is the first stage of Two-Stage Least Squares in estimating LATE (Imbens and Angrist 1994). Equation (I2) is the mean treatment among individuals without the offer of the treatment. Equation (I3) is one minus the mean treatment among individuals with the offer of the treatment.

The size of compliers among the treated is given by:

$$P(D_1 > D_0 \mid D = 1) = \frac{P(D = 1 \mid D_1 > D_0)P(D_1 > D_0)}{P(D = 1)}$$
$$= \frac{P(Z = 1)(E[D \mid Z = 1] - E[D \mid Z = 0])}{P(D = 1)} \quad . \tag{I4}$$

The first equality follows Bayes rule, and the second equality uses the facts that $P(D = 1 \mid D_1 > D_0) = P(Z = 1 \mid D_1 > D_0)$ among compliers and $P(Z = 1 \mid D_1 > D_0) = P(Z = 1)$ by randomization. Equation (I4) is the size of compliers weighted by the ratio of the proportion of the offer of the treatment to the proportion of the treated. Analogously, the size of compliers among the controlled is given by:

$$P(D_1 > D_0 \mid D = 0) = \frac{P(D = 0 \mid D_1 > D_0)P(D_1 > D_0)}{P(D = 0)}$$
$$= \frac{P(Z = 0)(E[D \mid Z = 1] - E[D \mid Z = 0])}{P(D = 0)} \quad . \tag{I5}$$

Binary characteristics

Let *X* denote a Bernoulli-distributed characteristic. The proportion of *X* among compliers is given by:

$$P(X = 1 \mid D_1 > D_0) = \frac{P(D_1 > D_0 \mid X = 1)P(X = 1)}{P(D_1 > D_0)}$$
$$= \frac{(E[D \mid Z = 1, X = 1] - E[D \mid Z = 0, X = 1])P(X = 1)}{E[D \mid Z = 1] - E[D \mid Z = 0]} \quad . \tag{I6}$$

The first equality follows Bayes rule, and the second equality follows equation (I1). Equation (I6) is the overall proportion of *X* weighted by the ratio of the complier size for individuals with $X = 1$ to the overall complier size.

Discrete and continuous characteristics

Let *X* denote a discrete or continuous characteristic. The mean of *X* among compliers is given by:

$$E[X \mid D_1 > D_0] = \frac{E[\kappa(X)X]}{E[\kappa(X)]}, \tag{I7}$$

where

$$\kappa(X) = 1 - \frac{D(1-Z)}{1-P(Z=1\mid X)} - \frac{(1-D)Z}{P(Z=1\mid X)}.$$

Theorem 3.1 in Abadie (2003) yields equation (I7), which is the weighted mean of X using the weighting function $\kappa(X)$, where by monotonicity, those with $D(1\text{-}Z)=1$ are always-takers and those with $(1\text{-}D)Z = 1$ are never-takers, and thus compliers are the left-out group. Following Abadie (2003), we estimate $P(Z = 1|X)$ by probit.

Empirical analysis

Section 6 conducts these analyses for each treatment group – incentive or combined – and the control group. Table A-14 reports the proportion of treatment (uptake 1) ($P(D = 1)$), the proportion of the offer of treatment (randomized intervention) ($P(Z = 1)$), equations (I2), (I1), (I4), and (I5). Table 8 reports the overall mean of X ($P(X = 1)$ or $E(X)$) and the conditional mean of X among compliers (equation I6 or I7).

References

Abadie, Alberto (2003). "Semiparametric instrumental variable estimation of treatment response models." *Journal of Econometrics*, 113, 231-263.

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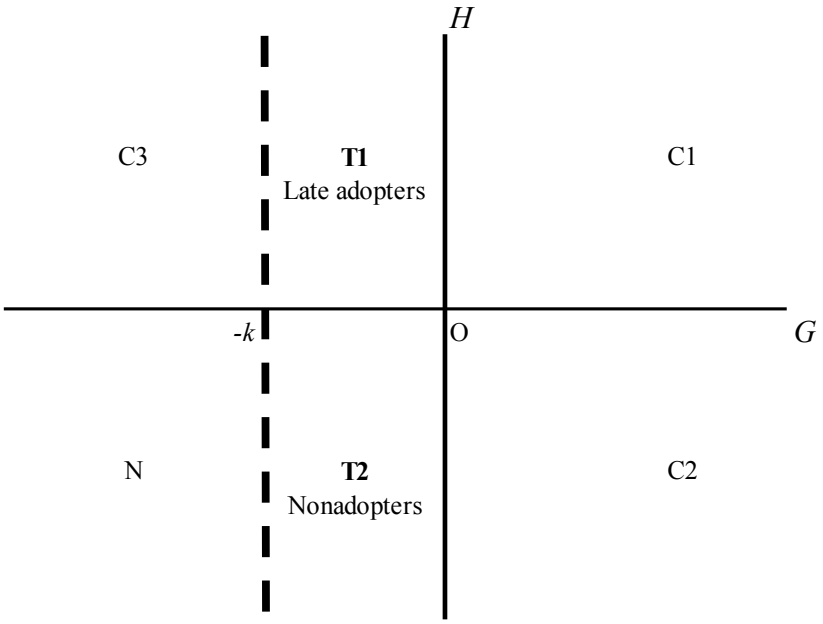
Imbens, Guido W and Joshua D Angrist (1994). "Identification and estimation of local average treatment effects." *Econometrica*, 62, 467-475.

Simkhada, Bibha, Edwin R van Teijlingen, Maureen Porter, and Padam Simkhada (2008). "Factors affecting the utilization of antenatal care in developing countries: systematic review of the literature." *Journal of Advanced Nursing*, 61, 244-260.

Figure A-1. A static model of sequential adoptions



Notes: $G = B_1(1)$ and $H = B_2(1)$, where $B_1(d_1)$ and $B_2(d_2)$, respectively, denote the perceived net benefits at periods 1 and 2, d_1 is a dummy for uptake 1, and d_2 is a dummy for uptake 2. k is cash incentive.

Table A-1. Treatment assignments

A. Treatment assignments across villages by strata

ID	LGA	Village health facility	Control	Incentive	Information	Combined	Total
A1. Randomized treatment assignments							
Baseline sample:							
1	Maiha	No	1	1	1	1	4
2	Maiha	Yes	4	3	3	3	13
3	Michika	No	4	2	2	2	10
4	Michika	Yes	4	2	2	2	10
5	Guyuk	No	3	4	3	4	14
6	Guyuk	Yes	3	3	4	4	14
7	Numan	No	2	1	1	1	5
8	Numan	Yes	2	2	2	2	8
9	Jada	No	3	3	3	3	12
10	Jada	Yes	1	3	3	3	10
	Total		27	24	24	25	100
Analysis sample:							
All			27	24	23	25	99
Baseline takers			27	23	19	23	92
Baseline nontakers			25	23	22	23	93
	Past takers		19	18	19	17	73
	Past nontakers		20	20	17	16	73
A2. Actual treatment assgnments in stratum with imperfect compliance - baseline sample[a]							
5	Guyuk	No	4	3	3	4	14
	Total		28	23	24	25	100

B. Comparision of randomized and actual treatment assgnments across villages in stratum 5

		Actual treatment assgnments				
		Control	Incentive	Information	Combined	Total
Randomized treatment assignments	Control	2	1	0	0	3
	Incentive	2	2	0	0	4
	Information	0	0	3	0	3
	Combined	0	0	0	4	4
	Total	4	3	3	4	14

(continued)

C. Number of women in three villages with imperfect compliance in stratum 5

	All		Noncompliance	
	Villages	Women	Villages	Women
Baseline sample:				
All	100	1032	3	31
Baseline takers	98	568	3	24
Baseline nontakers	96	450	2	7
Analysis sample:				
All	99	716	3	20
Baseline takers	92	384	3	17
Baseline nontakers	93	332	2	3

[a]Actual treatment assignments in the remaining 9 strata are identical to randomized assignments.

Table A-2. Correlates of baseline antenatal care uptake

	Baseline sample		Analysis sample	
	(1)	(2)	(3)	(4)
Second trimester	0.259*	0.262*	0.269*	0.275*
	(0.0420)	(0.0417)	(0.0471)	(0.0471)
Third trimester	0.389*	0.402*	0.397*	0.406*
	(0.0466)	(0.0456)	(0.0583)	(0.0568)
Past pregnancy		0.0289		0.00355
		(0.0503)		(0.0617)
Parity		-0.0137		-0.0151
		(0.0115)		(0.0136)
Age of woman		0.00155		0.000265
		(0.00332)		(0.00430)
Literate woman		0.130*		0.126*
		(0.0333)		(0.0369)
Household size		-0.000949		0.00224
		(0.00748)		(0.00921)
Children at age 5 or below		0.0211		0.0631
		(0.0418)		(0.0512)
Asset index (z-score)		0.0215		0.0233
		(0.0162)		(0.0194)
Free antenatal care		0.0638		0.0542
		(0.0449)		(0.0493)
Family planning		-0.0395		-0.0757
		(0.0453)		(0.0491)
HIV test		0.0132		0.0218
		(0.0473)		(0.0497)
Joint significance F (p-value):				
Strata fixed effects	0.00	0.00	0.00	0.01
No. observations	1014	979	714	692
R squared	0.120	0.153	0.122	0.155
Mean of dependent variables	0.558	0.559	0.536	0.536

Notes: The table reports OLS estimates. The dependent variable is a dummy for baseline uptake. Robust standard errors clustered by village are shown in parentheses. Strata fixed effects are controlled for. *p<0.05

Table A-3. Treatment effects on sample attrition

	All		Baseline nontakers		Baseline takers	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive	-0.0843	-0.0500	-0.0603	-0.0486	-0.0601	-0.0402
	(0.0522)	(0.0554)	(0.0603)	(0.0590)	(0.0710)	(0.0777)
Information	-0.0493	-0.0356	-0.0825	-0.0806	-0.00592	-0.000315
	(0.0557)	(0.0498)	(0.0711)	(0.0566)	(0.0677)	(0.0646)
Combined	-0.0424	-0.0368	-0.0810	-0.0838	0.0286	-0.000527
	(0.0461)	(0.0472)	(0.0614)	(0.0532)	(0.0599)	(0.0608)
Baseline uptake		0.0143				
		(0.0309)				
Second trimester		0.0585		0.0807		0.0348
		(0.0345)		(0.0429)		(0.0585)
Third trimester		0.245*		0.225*		0.245*
		(0.0463)		(0.0713)		(0.0639)
Joint significance F (p-value):						
Treatments	0.46	0.82	0.52	0.37	0.58	0.94
Strata fixed effects	0.16	0.27	0.02	0.03	0.24	0.20
Other covariates		0.07		0.16		0.13
No. observations	1032	979	450	432	568	547
R squared	0.023	0.088	0.034	0.096	0.029	0.111
Mean of dependent variables	0.306	0.293	0.262	0.257	0.324	0.322

Notes: The table reports OLS estimates. The sample is baseline sample. The dependent variable is a dummy for attrition from analysis sample. Robust standard errors clustered by village are shown in parentheses. Other covariates not shown here are past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. *p<0.05

Table A-4. Baseline balance - baseline nontakers and takers

	Baseline nontakers						Baseline takers					
	No. obs.	Control mean	Incentive - Control	Info. - Control	Com-bined - Control	Joint sig. F (p-value)	No. obs.	Control mean	Incentive - Control	Info. - Control	Com-bined - Control	Joint sig. F (p-value)
Stage of pregnancy at baseline (means among women)												
First trimester	93	0.383	-0.0387	-0.0863	-0.0447	0.838	92	0.188	-0.0949	-0.0916	-0.104	0.250
Second trimester	93	0.468	-0.00186	0.114	0.0716	0.510	92	0.626	-0.00953	0.0119	-0.105	0.570
Third trimester	93	0.149	0.0405	-0.0277	-0.0269	0.674	92	0.186	0.104	0.0797	0.209*	0.068
Baseline outcomes (means among women)												
Past pregnancy	93	0.825	-0.101	-0.0948	-0.0365	0.572	92	0.827	-0.149	-0.123	-0.0246	0.189
Parity	93	2.834	-0.896*	-0.691	-0.423	0.186	92	2.611	-0.842*	-0.680	0.0245	0.078
Any uptake in past pregnancies[a]	86	0.670	-0.0617	0.187	0.0600	0.158	84	0.913	0.00277	-0.0625	-0.0909	0.605
Any uptake in last pregnancy[a]	86	0.637	-0.115	0.115	0.0367	0.317	83	0.900	0.00514	-0.0675	-0.107	0.507
At least 4 uptakes in last pregnancy[a]	78	0.388	-0.0859	0.0921	0.0651	0.507	79	0.504	0.132	-0.00853	0.120	0.550
Woman/household characteristics (means among women)												
Age of woman	93	26.91	-2.778*	-0.298	-0.738	0.067	92	27.26	-2.838*	-2.551*	-0.186	0.026
Literate woman	93	0.250	0.129	0.150	0.0133	0.247	92	0.458	0.0868	-0.0677	-0.0927	0.312
Household size	93	5.341	-0.899	0.0331	-0.323	0.270	92	5.395	-0.804	-0.237	0.297	0.382
Children at age 5 or below	93	0.687	-0.0864	-0.0775	-0.0554	0.758	92	0.675	-0.0455	-0.0382	0.0147	0.904
Asset index (z-score)	93	-0.128	-0.0634	0.140	0.0723	0.768	92	-0.0493	0.273	0.341	0.135	0.275
Health facility characteristics												
Free antenatal care	93	0.760	-0.108	-0.124	-0.0643	0.804	92	0.778	-0.126	-0.146	-0.0821	0.708
Family planning	93	0.840	-0.275*	-0.0218	-0.0574	0.114	92	0.815	-0.206	-0.0253	-0.0757	0.389
HIV test	93	0.480	-0.263	-0.162	-0.132	0.298	92	0.444	-0.227	-0.129	-0.0966	0.415
Village characteristics												
Number of households (log)	91	5.281	-0.248	-0.272	-0.112	0.815	90	5.280	-0.270	-0.195	-0.197	0.834
Tapped water or piped/tubed well	91	0.333	0.0145	0.000	0.101	0.877	89	0.308	0.0401	0.0256	0.192	0.548
Any toilet	90	0.708	-0.0562	-0.00833	0.118	0.611	89	0.654	-0.00167	0.124	0.164	0.499

Notes: The samples are baseline nontakers and takers in the analysis sample. The table reports the difference in each variable between each of three treatment groups and the control group. The joint signficance of the difference for the three treatment groups is tested. See Table A-1 for the number of observations of each group. [a]Among non-first-time pregnant women. *p<0.05

Table A-5. Treatment effects on the timing of completion of pregnancy

	Baseline sample		Analysis sample		Baseline-nontakers anaysis sample		Baseline-takers analysis sample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentive	0.0304	0.0388	0.0547	0.0305	-0.0382	-0.0237	0.125	0.0624
	(0.0488)	(0.0382)	(0.0608)	(0.0528)	(0.0840)	(0.0618)	(0.0767)	(0.0815)
Information	0.00501	0.0459	0.0693	0.0581	-0.00298	-0.0323	0.151*	0.105
	(0.0507)	(0.0369)	(0.0567)	(0.0424)	(0.0772)	(0.0606)	(0.0744)	(0.0651)
Combined	0.0901	0.0437	0.125*	0.0594	0.0136	0.0196	0.243*	0.106
	(0.0488)	(0.0356)	(0.0569)	(0.0461)	(0.0749)	(0.0633)	(0.0691)	(0.0616)
Baseline uptake		0.117*		0.151*				
		(0.0281)		(0.0398)				
Second trimester		0.171*		0.183*		0.200*		0.119
		(0.0350)		(0.0392)		(0.0468)		(0.0817)
Third trimester		0.742*		0.758*		0.907*		0.634*
		(0.0306)		(0.0394)		(0.0342)		(0.0800)
Joint significance F (p-value):								
Treatments	0.27	0.54	0.19	0.52	0.94	0.77	0.01	0.33
Strata fixed effects	0.03	0.00	0.52	0.02	0.36	0.11	0.24	0.01
Other covariates		0.65		0.81		0.20		0.44
No. observations	1032	979	716	692	332	321	384	371
R squared	0.026	0.396	0.019	0.377	0.032	0.420	0.046	0.308
Mean of dependent variables	0.512	0.511	0.493	0.488	0.355	0.346	0.612	0.612

Notes: The table reports OLS estimates. The dependent variable is a dummy for the completion of pregnancy before second follow-up survey. Robust standard errors clustered by village are shown in parentheses. Other covariates not shown here are past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. *p<0.05

Table A-6. Robustness check: Treatment effects on antenatal care uptake - excluding stratum 5

	Uptake 1			Uptake 2				Uptake 12			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Incentive (α_t)	0.263*	0.239*	0.271*	0.0316	0.0687	0.0957	0.0833	0.157	0.155*	0.177*	0.172*
	(0.103)	(0.0917)	(0.0942)	(0.0940)	(0.0855)	(0.0763)	(0.0641)	(0.0909)	(0.0766)	(0.0804)	(0.0785)
	0.02	*0.02*	*0.01*	*0.73*	*0.46*	*0.28*	*0.22*	*0.10*	*0.06*	*0.05*	*0.05*
Information (β_t)	-0.0311	-0.0453	-0.00848	-0.0974	-0.0527	-0.0680	-0.0762	-0.0547	-0.0435	-0.0284	-0.0318
	(0.0875)	(0.0822)	(0.0912)	(0.0945)	(0.0934)	(0.0862)	(0.0801)	(0.0707)	(0.0758)	(0.0744)	(0.0744)
Combined (γ_t)	0.179	0.187	0.218*	-0.0600	-0.00297	0.00775	-0.00426	0.0161	0.0451	0.0715	0.0665
	(0.112)	(0.0947)	(0.0966)	(0.103)	(0.0866)	(0.0841)	(0.0725)	(0.0748)	(0.0635)	(0.0673)	(0.0661)
	0.15	*0.10*	*0.07*	*0.59*	*0.97*	*0.94*	*0.96*	*0.83*	*0.51*	*0.33*	*0.35*
Strata fixed effects	No	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Pregnancy completion	No	No	No	No	No	No	Yes	No	No	No	Yes
No. observations	307	307	297	307	307	297	297	307	307	297	297
R squared	0.061	0.131	0.181	0.010	0.070	0.185	0.312	0.034	0.101	0.149	0.178
Control mean	0.395	0.395	0.387	0.500	0.500	0.507	0.507	0.211	0.211	0.213	0.213
Channel - Chi-squared (p-value):											
Incentive ($\alpha_2 = \alpha_{t2}$)								0.11	0.26	0.25	0.19
Combined ($\gamma_2 = \gamma_{12}$)								0.36	0.52	0.36	0.28
Sustainability - Chi-squared (p-value):											
Incentive ($\alpha_1 = \alpha_t$)				0.07	0.17	0.10	0.06	0.16	0.26	0.17	0.14
Combined ($\gamma_1 = \gamma_t$)				0.08	0.10	0.06	0.03	0.07	0.08	0.07	0.06
Interaction/complementarity - F (p-value):											
$\alpha_t = \gamma_t$	0.47	0.59	0.57	0.40	0.45	0.29	0.22	0.13	0.10	0.10	0.09
$\alpha_t + \beta_t = \gamma_t$	0.71	0.96	0.74	0.97	0.89	0.87	0.92	0.46	0.52	0.43	0.44

Notes: The sample is baseline nontakers in the analysis sample excluding stratum 5 (defined in Online Table A-1). The number of clusters is 81. Robust standard errors clustered by village are shown in parentheses, below which wild cluster bootstrap p-vaues are shown in italics for incentive and combined treatment. Covariates are second and third trimesters of pregnancy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. A dummy for completion of pregnancy after second follow-up survey is also controlled for in columns (7) and (11). *p<0.05

Table A-7. Estimation results of covariates unreported in Table 5

	Uptake 1	Uptake 2		Uptake 12	
	(3)	(6)	(7)	(10)	(11)
Second trimester	0.136*	-0.156*	-0.0497	0.0286	0.0707
	(0.0610)	(0.0551)	(0.0548)	(0.0454)	(0.0476)
Third trimester	0.196*	-0.459*	0.0221	-0.102	0.0891
	(0.0931)	(0.0755)	(0.0884)	(0.0775)	(0.0819)
Completion after second follow-up			0.530*		0.211*
			(0.0582)		(0.0567)
Past pregnancy	0.155	0.0791	0.0551	0.130	0.120
	(0.0879)	(0.104)	(0.0900)	(0.0760)	(0.0763)
Parity	-0.0175	0.00222	-0.0108	-0.0313	-0.0364*
	(0.0240)	(0.0204)	(0.0178)	(0.0180)	(0.0174)
Age of woman	0.00145	-0.00445	0.000200	0.00160	0.00345
	(0.00477)	(0.00597)	(0.00510)	(0.00433)	(0.00432)
Literate woman	0.0497	0.0709	0.114*	0.00189	0.0192
	(0.0575)	(0.0601)	(0.0501)	(0.0550)	(0.0531)
Household size	-0.0116	0.000259	0.00748	0.00449	0.00736
	(0.0176)	(0.0143)	(0.0114)	(0.0122)	(0.0116)
Children at age 5 or below	0.00828	0.0456	0.0328	0.0812	0.0761
	(0.0727)	(0.0777)	(0.0695)	(0.0682)	(0.0670)
Asset index (z-score)	-0.0315	0.0182	0.0110	-0.0212	-0.0241
	(0.0261)	(0.0250)	(0.0211)	(0.0218)	(0.0210)
Free antenatal care	0.00879	-0.157*	-0.118	-0.0715	-0.0559
	(0.0787)	(0.0642)	(0.0626)	(0.0645)	(0.0642)
Family planning	-0.0306	0.0998	0.0768	0.0765	0.0674
	(0.0728)	(0.0679)	(0.0559)	(0.0540)	(0.0493)
HIV test	0.0826	-0.0109	-0.0282	0.0544	0.0475
	(0.0678)	(0.0677)	(0.0600)	(0.0557)	(0.0541)

Notes: The sample is baseline nontakers in the analysis sample. The column numbers correspond to Table 5. Robust standard errors clustered by village are shown in parentheses. *p<0.05

Table A-8. Antenatal care uptake sequence - other subsamples

	All		Control		Incentive		Information		Combined	
	Y_1	Y_2	Y_1	Y_2	Y_1	Y_2	Y_1	Y_2	Y_1	Y_2
A. Baseline takers										
$Y_1=Y_2=0$		0.11		0.09		0.07		0.22		0.06
$Y_1=0$	0.17		0.19		0.16		0.25		0.07	
$Y_1=0, Y_2=1$		0.06		0.10		0.09		0.03		0.01
$Y_1=1, Y_2=0$		0.56		0.45		0.53		0.57		0.71
$Y_1=1$	0.83		0.81		0.84		0.75		0.93	
$Y_1=Y_2=1$		0.27		0.36		0.31		0.17		0.22
No. observations	384		102		99		87		96	
B. Past takers among baseline nontakers										
$Y_1=Y_2=0$		0.25		0.35		0.10		0.32		0.24
$Y_1=0$	0.51		0.73		0.24		0.62		0.43	
$Y_1=0, Y_2=1$		0.25		0.38		0.15		0.30		0.20
$Y_1=1, Y_2=0$		0.24		0.08		0.27		0.22		0.39
$Y_1=1$	0.49		0.28		0.76		0.38		0.57	
$Y_1=Y_2=1$		0.25		0.20		0.49		0.16		0.17
No. observations	177		40		41		50		46	
C. Past nontakers among baseline nontakers										
$Y_1=Y_2=0$		0.32		0.29		0.26		0.47		0.28
$Y_1=0$	0.55		0.50		0.50		0.69		0.53	
$Y_1=0, Y_2=1$		0.23		0.21		0.24		0.22		0.25
$Y_1=1, Y_2=0$		0.24		0.29		0.26		0.16		0.25
$Y_1=1$	0.45		0.50		0.50		0.31		0.47	
$Y_1=Y_2=1$		0.21		0.21		0.24		0.16		0.22
No. observations	148		38		42		32		36	

Notes: The sample is baseline takers in the analysis sample in panel A, past takers among baseline nontakers in the analysis sample in panel B, and past nontakers among baseline nontakers in the analysis sample in panel C. Proportions for each squence up to the corresponding period are shown. Y_1: uptake 1, Y_2: uptake 2 (0: no uptake, 1: uptake)

Table A-9. Treatment effects on antenatal care uptake - baseline takers

	Uptake 1			Uptake 2				Uptake 12			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Incentive (α_t)	0.0247	0.0290	-0.00476	-0.0567	-0.0509	-0.0404	-0.0123	-0.0496	-0.0418	-0.0307	-0.00885
	(0.0697)	(0.0574)	(0.0599)	(0.0817)	(0.0807)	(0.0881)	(0.0756)	(0.0773)	(0.0739)	(0.0781)	(0.0731)
Information (β_t)	-0.0666	-0.0805	-0.0914	-0.254*	-0.230*	-0.216*	-0.169*	-0.190*	-0.171*	-0.147*	-0.110
	(0.0804)	(0.0659)	(0.0725)	(0.0641)	(0.0664)	(0.0693)	(0.0559)	(0.0585)	(0.0641)	(0.0690)	(0.0609)
Combined (γ_t)	0.113*	0.103*	0.0790	-0.232*	-0.213*	-0.144*	-0.0965	-0.144*	-0.128*	-0.0599	-0.0229
	(0.0545)	(0.0482)	(0.0483)	(0.0706)	(0.0623)	(0.0684)	(0.0600)	(0.0692)	(0.0585)	(0.0632)	(0.0586)
Strata fixed effects	No	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Covariates	No	No	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Pregnancy completion	No	No	No	No	No	No	Yes	No	No	No	Yes
No. observations	384	384	371	384	384	371	371	384	384	371	371
R squared	0.029	0.137	0.165	0.054	0.101	0.195	0.345	0.028	0.077	0.181	0.283
Control mean	0.814	0.814	0.825	0.461	0.461	0.454	0.454	0.363	0.363	0.361	0.361
Chi-squared (p-value):											
Information ($\beta_2 = \beta_{12}$)								0.08	0.08	0.04	0.07
Combined ($\gamma_2 = \gamma_{12}$)								0.01	0.00	0.00	0.01
Chi-squared (p-value):											
Information ($\beta_1 = \beta_t$)				0.06	0.08	0.16	0.31	0.13	0.21	0.47	0.78
Combined ($\gamma_1 = \gamma_t$)				0.00	0.00	0.02	0.03	0.00	0.00	0.07	0.14
Interaction/complementarity - F (p-value):											
$\beta_t = \gamma_t$	0.01	0.00	0.01	0.75	0.79	0.27	0.23	0.50	0.53	0.19	0.15
$\alpha_t + \beta_t = \gamma_t$	0.13	0.06	0.05	0.47	0.52	0.31	0.37	0.35	0.40	0.26	0.31
Baseline nontakers (Table 5) = *Baseline takers* - Chi-squared (p-value):											
Incentive (α_t)	0.03	0.02	0.01	0.30	0.15	0.10	0.15	0.05	0.04	0.03	0.05
Information (β_t)	0.58	0.39	0.29	0.06	0.04	0.05	0.15	0.06	0.07	0.11	0.24
Combined (γ_t)	0.75	0.42	0.16	0.09	0.03	0.17	0.28	0.06	0.02	0.15	0.27

Notes: The sample is baseline takers in the analysis sample. Robust standard errors clustered by village are shown in parentheses. The number of clusters is 92. Covariates are second and third trimesters of pregnancy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. A dummy for completion of pregnancy after second follow-up survey is also controlled for in columns (7) and (11). Estimation results among baseline nontakers are reported in Table 5. *p<0.05

Table A-10. Robustness check: Heterogeneity by the stage of pregnancy - excluding stratum 5

	Uptake 1		Uptake 2		Uptake 12	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive (α_t)						
Sample 0	0.239*	0.271*	0.0687	0.0957	0.155*	0.177*
	(0.0917)	(0.0942)	(0.0855)	(0.0763)	(0.0766)	(0.0804)
Sample 1	0.271*	0.306*	0.0643	0.139	0.199*	0.248*
	(0.0981)	(0.107)	(0.0890)	(0.0796)	(0.0865)	(0.0914)
Sample 2	0.290*	0.313*	0.0796	0.112	0.243*	0.277*
	(0.109)	(0.120)	(0.0984)	(0.0865)	(0.116)	(0.118)
Combined (γ_t)						
Sample 0	0.187	0.218*	-0.00297	0.00775	0.0451	0.0715
	(0.0947)	(0.0966)	(0.0866)	(0.0841)	(0.0635)	(0.0673)
Sample 1	0.173	0.202*	-0.0131	0.0150	0.0644	0.0958
	(0.0923)	(0.0995)	(0.0930)	(0.0945)	(0.0689)	(0.0761)
Sample 2	0.126	0.141	-0.00760	-0.0135	0.0745	0.0922
	(0.101)	(0.110)	(0.105)	(0.106)	(0.0881)	(0.0951)
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes
Test (p-value)	*Interaction*		*Channel*		*Sustainability*	
	F: $\alpha_1 = \gamma_1$		*Chi-sq.:* $\alpha_2 = \alpha_{12}$		*Chi-sq.:* $\alpha_1 = \alpha_{12}$	
Sample 0	0.59	0.57	0.26	0.25	0.26	0.17
Sample 1	0.32	0.30	0.11	0.20	0.35	0.42
Sample 2	0.14	0.15	0.15	0.13	0.52	0.63

Notes: Sample 0 is baseline nontakers in the analysis sample excluding stratum 5 (defined in Online Table A-1) (n=307 with no covariates, n=297 with covariates; 81 clusters). Sample 1 excludes women in the third trimester of pregnancy at the baseline (n=257 with no covariates, n=249 with covariates; 80 clusters). Sample 2 excludes women who completed pregnancy before the second follow-up survey (n=197 with no covariates, n=193 with covariates; 78 clusters). Robust standard errors clustered by village are shown in parentheses. Covariates are second and third trimesters of pregnancy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. The results for the information alone are not reported and only selected test results are shown for brevity. *p<0.05

Table A-11. Baseline knowledge and perceptions

		Past takers	Non-first-time preg. women with no past uptake - Past takers	First-time preg. women - Past takers	Joint sig. F (p-value)
1	Knew antenatal care	0.935	-0.114*	-0.0600*	0.000
2	Antenatal care could help keep you and your baby healthy	0.927	-0.128*	-0.0630*	0.000
3	Antenatal care could help detect potential problems early, prevent them, and direct you to appropriate specialists, hospitals, etc., if needed	0.895	-0.115*	-0.0666*	0.001
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	0.776	-0.142*	-0.0721	0.003
5	In Nigeria many children die before they reach the age of 5 because mothers did not take antenatal care	0.657	-0.178*	-0.0622	0.001
6	Antenatal care includes tetanus toxid vaccination	0.751	-0.0843	-0.0393	0.151
7	Antenatal care includes iron/folic acid supplementation	0.649	-0.0223	0.0296	0.652
8	Thought antenatal care was good for your and your baby's health	0.929	-0.0758*	0.0155	0.011
9	Importance of antenatal care (1: not important at all, 2: unimportant, 3: neither unimportant nor important, 4: important, 5: very important)	4.401	-0.474*	0.0382	0.000
10	Thought you could get good enough antental care	0.792	-0.0548	-0.0181	0.439
11	Quality of antenatal care you thought you could get (1: very poor, 2: poor, 3: fair, 4: good, 5: very good)	4.112	-0.335*	-0.229*	0.004

Notes: The sample is baseline sample. The maximum number of observations is 799 due to missing values. The table reports the difference in each variable between non-first-time pregnant women with no past uptake and past takers and between first-time pregnant women and past takers. The joint signficance of the difference for non-first-time pregnanent women with no past uptake and first-time pregnanent women is tested. *p<0.05

Table A-12. Correlates of past experience

	Baseline sample				Analysis sample				Baseline-nontakers analysis sample			
	Past pregancy	Past uptake[a]		Past takers	Past pregancy	Past uptake[a]		Past takers	Past pregancy	Past uptake[a]		Past takers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Age of woman	0.0267*	0.00125	-0.00317	0.0219*	0.0279*	0.00532*	0.000500	0.0267*	0.0252*	0.00988	0.000282	0.0231*
	(0.00213)	(0.00227)	(0.00300)	(0.00247)	(0.00244)	(0.00242)	(0.00311)	(0.00270)	(0.00389)	(0.00503)	(0.00590)	(0.00424)
Literate woman	-0.0933*	0.0580*	0.0666*	-0.0300	-0.126*	0.0103	0.0189	-0.0928*	-0.217*	-0.0778	-0.0664	-0.186*
	(0.0269)	(0.0292)	(0.0297)	(0.0328)	(0.0326)	(0.0326)	(0.0325)	(0.0399)	(0.0599)	(0.0736)	(0.0749)	(0.0671)
Asset index (z-score)	0.00853	0.0588*	0.0581*	0.0499*	-0.00133	0.0550*	0.0572*	0.0383*	0.0175	0.0815*	0.0878*	0.0678*
	(0.0126)	(0.0192)	(0.0191)	(0.0182)	(0.0143)	(0.0199)	(0.0198)	(0.0181)	(0.0204)	(0.0310)	(0.0300)	(0.0246)
Free antenatal care	0.0354	0.0766	0.0743	0.0779	0.0179	0.0878*	0.0839	0.0801	0.0404	0.144	0.138	0.130
	(0.0356)	(0.0434)	(0.0438)	(0.0479)	(0.0367)	(0.0441)	(0.0451)	(0.0482)	(0.0503)	(0.0746)	(0.0730)	(0.0667)
Family planning	-0.0526	-0.0470	-0.0515	-0.0832	-0.0483	-0.0173	-0.0264	-0.0572	-0.0820	-0.0211	-0.0396	-0.0800
	(0.0280)	(0.0404)	(0.0402)	(0.0420)	(0.0290)	(0.0432)	(0.0441)	(0.0450)	(0.0495)	(0.0793)	(0.0807)	(0.0709)
HIV test	-0.0257	0.0196	0.0237	-0.0136	-0.0118	-0.0130	-0.00444	-0.0279	-0.0301	-0.0640	-0.0419	-0.0851
	(0.0297)	(0.0587)	(0.0587)	(0.0506)	(0.0312)	(0.0588)	(0.0587)	(0.0495)	(0.0437)	(0.0842)	(0.0826)	(0.0681)
Parity			0.0233*				0.0261*				0.0479*	
			(0.00912)				(0.00955)				(0.0146)	
Joint significance F (p-value):												
Strata fixed effects	0.09	0.23	0.26	0.01	0.04	0.11	0.13	0.03	0.88	0.20	0.15	0.33
No. observations	986	747	747	966	694	528	528	682	322	242	242	315
R squared	0.212	0.086	0.094	0.146	0.230	0.085	0.096	0.185	0.256	0.136	0.165	0.215
Mean of dependent variable	0.778	0.801	0.801	0.619	0.778	0.828	0.828	0.641	0.773	0.711	0.711	0.546

Notes: The table reports OLS estimates. Robust standard errors clustered by village are shown in parentheses. Strata fixed effects are controlled for.

[a]among non-first-time pregnant women. *p<0.05

Table A-13. Robustness check: Heterogeneity by past experience - excluding stratum 5

	Uptake 1		Uptake 2		Uptake 12	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive * Past nontakers (α^0_t)	-0.0501	-0.0164	0.0868	0.0505	0.00119	-0.0102
	(0.127)	(0.125)	(0.110)	(0.0978)	(0.0868)	(0.0862)
Incentive * Past takers (α^1_t)	0.487*	0.529*	0.0562	0.149	0.300*	0.372*
	(0.115)	(0.115)	(0.114)	(0.112)	(0.106)	(0.103)
	0.00	*0.00*	*0.63*	*0.23*	*0.02*	*0.01*
Information * Past nontakers (β^0_t)	-0.268*	-0.193	-0.00965	-0.0965	-0.0680	-0.0547
	(0.123)	(0.129)	(0.135)	(0.121)	(0.104)	(0.107)
Information * Past takers (β^1_t)	0.0752	0.0859	-0.115	-0.0789	-0.0597	-0.0303
	(0.0991)	(0.111)	(0.127)	(0.125)	(0.0907)	(0.0930)
Combined * Past nontakers (γ^0_t)	-0.000293	0.0323	0.161	0.165	0.0387	0.0907
	(0.143)	(0.143)	(0.121)	(0.114)	(0.0910)	(0.0922)
Combined * Past takers (γ^1_t)	0.315*	0.360*	-0.169	-0.141	0.0170	0.0576
	(0.108)	(0.113)	(0.106)	(0.119)	(0.0727)	(0.0846)
	0.02	*0.01*	*0.12*	*0.27*	*0.81*	*0.52*
Strata fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Past takers	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes
No. observations	300	290	300	290	300	290
R squared	0.177	0.218	0.095	0.208	0.136	0.186
Control mean - past nontakers	0.514	0.500	0.432	0.444	0.216	0.222
Control mean - past takers	0.289	0.289	0.579	0.579	0.211	0.211
Channel - Chi-squared (p-value):						
Incentive * Past takers ($\alpha^1_2 = \alpha^1_{12}$)					0.01	0.02
Combined * Past takers ($\gamma^1_2 = \gamma^1_{12}$)					0.06	0.05
Sustainability - Chi-squared (p-value):						
Incentive * Past takers ($\alpha^1_1 = \alpha^1_t$)			0.01	0.01	0.08	0.10
Combined * Past takers ($\gamma^1_1 = \gamma^1_t$)			0.00	0.00	0.00	0.00
Interaction/complementarity - F (p-value)						
Past takers ($\alpha^1_t = \gamma^1_t$)	0.13	0.12	0.06	0.01	0.01	0.00
Past takers ($\alpha^1_t + \beta^1_t = \gamma^1_t$)	0.11	0.10	0.51	0.18	0.09	0.03
Past nontakers = Past takers - F (p-value):						
Incentive ($\alpha^0_t = \alpha^1_t$)	0.00	0.00	0.83	0.50	0.01	0.00
Combined ($\gamma^0_t = \gamma^1_t$)	0.07	0.06	0.02	0.05	0.84	0.79

Notes: The sample is the analysis sample excluding stratum 5 (defined in Online Table A-1). Robust standard errors clustered by village are shown in parentheses, below which wild cluster bootstrap p-values are shown in italics for incentive and combined treatment among past takers. The number of clusters is 80 (65 both among past nontakers and takers). A dummy for past takers is always controlled for. Other covariates are second and third trimesters of pregnacy, past pregnancy, parity, and woman/household/health facility characteristics reported in Table 1. *p<0.05

Table A-14. Reasons for not making antenatal care uptake

	Baseline nontakers					Past nontakers					Past takers				
	All	Control	Incen-tive	Infor-mation	Com-bined	All	Control	Incen-tive	Infor-mation	Com-bined	All	Control	Incen-tive	Infor-mation	Com-bined
Baseline uptake (proportion):															
High cost	0.43	0.45	0.43	0.47	0.36	0.39	0.36	0.34	0.50	0.38	0.47	0.53	0.53	0.44	0.38
Lack of services	0.06	0.05	0.07	0.06	0.07	0.08	0.09	0.09	0.08	0.06	0.05	0.00	0.06	0.04	0.08
Long distance	0.08	0.09	0.09	0.03	0.09	0.09	0.15	0.09	0.00	0.12	0.06	0.03	0.09	0.04	0.05
Not necessary	0.28	0.23	0.26	0.31	0.32	0.31	0.24	0.37	0.27	0.35	0.26	0.23	0.16	0.33	0.27
Other	0.15	0.17	0.15	0.14	0.15	0.13	0.15	0.11	0.15	0.09	0.17	0.20	0.16	0.13	0.22
No. observations	278	64	68	72	74	128	33	35	26	34	144	30	32	45	37
Uptake 1 (proportion):															
High cost	0.49	0.62	0.48	0.37	0.52	0.54	0.56	0.53	0.53	0.53	0.45	0.65	0.38	0.27	0.54
Lack of services	0.04	0.00	0.08	0.02	0.10	0.07	0.00	0.12	0.06	0.12	0.01	0.00	0.00	0.00	0.08
Long distance	0.04	0.08	0.04	0.02	0.00	0.04	0.13	0.06	0.00	0.00	0.03	0.05	0.00	0.04	0.00
Not necessary	0.21	0.16	0.16	0.30	0.19	0.16	0.25	0.12	0.18	0.12	0.27	0.10	0.25	0.38	0.31
Other	0.21	0.14	0.24	0.28	0.19	0.18	0.06	0.18	0.24	0.24	0.24	0.20	0.38	0.31	0.08
No. observations	136	37	25	43	31	67	16	17	17	17	67	20	8	26	13

Note: The sample is baseline nontakers in the analysis sample.

Table A-15. Size of compliers

	Baseline nontakers		Past takers	
Treatment:	Incentive	Combined	Incentive	Combined
Proportions in the corresponding treatment and control groups:				
Uptake 1	0.53	0.48	0.55	0.45
Treatment	0.51	0.50	0.51	0.54
Always-takers	0.39	0.39	0.28	0.29
Compliers	0.26	0.18	0.51	0.30
Made uptake 1	0.25	0.18	0.47	0.36
Did not make uptake 1	0.27	0.17	0.55	0.25
No. observations	155	151	77	82

Notes: The sample is the analysis sample excluding stratum 5 (defined in Table A-1). See Online Appendix I for procedures.