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WHEN INCENTIVES AREN'T ENOUGH: EVIDENCE ON INATTENTION AND IMPERFECT MEMORY FROM HIV MEDICATION ADHERENCE

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When Incentives Aren't Enough: Evidence on Inattention and Imperfect Memory from HIV Medication Adherence

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ABSTRACT

Financial incentives are widely used to encourage beneficial behaviors, but their effectiveness may be limited by inattention and imperfect memory. We study this in a randomized trial of HIV medication adherence in Mozambique. Financial incentives alone increase adherence by 10.6 percentage points, while pairing incentives with reminders increases adherence by 24.3 percentage points. We develop a model in which inattention to daily adherence and imperfect memory of payment eligibility reduce incentive effectiveness and show that reminders mitigate both frictions. Detailed medication refill data support the model's predictions. The results suggest combining incentives with reminders can substantially increase program effectiveness.

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A randomized controlled trials registry entry is available at
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1. Introduction

Financial incentives are a cornerstone of policy design. Governments and organizations use them to encourage desired behaviors: conditional cash transfers for school attendance, performance pay for public health workers, subsidies for preventive care, rebates for energy-efficient purchases, tax incentives for retirement savings. Yet incentive programs often underperform expectations. Understanding why incentives fail – and how to fix them – has first-order policy implications.

We propose that two behavioral biases can systematically undermine incentive effectiveness: inattention to the incentivized task and imperfect memory of eligibility conditions. Even well-designed incentive schemes fail when individuals forget to perform the underlying behavior (e.g., forget to take medication daily, forget to submit paperwork) or misremember when they're eligible for payment (e.g., confuse deadline dates, don't realize a time window has closed). These frictions are particularly problematic for incentives tied to repeated, time-sensitive behaviors – a common context in which policymakers deploy conditional payments.

We test this hypothesis in a high-stakes setting: medication adherence among HIV patients newly enrolled in antiretroviral therapy (ART) in urban Mozambique. We randomly assigned approximately 800 patients to receive: (1) financial incentives for timely medication refills, (2) reminder phone calls before refill due dates, (3) the combination of both interventions, or (4) standard care. Financial incentives alone modestly increase adherence. Reminders alone have a similar modest effect. But the combination treatment more than doubles adherence rates relative to the control group — financial incentives are much more effective when combined with reminders than when offered alone.

We develop a simple theoretical model in which financial incentives' effectiveness depends critically on patients remembering both to take daily medication (which determines when pills run out) and the exact eligibility date for payments. Reminders address both frictions: they increase attentiveness to daily pill-taking (reducing the probability of forgetting doses) and provide information about payment eligibility timing. The model generates sharp predictions about refill-timing patterns across treatment arms, which we test using detailed pharmacy records. Patients receiving incentives alone exhibit their highest refill rates immediately *after* payment eligibility expires – consistent with confusion about due dates. This

pattern is absent among patients receiving both reminders and incentives. Instead, these patients cluster refills just *before* the due date. These patterns provide direct evidence that memory and attention frictions undermine incentive effectiveness.

Our primary contribution is to demonstrate *how* behavioral frictions limit the effectiveness of incentives. A substantial literature in health and development has found that incentives are effective at encouraging beneficial health behaviors (Chandir et al., 2022, Björkman Nyqvist et al., 2018, McCoy et al., 2017, Barte and Wendel-Vos, 2017, Gibson et al., 2017, Bassani et al., 2013, Pop-Eleches et al., 2011, Banerjee et al., 2010, Thornton, 2008). Yet incentive programs fall far short of full compliance (Carter et al., 2021, Bhargava and Manoli, 2015, Lacetera et al., 2014, Kamenica, 2012), and are often not cost effective compared to approaches targeting behavioral frictions (Banerjee et al., 2025, Yu, 2023). Previous studies have found that incentives are more effective when provided in combination with other nudges (Banerjee et al., 2025, Reñosa et al., 2021, Corgnet et al., 2015); we show why this is the case using a simple model and our detailed data on ART refill-timings. We isolate specific frictions that (a) reduce incentive effectiveness and (b) are alleviated by reminders.

Further, we show how the interaction between imperfect memory and inattention leads to novel behavioral patterns and policy recommendations. While imperfect memory (Hirani and Wüst, 2024, Louw et al., 2024, Augenblick et al., 2023) and inattention (Barron et al., 2022, Habla and Muller, 2021, Gabaix, 2019, Karlan et al., 2016, Stango and Zinman, 2014) have both been shown to distort dynamic choices, their interaction has received little attention. We show that neither friction alone can explain the patterns in our data, or why incentives and reminders are more than twice as effective when combined. Instead, these two frictions interact to create distinct patterns of adherence and treatment effects.

The framework applies broadly: any incentive program tied to repeated, time-sensitive behaviors – from tax compliance to vaccination to educational programs – may be significantly undermined by inattention and imperfect memory, and improved through reminder interventions.

Beyond its primary contributions, this study also advances health economics by shedding light on complex demand-side barriers to health in a high-prevalence, low-resource setting. In particular, we add to the literature on obstacles individuals face in combating HIV and strategies to address them (McCoy et al., 2017, Kiene et al., 2017, Yotebieng et al., 2017, Ivers et al., 2014, Ssewamala et al., 2009,

Thornton, 2008). We evaluate a broad set of interventions and complement prior evidence on demand barriers for health – ranging from social factors (Karing, 2024, Allen IV et al., 2024, Yu, 2023, Derksen et al., 2022), imperfect information (Yang et al., 2023, Kim et al., 2017, Duflo et al., 2015, Dupas, 2011), to psychological factors (Dai et al., 2021, Milkman et al., 2021, 2011, Giné et al., 2010) – that are typically studied in isolation. We contribute to this literature by providing evidence of complementarities between financial incentives and an intervention (reminders) that alleviates inattention and imperfect memory.

2. Experimental Design

We registered a pre-analysis plan (PAP) at the AEA RCT Registry on August 14, 2019 (www.socialscienceregistry.org/trials/3184).

2.1. Setting

HIV medication adherence provides an ideal setting to study how behavioral biases undermine incentive effectiveness. The stakes are high: imperfect adherence worsens health outcomes, increases transmission, and promotes drug resistance.[1] The behavior is precisely measurable through pharmacy refill records, avoiding self-reporting biases. Adherence requires both daily pill-taking (where inattention matters) and timely refills at 30-day intervals (where memory of due dates matters). Despite widespread availability of free ART in Mozambique, adherence rates remain low – only 23% of our control group achieved the recommended 95% threshold. This is a context in which evidence on approaches to improving outcomes is urgently needed.

We conducted a randomized controlled trial in collaboration with Munhava Health Center, a public facility in the Munhava district of Beira City, the third largest city in Mozambique and a key regional port. The health center provides free HIV testing and treatment under national policy, with ART medications supplied through the U.S. President's Emergency Plan for AIDS Relief (PEPFAR).

[1] While newer ART regimens are more forgiving, previous studies suggest that adherence rates of at least 95% are required to achieve durable viral suppression in resource-limited settings Bezabhe et al. (2016), Sangeda et al. (2014), McNabb et al. (2001), Paterson et al. (2000). Further discussion is provided in Appendix A 3.

At the time of the study, the center diagnosed 10 to 20 new HIV-positive patients per weekday, all of whom were initiated on ART the same day. In June 2019, just prior to our intervention, the clinic was serving over 2,000 active ART patients.[2]

Initial ART refills were dispensed in 30-day increments, and patients were expected to return monthly for both medication and follow-up consultations. The vast majority of our study sample were on a 30-day refill schedule as they were newly-enrolled in or in early stage of ART. Per national protocol, patients demonstrating good adherence became eligible for 90-day refills after six months on medication.

2.2. *Study Protocol*

Participant Recruitment

We recruited potential participants at Munhava Health Center with the support of clinic staff. After completing their routine medical consultations, eligible patients were referred to the study team for enrollment. Individuals were eligible if they were at least 18 years old, living with HIV, and had either not yet initiated ART or had initiated ART no more than 90 days prior to enrollment. Due to the nature of our interventions—which included phone-call reminders and mobile money transfers—we excluded individuals who did not have access to a phone number through which they felt comfortable discussing personal health matters. Applying these criteria, we enrolled an average of six participants per work day.

Participants completed a 20-minute baseline survey immediately following enrollment. A brief phone follow-up was conducted one to two months later, and administrative data tracked refill behavior over the next 180 days.

COVID-19 Interruption

The study was impacted by the onset of the COVID-19 pandemic in Mozambique and new official protocols for Health Center management beginning March 24, 2020. Although Munhava Health Center continued to offer free HIV testing and ART treatment to both new and existing patients, many HIV-related consultation services were disrupted.

[2]Estimate provided by facility staff. Patient-level tracking is limited due to adherence challenges and data system constraints.

The most significant disruption to our study design was a change in the medication dispensing protocol: to reduce the frequency of in-person clinic visits, the Health Center shifted from monthly to 90-day ART medication refills. This change limited our ability to measure adherence using the originally pre-specified definition of the medication possession ratio (MPR), which relied on monthly variation in refill behavior.

As a result, we define our **primary analysis sample** as all participants who were recruited on or before February 21, 2020 to ensure a minimum of 32 days of observation. We redefine the primary outcome as an indicator for whether the MPR is at least 95% during their individual observation window, where the observation window is truncated on March 24, 2020 and varies in length. This primary sample consists of 821 participants.[3]

Randomization and Interventions

We randomly assigned study participants into six groups: five treatment groups and one control group. Randomization was implemented at the level of the recruitment day, so that all participants recruited on the same day were assigned to the same group to prevent information spillovers.[4] The treatment conditions were as follows:

1. **Financial Incentives Group** – Participants were paid 200 Mt (US$3.22) each time they refilled their ART medication on time, defined as on or before their refill due day (i.e., the day their medication was expected to run out).

2. **Reminders Group** – Participants received a phone call reminding them to refill their ART medication 0–6 days before each refill due day.

3. **Combination Group** – Participants received both the financial incentive and the reminder phone calls. In these calls, they were reminded of the upcoming refill deadline and the associated financial reward for timely refill.

[3]The pre-specified primary outcome of interest is MPR $\geq$ 95% within a 180-day window, which may only be truncated if the participant dies, transfers clinics, is lost to follow-up, or withdraws from the study. Participants recruited on or before September 26, 2019, completed the full 180-day observation period by March 24, 2020. We refer to this subgroup as the "restricted sample" ($N = 378$) and present key analyses based on this sample in Appendix Table A3.

[4]Participants were informed only of their own intervention to avoid cross-group comparisons that could alter expectations or behavior. Concealing the financial-incentive arm reduced risks of coercion at enrollment and dissatisfaction among those not receiving incentives.

4. **Information Group** – Upon completing the recruitment survey, participants were shown a short video explaining the risks of HIV and the benefits of proper adherence to ART.[5]

5. **Stigma-Relieving Group** – Our research team had previously conducted an HIV Stigma Survey in the Munhava neighborhood. Following Yu (2023), if a participant in this group overestimated the level of HIV-related stigma in the community, they were informed – at the end of the enrollment survey – that stigma levels were lower than they believed, based on local survey evidence.[6]

6. **Control Group** – Participants received standard care provided by the Health Center and no additional intervention.

In the primary analysis sample, a total of 821 participants were assigned as follows: 106 to the Control Group, 110 to the Financial Incentives Group, 107 to the Reminders Group, 135 to the Combination Group, 254 to the Information Group, and 109 to the Stigma-Relieving Group.[7]

2.3. Data Collection

We utilize multiple data sources for our analysis. We summarize these data sources here, and provide further details in Appendix Section A 1.

To obtain our primary outcome variable, the medication possession ratio (MPR), we manually digitized physical paper pharmacy records on refills at the Munhava Health Center. Our research team also used these pharmacy data in real time for implementation of reminder phone calls and the distribution of financial incentives for on-time refills.

Our research team also digitized study participants' physical medical files at Munhava Health Center. We used data from these patient files primarily to construct the appointment attendance ratio (AAR), a key secondary outcome.

We also conducted three surveys as part of the study: a baseline survey at the time of recruitment, an intermediate follow-up survey administered by phone one

[5]See Appendix A 3.1 for the English transcripts and linkes to the videos in Portuguese and Sena.

[6]See Appendix A 3.2 and A 3.3 for the HIV Stigma Survey in Munhava and the stigma-relieving intervention protocol.

[7]For more details on randomization and intervention fidelity, please see Appendix Section A 3.4.

to two months after enrollment, and an endline phone survey after the end of each participant's observation window.

2.4. Sample Description

Appendix Table A2 presents key baseline characteristics of the primary sample. The sample is 59 percent female, with an average of 7 years of education. On average, participants reported travel time of 36 minutes to reach the health center. More than half of the sample experienced food insecurity, defined as having gone without food or reduced meal size due to household food shortages in the past 12 months. Participants demonstrated relatively strong knowledge about the importance of adhering to ART, with an average score of 3.15 out of 4 on the HIV knowledge index. All characteristics are balanced across groups (see table notes for joint p-values).

3. Main Results

3.1. Patterns in Refill Behavior and Adherence

Table 1 reports some key outcomes of interest.

The medication possession ratio (MPR) is defined as:

$$MPR = \frac{\text{\# of days with medicine in possession}}{\text{\# of days in observation window}} \tag{1}$$

In calculating the numerator, we assume that patients take one dose of ART medication per day as long as they have pills on hand. The observation window is truncated at the earliest of the following events: death, transfer to another clinic, voluntary withdrawal, loss to follow-up, completion of 180 days, or March 24, 2020—the date on which COVID-related protocol changes were implemented. MPR is constructed using pharmacy refill records, supplemented by information from medical files where necessary. Our primary outcome of interest is an indicator for whether a participant achieves MPR $\geq$ 95%. Adherence as measured by MPR is low across the sample: only one-third of participants met the recommended 95% threshold, and in the control group, just 23% reached this level.

Appendix Figure A2 displays the fraction of respondents refilling daily, across the full range of 180 days in the observation window. The figure displays spikes every 30 days corresponding to standard refill intervals. These spikes become progressively smaller and more diffuse over time, suggesting gradual deterioration in adherence. In the control group, the refill spikes virtually disappear after the third cycle, while in the intervention groups – particularly the combination group – the refill rhythm is better preserved.

Figure 1 tracks the share of participants achieving MPR $\geq$ 95% over time, providing a dynamic view of cumulative adherence.

The gray bars at the bottom of the figure indicate the number of participants with observation windows reaching each day—recall that the study activities were truncated at the onset of COVID-19 protocol changes, leaving varying lengths of observation windows across patients.

The control group exhibits the lowest cumulative MPR $\geq$ 95% rate, leveling off at 22.6% by day 180. In contrast, all intervention groups show higher adherence, with the combination group achieving the largest and most sustained gains – reaching 47.4%, more than doubling the control rate. Notably, the treatment effects emerge gradually: group differences are minimal in the first 30–60 days, but diverge substantially after the second refill cycle and remain persistent through the observation period.

Several secondary outcomes of interest are also reported in Table 1. A participant is considered lost to follow up (LTFU) during the observation window if they were not seen in records (refilled medicine or attended a clinic visit) for 120 days. This variable is only defined for those with an observation window longer than 120 days. Ten percent of the sample were lost to follow up. During the observation window, five participants died and two transferred to other health centers.[8]

The appointment attendance rate (AAR) is defined as

$$AAR = \frac{\text{\# of Appointments Attended}}{\text{\# of Expected Appointments Conditional on Perfect Attendance}} \quad (2)$$

In practice, new appointments are only scheduled after a patient attends an appointment. Therefore, the denominator in our AAR equation is based on the number of appointments a patient would have scheduled if they attended every appointment on time. Appointments are typically once a month.

[8]Death is also counted as lost to follow up, while transfers were excluded from the lost to follow up denominator.

Test referral is a binary variable indicating whether at least one of the HIV testing coupons distributed to a participant was redeemed within one month by someone at Munhava Health Center. Changes in knowledge score and stigma belief are calculated as the differences in responses to HIV-related knowledge questions and perceived community stigma between the baseline survey and the intermediate phone follow-up. See the Appendix A 2 for detailed definitions of these measures.

3.2. Treatment Effect Estimates

The descriptive analysis above reveals substantial non-adherence across the sample, particularly in the control group, and visual evidence suggests that study interventions help maintain refill regularity over time. We now use a pre-specified regression framework to estimate the causal impact of each intervention on ART adherence. We use the following OLS regression specification:

$$Y_i = \alpha + \beta_1 G_i^1 + \beta_2 G_i^2 + \beta_3 G_i^3 + \beta_4 G_i^4 + \beta_5 G_i^5 + \xi \mathbf{X}_i + \epsilon_i \quad (3)$$

Y_i is the outcome of interest for individual i. G_i^j is an indicator for treatment group j. $\mathbf{X}_i$ is a vector of pre-specified individual-level control variables: a female indicator, years of education, an indicator for food security, travel time to the Health Center in minutes, and a knowledge score (1-4) about ART at baseline. Standard errors are clustered by date of recruitment.

Table 2 presents the effects of the different interventions on ART adherence. Column (1) shows results for the primary outcome. To account for the presence of multiple treatment arms, we adjust p-values using the procedure proposed by List et al. (2019), which controls for the family-wise error rate in the context of multiple hypothesis testing. Among all interventions, the combination treatment – which combined reminder calls and financial incentives – had the largest effect, increasing the likelihood of optimal adherence by 24.3 percentage points, more than doubling the control group's adherence level of 22.6 percentage points. The coefficient on the combination treatment remains statistically significant after correcting for multiple hypothesis testing, with a p-value of 0.004.

All other interventions also yielded positive treatment effects. None of the other coefficients are statistically significant at conventional levels after adjustment for multiple hypothesis testing, but the magnitudes are economically meaningful. The stigma-relieving intervention increased adherence by 14.7 percentage points,

a 65% improvement over the control group. Financial incentives and reminders, when implemented individually, each raised adherence by more than 10 percentage points. Even the light-touch information intervention produced a 7.8 percentage point increase.

The results are similar when using alternative adherence outcomes: MPR $\geq$ 0.80 and continuous MPR. Appendix Table A3 replicates the results on the restricted 180-day-window sample, and the patterns are robust.

As pre-specified, we estimated treatment effects separately in sample subgroups. Results are in Appendix Table A4. We find that the combination treatment has a significantly larger impact on males than on females (39.2 percentage points v.s. 14.3 percentage points; p-value of difference 0.016). Other studies have not demonstrated differences in men versus women with regard to incentive responsiveness (Fahey et al., 2020). We do not find significant treatment heterogeneity by food security, education, travel distance, or baseline ART knowledge.

We also examine impacts on a pre-specified set of secondary outcomes relevant to HIV care: clinic visit attendance, knowledge of ART, perceived HIV-related stigma, and test referral behavior. Regression results are in Appendix Table A5. Overall, we find limited evidence of meaningful treatment effects across these secondary measures. We do find that the combination treatment – offering both reminders and financial incentives – significantly improves the appointment attendance rate (AAR), consistent with the primary analysis showing that this treatment has the largest effect on ART adherence. This alignment is expected, as clinic appointments typically coincide with medication refill appointments. Treatment effects on the other secondary outcomes are generally small and not statistically significant.

4. Theoretical Model and Additional Analyses

We develop a model to understand when and why behavioral frictions undermine financial incentives, and how reminders can alleviate these biases. The model focuses on two key frictions: inattention to repeated tasks (forgetting daily pill-taking) and imperfect memory of conditional eligibility (forgetting refill due dates). We show that these frictions interact: with perfect attention, patients do not need to remember their incentive expiration date, as this date coincides with the date patients run out of pills. Inattention to daily pill-taking creates

a wedge between these two dates, making a patient's remaining pill count a noisy signal of incentive eligibility. The resulting uncertainty about payment eligibility undermines the effectiveness of incentives. Reminders address both frictions simultaneously – increasing attention to daily pill taking and eliminating uncertainty about eligibility dates – creating complementarity between reminders and incentives.

The model makes sharp predictions about refill-timing across treatment groups. We test these predictions with the patterns in our administrative refill data.

4.1. Basic Model with Perfect Memory

Consider a basic model of refilling. For clarity, we focus on a single refill window and coarsen the window into four periods. The patient is endowed with two periods worth of pills. In each period, the patient first takes a pill, then draws a cost shock c_t from the distribution $F_c(\cdot)$. Next, the patient makes a binary choice: if they refill, their payout is $-c_t$ and the game ends. If they don't, they continue to the next period. In periods 3 and 4 – given perfect memory – the patient has run out of pills and pays a health cost $h(\cdot)$. We assume the patient permanently drops out after two periods without pills or refilling. The patient discounts the future at a value δ. Figure 2 describes this model.

4.2. Empirical Analysis

In our empirical analysis, we treat every refill window as an independent observation, conditional on refill-number fixed effects. We define empirical analogs to the periods in the model, grouping days six or more days early into Period 1, 0–5 days early into Period 2, 1–5 days late into Period 3, and six or more days late into Period 4.[9]

Let a_t denote an indicator for refilling in period t. Define the conditional refill rate in period t as:

$$R(t) = \Pr(a_t = 1 \mid a_{t'} = 0 \; \forall t' < t). \tag{4}$$

[9]The model assumes reminders arrive before period-2 refill decisions. In later cohorts, contact delays from unanswered reminder calls (Appendix Section A 3.4) meant some patients were reached only on the final day of period 2, shifting reminder-induced refills into period 3. We therefore restrict refill-timing analyses to earlier cohorts observed for the full 180 days, where contact delays were less common.

This object captures the refill probability among patients who have not yet refilled and can be calculated in the model by integrating over the distribution of c_t. The regression specification below is designed so that treatment effects map directly to differences in this conditional refill rate.

We estimate the following regression and report results in Table 3:

$$a_{ikt} = \sum_{\tau=1}^{4} \gamma_\tau \, G_{ik}^{(T)} \mathbb{I}_{\{t=\tau\}} + \lambda_t + \delta_k + \varepsilon_{ikt}. \tag{5}$$

Every participant enters their first refill window at enrollment and progresses to the next refill window each time they complete a refill. The outcome a_{ikt} is an indicator for participant i refilling medication in period t of window k. To match the conditional refill-rate object in the model, once a patient refills in period s of window k, subsequent periods ($t > s$) of that same window are excluded from the analysis. $G_{ik}^{(T)}$ is an indicator for assignment to the treatment group.

Each column of Table 3 compares two study groups. In columns (1), (2), and (3), the treatment groups are the reminder, financial, and combination groups, respectively. The comparison group is the control group in columns (1) and (2), and the financial group in column (3). The regression includes period fixed effects (λ_t) and refill-number fixed effects (δ_k), where k indexes the ordinal refill window for an individual patient. In this specification, γ_τ captures the difference in the conditional refill rate in period τ between the treatment and comparison groups.

4.3. Financial Incentives

We model the financial incentives treatment as increasing utility by γ for all patients that refill in period 1 or 2. This is consistent with our implementation, in which patients only get the financial incentive if they refill before their due date.

With perfect memory, this model implies that conditional refill rates will go up in period 1 and 2 but stay the same in subsequent periods. Table 3 column 2 compares conditional refill rates of the financial group and the control group. Contrary to what perfect memory predicts, we find a significant increase in period 1 and a large but marginally insignificant increase in period 3, but no increase in period 2 – a pattern indicating imperfect memory.

4.4. Behavioral Frictions

We make two assumptions regarding behavioral frictions:

1. **Inattention to daily pill-taking**: In each period, the patient forgets to take a pill with probability θ, and remembers with probability $1-\theta$. If the patient forgets, they transition to the next period without taking a pill or refilling.

2. **Imperfect memory of due date**: In the absence of reminders, patients do not remember the due date perfectly. They use their number of pills on hand to form expectations about their probability of being past the due date, and thus of getting the incentive.

The first assumption implies that patients will run out of pills after our official due date. In the absence of assumption 2, this should have no impact on the effect of financial incentives. However, if patients do not perfectly remember their due date *and* forget to take pills, many patients who are in Period 3 may *believe* they are in Period 2. Thus, financial incentives can increase refills in Period 3.

4.5. Adding Reminders

We now derive theoretical insights that explain why reminders matter, and test other model predictions against additional moments in the data.

4.5.1. Combination Group

First consider the combination group. The primary purpose of reminders is to inform patients of their official due date. In our model, this allows patients in the combination group to always know t with certainty in Periods 1 and 2. Period 1 is the only period in which the patient has not yet received the reminder, while Period 2 is the only period in which the patient receives the reminder. Relative to the financial-incentives-only group, we should expect refilling in period 1 to fall, since patients know they have one more period to refill before losing the financial incentive. We also expect refilling in period 2 to rise, since patients know this is their last period to get the financial incentive. This is exactly what we find in the data in Table 3, column 3. Compared to the incentive-only group, the conditional refill rate of the combination group is higher in period 2 and lower in period 1.

4.5.2. Reminder Group

We do not need reminders to have any effects on pill-taking inattention to explain patterns in the combination group. However, the existence of a positive treatment

effect in the reminder group suggests that reminders have effects other than information since, without financial incentives, the information contained in the reminder is irrelevant to payoffs.

Table 3 column 1 demonstrates that reminders only increased refills in period 1, before the reminder is actually received. This result suggests that reminders do not work by increasing memory or salience of ART in the aftermath of receiving the reminder. Instead, this finding is consistent with knowledge of a forthcoming call increasing attentiveness to pill-taking: that is, reducing θ. A decrease in θ mechanically increases refills in period 1, since more patients remember ART and thus can refill. Effects of decreased θ in other periods are ambiguous, consistent with our null findings in the other three periods.

4.6. MPR Effects in the Model

Treatment effects on the continuous measure of MPR (Table 2, column 3) further support the model: both reminders and financial incentives increase MPR, and the two treatments are approximately additive in effect. While an analytic proof is not tractable due to the multiple frictions and sources of uncertainty in the model, we ran simulations for a large range of reasonable parameter values and found: (a) all treatments have positive effects on MPR > .95, (b) the combination group always has a larger effect than reminders or incentives alone, and (c) the treatments are approximately additive on average across parameter values.

In further simulations, we remove each of the behavioral frictions in the model. Removing inattentiveness to daily pill-taking (i.e. setting $\theta = 0$) removes all effects of reminders: the reminder group has the same MPR as the control group, and the combination group has the same MPR as the financial group. Removing the imperfect memory of the due date eliminates the increase in the conditional refill rate in period 3 for the financial group. Furthermore, it removes the reduction in the period 1 refill rate in the combination group.

For further details, see Online Appendix A 5.

4.7. Implications of Model

The model establishes that reminders and financial incentives have a larger effect when combined. The effectiveness of financial incentives is undercut by the combination of two behavioral frictions: inattention to daily pill-taking and imperfect

memory of the due date. The memory effect of reminders alleviates the inattention problem. The information effect of reminders eliminates the imperfect memory problem. Since the imperfect memory is only payoff-relevant with financial incentives, this generates an interaction between the two treatments, magnifying their effectiveness.

The key finding is that financial incentives alone achieve only half the effect of the combination treatment. This is not because financial incentives are weak; a $3.22 payment per refill represents substantial value in this context. Rather, it suggests that behavioral frictions substantially undermine incentive effectiveness. When we add reminders that cost $0.20 per call, we more than double the impact of the incentive payment. Programs leaving behavioral frictions unaddressed are forgoing opportunities to raise the effectiveness of incentive payments.

5. Conclusion

Our study among ART patients in Mozambique demonstrates that behavioral interventions can meaningfully improve medication adherence in resource-limited settings, but also reveals the complexity of the adherence problem and the limitations of single-channel approaches.

Our key finding is that the combination treatment – providing both financial incentives and reminder phone calls – more than doubled adherence rates compared to the control group, increasing the proportion of patients achieving optimal adherence (MPR $\geq$ 95%) by 24.3 percentage points. This effect remained statistically significant after corrections for multiple hypothesis testing and proved robust across alternative adherence measures and sample specifications. In contrast, financial incentives alone or reminders alone each produced positive but substantially smaller effects that did not survive multiple testing corrections, suggesting that neither intervention is sufficient on its own.

To understand these patterns, we developed a simple theoretical model incorporating two key behavioral frictions: inattention to daily pill-taking and imperfect memory of refill due dates. The model shows how these frictions interact to undermine adherence, and crucially, how reminders and incentives address different aspects of the problem. Reminders work through both a memory effect – informing patients when they are eligible for incentives – and an attention effect – increasing vigilance around pill-taking. Financial incentives motivate

timely refills but are only effective when patients accurately remember their due dates. When offered together in the "combination" treatment, reminders and financial incentives do not substitute for one another. Rather, their effects are additive, reinforcing one another. Reminders enhance the effectiveness of financial incentives by addressing the imperfect memory (about due dates) that otherwise limits their impact. And financial incentives – by providing monetary returns for getting refills on time – enhance the effectiveness of reminders about due dates (without financial incentives, there would be much less reason for patients to refill by the exact due date).

We find empirical support for these mechanisms in detailed refill-timing data. Consistent with model predictions, reminders increase early refills, financial incentives increase late refills (when patients mistakenly believe they are still eligible), and the combination treatment generates distinct early-refill patterns relative to incentives alone. Model simulations confirm that these empirical patterns emerge across a wide range of plausible parameter values.

Our findings suggest three principles for designing effective incentive programs in settings requiring repeated, time-sensitive compliance. First, recognize that behavioral biases reduce incentive effectiveness. Even well-designed, appropriately-sized incentive payments may fall short if participants are inattentive to the underlying behavior or confused about eligibility timing. Program evaluations should explicitly test whether behavioral biases limit effectiveness. Second, pair incentives with interventions addressing behavioral biases. In our setting, inexpensive reminder calls (costing $0.20 each) more than doubled the effectiveness of $3.22 incentive payments. This suggests that at the margin, investment in addressing behavioral biases may be more cost-effective than larger incentive payments. Similar logic may apply to other low-cost interventions: simplified eligibility rules, salient deadline notifications, or automatic enrollment with default options. Third, use detailed behavioral data to diagnose which frictions bind. Our ability to identify inattention and memory frictions separately relied on high-frequency timing data showing exactly when participants refilled medications. Policymakers implementing incentive programs should consider collecting similar process data to understand where programs break down and how to improve them.

The behavioral biases we identify – inattention to repeated tasks and imperfect memory of deadlines – are present in many economically important settings:

- **Conditional cash transfers:** Programs conditioning payments on school attendance, health checkups, or job training may be undermined when participants forget to attend or misunderstand timing requirements.
- **Tax compliance:** Many tax incentives (EITC, retirement savings credits) require specific actions by fixed deadlines. Inattention and memory frictions may explain low take-up.
- **Public benefits:** SNAP recertification, unemployment insurance filings, and other benefit programs require repeated compliance with deadlines.
- **Preventative healthcare:** Multi-dose vaccination schedules, cancer screening programs, and chronic disease management all involve repeated, time-sensitive behaviors with long-term benefits.

In each case, the logic is similar: conditional incentives create value, but only if individuals remember both to perform the underlying behavior and when they're eligible for payment. Low-cost interventions addressing these frictions may dramatically improve program effectiveness.

The study also provides suggestive evidence that HIV-related stigma and knowledge gaps remain important barriers. The stigma-relieving intervention – correcting misperceptions about community attitudes – produced economically meaningful increases in adherence, though effects did not survive multiple testing corrections. Similarly, the information intervention showed positive point estimates. These patterns suggest there are gains from future research exploring stigma and information interventions with larger sample sizes.

From a policy perspective, our results point to the value of multi-dimensional adherence strategies that simultaneously address financial and behavioral barriers. The combination approach tested here is low-cost, operationally feasible, and scalable. The theoretical framework suggests that other interventions targeting pill-taking inattention (such as pill-taking reminders or simplified regimens) could yield additional gains when combined with refill reminders and incentives. More broadly, our findings demonstrate that addressing behavioral frictions through complementary interventions can generate synergistic effects that exceed the impact of any single approach – a principle likely applicable to other contexts requiring sustained behavior change, from chronic disease management to educational interventions.

Future research should explore additional mechanisms to reduce inattention to daily pill-taking, test alternative methods to improve memory of refill due dates at lower cost, and investigate whether treatment effects persist after interventions are withdrawn. Understanding how to maintain high adherence over longer time horizons remains critical for achieving global HIV elimination targets.

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Table 1: Outcomes Variables

Variables	**Full Primary Sample**			**Control Group Only**			p-value of
	obs	mean	s.d.	obs	mean	s.d.	joint test
MPR $\geq$ 95%, obs window	821	0.33	0.47	106	0.23	0.42	0.001
MPR, continuous	821	0.74	0.25	106	0.69	0.25	0.003
Lost to follow up	588	0.10	0.30	77	0.06	0.25	0.885
AAR	734	0.30	0.35	95	0.29	0.35	0.218
Test referral	821	0.14	0.34	106	0.11	0.32	0.204
Change in knowledge score	518	0.27	1.25	60	0.35	1.25	0.011
Change in stigma belief	511	-1.82	2.40	59	-1.95	2.03	0.690

Notes: MPR stands for medication possession ratio. "Lost to follow-up" is a binary variable that indicates whether a patient missed their most recent appointment by at least 90 days. AAR stands for appointment attendance ratio. "Test referral" is an indicator for whether the patient referred others for HIV testing using coupons. "Change in knowledge score" and "change in stigma belief" measure the difference between the baseline survey at recruitment and the one-month follow-up survey.

Table 2: Effects of Interventions on MPR

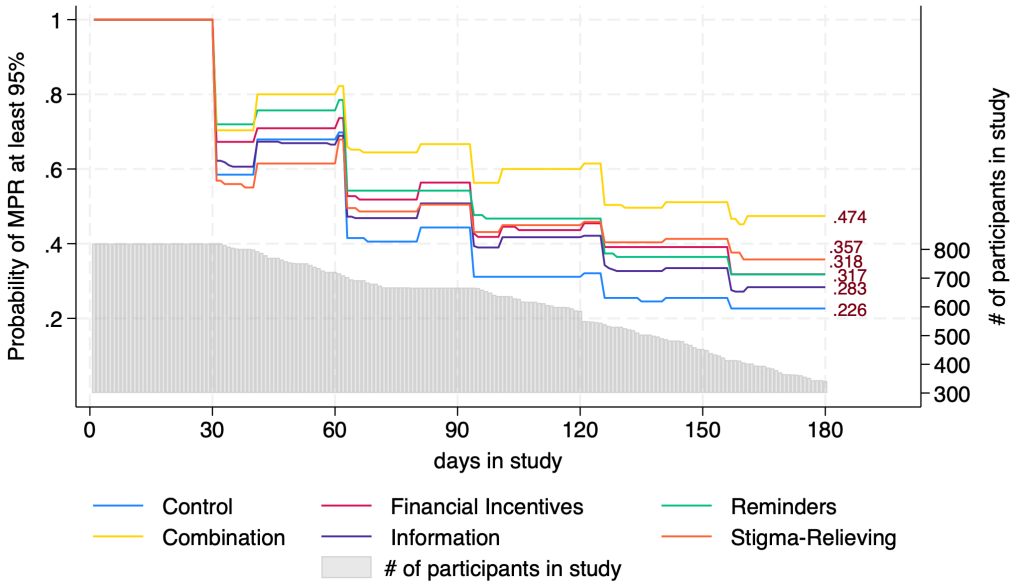
Outcome Variable	(1) MPR $\geq$ 95%	(2) MPR $\geq$ 80%	(3) MPR (continuous)
β_1: Financial Incentives	0.101*	0.135**	0.0751**
	(0.0579)	(0.0667)	(0.0323)
	[0.194]		
β_2: Reminders	0.116*	0.0894	0.0571*
	(0.0642)	(0.0642)	(0.0312)
	[0.197]		
β_3: Combination	0.243***	0.228***	0.124***
	(0.0547)	(0.0606)	(0.0294)
	[0.00350]		
β_4: Information	0.0783	0.103*	0.0406
	(0.0547)	(0.0581)	(0.0286)
	[0.209]		
β_5: Stigma-Relieving	0.147**	0.111	0.0617
	(0.0632)	(0.0825)	(0.0390)
	[0.143]		
Female	0.0316	0.102***	0.0589***
	(0.0309)	(0.0334)	(0.0175)
Education	0.00182	0.00773*	0.00343*
	(0.00416)	(0.00432)	(0.00195)
Food security	-0.0169	0.00828	-0.00607
	(0.0295)	(0.0345)	(0.0170)
Travel time	-0.000415	-0.000560	-0.000432
	(0.000421)	(0.000650)	(0.000317)
ART knowledge	0.0162	-0.00295	0.00231
	(0.0144)	(0.0155)	(0.00744)
Observations	821	821	821
R-squared	0.156	0.131	0.174
Other PAP controls	Yes	Yes	Yes
Control mean	0.226	0.443	0.689
Test: $\beta_3 = \beta_1 + \beta_2$	0.743	0.968	0.839
Test: $\beta_3 = \beta_1$	0.00619	0.0954	0.0617
Test: $\beta_3 = \beta_2$	0.0149	0.00924	0.00769

Notes: Robust standard errors clustered at the recruitment day level are in parentheses. Asterisks denote raw p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Adjusted p-values based on the multiple-hypothesis LSX test are shown in square brackets. We use 10,000-replicate bootstrapped tests to compute joint significance levels. Other PAP controls include indicators for missing years of education and food security, and fixed effects for recruitment enumerator and randomization strata as described in Appendix Section A 3.4.

Table 3: Patterns of Refill Timing

	(1) vs. Control	(2) vs. Control	(3) vs. Incentive
Reminders × t = 1	0.0872*		
	(0.0472)		
Reminders × t = 2	0.0331		
	(0.0782)		
Reminders × t = 3	0.0254		
	(0.0950)		
Reminders × t = 4	-0.0875		
	(0.108)		
Financial Incentives × t = 1		0.0977***	
		(0.0320)	
Financial Incentives × t = 2		-0.0354	
		(0.0763)	
Financial Incentives × t = 3		0.105	
		(0.0778)	
Financial Incentives × t = 4		0.0122	
		(0.103)	
Combination × t = 1			-0.0565
			(0.0323)
Combination × t = 2			0.0866*
			(0.0439)
Combination × t = 3			0.0357
			(0.0657)
Combination × t = 4			0.0239
			(0.112)
Observations	889	1032	1288
Time Period FE	Yes	Yes	Yes
Refill Num. FE	Yes	Yes	Yes
DV Mean	0.283	0.291	0.307

Notes: Robust standard errors clustered at the recruitment day level are in parentheses. Asterisks denote raw p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In all columns, the outcome is a dummy for if the patient refilled in that period in that refill window. We control for t fixed effects and refill number fixed effects in all specifications.



Notes: Each line shows, by study group, the share of participants whose cumulative MPR is at least 95% on each day in the study. For participants whose observation ends before day 180, their indicator for "MPR≥95%" remains fixed from their last observed day onward and continues to be included in the group average. The number shown at the right end of each line is the group mean on day 180, corresponding to the outcome "MPR≥95%, obs window" in Table 1. The gray bars represent the number of observation windows that reach at least X days in length.

Figure 1: MPR≥95% Over Time in the Observation Window, by Study Group

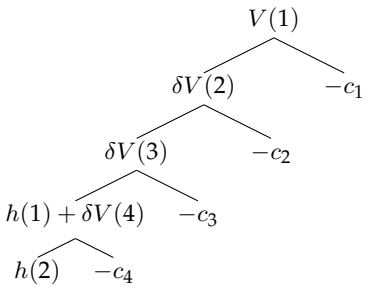


Figure 2: Decision Tree for Model with Perfect Memory

Supplementary Appendix for Online Publication

A 1. Data Collection

Pharmacy Records

The pharmacy at Munhava Health Center maintained records of ART medication refills dispensed in hard copy, including patient identifiers and the number of doses provided at each visit. Our research assistants manually entered these data, enabling us to construct a detailed dataset of each participant's refill history. We regularly updated the refill logs by extracting all relevant entries for study participants during their respective observation windows. This pharmacy refill log served as the primary data source for calculating the Medication Possession Ratio (MPR), our primary outcome variable. Additionally, we used the log in real time to update each participant's refill deadline, enabling the timely implementation of reminder phone calls and the distribution of financial incentives for on-time refills, where applicable.

Medical Files

At the end of each participant's observation window, we digitized their physical medical file maintained by Munhava Health Center. These files were initiated at the time of the patient's initial HIV diagnosis and contain a wide range of clinical information, including demographic details, laboratory test results, ART medication history, and documentation of other existing health conditions. They also record the scheduled and actual dates of each clinic visit, including sessions with psycho-social support staff. The digitized information served two main purposes. First, it allowed us to retrospectively supplement and verify pharmacy refill data, enhancing the accuracy of Medication Possession Ratio (MPR) calculations. Second, the visit records formed the basis for constructing the Appointment Attendance Ratio (AAR), a key secondary outcome used to measure patient retention in care during the study period.

Survey Data

We conducted three surveys as part of the study: a baseline survey at the time of recruitment, an intermediate follow-up survey administered by phone one to

two months after enrollment, and an endline phone survey conducted after the end of each participant's observation window.

Baseline Survey

The baseline survey was administered in person immediately after enrollment, in a private space at the clinic to ensure confidentiality. The survey collected key demographic, socio-economic, and health-related information. Participants were also asked about their knowledge of HIV, personal experiences with HIV-related stigma, and perceptions of stigma within their community. In addition, they were asked how many ART pills they had on hand at the time of enrollment, a measure that contributes to the construction of adherence outcomes.

All participants, regardless of study group, were encouraged to adhere to their ART regimen as instructed by clinic staff. At the conclusion of the survey, participants received three to five HIV test referral coupons. These coupons could be given to family members or sexual partners and were redeemable at Munhava Health Center for 100 MT upon proof of HIV testing—without disclosing test results. Each coupon contained a unique, machine-readable barcode that anonymously linked it to the participant. Coupons were valid for one month. If at least one coupon linked to a participant was redeemed, the participant received an additional reward of 100 MT. This referral tracking system enabled us to measure participants' test referral behavior, which serves as a key secondary outcome in the study.

Intermediate Follow-up Survey

The intermediate follow-up survey was conducted by phone approximately one to two months after participant enrollment. The survey lasted around five minutes and focused on assessing participants' knowledge of HIV and their perceptions of HIV-related stigma. The primary purpose of this survey was to identify potential short-term effects of the informational and stigma-relieving interventions. By re-measuring key attitudinal and knowledge-based outcomes shortly after the interventions were delivered, we aimed to evaluate whether these treatments produced detectable intermediate impacts.

Endline Survey

The endline survey occurred in November and December of 2020, and was conducted over the phone due to the COVID-19 pandemic. The survey collected

data on self-reported adherence, the impact of COVID on adherence, the impact of COVID-related clinic protocol changes on adherence, and perceived effectiveness of ART. This final round of data collection provided complementary insights into patient experiences that could not be captured through administrative data alone.

There was non-negligible attrition for the phone-based intermediate follow-up and post-graduation survey. Results based on these two rounds of surveys should be interpreted with caution. Appendix Table A6 provides more detailed discussion.

A 2. Definitions of Some Secondary Outcomes of Interest

Change in knowledge about HIV and ART

Knowledge about HIV and ART is measured by the number of correct answers to four evaluating questions, and so takes integer values from 0 to 4. The knowledge is measured in both the baseline survey and the intermediate phone follow-up one month after recruitment. The change in knowledge equals knowledge measured at the time of the phone call follow up minus knowledge measured at the time of the recruitment. The four knowledge questions (and their correct answers) are:

1. Can people get HIV from mosquito bites? (**No**)
2. Can people get HIV from kissing an infected person? (**No**)
3. Can HIV be transmitted from a mother to her baby during delivery? (**Yes**)
4. Can HIV be transmitted from a mother to her baby by breastfeeding? (**Yes**)

Change in belief about social stigma

Belief in social stigma is measured by answers to five questions. Each answer takes value 0 to 10. The belief about social stigma is the sum of the five answers and it is measured in both the baseline survey and the intermediate phone follow-up. The change in belief about social stigma equals the belief measured at the time of phone-call follow up minus the belief measured at the time of recruitment. The five questions measuring belief about stigma are:

If, for each of the following questions, I ask 10 people living in the neighboring community, how many do you think would answer "Yes"? (Choose from 0, 1, 2, . . . , 10 for each question.)

1. Do you think that people living with HIV should always use separate dishware when sharing food with others to protect other's health?
   (Stigmatizing guess = number of "Yes" answers guessed)

2. Do you agree: I would be ashamed if someone in my family had HIV?
   (Stigmatizing guess = number of "Yes" answers guessed)

3. If a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in school?
   (Stigmatizing guess = 10 minus number of "Yes" answers guessed)

4. Do you think that children living with HIV should be able to attend school with children who are HIV negative?
   (Stigmatizing guess = 10 minus number of "Yes" answers guessed)

5. If a member of your family became sick with the AIDS virus, would you be willing to care for him or her in your household?
   (Stigmatizing guess = 10 minus number of "Yes" answers guessed)

Test Referral

This is an indicator variable, which takes value 1 if the participant makes a successful referral to test for HIV within 1 month of recruitment, and 0 otherwise. A participant is considered as having made a successful referral if someone approaches our study team in the clinic, and presents us with the proof of an HIV testing together with the barcode card we distributed to the participant upon recruitment.

A 3. More Project Details

Clinical Motivation and Adherence Benchmarks

The clinical rationale for promoting adherence to antiretroviral therapy (ART) is well established in the medical literature. ART suppresses HIV viral replication to

undetectable levels, thereby preserving immune function—especially the integrity of CD4+ T-cells, which play a central role in preventing opportunistic infections. Non-adherence, even intermittently, increases the risk of viral rebound, accelerates immune system deterioration, and heightens the likelihood of transmitting HIV to others (Hughes et al., 2012, McNabb et al., 2001, Bangsberg et al., 2001, Paterson et al., 2000).

Adherence thresholds for clinical effectiveness vary by ART regimen. Earlier-generation therapies—still common in resource-limited settings—typically require adherence rates of at least 95% (equivalent to missing no more than one dose per month) to achieve durable viral suppression (Bezabhe et al., 2016, Sangeda et al., 2014). Poor adherence can also lead to the emergence of drug-resistant viral strains, which undermine future treatment options and raise public health concerns.

Given these biological and epidemiological dynamics, adherence to ART is a key outcome in both clinical and economic evaluations of HIV programs. Our primary adherence measure, the medication possession ratio (MPR), is widely used in public health and health economics to approximate the degree to which patients follow prescribed regimens.

A 3.1. Information Intervention Protocol

We originally planned a sixth treatment group, the Combination-2 Group, in which participants would receive both the Information and Stigma-Alleviation interventions. Due to a technical error, participants assigned to this group received only the Information intervention. In this paper, we classify these participants as part of the Information Group. Our results remain robust when excluding these participants or treating them as a separate group with its own estimated treatment effect.

The videos we used for information intervention can be accessed here:

Portuguese version:

`https://drive.google.com/file/d/1W3pJOJVmzuRTOKkrhydlk65-3Ojz9x6q/view?usp=sharing`

Sena version:

`https://drive.google.com/file/d/11likL7lYE73J35_1QkXw7LzekfcByHRs/view?usp=sharing.`

Below are the English transcripts.

What is HIV infection?
HIV stands for Human Immunodeficiency Virus. When this virus infects someone, it attacks and eventually destroys the immune system over several years. The immune system is a part of your body that protects you from diseases. Most people with HIV feel normal at first until their immune system is destroyed and they develop severe infections and cancers that may be fatal.

HIV is not witchcraft or supernatural power. HIV is transmitted from one person to another through semen, vaginal fluid, blood, and breast milk. HIV is not transmitted through kissing, shaking hands or sharing dishes.

Why can't I leave my HIV untreated?
If left untreated, HIV will multiply in the body very quickly and over time destroy your body's defenses to infections and cancers. These infections and cancers are often fatal. Without treatment, an infected person develops AIDS in ten years on average. Common symptoms of AIDS include rapid weight loss; recurring fever; extreme tiredness; long-lasting diarrhea; swelling of the lymph glands; blotches on or under the skin or inside the mouth, nose, or eyelids; and memory loss. Without treatment, someone with AIDS typically survives about three years before they die. Also, without treatment, you are much more likely to transmit HIV to your sexual partners.

How can the treatment help me?
Antiretroviral therapy also known as ART is a medication that stops HIV dead in its tracks. It prevents HIV from destroying the immune system to keep you healthy. It does not eliminate the virus from your body but prevents it from harming you or transmitting it to others. Everyone with HIV infection should start treatment immediately. If you correctly take ART medication, you have a good chance to stay healthy and live for as long as uninfected people and prevent the spread of HIV to others.

How does ART work?

- *ART is entirely free at this health center. You do not need to pay anything to receive the treatment.*
- *To make the treatment most effective, you need to keep taking it once started. Quitting ART after you started is bad for your health and will make future treatment more difficult.*

- *It is normal to have some side effects when you first start ART. So, do not be frightened if you have some loss or increase of appetite, fatigue or diarrhea. If your side effects are severe, come to the clinic. The health workers can help you manage them or change to a different ART regimen.*
- *Take the medication every day and do not miss any doses. The ART can effectively stop the virus only if you take the medication at least 90% of the times (missing no more than one or two doses per month). So, do not worry too much if you miss a dose by accident, but try to always keep up with the schedule.*
- *Here are a few tips to help your ART work best for you:*
  - *Make sure you always have ART drugs in stock*
  - *Regularly get your monthly refill at the health facility before you have run out*
  - *Keep a small supply of your medication with you at all times*
  - *Attend your scheduled health clinic visits regularly*

A 3.2. Munhava Stigma Belief Survey

We conducted a survey of residents living in the neighborhood of Munhava to assess prevailing social attitudes toward people living with HIV. This data collection, hereafter referred to as the *Munhava Stigma Survey*, was part of the study titled "Pilot Study: Beliefs and Stigmatizing Public Attitudes towards People Living with HIV" (University of Michigan IRB exemption approval number: HUM00141271). The survey was administered in June 2019 and included 442 randomly selected adult residents using a random-walk sampling procedure centered at Munhava Health Center to ensure geographic representativeness.

Participants were asked five questions designed to measure HIV-related stigma. Table A1 below summarizes the survey items, identifies which responses were classified as stigmatizing, and reports the proportion of respondents giving those answers.

The results indicate relatively low levels of HIV-related stigma in the Munhava community. Across all five measures, the proportion of respondents expressing stigmatizing beliefs was low, with the highest being 18.6% and the lowest just 1.9%.

Table A1: Munhava Stigma Survey Summary

No	Stigma Measure Question	Stigmatizing Answer	Share of Respondents
1	Do you think that people living with HIV should always use separate dishware when sharing food with others to protect other's health?	Yes	0.186
2	Do you agree? I would be ashamed if someone in my family had HIV	Yes	0.147
3	If a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in school?	No	0.062
4	Do you think that children living with HIV should be able to attend school with children who are HIV negative?	No	0.074
5	If a member of your family became sick with the AIDS virus, would you be willing to care for him or her in your household?	No	0.019

A 3.3. Stigma-Relieving Intervention Protocol

The stigma-relieving treatment in this study was designed to reduce concerns about HIV-related stigma by revealing to participants the relatively low prevalence of stigmatizing attitudes in their community, as measured by the Munhava Stigma Survey. By correcting possible misperceptions about the level of stigma, the intervention aimed to encourage treatment uptake and adherence.

During baseline recruitment survey, participants were asked to estimate the share of community members who had given stigmatizing responses to the five HIV-related questions in the Munhava Stigma Survey (i.e., the final column of Table A1). To avoid introducing pessimism or reinforcing stigma, the intervention was administered only when a participant overestimated the prevalence of stigmatizing attitudes—i.e., when the participant's guess exceeded the actual proportion recorded in the survey.

If the condition for intervention was met, enumerators followed a structured script to reveal the correct information. For each applicable question, the enumerator read a prewritten paragraph that compared the participant's guess to the actual community response, highlighting that most residents in Munhava hold non-stigmatizing views. The following standardized language was used:

In a population survey conducted in a neighboring community, we asked people questions about their attitudes towards HIV/AIDS. We would like to share with you how people

responded.

- *In the survey we just finished, you guessed that [X%] would answer "yes" to the question, "Do you think that people living with HIV should always use separate dishware when sharing food with others to protect other's health?" We did ask this question in the population survey in Munhava. People answered "yes" to this question less often than you think they would. Our data show that less than 2 out of 10 people (or 18.6%) answered "yes," indicating that the majority of the people living in this neighborhood are supportive of people living with HIV.*

- *You guessed that [X%] would answer "yes" to the question, "Do you agree? I would be ashamed if someone in my family had HIV." Our data show that only about 1 out of 10 people (or 14.7%) agreed, again showing broad support for people living with HIV in the neighborhood.*

- *You guessed that [X%] would answer "yes" to the question, "In your opinion, if a teacher has HIV but is not sick, should they be allowed to continue teaching at school?" In fact, more than 9 out of 10 people (or 93.8%) answered "yes," indicating strong community support.*

- *You guessed that [X%] would answer "yes" to the question, "Do you think that children living with HIV should be able to attend school with children who are HIV negative?" Our data show that 92.7% agreed with this statement.*

- *You guessed that [X%] would answer "yes" to the question, "If a member of your family became sick with AIDS would you be willing to care for them in your own household?" In reality, almost all people (98.1%) said "yes," showing a very high level of community care and support.*

Each paragraph was displayed and read only if the participant's guess exceeded the actual community response for that item. The dynamic insertion of their guessed value (indicated above by `X%`) was handled through the survey software.

A 3.4. Randomization and Intervention Fidelity

Random Assignment of Treatment

We randomized treatment assignment on a predefined list of work days of Muhnava Health Center, patients recruited on the same work day were assigned to the same study group. Every seven consecutive work days consist a randomization stratum within which study groups appear in random order. The randomization was coded into survey instruments and concealed from field enumerators. Enumerators recruit patient to study groups according to survey instrument prompts, without knowing what groups future days belong to. The original pre-analysis plan has a sixth treatment group in addition to the five described in section 2.2:

- **Combination Group-2** – Upon completing the recruitment survey, participants receive both the information intervention and the stigma-relieving intervention.

Due to a technical error on the survey instrument, patients assigned to **Combination Group-2** only received the information intervention but not the stigma-relieving intervention, making it, in effect, equivalent to the **Information Group**. As a result, we pool participants assigned to both groups and label their treatment status as **Information Group** in all analyses. Therefore, the **Information Group** has around twice the size of any other group.

Incentive Payments

Among all observed on-time refill events in the financial incentives and combination treatment groups, incentive payments were successfully delivered in 55% of cases. The average delay between the refill date and receipt of payment was 8.5 days. Payments were transferred via M-Pesa or another mobile money platform of the participant's choice, as recorded during the baseline survey. Treatment group assignments were concealed from the technicians responsible for processing payments. The most common reasons for failed or delayed transfers were inactive phone numbers or phones being unreachable at the time of payment.

Reminders

Among all refills in the reminder and combination groups, approximately 27% were refilled early – that is, before the scheduled reminder call. In such cases, no reminder call was made for that particular refill cycle (future reminders remained scheduled as planned). Of the remaining intended reminders, about

41% were successfully delivered. A reminder was considered unsuccessful if the participant's phone number was out of service or if no call (out of multiple call attempts) was answered before the refill due date. On average, successfully delivered reminders were received 2.3 days prior to the refill due date.

Reminder phone calls were made by one full time worker at a salary of 5000MT per month for 9 months during the study follow-up period. The worker made approximately 3600 refill reminder calls (one reminder for each patient-month in their observation window). Including minor airtime costs of the calls, reminder calls cost on average 12.5 Meticais (around 0.2 USD) each.

Information Intervention

The information intervention lasted an average of 4.0 minutes. Following the intervention, participants were asked seven questions assessing their understanding of HIV, antiretroviral therapy (ART), and proper adherence practices—all of which were covered in the informational video. Participants in the information group answered an average of 5.4 questions correctly, significantly higher than both the control group (mean = 4.5, $p < 0.001$) and all other groups combined excluding the Information group (mean = 4.5, $p < 0.001$). These results suggest that participants in the information group effectively absorbed the content presented in the video.

Stigma-Relieving Intervention

83% of participants in the stigma-relieving group overestimated the level of stigma in their neighborhood on at least one of the five stigma-belief questions and therefore received corrective information intended to alleviate their concerns.[10] On average, participants in the stigma-relieving group received 2.7 pieces of corrective information. The intervention was brief, lasting an average of 47 seconds per person. Approximately five minutes after the intervention – following a brief period in which participants continued with other baseline survey questions – we re-administered the same stigma-belief questions. Among those in the stigma-relieving group, 75% revised their beliefs in the intended direction, i.e., they came to perceive their community as more supportive of people living with HIV than they initially believed. For additional details, see Appendix A 3.3.

[10]The corresponding share in the full primary sample is 87%.

A 4. Additional Figures, Tables, and Analyses

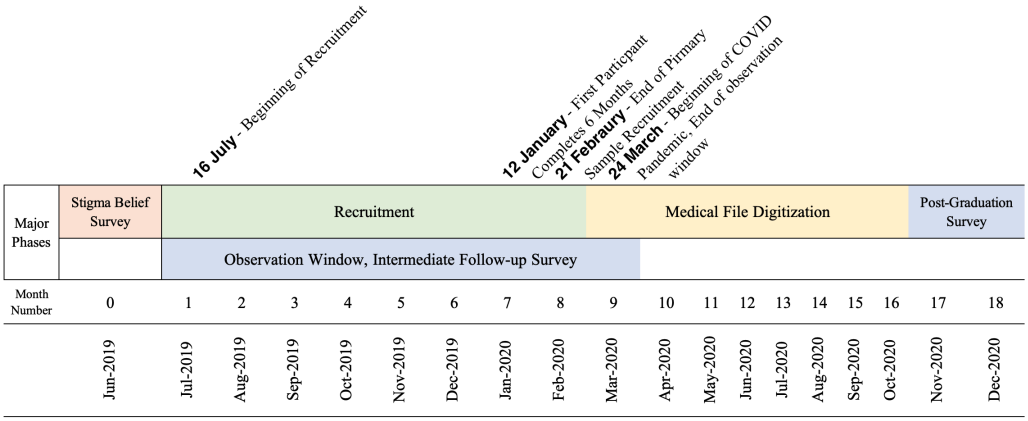


Figure A1: Study Timeline

Table A2: Sample Characteristics and Balance Tests

Variables	**Full Primary Sample** N = 821		**Control Group Only** N = 106		p-value of
	mean	s.d.	mean	s.d.	joint test
Female	0.59	0.49	0.61	0.49	0.669
Years of education	6.96	3.68	6.79	3.67	0.878
Food insecurity	0.55	0.50	0.55	0.50	0.484
Travel time to clinic (min)	35.95	30.32	33.93	20.72	0.246
Knowledge score (out of 4)	3.15	1.13	3.02	1.22	0.477

Notes: The last column reports the p-value of joint significance test (F-test) from a regression of the baseline characteristic on intervention group indicators.

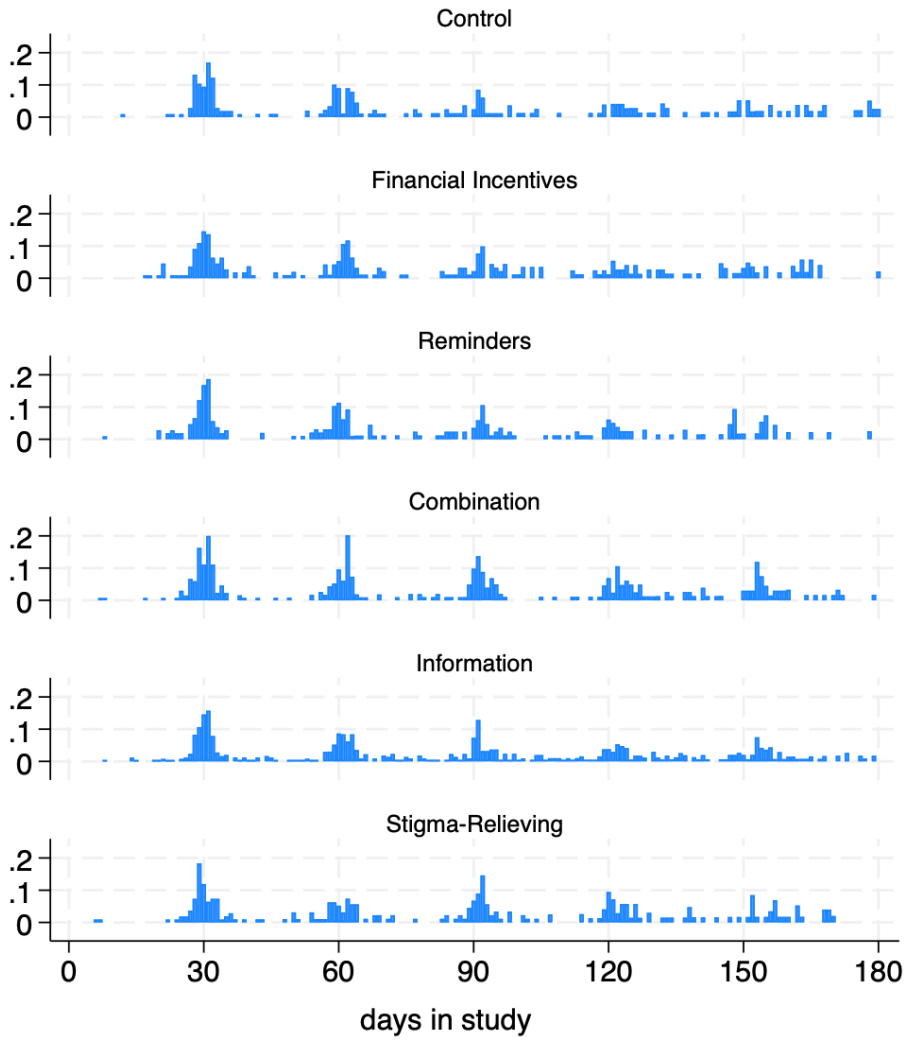


Figure A2: Refill probability by days in study by groups

Table A3: Effects of Interventions on MPR (Restricted Sample)

Outcome Variable	(1) MPR $\geq$ 95% in 180-day window	(2) MPR $\geq$ 80% in 180-day window	(3) MPR in 180-day window
β_1: Financial Incentives	0.0252	0.115	0.0842
	(0.0677)	(0.119)	(0.0560)
	[0.740]		
β_2: Reminders	0.0882	0.00918	0.0739
	(0.0735)	(0.117)	(0.0511)
	[0.506]		
β_3: Combination	0.191***	0.158	0.118**
	(0.0706)	(0.117)	(0.0523)
	[0.224]		
β_4: Information	0.152**	0.121	0.0775
	(0.0700)	(0.115)	(0.0518)
	[0.274]		
β_5: Stigma-Relieving	0.184**	0.102	0.101
	(0.0887)	(0.160)	(0.0712)
	[0.326]		
Female	0.00805	0.0752	0.0431
	(0.0442)	(0.0535)	(0.0281)
Education	-0.00140	0.0103*	0.00413
	(0.00478)	(0.00579)	(0.00248)
Food security	-0.0577	-0.00947	-0.0110
	(0.0452)	(0.0532)	(0.0252)
Travel time	-0.000673	-5.53e-05	-0.000188
	(0.000561)	(0.000687)	(0.000315)
ART knowledge	0.00287	-0.0113	0.000259
	(0.0227)	(0.0238)	(0.0117)
Observations	378	378	378
R-squared	0.059	0.076	0.075
PAP controls	yes	yes	yes
Control mean	0.140	0.395	0.626
Test: $\beta_3 = \beta_1 + \beta_2$	0.371	0.803	0.509

Notes: Robust standard errors clustered at the recruitment day level are in parentheses. Asterisks denote raw p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Adjusted p-values based on the multiple-hypothesis LSX test are shown in square brackets. We use 10,000-replicate bootstrapped tests to compute joint significance levels. Other PAP controls include missing indicators for years of education and food security, as well as recruitment enumerator and strata fixed effects.

Table A4: Subgroup Analyses on MPR $\geq$ 95% in Observation Window, Primary Sample

Subsample	(1) Male	(2) Female	(3) Low edu	(4) High edu	(5) Food insecure	(6) Food secure	(7) Close	(8) Far	(9) Low knowledge	(10) High knowledge
β_1: Financial	0.146*	0.0955	0.196**	0.0321	0.0960	0.0675	0.0644	0.0814	0.0131	0.159**
Incentives	(0.0748)	(0.0825)	(0.0818)	(0.0755)	(0.0807)	(0.0629)	(0.0799)	(0.0781)	(0.0902)	(0.0719)
β_2: Reminders	0.229***	0.0426	0.153*	0.107	0.0285	0.136**	0.0957	0.0959	0.0893	0.104
	(0.0862)	(0.0698)	(0.0899)	(0.0860)	(0.0886)	(0.0671)	(0.0891)	(0.0717)	(0.0873)	(0.0778)
β_3: Combination	0.392***	0.143*	0.306***	0.216***	0.285***	0.224***	0.293***	0.181**	0.177**	0.288***
	(0.0839)	(0.0733)	(0.0824)	(0.0754)	(0.0688)	(0.0693)	(0.0949)	(0.0718)	(0.0739)	(0.0733)
β_4: Information	0.125	0.0548	0.184**	0.0126	0.110	0.0397	0.0299	0.0920	0.0185	0.112*
	(0.0800)	(0.0669)	(0.0745)	(0.0692)	(0.0723)	(0.0600)	(0.0792)	(0.0658)	(0.0794)	(0.0674)
β_5: Stigma-	0.229***	0.112	0.204**	0.145**	0.179*	0.107	0.117	0.142*	0.188**	0.0816
Relieving	(0.0790)	(0.0845)	(0.0861)	(0.0706)	(0.0913)	(0.0758)	(0.0905)	(0.0718)	(0.0756)	(0.0814)
Observations	335	486	304	511	361	449	344	477	385	436
R-squared	0.256	0.122	0.152	0.202	0.182	0.210	0.218	0.158	0.138	0.236
PAP controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.171	0.262	0.158	0.258	0.222	0.224	0.209	0.238	0.268	0.180
p-value test of equality β_1	0.626		0.129		0.726		0.871		0.196	
p-value test of equality β_2	0.053		0.710		0.302		0.998		0.892	
p-value test of equality β_3	0.016		0.434		0.496		0.358		0.276	
p-value test of equality β_4	0.436		0.079		0.374		0.508		0.337	
p-value test of equality β_5	0.281		0.536		0.508		0.806		0.279	

Notes: Robust standard errors clustered at the recruitment day level are in parentheses. Asterisks represent raw significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A 4.1. Secondary Regression Analyses

The treatment may also influence other important behaviors and attitudes relevant to HIV care, other than ART adherence. In this section, we examine treatment effects on several secondary outcomes, including clinic visit attendance, knowledge of ART, perceived HIV-related stigma, and test referral behavior.

Table A5: Effects of Interventions on Secondary Outcomes

	(1) Lost to follow-up	(2) AAR	(3) Test referral	(4) Change in knowledge	(5) Change in stigma belief
β_1: Financial Incentives	0.0548	0.0520	-0.0286	0.111	0.471
	(0.0359)	(0.0434)	(0.0395)	(0.225)	(0.331)
β_2: Reminders	0.0417	-0.0245	0.0683	-0.450**	0.00210
	(0.0346)	(0.0485)	(0.0462)	(0.190)	(0.390)
β_3: Combination	0.0177	0.0743**	0.0584	-0.101	-0.0179
	(0.0316)	(0.0366)	(0.0413)	(0.181)	(0.368)
β_4: Information	0.0479*	0.000801	0.0310	0.238	0.468
	(0.0287)	(0.0357)	(0.0389)	(0.172)	(0.356)
β_5: Stigma-Relieving	0.0398	0.0807	0.0225	-0.200	0.223
	(0.0377)	(0.0505)	(0.0411)	(0.179)	(0.318)
Observations	588	734	821	518	511
R-squared	0.060	0.131	0.051	0.149	0.177
PAP controls	yes	yes	yes	yes	yes
Control mean	0.0649	0.292	0.113	0.417	-1.953

Notes: Robust standard errors clustered at the recruitment day level are in parentheses. Asterisks denote raw p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5 presents treatment effects on key secondary outcomes, including retention in care, knowledge, stigma, and test referral behavior. Overall, we find limited evidence of meaningful treatment effects across these secondary measures. One likely reason is the substantially reduced sample sizes for several outcomes. For instance, loss to follow-up (LTFU) is only defined for participants with observation windows longer than 120 days, and the Appointment Attendance Ratio (AAR) is constructed only for participants with non-missing medical records. The outcomes measuring change in knowledge and change in perceived stigma suffer the most attrition, as they rely on successful completion of the intermediate phone follow-up survey.

Despite these limitations, the data suggest that the combination treatment – combining reminders and financial incentives – significantly improves AAR, consistent with the primary analysis showing that this intervention most effectively enhances ART adherence. This alignment is expected, as clinic visits typically coincide with medication refill appointments. Other treatment effects are generally small and not statistically significant.

A 4.2. Attrition Analysis

Table A6 reports the completion rates—and thus attrition patterns—for each round of data collection. On average, 63.1% of participants completed the intermediate phone follow-up survey, 98.1% had their medical records successfully digitized, and 50.8% completed the post-graduation phone survey. The two phone-based surveys experienced relatively high attrition, and notably, they were conducted more than a year apart. The correlation between completing the intermediate and post-graduation surveys is modest (correlation coefficient = 0.39), suggesting that attrition is unlikely to be explained solely by participants losing contact over time. Instead, it may reflect persistent challenges related to mobile network reliability in Mozambique.

We also conducted joint significance tests for the set of intervention indicators; p-values are reported in the "p-value of F test" row. Although none of the joint tests reach conventional levels of significance, the reminder intervention is strongly associated with higher completion of the intermediate phone follow-up, and the stigma-relieving intervention is significantly associated with post-graduation survey completion. Therefore, findings from these two surveys should be interpreted with caution. Additionally, higher levels of education are consistently associated with greater likelihood of survey completion in the phone-based rounds.

A 5. Model Simulations

To run the model simulations as described in Section 4.6, we parameterized the cost shock distribution as normal, normalized the standard deviation of cost shocks to 1, and fixed the discount rate at .8. There are 6 other parameters in the model, described in columns 1 and 2 of Table A7.

Table A6: Predictors of Survey Completion and Record Availability

	(1) Complete Phone Follow-up	(2) Complete Medical Files Digitization	(3) Complete Post-Graduation Survey
β_1: Financial Incentives	0.0215	0.00147	0.0829
	(0.0567)	(0.0151)	(0.0583)
β_2: Reminders	0.128**	0.0139	0.112*
	(0.0639)	(0.0141)	(0.0609)
β_3: Combination	0.0738	-0.00320	0.0302
	(0.0653)	(0.0149)	(0.0546)
β_4: Information	0.0444	0.00486	0.0821
	(0.0509)	(0.0138)	(0.0534)
β_5: Stigma-Relieving	0.0184	-0.00856	0.143**
	(0.0720)	(0.0171)	(0.0587)
Female	0.00524	-0.00932	0.00991
	(0.0352)	(0.00880)	(0.0347)
Education	0.0191***	0.000502	0.0293***
	(0.00443)	(0.00130)	(0.00475)
Food security	0.0398	-0.0118	-0.00480
	(0.0335)	(0.0106)	(0.0370)
Travel time	-0.000410	-0.000241	0.000246
	(0.000457)	(0.000171)	(0.000497)
ART knowledge	0.0319*	-0.00455	0.0126
	(0.0166)	(0.00313)	(0.0171)
Observations	821	821	821
R-squared	0.091	0.037	0.090
Other PAP Controls	Yes	Yes	Yes
Outcome mean	0.631	0.981	0.508
Control mean	0.566	0.981	0.425
p-value of F test	0.408	0.813	0.109

Notes: Robust standard errors clustered at the recruitment day level are in parentheses. Asterisks denote raw p-values: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Parameters in Model Simulations

Parameter	Interpretation	Simulation Range	Restrictions
$h(1)$	Cost of 1 period without medication	$[1, 20]$	
$h(2)$	Cost of 2 periods without medication + dropout	$[1, 20]$	$\geq h(1)$
θ	Forgetting rate without reminders	$[0, 0.8]$	
$\Delta\theta$	Effect of reminders on forgetting rate	$[-0.8, 0]$	$\theta + \Delta\theta \geq 0$
γ	Value of incentive in utils	$[1, 50]$	
μ_c	Mean cost of refilling	$[1, 20]$	
σ_c	Standard deviation of refill costs	1	

We then generated a grid of parameter vectors within the ranges described in A7, column 3. We chose 5 equally spaced points within each range for each parameters, then removed all parameter vectors in which $h(2) < h(1)$ or $\theta + \Delta\theta < 0$. This left us with 3,585 parameter vectors. We simulated the model for each parameter vector, then calculated the MPR and conditional refill rates for each group.

1,500 of the parameter vectors returned a nonzero MPR in the control group. Within this group, we find the following:

1. MPR in the reminder group is higher than control if and only if $\Delta\theta$ is nonzero
2. MPR in the financial group is always higher than the control
3. MPR in the Combination group is strictly higher than all other groups, provided $\Delta\theta$ is nonzero

These findings align with the pattern in our main empirical results in Table 2. Further the simulations confirm that, in this model:

1. Reminders can increase refills in period 1 relative to control
2. Financial incentives can increase refills in period 3 relative to control
3. Combination can reduce refills in period 1 relative to incentives only
4. Combination lead to a greater spike in refills in period 2 relative to incentives only

These findings align with the analysis in Table 3

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