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. In a randomized trial of HIV medication adherence in Mozambique, pairing incentives with reminder calls increases the probability of achieving at least 95 percent medication coverage by 23.3 percentage points relative to standard care and by 14.7 points relative to incentives alone. We develop a model in which inattention to daily pill-taking leaves extra pills that can mislead patients about reward eligibility when deadlines are imperfectly remembered. Reminders help patients locate and act on reward opportunities; refill-timing patterns are consistent with this mechanism. Cumulative refill histories further show that the combination increases timely replenishment across repeated opportunities relative to incentives alone. In our preferred cost analysis, adding reminders increases cost per participant by approximately 23 percent, while the combination's estimated treatment effect is 2.7 times that of incentives alone. The results suggest that reminder communication is a cost-effective way to strengthen conditional incentive programs.

KEYWORDS: Financial incentives; reminders; inattention; imperfect memory; medication adherence.
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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. Evidence shows that information and communication can substantially affect how people respond to financial opportunities (Bhargava and Manoli, 2015, Jessoe and Rapson, 2014). Understanding why incentives fall short—and how to improve their effectiveness—has first-order policy implications.

We propose that two behavioral frictions can systematically undermine incentive effectiveness: imperfect memory of eligibility conditions and inattention to the underlying task (Mullainathan, 2002, DellaVigna, 2009). Even a well-designed incentive scheme can fall short when individuals misremember when they are eligible for payment, for example by confusing deadline dates or overlooking that an eligibility window has closed. Inattention to the underlying task can contribute to such memory errors: lapses in earlier actions may distort the cues people use to judge their progress and the time remaining to qualify for a reward. These frictions are particularly problematic for incentives tied to repeated, time-sensitive behaviors—a common context in which policymakers deploy conditional payments. Reminders offer an inexpensive way to address these barriers. Field experiments show that reminders can increase saving (Karlan et al., 2016) and vaccination uptake (Dai et al., 2021). We ask whether helping people remember when and how to act on a reward can improve an incentive program's performance.

We study this question in a high-stakes setting: medication adherence among patients newly diagnosed with HIV infection and initiated on antiretroviral therapy (ART) in urban Mozambique. Treatment is available free of charge, but maintaining it requires both daily medication use and regular visits to replenish supplies of medication. Our randomized trial evaluates financial incentives for timely medication refills, reminder phone calls before refill due dates, and their combination, alongside interventions providing treatment information or relieving concerns about social stigma associated with HIV/AIDS. The primary analysis includes 821 patients. Our central comparison concerns incentives, reminders, their combination, and standard care. The promised reward is the same in the two incentive groups; combination calls remind patients of both the upcoming

refill date and the reward for returning on time. Administrative refill histories from the clinic pharmacy allow us to measure medication coverage and examine when patients return across successive refill opportunities.

The combination of incentives and reminders substantially improves medication coverage. It increases the probability of having medication available for at least 95 percent of the observation window by 23.3 percentage points, nearly doubling the control-group rate. Incentives and reminders alone produce smaller, less precisely estimated gains. Adding reminders to incentives increases high medication coverage by 14.7 percentage points. The combination also improves average coverage and the share of patients meeting a lower coverage threshold. Its positive effect persists across alternative measurement conventions. These findings show substantial scope to improve the effectiveness of conditional rewards by supporting patients' ability to act on them.

We develop a simple model to explain how imperfect memory can weaken the response to a financial incentive. Patients can use their remaining pills as a cue for when to return to the clinic. Inattention to daily pill-taking, however, leaves more pills than prescribed use would imply, making remaining stock an imperfect guide to elapsed time and reward eligibility. A patient may consequently believe that a reward remains available after its deadline has passed. Remembering the due date provides a benchmark for locating the reward opportunity and potentially for monitoring correct, daily medication use. The model predicts that imperfect memory can extend the incentive response beyond the eligibility deadline, while reminders strengthen refilling near that deadline.

Dated refill records provide exploratory evidence consistent with this account. Incentives alone have a positive post-deadline refill effect, while adding reminders produces a positive effect near the deadline; both estimates are economically large but imprecise. Cumulative outcomes at the patient level show that the combination increases timely replenishment across repeated opportunities. The timing and cumulative analyses thus connect the model's predictions to behavior beyond average medication coverage.

In our preferred cost analysis, adding reminders increases cost by $1.49 per participant, approximately 23 percent compared to the cost of incentives alone, while the combination's estimated effect on high medication coverage is 2.7 times that of incentives alone. The incremental cost is approximately $10 per additional patient achieving high coverage.

Our primary contribution is experimental evidence that **reminder commu-**

nication improves the effectiveness of conditional incentives. A substantial literature shows that responses to financial opportunities, from energy prices to tax benefits, depend on how people perceive and act on them (Kamenica, 2012, Jessoe and Rapson, 2014, Bhargava and Manoli, 2015). Evidence from health and development shows that incentives can increase HIV test-result collection (Thornton, 2008), raise childhood immunization rates (Banerjee et al., 2010), and reduce HIV incidence (Björkman Nyqvist et al., 2018). Prior experiments examine incentives alongside attention aids (Bronchetti et al., 2023) and combine incentives with reminders for childhood immunization (Banerjee et al., 2025) and opioid disposal (Toma et al., 2026). Savings reminders can also be especially effective when they mention both savings goals and financial incentives (Karlan et al., 2016). We show that adding reminders to an existing reward offer substantially increases high medication coverage. This direct comparison identifies communication as a consequential component of incentive design. We further reveal how these gains accumulate across successive reward opportunities.

We also contribute to understanding how **imperfect memory and inattention** shape responses to economic incentives. Research has established the importance of memory for intertemporal decisions (Ericson, 2017, Augenblick et al., 2026) and of attention for economic behavior (Stango and Zinman, 2014, Karlan et al., 2016, Gabaix, 2019). Reminder content also affects self-reported medication adherence (Barron et al., 2026). Building on work linking reminders, incentives, and deadlines (Taubinsky, 2014, Altmann et al., 2022), our model explains how lapses in an ongoing task can distort the information used to assess later reward opportunities. The model yields explicit timing predictions, which we examine through the corresponding experimental contrasts. The contribution is to connect lapses in task execution to subsequent decisions about whether and when to act on a reward, supported by exploratory evidence on refill timing and cumulative replenishment.

Our study also advances research on **health behavior and demand barriers** by comparing interventions addressing financial, behavioral, informational, and social barriers within the same setting. A common study population and outcome framework allow us to assess these approaches and the incremental value of combining incentives with reminders. We build on evidence that health investments respond to social factors (Yu, 2023, Karing, 2024), information (Dupas, 2011, Yang et al., 2023), and interventions addressing attention and planning (Milkman et al., 2011, Dai et al., 2021). HIV-testing appointments with reminder calls can

also substitute for deposit-based commitment devices (Derksen et al., 2025). We extend research on reminders and incentives for ART adherence (Pop-Eleches et al., 2011, McCoy et al., 2017, Stecher et al., 2021, Linnemayr et al., 2024) with evidence on timely replenishment across repeated opportunities. The information and stigma-relieving interventions extend the analysis of demand barriers from HIV testing to continued treatment after ART initiation. These experimental comparisons inform program design under resource-limited delivery conditions faced by a wide range of countries in the developing world.

2. Setting, Experimental Design, and Measurement

2.1. Setting and Standard Care

HIV medication adherence provides an ideal setting to study how behavioral biases undermine incentive effectiveness. The stakes are high: imperfect adherence to ART worsens health outcomes, increases HIV transmission, and promotes ART drug resistance.[1] Adherence requires both daily pill-taking (where inattention matters) and timely refills at 30-day intervals (where memory of due dates matters). Gaps in effective HIV treatment remained substantial around the time of our study. UNAIDS estimated that 44% of adults living with HIV in Mozambique had unsuppressed viral loads in 2020 (UNAIDS, 2021). 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, 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).

ART suppresses HIV replication, allowing immune function to recover and thus reducing risk for opportunistic infection, mortality, and transmission of HIV to others. HIV medication controls rather than cures the infection, so treatment must continue long term in an uninterrupted fashion to remain effective. The oral regimens relevant to this study combine three active drugs in a single tablet and are taken daily. Taking medication on a regular daily schedule and replenishing it before supplies run out are therefore recurring tasks even for patients who feel

[1] See World Health Organization (2023) on viral suppression and transmission, and World Health Organization (2016) on adherence and drug resistance. Further discussion is provided in Appendix A 2.5.

healthy (National Institutes of Health, 2025b, 2026a). Appendix A 2.5 describes the regimen information in more detail.

At the time of the study, the center diagnosed 10 to 20 new HIV-positive patients per workday, 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 patients living with HIV infection.[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 recorded refills dispensed 30 doses (97.1% of refills within our primary observation window) consistent with recruitment early in ART. Per national guidelines[3], clinically stable adults could receive three-month ART supplies after at least six months on treatment.

2.2. *Experimental Design and Implementation*

We recruited participants at Munhava Health Center with the support of clinic staff. The study design was registered at ClinicalTrials.gov (NCT03618511), and the prespecified analysis plan was filed in the AEA RCT Registry (AEARCTR-0003184). After patients completed their routine medical consultations, clinicians who remained unaware of our treatment assignment referred eligible patients to the on-site study team, where enumerators obtained consent and enrolled them. Individuals were eligible if they were at least 18 years old, living with HIV, and had not yet initiated ART or were within 90 days on ART. 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 6.0 participants per recruitment day.

The delivered interventions are reported as 6 groups: 5 treatment groups and one control group. Randomization was coded into the end of the recruitment survey, clustered at the recruitment day to prevent information spillovers, and

[2]These figures are estimates provided by the clinic leadership and staff before the study.

[3]Clinical stability required regularity of the last three medication pickups or clinical visits, no active WHO stage III/IV condition or adverse drug reaction requiring regular monitoring, and viral suppression or an eligible CD_4 result where viral-load testing was unavailable (MISAU, 2018).

stratified by blocks of consecutive work days. Appendix A 1.2 explains the assignment schedule and related adaptations.[4]

The conditions of the parallel groups were as follows:

1. **Financial Incentives Group** – Participants to be paid 200 Mt (US$3.22) for each eligible refill on or before their refill due date.

2. **Reminders Group** – Participants to receive a phone call reminding them to refill their ART medication, with attempts starting three work days before the refill due date and continuing until the due date or refill, whichever comes first.

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

4. **Information Group** – Participants to be shown a short video on a tablet explaining the risks of HIV and the benefits of proper adherence to ART upon completing the recruitment survey, with an enumerator available to assist.[5]

5. **Stigma-Relieving Group** – Participants to receive information to relieve concerns about stigma upon completing the recruitment survey – that stigma levels were lower than they believed based on local survey evidence – if they overestimated the level of HIV-related stigma in Munhava.[6]

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

The refill due date is the day a patient's medication supply is expected to run out under prescribed daily use. This date governs the reward eligibility rule and reminder schedule: patients qualify for a reward by refilling on or before the due date, and reminder calls communicate that date. We use the same definition to assess refill timing in the analysis, calculating expected exhaustion from recorded

[4]Participants were informed only of their own intervention to avoid cross-group comparisons that could alter expectations or behavior.

[5]See Appendix A 1.3 for the English transcripts and links to the videos in Portuguese and Sena.

[6]Following protocol in Yu (2023), our research team conducted an HIV Stigma Survey (IRB exemption HUM00141271) in the Munhava neighborhood prior to the experiment. See Appendix A 1.4 and A 1.5 for the HIV Stigma Survey in Munhava and the stigma-relieving intervention details.

medication supplies and prescribed daily use. We planned 180 days of administrative follow-up; Appendix A 1.6 gives the reminder and graduation scripts.

Implementation records describe intervention delivery (Appendix Table A4). Across 730 evaluable cycles in the reminder arms, 27.8% ended in an on-time refill before a qualifying reminder, and 41.4% received a reminder by the due date. The remaining 30.8% had neither an on-time refill nor a timely reminder. Successful reminders arrived a median of 2 days before the due date. Among on-time refills in the payment arms, 62.5% have a matched successful transfer, arriving on average 11.6 days after the due date (median 7). Common practical obstacles included unstable network coverage and phones that were out of service, conditions encountered frequently in Beira during the study. Imperfect real-time refill record tracking is another source of undelivered calls or payments. The appendix describes delivery timing, practical constraints, and the completeness of the records.[7] Treatment effects below compare randomized assignments, without conditioning on successful contact or payment receipt. They measure the effectiveness of offering the programs under the natural operating conditions. Reminder and payment delivery rates do not differ significantly between the relevant treatment arms (Appendix A 1.7).

The information intervention lasted an average of 4.0 minutes. Following the intervention, participants answered 5.4 out of 7 questions correctly about HIV and ART, compared with 4.5 in the control group (recruitment-day clustered test, $p < 0.001$). In the Stigma-Relieving Group, 83% of participants overestimated the level of stigma in their neighborhood. Across all participants assigned to this group, the mean number of corrective items was 2.7, and 75% updated their beliefs toward a more supportive community at the immediate belief reassessment.

The COVID-19 pandemic interrupted recruitment and in-person intervention delivery. Munhava Health Center continued providing HIV testing and ART, but new protocols effective from March 24, 2020 changed dispensing and consultation practices. We discuss the resulting adaptations to outcome measurement below and their implications for the analysis sample and statistical power in Sections 2.4 and 3.1.2. Appendix Figure A1 summarizes the program timeline.

[7] A regional randomized trial reports similar implementation challenges. In a western Kenyan vaccination trial, 67% and 81% of caregivers in the two intervention arms reported receiving at least one SMS reminder, and 71% in the incentive arm reported receiving the transfer (Kagucia et al., 2021). These participant-reported receipt measures provide context.

2.3. Data and Outcome Measurement

2.3.1. Data Sources

We utilize multiple data sources for our analysis. They are summarized here, and further details can be found in Appendix Section A 2.1.

Two surveys supply the covariates and survey outcomes analyzed here. Participants completed a 20-minute baseline survey immediately at the time of enrollment. The purpose of the baseline survey is threefold: (1) collect demographic and socioeconomic characteristics and patients' knowledge and beliefs as control variables and foundations for subgroup analysis; (2) collect a patient's ART history and initial pill holdings to facilitate pill counts and define refill due dates for the follow-up observation window; (3) use stigma beliefs as inputs for the stigma-relieving intervention when applicable. An intermediate phone follow-up was administered one to two months after enrollment to assess knowledge and stigma beliefs.

Three distinct administrative streams support measurement. First, research assistants checked handwritten paper pharmacy dispensing records daily during the trial. Second, retrospective refill histories were transcribed from the clinic statistical department's electronic information system at the end of the study. The reconciled histories of refills based on these two sources supply the refill events used for MPR-related analysis. Third, patient medical dossiers were digitized separately at the end of each patient's observation window; their clinical and psychosocial visit dates supply clinic-attendance measures. Appendix A 2.2 and A 2.3 detail outcome constructions.

2.3.2. Outcome Definitions

The prespecified primary outcome of interest is an indicator for medication possession ratio (MPR) during the observation window to be at least 95%, where MPR is defined as

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

In calculating the numerator, we assume that patients take one dose of ART medication per day as long as they have pills on hand. A day counts as covered when the daily medication-stock measure is positive or a new refill is recorded that day.

Following McCoy et al. (2017), our primary outcome is MPR $\geq$ 95%, with MPR $\geq$ 80% and continuous MPR as secondary outcomes. MPR provides a practical measure of medication availability from routine refill records, avoiding reliance on patients' recall while capturing an essential prerequisite for uninterrupted treatment (World Health Organization, 2021), and is associated with viral suppression (Bisson et al., 2008, Tchakoute et al., 2022). The 95% threshold is a historical high-adherence benchmark from studies of protease-inhibitor regimens (Paterson et al., 2000), facilitating comparison with prior trials. The 80% threshold also has precedent in medication-adherence trials (Choudhry et al., 2017). MPR remains in use in recent Mozambican trials (Audet et al., 2024), while pharmacy records also underpin medication-use research in economics (Chandra et al., 2024). These outcomes capture changes in average medication coverage and the share of patients meeting established coverage benchmarks. Viral-load testing was not available to the study population at the time of the study.

The primary observation window begins the day after enrollment and ends after 180 days or on 24 March 2020, whichever comes first, with earlier pre-specified endpoints for death, transfer, or loss to follow-up as defined below. The COVID protocol expanded dispensing from 30-day to 90-day supplies to reduce clinic contacts. These larger supplies changed refill opportunities and could cover much of the remaining study period without a further visit, motivating the pre-COVID cutoff.

We measure documented clinic attendance using a monthly adaptation of visit constancy, the proportion of follow-up intervals containing a clinic visit (Mugavero et al., 2010, Ulloa et al., 2019). Our *appointment attendance rate* (AAR) is the weighted share of enrollment-based 30-day intervals containing a documented clinical or psychosocial attendance episode, with proportional weight for a partial final interval. AAR uses the primary MPR observation window; Appendix A 2.3 provides details.

We define loss to follow-up (LTFU) using the last recorded contact in the refill and clinic records. A participant must have more than 120 days without contact before the observation cutoff, and a retrospective check must confirm that the absence persisted under the 180-day follow-up rule. Death and loss to follow-up form a composite secondary outcome and also determine early endpoints for MPR. LTFU ascertainment requires at least 120 days of observation; participants with a recorded death are included regardless of observation length. After excluding transfers, the composite outcome is evaluable for 588 participants.

Appendix A 1.8 explains how the available records implement the registered outcome concepts. Appendix A 2.4 defines the remaining secondary outcomes.

2.4. Analysis Sample

The *primary analysis sample* contains 821 participants recruited between July 16, 2019 and February 21, 2020, after excluding records without refill linkage or starting pill quantity. This enrollment cutoff allows at least 32 days before the COVID cutoff, so a standard 30-day supply alone need not imply MPR above 95%. The *restricted sample* consists of 378 participants enrolled on or before 26 September 2019, allowing 180 days before the COVID cutoff for participants who remain alive and in care at the clinic. Appendix Table A1 traces the sample from assigned recruits through the primary and restricted cohorts.

The reported arms contain 106 control, 110 incentives, 107 reminders, 135 combination, 254 information, and 109 stigma-relieving participants.[8]

The primary sample is 59.2% female, with an average of 7.0 years of education. On average, participants reported travel time of 35.9 minutes to reach the health center. Just under half of the sample (44.6%) experienced food insecurity, defined as having gone without food or reduced meal size due to household food shortages in the past 12 months. Participants scored an average of 3.15 out of 4 on the index of knowledge about the risks of missing ART doses. Baseline characteristics are broadly balanced across treatment arms. Appendix Table A2 reports the baseline balance.

In the full primary analysis sample, 33.5% met the MPR $\geq$ 95% benchmark during their observation window (24.5% in the control group). Mean AAR is 25.2% among the 808 primary-sample participants with linked medical files (22.6% in the control group). The death/LTFU composite is evaluable for 588 participants, of whom 9.7% meet its definition. Within the primary observation windows, we observe 5 deaths and 2 transfers. Appendix Table A6 reports descriptive statistics for other outcomes.

Appendix A 1.8 maps the registered design and administrative outcomes to their implementation, including COVID censoring and field adaptions.

[8] An original seventh assignment category (120 participants) was intended to receive information plus stigma relief but received information only because of a coding error. It is pooled with the 134 participants originally assigned to information. Column (4) of Appendix Table A8 replicates the primary-outcome column of Table 1 using intended assignment, reporting the information-plus-stigma category separately as "Combination 2." Key results are not affected by splitting the two intended arms.

3. Treatment Effects

3.1. Empirical Strategy

3.1.1. Analysis Specifications

We estimate treatment effects and prespecified heterogeneity using the following specification:

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

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, a 4-item score for knowledge of the risks of missing ART doses at baseline, missing-value indicators for controls, enumerator fixed effects, and stratum fixed effects. The primary sample spans 137 recruitment days and 17 analysis strata. Standard errors are clustered by date of recruitment.

For the primary outcome and its two horizon checks, we test combination versus incentives ($\beta_3 - \beta_1 = 0$), combination versus reminders ($\beta_3 - \beta_2 = 0$), and additivity ($\beta_3 - \beta_1 - \beta_2 = 0$).

We report treatment effects within the prespecified subgroups defined by gender, education, food security, travel time, and baseline knowledge of the risks of missing ART doses. Formal equality tests cover all five treatment arms for each of the five subgroup splits.

3.1.2. Power Design and Multiple Hypothesis Testing

The original design planned for a sample size of 300 observations per study group.[9] We conducted the following power calculation. We assumed equal allocation of 300 individuals to treatment and control, with cluster-level randomization in 30 clusters of 10 individuals per arm. We assumed a small intra-class correlation coefficient (ICC), set at $\rho = 0.01$. For a binary outcome variable, we assumed the control-group rate of 50% to be conservative in power. Under these assumptions, the study had 80% power at a two-sided 5% significance level to detect an absolute treatment–control difference of about 11.8 percentage points.

The COVID-19 disruption shortened our implementation time and reduced sample size. A typical study group in the realized experiment has 19 clusters of 6

[9]Registered at ClinicalTrials.gov: https://clinicaltrials.gov/study/NCT03618511

individuals. The control-group high-adherence rate is 24.5%. The estimated intra-class correlation coefficient is 0.000 after adjustment for treatment assignment and strata. As a result, at 80% power and 0.05 significance level, the approximate nominal pairwise minimum detectable effect (MDE) is 17.4 percentage points. The realized sample therefore has limited power to detect smaller effects. We interpret effect sizes alongside their confidence intervals; imprecise estimates can remain compatible with economically meaningful gains.

The pre-analysis plan (PAP) also specified the method of List et al. (2019) to control the family-wise error rate (FWER) in the primary analysis (β's in equation 2 with the primary outcome of interest). Although the PAP lists treatment-effect hypotheses as one-sided tests, we report and interpret primary results based on the more conservative two-sided FWER-adjusted p-values.[10]

The primary-outcome column in Table 1 and its counterparts in Appendix Tables A7 and A8 apply the same procedure to control FWER within each reported family. The PAP did not specify a multiple-hypothesis-testing adjustment for analyses beyond the primary analysis. We adjust for multiple hypothesis testing by calculating sharpened q-values using the two-stage procedure of Benjamini et al. (2006), following Anderson (2008), across the 242 unique treatment-effect tests reported in this section and its associated appendix.

Regression tables report clustered standard errors in parentheses and stars based on unadjusted two-sided tests; square brackets report sharpened q-values, and curly braces report family-specific FWER-adjusted p-values. We interpret the primary treatment effects using the FWER-adjusted p-values and other effects in this section using the sharpened q-values.

3.2. Primary Results and Direct Comparisons

Table 1 reports all five treatment effects for the primary and up-to-90-day MPR outcomes. The combination increases the probability of primary-window MPR at least 95% by 23.3 percentage points, relative to a control mean of 24.5%. Its FWER-adjusted p-value is < 0.001. Incentives and reminders alone have positive estimates of 8.6 and 10.0 points, respectively, with greater uncertainty after primary-family adjustment.

[10]Supplementary FWER-adjusted p-values for the PAP-specified one-sided tests, computed from the same bootstrap draws using a custom signed-statistic implementation, are reported in the notes to Table 1.

The direct incremental comparisons are central. Combination exceeds incentives by 14.7 points ($q = 0.035$), and reminders by 13.3 points ($q = 0.063$). The point estimates favor the combination over each component. The combination also exceeds the sum of the individual point estimates by 4.7 percentage points, although this additivity contrast is not statistically significant ($q = 0.474$).

The stigma-relieving intervention increases the primary outcome by 14.1 percentage points (two-sided primary FWER $p = 0.137$; supplementary PAP-specified one-sided FWER $p = 0.0522$). This economically substantial estimate suggests that correcting pessimistic beliefs about community attitudes may support continued treatment. It extends the question studied by Yu (2023), who finds that correcting stigma misperceptions increases HIV testing in Mozambique, to patients already receiving ART.

The remaining columns of Table 1 report the 80% benchmark and continuous MPR at both horizons. The combination estimates are positive across these alternative measures, consistent with the primary result. They indicate gains in both average medication coverage and the share meeting the lower adherence benchmark.

In the 378-person restricted sample, the combination increases primary-window MPR $\geq 95\%$ by 22.4 percentage points (Appendix Table A7, column (1)). The effect is similar in magnitude to the primary estimate, although the smaller sample yields less precise inference (restricted-family FWER $p = 0.144$). The combination also exceeds incentives alone by 20.4 percentage points ($q = 0.005$). The combination effect also remains positive and significant when we extend follow-up beyond the COVID cutoff. This estimate of 16.0 percentage points is smaller, consistent with longer dispensing intervals reducing variation in measured coverage, but remains significant (FWER-adjusted $p = 0.019$; Appendix Table A7, column (4)). Under this extended follow-up, the combination-minus-incentives estimate is 7.1 percentage points ($q = 0.276$). The incremental estimates therefore remain positive in both follow-up checks, with less precise evidence under the extended horizon.

The combination's effect is robust across a range of alternative specifications and variable constructions (Appendix Table A8). The effect remains between 22.0 and 22.6 points when varying the enrollment-day dose deduction, death and loss-to-follow-up censoring, and assignment-block controls (Appendix Table A8, columns (1)–(3)); each estimate is significant at the 1% level using its family-specific FWER adjustment (adjusted $p < 0.001$, $p < 0.001$, and $p < 0.001$,

respectively). The combination effect also remains positive and significant when we recover the intended group assignment (column (4)). Using only the real-time refill log or only the retrospective source yields combination effects of 15.0 and 19.5 percentage points, respectively (Appendix Table A8, columns (5)–(6)); both remain significant at the 5% level after their separate FWER adjustments ($p = 0.016$ and $p = 0.001$).

Figure 1 shows the dynamics of high adherence (cumulative MPR $\geq$ 95%). Adherence falls and partially recovers around successive 30-day refill cycles, consistent with interruptions as initial supplies run out and subsequent refills restore coverage. The combination separates from control before month three and remains higher in the plotted series. Section 4 uses refill timing to explore behavioral mechanisms in more detail.

3.3. Other Outcomes and Heterogeneity

We also examine other adherence measures prespecified as secondary outcomes. These measures are coarser and less responsive than MPR. The combination increases AAR by 7.6 percentage points ($q = 0.093$), providing suggestive evidence that the combination benefit also appears in this coarser measure of clinic attendance. The stigma-relieving intervention also increases AAR, by 8.3 points ($q = 0.093$). Appendix Table A9 reports these estimates and the other secondary outcomes, which show no significant pattern across interventions. Survey observation rates vary across arms; Appendix Table A6 reports their ranges.

The combination's estimated effect differs most by gender among the prespecified subgroup splits (Table 2): 36.6 percentage points for men versus 14.5 for women. This difference is substantial but not statistically significant after multiple-testing adjustment ($q = 0.121$). The larger point estimate for men is relevant to persistent gaps in HIV care. Global ART coverage in 2019 was 12 percentage points lower among men than women (UNAIDS, 2020); studies in Mozambique and South Africa also find that men start treatment with more advanced disease and are more likely to be lost to follow-up (Lopez-Varela et al., 2021, Cornell et al., 2012). Incentives paired with reminders may help engage this underserved group.

4. Mechanisms: Incentives and Imperfect Memory

The gains from combining reminders and incentives raise a question about incentive effectiveness: what does a reminder add to a financial reward? Section 3 shows that the combination improves high medication coverage relative to either component. We examine how remembering the reward deadline can help patients translate that financial opportunity into timely replenishment.

The central friction is *imperfect memory* of when the reward can be earned. Medication use gives this problem a distinctive form. Patients can use the pills remaining at home as a cue for when to return. But another friction, *inattention* to everyday pill-taking, leads to missed doses and leaves more pills than prescribed use would imply. Stock then becomes an imperfect guide to the calendar. Remembering the expected exhaustion date provides another benchmark: it locates the reward opportunity and can help patients assess whether their remaining supply is consistent with daily medication use.

We develop this connection in a model of refill decisions, derive predictions for behavior around the deadline, and examine them using dated dispensing records. We then consider timely replenishment over successive refill opportunities and the role of reminder communication within routine appointment-based care.

4.1. A Model of Incentives and Imperfect Memory

4.1.1. The refill problem

We simplify the medication supply in a refill window to two periods, followed by two periods without medication. This distinguishes an early opportunity to refill, the last on-time opportunity, and subsequent delays. Patients share the same preferences and cost distribution across treatment arms and are homogeneous in attention probabilities and beliefs within each arm; realized cost draws and attention lapses vary across patients and days. A patient begins with two pills and is prescribed one per day. The supply should cover days 1 and 2; day 2 is the due date, and a financial incentive of $\gamma > 0$ is available for refilling on either day. There are four pill states: two pills, one pill, the first state without medication, and a terminal state without medication, indexed by $s = 1,2,3,4$. With daily attention these states coincide with the first four calendar days. With lapses, a state can persist for more than one day. The four states track medication availability as calendar time advances.

Figure 3 shows the sequence of refill and waiting decisions. On a decision day, the patient draws a refill cost c from a continuous distribution F_c, strictly increasing on its support in $[0,\infty)$. Cost draws are independent across days and of attention. Refilling ends the window. Without a refill, the first exhausted state incurs health cost $h(1) > 0$ and the terminal state incurs health cost $h(2) \geq h(1)$, including the cost of leaving care. A refill avoids that day's health cost. These costs summarize the health consequences of delayed replenishment. With discount factor $\delta \in (0,1)$, the no-incentive continuation values in the benchmark with no anticipated future lapses are

$$\begin{aligned} V(4) &= \mathbb{E}\max\{-c, -h(2)\}, \quad V(3) = \mathbb{E}\max\{-c, -h(1) + \delta V(4)\}, \\ V(2) &= \mathbb{E}\max\{-c, \delta V(3)\}, \quad\quad V(1) = \mathbb{E}\max\{-c, \delta V(2)\}. \end{aligned} \tag{3}$$

The patient refills when the cost draw is below a state-dependent threshold. Denote the refill probability of an attentive patient in pill state s by $q(s)$ and assume interior thresholds. With perfect attention, state s is reached on day s, so $q(s)$ is also the refill probability on that day conditional on no earlier refill. Appendix A 4.1 shows that $q(1) < q(2) < q(3)$: as the stock runs down, waiting for a cheaper refill opportunity becomes less attractive.

The model's timing object is the conditional refill rate,

$$R(t) = \Pr(\text{refill on day } t \mid \text{no previous refill}). \tag{4}$$

With perfect attention and knowledge of the deadline, $R(t) = q(t)$ for the first four days.

4.1.2. Memory, pill stocks, and attention

Imperfect deadline memory makes the information in pill stocks economically important. We first specify the daily-use process that determines this information, then the patient's beliefs about elapsed time and her continuation rule.

Assumption 1 (Daily attention) *Each day the patient is attentive with probability* $1-\theta$*, independently across days, where* $0 \leq \theta < 1$*. On an attentive day she takes a pill if available and considers refilling. On an inattentive day she does neither. Before the terminal state, her state does not advance. In state 4, failure to refill ends the window, including when she is inattentive.*

Daily attention links medication use to replenishment. After a missed dose, the patient holds more pills than prescribed daily use would imply, while the reward deadline remains fixed.

Assumption 2 (Imperfect memory of the deadline) *The patient observes her pill state but does not recall where she is in the refill window. Conditional on that state and not having refilled, she weights possible elapsed days in proportion to the probability of reaching them under the pill-state process.*

Here t measures time since the start of this particular supply. The patient uses her remaining pills to infer her position relative to the refill deadline. Following Piccione and Rubinstein (1997), she weights possible elapsed days by the probability of reaching the observed pill state without an earlier refill.

Assumption 3 (Naive continuation) *When evaluating continuation, the patient acts as if she will be attentive on every subsequent day. Without a reminder, she evaluates future eligibility using the belief associated with each future pill state; with a reminder, she knows the calendar deadline.*

Naivety concerns future attention: current eligibility beliefs still reflect past lapses through Assumption 2, but the patient does not anticipate further lapses when evaluating whether to wait. This gives the continuation values in equation (3) and the analytical predictions below. Appendix A 4.2 relaxes naive continuation and examines numerical robustness under partial and accurate anticipation of future inattention.

Lemma 1 (Perceived eligibility) *The perceived probability that the reward has not expired is* $1-\theta^2$ *with two pills,* $(1-\theta)^2$ *with one pill, and zero once no pills remain.*

In particular, a patient can still hold pills after the due date and assign positive probability to being eligible. Conversely, before the deadline, uncertainty discounts the reward relative to a known eligible payment. Pill stock is informative, but cannot replace remembering the date.

4.1.3. How the treatments operate

With incentives alone, the perceived current bonus is

$$g(1)=\gamma(1-\theta^2), \qquad g(2)=\gamma(1-\theta)^2, \qquad g(3)=g(4)=0. \tag{5}$$

The financial-arm decision rule adds $g(s)$ to the refill payoff in equation (3); write its refill probabilities as $q^F(s)$.

Reminders have two possible effects. First, they make the deadline available to the patient. To isolate this information channel, a perfectly anticipated call arrives

at day 2. Knowing the schedule reveals whether today is before, at, or after the deadline.

Second, the reminder intervention can reduce inattention from θ to $\theta^R = \theta + \Delta\theta$, where $-\theta \leq \Delta\theta \leq 0$. Remembering the refill date can make medication use easier to monitor: a patient who should have only a few pills left may recognize that a larger stock reflects missed doses. Reminder communication can also prompt medication-taking and pickup routines, reducing occasions on which these actions fail to come to mind. We summarize these possible salience, self-monitoring and planning responses through $\Delta\theta$, without requiring a particular psychological mechanism or an endogenous choice of attention. Because the model applies the attention change throughout the window, it allows anticipation of the intervention as well as a response to the call.

In the reminders-only arm, information about the date operates through the attention response $\Delta\theta$, which captures the salience and monitoring benefits of the remembered schedule. With incentives, remembering the date also affects the perceived reward directly: it replaces $g(s)$ with the actual bonus $\gamma\mathbb{I}\{t \leq 2\}$.

4.1.4. Model predictions

The following propositions use Assumptions 1–3 and the interior-threshold condition in Appendix A 4.1.

Proposition 1 (Known-deadline benchmark) *Without either friction, incentives raise conditional refilling on days 1 and 2 and leave it unchanged on days 3 and 4.*

The bonus raises the payoff to acting before expiry; after expiry it enters neither the current payoff nor the value of waiting. In this benchmark, knowledge of the deadline concentrates the incentive response in the eligible period.

Proposition 2 (Reminders alone) *If $\Delta\theta < 0$, reminders raise the conditional refill rate on days 1, 2, and 3 relative to control. If $\theta = 0$, there is no initial inattention to reduce and reminders alone have no effect.*

Greater attention increases decision opportunities and advances the stock toward states where replenishment is more urgent. More generally, reminders alone have no effect in the benchmark whenever $\Delta\theta = 0$.

Proposition 3 (Incentives with imperfect memory) *Incentives raise conditional refilling on days 1 and 2. They also raise it on day 3 when $\theta > 0$; with $\theta = 0$, the day-3 effect is zero.*

A patient who missed a dose can reach day 3 with pills still in hand. Using that stock to judge when to return, she may believe that the reward is still available. The perceived reward then makes her more willing to refill, even though the calendar deadline has passed. Imperfect memory thus extends the incentive response beyond the eligible period.

Proposition 4 (Adding reminders to incentives) *(a) The combination raises the day-2 conditional refill rate relative to incentives alone if $\theta > 0$; the rates coincide if $\theta = 0$. (b) The day-1 difference is ambiguous. Information can encourage waiting until the last eligible day, while improved attention increases opportunities to act.*

Information changes both the payoff to acting now and the value of waiting. Before the deadline, learning that another eligible day remains can make waiting attractive. On the deadline, it removes uncertainty about eligibility and about whether another opportunity remains. Information can therefore shift refilling toward the final eligible day.

Proposition 5 (Aggregate on-time refilling) *The on-time refill share is $\Pi = 1 - (1 - R(1))(1 - R(2))$, increasing in each rate. Incentives raise Π, and reminders alone raise it if $\Delta\theta < 0$. For $\theta > 0$, the combination raises Π relative to incentives alone whenever its day-1 rate is no lower; with $\theta = 0$, the two arms coincide.*

Remembering the deadline and paying attention to medication use reinforce one another. Information helps patients target the eligible period, while attention increases their opportunities to act. The aggregate gain depends on the balance between informed waiting and attention: a patient who waits may face a high cost or a lapse on the final eligible day. Appendix A 4.1 derives the timing predictions and the aggregate on-time implication. The numerical exercise in Appendix A 4.2 finds a higher on-time share under the combination in 96.6% of parameter vectors with improved attention under naive continuation, 94.3% under partial anticipation, and 93.3% under accurate anticipation of future inattention. The positive comparison is therefore common across the illustrated parameter ranges, including when patients anticipate future lapses. These uncalibrated numerical comparisons illustrate the mechanism rather than estimate the distribution of patient responses.

From predictions to evidence. The propositions organize the evidence around two timing contrasts: incentives versus control after the deadline (Propositions 1

and 3), and combination versus incentives near the deadline (Proposition 4). Reminders versus control provides a further comparison for the attention response in Proposition 2. We then examine whether gains in timely replenishment extend across repeated opportunities, using cumulative outcomes that retain non-returners.

4.2. Taking the Model to the Data

The model generates distinct predictions for when incentives and reminders affect refilling. We examine these predictions using dated refill records, then assess whether the gains extend to cumulative timely replenishment. These exploratory analyses provide evidence on the proposed mechanism beyond the average treatment effects.

4.2.1. Incentives beyond the deadline and reminders near the deadline

Timing is measured relative to the refill due date defined in Section 2.2, when medication is expected to run out under prescribed daily use. Figure 2 presents the two central timing comparisons. Incentives are associated with higher refilling just after the due date, while adding reminders produces a positive increment near the deadline. To examine these patterns systematically, we divide each refill window into four periods: at least six days early, zero to five days early, one to five days late, and at least six days late. These intervals distinguish early action, proximity to the deadline, and delay. The zero-to-five-days-early interval captures the scheduled reminder window and corresponds to day 2 in the model, when the reminder makes the impending deadline salient.

We estimate the following equation separately for reminders versus control, incentives versus control, and combination versus incentives:

$$a_{ik\tau} = \sum_{j=1}^{4} \beta_j G_i \mathbb{I}\{\tau = j\} + \lambda_\tau + \psi_k + X_i'\kappa + \varepsilon_{ik\tau}. \tag{6}$$

Here $a_{ik\tau}$ indicates a refill by patient i in period τ of refill window k, and G_i indicates assignment to the first arm in the comparison. The observation is a patient–refill-window–period: each observed refill window contributes one observation for each timing period it reaches. Patients with more observed refill windows therefore contribute more observations. Period and refill-number fixed effects, λ_τ and ψ_k, absorb common differences across periods and windows.

The vector X_i contains the PAP controls used in the main analysis. A window contributes while open and leaves the risk set when a refill occurs; each β_j is therefore a covariate-adjusted difference in conditional refill rates. Standard errors are clustered by recruitment day.[11]

Propositions 1 and 3 distinguish the response to incentives under perfect and imperfect deadline memory: only the latter allows a perceived reward to persist after eligibility has expired. We examine this prediction by comparing incentives with control across the four periods in Table 3. Incentives increase early refilling by 7.9 percentage points; their estimated effect one to five days after the due date is 7.0 points (standard error 5.7). The positive post-deadline estimate is consistent with patients using remaining pills to judge eligibility after the reward has expired, although it is imprecisely estimated.

Proposition 4(a) predicts that adding reminders to incentives strengthens refilling near the deadline, when knowing that the reward is still available can encourage action. The corresponding comparison is combination versus incentives. In the zero-to-five-days-early period, the estimated increment is 8.0 percentage points (standard error 4.3). This positive estimate is consistent with the predicted role of a remembered deadline in helping patients use an available reward.

Proposition 2 predicts that reminders alone raise refilling when they improve attention. Comparing reminders with control, we find a 10.2-percentage-point increase in early refilling. This result is consistent with the model's inattention mechanism.[12]

All of the 7 point estimates in Table 3 for which the model gives a positive directional prediction have the predicted sign: reminders versus control in the first three periods, incentives versus control in the first three periods, and combination versus incentives near the deadline.

[11] Only participants who have not yet refilled contribute to later periods, and a new window begins after a refill. Treatment can therefore change which observations enter each comparison. The main specification excludes windows due in January, when calling operations were disrupted and led to a large number of reminders delivered only after the due date; Appendix Table A11 includes all dates for both the full and early-enrollment restricted samples. Appendix A 1.7 documents delivery.

[12] A possible additional mechanism is contact avoidance: because refilling stopped reminder attempts, patients expecting discomfort or privacy costs from contact could also return early. We expect privacy-related avoidance to be limited by our recruitment screen, which required access to a phone number through which participants felt comfortable discussing personal health matters. Contact avoidance can coexist with, and does not contradict, the attention and imperfect-memory mechanisms developed here.

4.2.2. Accumulation of timely refills

The cumulative analysis retains all administratively eligible patients, including non-returners, and gives each patient equal weight at a given horizon. Treatment-induced changes in the number of observed refills therefore do not change patients' relative weights, addressing one limitation of the conditional risk sets in Table 3. Proposition 5 connects conditional refill rates to the on-time share within a refill window; we extend the empirical analysis to timely replenishment across repeated opportunities. Holding the cohort fixed across horizons separately addresses changes in administrative eligibility. For a patient with initial supply B_i, define enrollment-based horizons $H_{ik} = B_i + 30(k-1)$ for $k = 1,2,3,4$. These correspond to the first four evaluation horizons based on initial supply and a standard 30-day dispensing cycle. At each horizon we count on-time events among the first k recorded refills:

$$C_{ik} = \sum_{j=1}^{k} \mathbb{I}\{T_{ij} \leq H_{ik}, \text{ refill } j \text{ is on time}\}. \tag{7}$$

Here T_{ij} is the date of the jth recorded refill, measured from enrollment, and each event is assessed against its refill due date. Every qualifying refill contributes to the count, including an on-time return following an earlier late return. Participants with no qualifying event have a count of zero. At each horizon, every eligible patient contributes exactly one observation, regardless of how many refill visits she makes. This gives patients equal weight within a column, so patients with more observed refill sessions do not receive extra weight as they do in the pooled timing analysis.

We estimate separate regressions for each horizon,

$$C_{ik} = \alpha_k + \sum_{g \neq \text{Control}} \tau_{gk} \mathbb{I}\{G_i = g\} + X_i' \eta_k + u_{ik}, \tag{8}$$

where G_i denotes randomized arm, X_i contains the main analysis's baseline covariates, and standard errors are clustered by recruitment day. As in the timing analysis, we retain only control, incentives, reminders and combination. Each column includes participants whose enrollment date and initial supply allow administrative follow-up through its horizon.[13]

[13]The four-arm regressions use the PAP covariates. Eligibility depends on enrollment information and the administrative cutoff, not on reaching a later refill. Non-returners and earlier death, transfer or loss-to-follow-up endpoints remain included.

Table 4 follows timely replenishment through all four milestones. The combination effects relative to control are 0.09, 0.10, 0.27 and 0.41 additional on-time refills, respectively. The fourth-milestone estimate has a standard error of 0.18 and a p-value of 0.022. At that milestone, the combination also produces 0.34 more on-time refills than incentives alone (standard error 0.15; $p = 0.023$). Appendix Table A12 holds the cohort fixed at the 330 patients eligible through all four milestones and uses the same four arms and specification. In this cohort, the combination effect relative to control rises from approximately zero at the first milestone to 0.41 at the fourth; the fourth-minus-first difference is 0.41 (standard error 0.12; unadjusted $p = 0.002$). These cumulative gains extend the evidence beyond conditional refill risk sets and are not explained solely by changes in the eligible cohort.

4.3. Alternative Channels and the Role of Reminder Communication

Appointments as a framework for action. Appointments can provide a concrete plan for action and help patients commit to a visit. Derksen et al. (2025) show that offering HIV-testing appointments, accompanied by a reminder call, increases testing. Our setting already has routine appointments for medication pickup. At enrollment, before the scheduled visit, patients reported the date of their next pickup appointment. We compare that previously reported date with the first medication refill observed after enrollment, then extend the comparison to subsequent reported appointments. These checks ask whether refill timing aligns with the patient's appointment and whether that alignment differs across intervention groups.

Appendix Figure A2, Panel A, compares first refills with the date reported at enrollment: 29.2% of 761 participants refill exactly on that date, with no detected difference across arms (covariate-adjusted joint $p = 0.713$). Panel B includes initial and subsequent reported appointments: 12.9% of 2382 appointments among 780 participants have an exact-date return (joint $p = 0.956$). Both panels retain non-returners for eligible appointments. Later appointment availability depends on prior refilling, so this broader comparison is descriptive. The alignment illustrates an existing appointment framework; it does not identify the causal effect of offering an appointment. The incremental question in our experiment is how reward-linked reminders help patients act within that framework.

Connecting the date with the reward. Reward-linked communication can help patients connect the refill due date with the benefit of returning on time. In our setting, combination calls communicate both the upcoming refill date and the financial reward for timely return. Remembering a date makes the opportunity easier to locate; remembering its connection to a reward gives patients a reason to act on it. Calls can also clarify the conditions and prompt practical planning for a clinic visit. Personal contact may reinforce ongoing engagement with treatment. These features make communication a useful complement to financial motivation. Our model develops the deadline-memory channel explicitly, with the attention response allowing a remembered benchmark to support medication management. Because the experiment does not separately randomize the recall, salience and planning functions of reminder calls, the evidence supports the proposed account without isolating each component. The broader economic insight is that making an incentive available and helping people use it are distinct parts of program design.

Maintaining engagement across opportunities. Future reward opportunities may also encourage patients to return after a missed deadline, since missing one reward does not invalidate later opportunities. This continuation motive is a separate potential channel. When a short delay postpones subsequent opportunities, returning sooner brings future rewards forward by a few days. Their value is discounted for timing and weighted by the probability of earning them: even with accurate deadline recall, costs or other constraints may prevent timely return at the next opportunity. This motive operates both before and after the current deadline. Appendix A 4.3 formalizes this channel, allowing returns to advance or preserve subsequent opportunities. Reminders may also strengthen this motive by making future rewards more salient and helping patients plan and follow through on timely returns, consistent with the experiment's broader finding that communication adds value to financial incentives.

The experiment establishes that adding reminders improves the incentive program's effect on high medication coverage. The model explains how pill-taking lapses can make remaining stock an imperfect guide to reward eligibility, and how a remembered deadline supplies a benchmark for acting. The timing comparisons examine the predicted post-deadline incentive response and near-deadline reminder increment; the cumulative outcomes show gains in timely replenishment

while retaining non-returners. This evidence supports the role of temporal communication in helping patients use conditional rewards across repeated decisions. The next section considers those gains alongside the program's implementation costs.

5. Discussion and Conclusion

5.1. Cost Effectiveness and Policy Implications

Adding reminders substantially improves the effectiveness of the incentive program at a modest additional cost. In our preferred cost analysis, the assumed implementation budget rises from \$6.50 to \$7.98 per participant, while the estimated increase in high medication coverage (MPR $\geq$ 95%) rises from 8.6 to 23.3 percentage points. Cost per participant increases by approximately 23%, while the estimated treatment effect rises to about 2.7 times its original size. The promised reward per qualifying refill is unchanged.

Panel A of Table 5 reports the preferred implementation budget. Adding reminders costs \$1.49 per participant and raises high coverage by 14.7 percentage points, implying \$10.12 per additional patient achieving high coverage. For every 100 patients offered incentives, adding reminders costs approximately \$149 and produces an estimated 14.7 additional high-coverage patients. The additional budget includes calling costs, rewards for eligible on-time refills and an administrative allowance.

Reminders alone have the lowest estimated cost per additional high-coverage patient, at \$9.01 (Panel A). Their estimated adherence gain is smaller, however, and its confidence interval includes zero. The results describe a choice between smaller gains at low cost and larger gains requiring a larger budget; they do not establish one intervention as the preferred option for every program budget or objective.

All delivery quantities used in the budgets come from Table A4, and costs are averaged over all assigned participants. The preferred budget values each qualifying reminder at \$0.80, allowing for 4 calls at \$0.20 each, and each eligible on-time refill at \$4.83, including the \$3.22 reward and a 50% administrative allowance. Appendix A 6 explains these assumptions. Panel B doubles the call price, raising the incremental cost of adding reminders to \$17.47 per additional high-coverage patient. Panel C budgets one call per eligible reminder cycle and one reward per eligible on-time refill at base prices, without administrative allowances; the

corresponding incremental cost is $6.04. All three scenarios hold observed refill behavior and estimated treatment effects fixed.

The substantial combination effect demonstrates that reminders can improve an incentive program even when delivery is incomplete. More reliable delivery may generate additional gains; the experiment establishes the performance of the programs under the observed operating conditions. Reminder communication is one part of incentive design, alongside the amount and conditions of payment. The experiment estimates the additional medication coverage from combining these tools, and the budget scenarios illustrate its resource implications, while leaving the appropriate balance between program reach and gains per patient to the policymaker's objectives and budget.

5.2. Concluding Assessment

Our study performed among people living with HIV newly initiated on ART in Mozambique shows that financial incentives can achieve substantially more when accompanied by support for remembering when and how to act on them. Adding reminder calls increased high medication coverage by 14.7 percentage points relative to incentives alone. This assignment-based effect establishes the benefit of offering reward-linked reminders under the clinic's operating conditions. The model explains how inattention to an ongoing task can make it harder to keep track of subsequent reward opportunities, and the exploratory timing evidence supports this account. Cumulative outcomes that retain non-returners show gains in timely replenishment across repeated opportunities, including within a fixed cohort.

A central policy implication is that the amount and conditions of payment are only part of incentive design. Participants must also recognize an opportunity, remember its requirements, and organize their actions in time to benefit. Programs can influence each of these steps through the clarity of eligibility rules, the timing and content of communication, and support for planning. Our analysis suggests that such additional assistance can be highly cost-effective. Policymakers should therefore consider communication and follow-through support alongside payment levels when allocating program resources. The relevant question is how different combinations of financial rewards and behavioral support translate a given budget into desired outcomes.

This perspective is especially useful for programs requiring repeated action. Conditional cash transfers may depend on recurring school attendance or health visits; public benefits may require periodic reporting and renewal; chronic disease management requires patients to replenish supplies and maintain daily routines. In these settings, each decision occurs in the context of earlier actions and lapses. Our model illustrates how that history can affect the information people use to judge what they should do next. A clear deadline or timely reminder can provide a benchmark for reassessing progress and acting on the next opportunity. This suggests a role for communication throughout a program, including helping participants resume participation after a missed deadline.

Detailed behavioral data can help make such support more effective. An average treatment effect reveals whether a program changes outcomes, but records of when people act can also reveal early responses, missed deadlines, delayed returns, and continued participation. These patterns can guide the design of reminder schedules, eligibility messages, and opportunities to re-engage. Experiments that vary these features can then distinguish among competing explanations and identify which forms of support are most useful. In health programs, this approach can complement efforts to address financial constraints, information gaps, and social concerns, including the stigma misperceptions examined in this study.

The broader economic lesson is that responses to incentives depend on people's ability to keep track of opportunities and carry intentions into action. Future research can examine how the value of behavioral support varies with task complexity, repetition, and the duration of incentives. For policy design, our findings establish a concrete starting point: helping people use an available reward can substantially increase what an incentive program achieves.

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Table 1: *Effects on medication coverage*

	Primary window (up to 180 days)			Up to 90 days		
	(1)	(2)	(3)	(4)	(5)	(6)
	MPR ≥ 95%	MPR ≥ 80%	MPR (continuous)	MPR ≥ 95%	MPR ≥ 80%	MPR (continuous)
Incentives	0.086	0.128*	0.073**	0.102	0.081	0.048
	(0.052)	(0.065)	(0.032)	(0.079)	(0.077)	(0.029)
	[0.242]	[0.164]	[0.110]	[0.286]	[0.367]	[0.243]
	{0.230}					
Reminders	0.100*	0.086	0.058*	0.119*	0.088	0.040
	(0.053)	(0.063)	(0.031)	(0.066)	(0.064)	(0.028)
	[0.180]	[0.276]	[0.185]	[0.193]	[0.276]	[0.269]
	{0.224}					
Combination	0.233***	0.203***	0.121***	0.197***	0.138**	0.067**
	(0.044)	(0.058)	(0.029)	(0.064)	(0.062)	(0.026)
	[0.001]	[0.010]	[0.002]	[0.027]	[0.118]	[0.089]
	{0.000}					
Information	0.075	0.087	0.037	0.046	0.045	0.005
	(0.048)	(0.057)	(0.029)	(0.064)	(0.062)	(0.026)
	[0.257]	[0.262]	[0.287]	[0.458]	[0.458]	[0.598]
	{0.230}					
Stigma-relieving	0.141**	0.113	0.060	0.062	0.022	0.004
	(0.059)	(0.084)	(0.039)	(0.084)	(0.082)	(0.036)
	[0.104]	[0.276]	[0.258]	[0.458]	[0.566]	[0.611]
	{0.137}					
PAP covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	821	821	821	821	821	821
Recruitment-day clusters	137	137	137	137	137	137
Control mean	0.245	0.453	0.691	0.472	0.613	0.809
Combination minus incentives	0.147***	0.075	0.048*	0.095	0.057	0.019
	(0.050)	(0.052)	(0.025)	(0.069)	(0.060)	(0.024)
	[0.035]	[0.269]	[0.177]	[0.276]	[0.414]	[0.457]
Combination minus reminders	0.133***	0.117**	0.063**	0.078	0.050	0.027
	(0.049)	(0.050)	(0.024)	(0.052)	(0.044)	(0.021)
	[0.063]	[0.110]	[0.077]	[0.264]	[0.356]	[0.292]
Departure from additivity	0.047	-0.011	-0.010	-0.024	-0.031	-0.021
	(0.072)	(0.083)	(0.041)	(0.097)	(0.091)	(0.037)
	[0.474]	[0.611]	[0.572]	[0.572]	[0.533]	[0.483]

Notes: Square brackets report Section 3 sharpened q-values. Covariates follow equation (2). Parentheses contain recruitment-day clustered standard errors. Stars indicate unadjusted two-sided tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Curly braces report two-sided List–Shaikh–Xu FWER-adjusted p-values from `mhtreg` (Steinmayr, 2020) with 10,000 recruitment-day cluster bootstrap replications (seed 31415) for the five treatment-versus-control hypotheses in column (1), with cumulative maxima in bootstrap-p order. Both horizons use the same 821 participants; individual endpoints may precede 90 days. As a separately computed supplement for the prespecified positive alternatives in column (1), the one-sided List–Shaikh–Xu step-down FWER-adjusted p-values are 0.1070 (incentives), 0.0834 (reminders), 0.0001 (combination), 0.1070 (information), and 0.0522 (stigma-relieving), using the same 10,000 recruitment-day bootstrap draws and the cumulative-maximum sequential convention.

Table 2: *Prespecified treatment heterogeneity: MPR* $\geq$ 95%

	Gender		Education		Food security		Travel time		ART missed-dose knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Men	Women	0–6 years	7+ years	Insecure	Secure	<30 min.	≥30 min.	0–3 correct	4 correct
Incentives	0.125*	0.087	0.212**	0.010	0.100	0.042	0.004	0.106	-0.022	0.159**
	(0.070)	(0.078)	(0.083)	(0.064)	(0.081)	(0.057)	(0.071)	(0.073)	(0.091)	(0.064)
	[0.198]	[0.356]	[0.086]	[0.603]	[0.314]	[0.458]	[0.611]	[0.269]	[0.572]	[0.094]
Reminders	0.201**	0.029	0.169*	0.090	0.025	0.111*	0.038	0.118*	0.047	0.109
	(0.084)	(0.061)	(0.091)	(0.072)	(0.087)	(0.061)	(0.079)	(0.064)	(0.089)	(0.070)
	[0.105]	[0.514]	[0.185]	[0.314]	[0.559]	[0.189]	[0.514]	[0.185]	[0.490]	[0.257]
Combination	0.366***	0.145**	0.292***	0.216***	0.284***	0.207***	0.228***	0.224***	0.164**	0.283***
	(0.079)	(0.063)	(0.080)	(0.059)	(0.067)	(0.062)	(0.081)	(0.065)	(0.071)	(0.065)
	[0.001]	[0.110]	[0.008]	[0.007]	[0.002]	[0.015]	[0.050]	[0.012]	[0.110]	[0.001]
Information	0.099	0.062	0.206***	0.008	0.137*	0.010	-0.018	0.119**	0.009	0.120**
	(0.076)	(0.059)	(0.076)	(0.055)	(0.072)	(0.055)	(0.067)	(0.056)	(0.077)	(0.059)
	[0.286]	[0.369]	[0.063]	[0.605]	[0.177]	[0.598]	[0.564]	[0.131]	[0.611]	[0.153]
Stigma-relieving	0.213***	0.116	0.246***	0.125*	0.158*	0.118	0.029	0.201***	0.176**	0.089
	(0.076)	(0.080)	(0.082)	(0.065)	(0.086)	(0.077)	(0.076)	(0.064)	(0.084)	(0.074)
	[0.050]	[0.269]	[0.031]	[0.177]	[0.189]	[0.264]	[0.528]	[0.026]	[0.134]	[0.318]
PAP covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	335	486	304	511	361	449	344	477	385	436
Recruitment-day clusters	120	131	125	132	124	134	123	130	121	133
Control mean	0.195	0.277	0.158	0.288	0.222	0.259	0.279	0.222	0.304	0.180
Difference between subgroups (second minus first)										
Incentives	-0.038		-0.202**		-0.057		0.102		0.181	
	(0.101)		(0.100)		(0.084)		(0.096)		(0.112)	
	[0.528]		[0.148]		[0.467]		[0.367]		[0.245]	
Reminders	-0.173*		-0.079		0.086		0.081		0.062	
	(0.097)		(0.114)		(0.105)		(0.101)		(0.115)	
	[0.194]		[0.458]		[0.450]		[0.457]		[0.487]	
Combination	-0.221**		-0.077		-0.077		-0.005		0.119	
	(0.102)		(0.103)		(0.094)		(0.109)		(0.102)	
	[0.121]		[0.458]		[0.450]		[0.611]		[0.325]	
Information	-0.037		-0.198**		-0.127		0.138*		0.110	
	(0.087)		(0.087)		(0.083)		(0.078)		(0.096)	
	[0.527]		[0.110]		[0.262]		[0.198]		[0.334]	
Stigma-relieving	-0.096		-0.122		-0.040		0.172**		-0.087	
	(0.107)		(0.085)		(0.109)		(0.086)		(0.105)	
	[0.424]		[0.269]		[0.528]		[0.153]		[0.450]	

Notes: Square brackets report Section 3 sharpened q-values. Parentheses contain recruitment-day clustered standard errors. Stars use unadjusted two-sided tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Each regression omits the split-defining covariate and its missingness indicator. Education and food-security splits exclude missing values; knowledge uses the four-item index. Bottom-panel differences use joint recruitment-day clustered covariance.

Table 3: *Treatment differences in conditional refill rates*

	(1) Reminders vs. control	(2) Incentives vs. control	(3) Combination vs. incentives
At least 6 days early	10.158***	7.906***	-2.171
	(2.755)	(2.436)	(2.689)
0–5 days early	3.794	1.202	8.029*
	(4.668)	(5.387)	(4.261)
1–5 days late	1.958	7.042	5.361
	(6.223)	(5.725)	(6.068)
At least 6 days late	3.640	5.023	-2.921
	(7.855)	(8.163)	(10.740)
PAP covariates	Yes	Yes	Yes
Period fixed effects	Yes	Yes	Yes
Refill-number fixed effects	Yes	Yes	Yes
Person-period observations	1539	1586	1819
Participants	213	215	244
Recruitment-day clusters	37	37	41

Notes: Each column is a pairwise regression of the refill indicator on treatment interacted with the four timing periods, period fixed effects, refill-number fixed effects, and the PAP controls. Coefficients are percentage points. Recruitment-day clustered standard errors are in parentheses. Windows due in January 2020 are excluded; Appendix Table A11 reports the inclusive specification. Periods contribute while a refill window remains open. Stars denote nominal two-sided p-values: *** $p < .01$, ** $p < .05$, * $p < .10$.

Table 4: *Treatment effects on cumulative on-time refills*

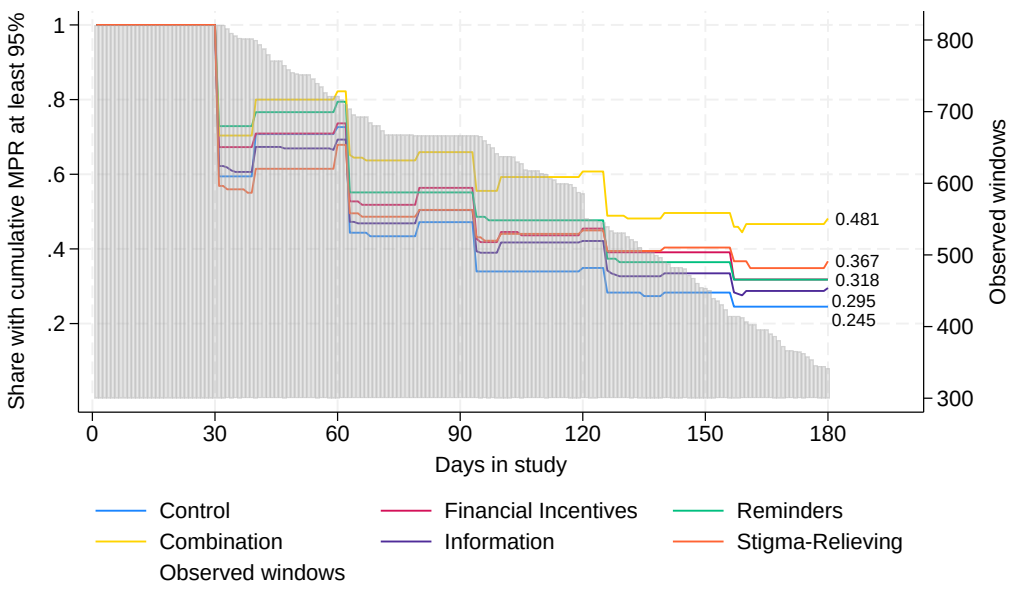
	(1) First milestone	(2) Second milestone	(3) Third milestone	(4) Fourth milestone
Financial incentives	0.092	0.006	0.124	0.074
	(0.098)	(0.152)	(0.161)	(0.179)
Reminders	0.130	0.159	0.165	0.240
	(0.086)	(0.122)	(0.153)	(0.180)
Combination	0.092	0.100	0.270*	0.414**
	(0.079)	(0.122)	(0.154)	(0.175)
Combination − incentives	-0.000	0.094	0.146	0.340**
	(0.090)	(0.134)	(0.138)	(0.145)
Count horizon	B_i	$B_i + 30$	$B_i + 60$	$B_i + 90$
Outcome range	0–1	0–2	0–3	0–4
PAP covariates	Yes	Yes	Yes	Yes
Participants	458	395	362	330
Recruitment-day clusters	78	67	61	52
Control mean (refills)	0.443	0.764	0.902	1.091

Notes: Each column includes all participants in control, incentives, reminders and combination with administrative follow-up available through its horizon. Treatment rows report effects relative to control; the final effect row compares combination with incentives. Outcomes count on-time events among the first k recorded refills by the stated horizon; B_i is initial pill supply. Non-returners and earlier death, transfer or loss-to-follow-up endpoints remain included. Covariates follow equation (8). Parentheses contain recruitment-day clustered standard errors. Stars denote nominal two-sided p-values: *** $p < .01$, ** $p < .05$, * $p < .10$.

Table 5: *Assumed implementation budgets and medication coverage*

Comparison	Additional cost per participant (US$)	High-coverage gain pp (95% CI)	Cost per additional high-coverage patient (US$)
A. Preferred implementation budget			
Add reminders to incentives	1.49	14.7 (4.9, 24.5)	10.12
Add incentives to reminders	7.09	13.3 (3.5, 23.1)	53.25
Incentives vs. control	6.50	8.6 (-1.8, 18.9)	75.62
Reminders vs. control	0.90	10.0 (-0.4, 20.4)	9.01
Combination vs. control	7.98	23.3 (14.5, 32.0)	34.31
B. Sensitivity: doubled call price			
Add reminders to incentives	2.56	14.7 (4.9, 24.5)	17.47
Add incentives to reminders	7.27	13.3 (3.5, 23.1)	54.62
Incentives vs. control	6.50	8.6 (-1.8, 18.9)	75.62
Reminders vs. control	1.79	10.0 (-0.4, 20.4)	18.02
Combination vs. control	9.06	23.3 (14.5, 32.0)	38.95
C. Sensitivity: simple budget without administrative allowances			
Add reminders to incentives	0.89	14.7 (4.9, 24.5)	6.04
Add incentives to reminders	4.63	13.3 (3.5, 23.1)	34.79
Incentives vs. control	4.33	8.6 (-1.8, 18.9)	50.42
Reminders vs. control	0.59	10.0 (-0.4, 20.4)	5.91
Combination vs. control	5.22	23.3 (14.5, 32.0)	22.43

Notes: Costs are assumed implementation budgets per assigned participant over primary follow-up. Panel A budgets four calls at US$0.20 per qualifying reminder in Table A4, and US$4.83 per eligible on-time refill (the US$3.22 reward plus a 50% administrative allowance). Panel B doubles the call price, holding other assumptions fixed. Panel C budgets one call per eligible reminder cycle and one reward per eligible on-time refill at base prices, without allowances. All panels hold refill behavior and Table 1 effects on MPR $\geq$ 95% fixed. Intervals are ordinary recruitment-day-clustered 95% confidence intervals; ratios use unrounded probability effects. Research, routine care and costs outside the stated allowances are excluded. Appendix A 6 gives the calculation.



Notes: Lines show group shares with cumulative MPR $\geq$ 95%. Indicators remain fixed after individual observation endpoints through day 180; end labels therefore give primary-window means. Gray bars count participants still under observation.

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

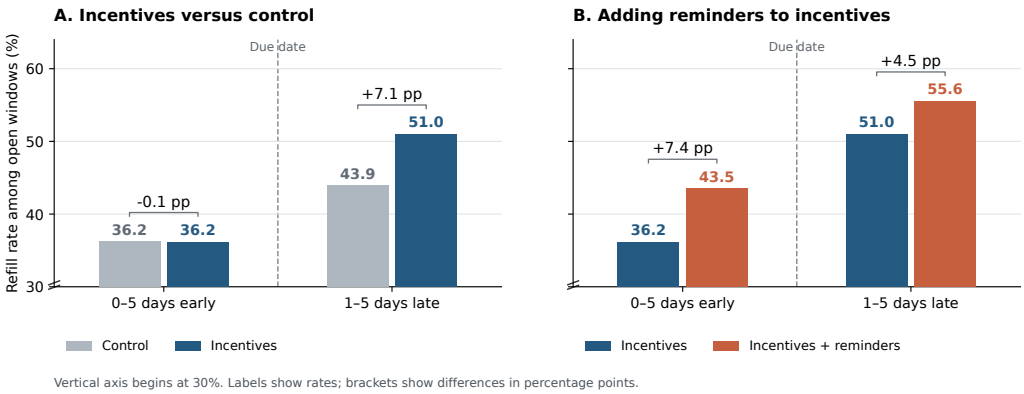


Figure 2: Incentives, reminders, and the timing of refills

Notes: Each bar is the share of open refill windows ending in a refill during the indicated period. Brackets show unadjusted differences in percentage points. The figure focuses on the two periods adjacent to the due date. Windows due in January 2020 are excluded. Panel A uses 503 refill windows from 211 participants, and Panel B uses 593 windows from 240 participants; later-period rates use windows still open.

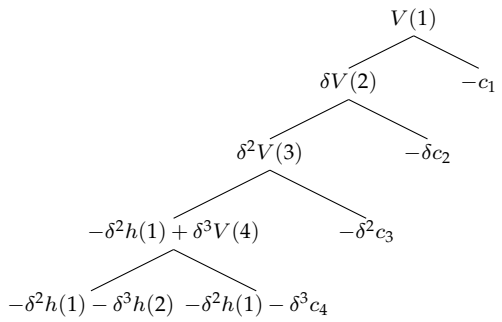


Figure 3: The four-period refill decision without frictions

Notes: Right branches represent refilling at cost c_t; left branches represent waiting. Initial supply covers periods 1–2; waiting without medication incurs $h(1)$ in period 3 and terminal cost $h(2)$ in period 4. Values are discounted to day 1; continuation values average over future cost draws and optimal choices.

Supplementary Appendix for Online Publication

The appendix is organized by themes.

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A 1. Design and Implementation

This appendix documents the study chronology, sample, intervention content and delivery, and implementation of the registered plan.

A 1.1. Study Chronology

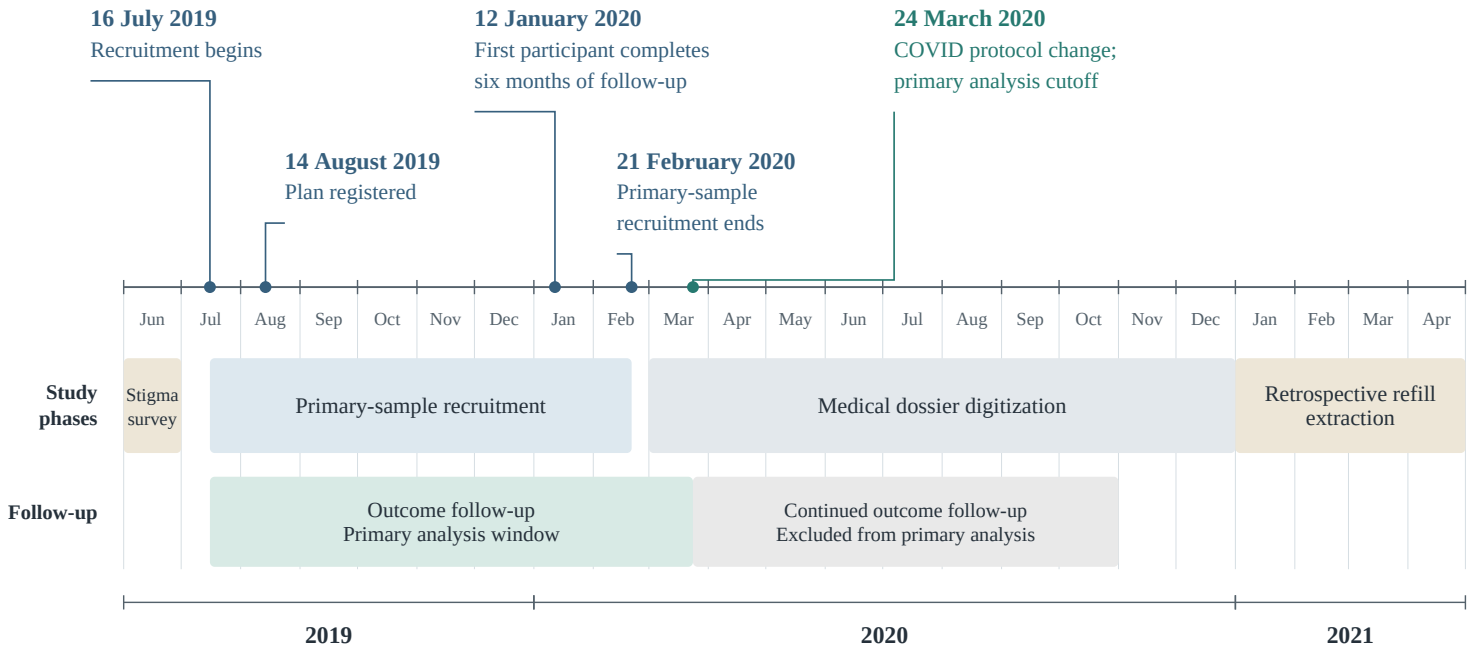


Figure A1: Study Chronology

A 1.2. Randomization, Sample Construction, and Baseline Characteristics

We randomized a prespecified list of working days in blocks of seven consecutive working days, assigning each of the seven original study groups to one day within each block. All participants recruited on a given day received that day's assignment. Assignment appeared at the end of the recruitment survey. Recruiters were not told future assignments or the block structure, but could infer the current day's group after their first interview. The investigators do not expect this knowledge to have affected recruitment, because patients arrived through the clinic's usual flow rather than in response to study assignment.

Unexpected suspensions of recruitment left some scheduled working days without recruits; later dates retained their scheduled assignments. We adjusted the analysis strata to ensure that each contains every study group. The primary sample spans 17 analysis strata and 137 recruitment-day clusters. Standard errors are clustered by recruitment day; Table A8 separately uses the original assignment-block controls.

Table A1 traces the primary sample and defines the early-enrollment restricted cohort. Table A2 reports participants and recruitment-day clusters by arm, together with baseline characteristics and their availability.

Table A1: *Primary sample construction*

	N
Assigned in-person recruits by 21 February 2020	837
Linked to refill records	832
With starting pill quantity (primary sample)	821
Restricted cohort: enrolled by 26 September 2019 (allows 180 days before the COVID cutoff)	378

Table A2: *Baseline characteristics by arm*

	Control	Incentives	Reminders	Combination	Information	Stigma-relieving	Joint p
Female	0.613	0.582	0.579	0.533	0.606	0.633	0.642
	(0.489)	(0.496)	(0.496)	(0.501)	(0.490)	(0.484)	
Years of education	6.923	6.873	7.000	7.358	7.008	6.824	0.904
	(3.579)	(3.812)	(3.852)	(3.390)	(3.613)	(3.761)	
Food secure	0.563	0.591	0.594	0.541	0.560	0.472	0.137
	(0.498)	(0.494)	(0.493)	(0.500)	(0.497)	(0.502)	
Travel time (minutes)	33.934	33.091	41.785	34.222	35.398	38.459	0.408
	(20.722)	(26.938)	(51.013)	(24.295)	(27.498)	(26.728)	
ART missed-dose knowledge (0–4)	3.019	3.218	3.271	3.185	3.169	3.028	0.071
	(1.219)	(1.120)	(0.996)	(1.038)	(1.166)	(1.205)	
Participants	106	110	107	135	254	109	
Recruitment-day clusters	18	19	19	22	41	18	

Notes: Parentheses contain standard deviations. Joint tests use assignment-block fixed effects and recruitment-day clustering. Available education counts, in column order: 104, 110, 106, 134, 253, 108. Available food-security counts: 103, 110, 106, 133, 250, 108. Other baseline variables are complete.

A 1.3. Information Intervention Protocol

Participants watched the information video on a tablet, with an enumerator available to assist and clarify. The videos are available in Portuguese and Sena.[14] An English rendering follows.

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-

[14]Portuguese: https://drive.google.com/file/d/1W3pJOJVmzuRTOKkrhydlk65-3Ojz9x6q/view. Sena: https://drive.google.com/file/d/11likL7lYE73J35_1QkXw7LzekfcByHRs/view.

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 try not to miss any doses. ART works best when you follow the prescribed schedule closely. Do not worry too much if you miss a dose by accident, but try to keep up with the schedule and avoid missing more than one or two doses per month.*
- *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 1.4. Munhava Stigma Belief Survey

The *Munhava Stigma Survey*, administered in June 2019, sampled 442 adult residents through a random-walk procedure centered at Munhava Health Center

(University of Michigan IRB exemption HUM00141271, 14 February 2018). Table A3 reports its five questions, the responses classified as stigmatizing, and the corresponding shares used in the intervention.

Table A3: *Stigma information: protocol benchmarks*

Question	Stigmatizing answer	Share
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
Do you agree? I would be ashamed if someone in my family had HIV	Yes	0.147
If a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in school?	No	0.062
Do you think that children living with HIV should be able to attend school with children who are HIV negative?	No	0.073
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 1.5. Stigma-Relieving Intervention Protocol

In the baseline recruitment survey, participants estimated community responses to the five questions in Table A3. At the end of the recruitment survey, once assignment to the stigma-relieving group was revealed, the enumerator read the corresponding paragraph below for each item on which the participant overestimated stigma, comparing the participant's guess with the survey benchmark. Information was withheld for other items to avoid increasing perceived stigma.

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 overestimated the share giving the stigmatizing response for that item. For the first two questions this corresponds to a higher guessed share of "Yes" answers; for the three supportive questions it corresponds to a lower guessed share of "Yes" answers. The dynamic insertion of their guessed value (indicated above by X%) was handled through the survey software.

A 1.6. Reminder Calls and Graduation Protocol

The study followed recruited patients on a 180-day schedule after enrollment, with graduation at the end of that period. This schedule applied to all recruits, including those whose 180-day period extended beyond 24 March 2020. The COVID cutoff limits the primary analysis window; it does not redefine the study's 180-day follow-up and graduation schedule. Death, transfer, and inactivity can also shorten individual analysis windows under the measurement rules below.

The English scripts below are edited for presentation. Shared introductions and closing instructions are shown once, with the arm-specific text separately.

The scheduled refill date communicated in these calls is the refill due date defined in Section 2.2: the day medication is expected to run out under prescribed daily use.

Reminder calls. The caller first confirms the participant's name and introduces the team:

> *I am calling on behalf of the "Interventions to improve HIV antiretroviral therapy adherence" research team.*

Both reminder arms receive the following message:

> *We would like to remind you that your next ART medication refill is scheduled on [date]. It is essential to refill medication before you run out of stock. Please plan to refill accordingly. Thanks for your attention.*

For the combination arm, the caller adds:

> *If you refill on or before the scheduled date, we will send you a reward of 200 meticais through M-Pesa to your phone number shortly.*

Graduation calls at the end of 180 days. The graduation scripts for the incentive, reminder, and combination arms share this introduction:

> *Good morning / good afternoon. My name is [research team member]. I call on behalf of the University of Michigan and the Beira Operational Research Center (CIOB), who are conducting a study called "Interventions to improve adherence to HIV antiretroviral therapy in Beira."*

The caller then reads the message for the participant's arm:

Incentives. *We want to inform you that your participation in our study has ended. This means that we will no longer continue to send you the 200 meticais amount corresponding to the incentive for your on-time refill of the medication. The research team thanks you for your participation.*

Reminders. *We want to inform you that your participation in our study has ended. This means that we will no longer continue to call you to remind you about your medication refill date. The research team thanks you for your participation.*

Combination. *We want to inform you that your participation in our study has ended. This means that we will no longer call you to remind you about your next refill date, and we will not continue to send you the 200 meticais amount corresponding to the incentive for your on-time refill of medication. The research team thanks you for your participation.*

All three scripts close with the instruction:

> *You must continue to attend appointments at the health center, pick up your medications and take them daily, as instructed by doctors or health clinic staff.*

A 1.7. Intervention Delivery

The intervention used the refill due date defined in Section 2.2 for reminder scheduling and reward eligibility. Delivery was subject to practical field constraints: patients could be unreachable, calls or transfers could be delayed or

unsuccessful, and the real-time administrative records could be incomplete or have reporting lags. These imperfections concern delivery and documentation of the intended intervention; they do not change its due-date rule. Table A4 summarizes documented delivery in person-refill cycles that either end in an observed refill or reach their due date within primary follow-up. Cycles still unfinished before that date are excluded from all denominators.

Reminders. Calls were scheduled to begin three working days before the refill due date, with repeated attempts until the due date or refill, whichever came first. Table A4 reports qualifying successful contact by the due date within the corresponding refill cycle, including contacts on the refill date. By design, reminders stopped once a patient refilled. On-time refills before a reminder therefore fulfilled the intended rule and did not constitute reminder-delivery failures. Among 240 patients with at least one evaluable cycle requiring a reminder, 75 have no qualifying successful contact during those cycles. Unavailable or switched-off phone numbers were common recorded obstacles. Calling operations were disrupted in January.

Payments. Patients qualified for a reward by refilling on or before the refill due date. Successful transfers are linked to the corresponding participant and refill date using the reconciled refill history, including transfers sent after the analysis window. Recorded obstacles included unavailable phones, unavailable mobile-money accounts and account-name mismatches. Table A4 measures payment delay from the refill due date to the transfer date.

Treatment effects are estimated by randomized assignment, without conditioning on successful contact or payment receipt. They therefore measure the effectiveness of offering the programs under the operating conditions of the study.

Delivery comparisons. The combination-minus-reminders difference in timely contact rates is 5.8 percentage points (standard error 6.3; two-sided $p = 0.367$). The combination-minus-incentives difference in matched payment rates is -5.4 percentage points (standard error 6.4; two-sided $p = 0.400$). These descriptive comparisons use the cycle definitions and denominators in Table A4.

Table A4: *Reminder and payment delivery by treatment arm*

	Incentives	Reminders	Combination
Eligible person-refill cycles	333	315	415
On-time refill before reminder (%)	—	30.79	25.54
Reminder delivered (%)	—	38.10	43.86
Neither on-time refill nor reminder (%)	—	31.11	30.60
Mean / median reminder lead (days)	—	2.4 / 2	2.3 / 2
Eligible on-time refills for payment	148	—	193
On-time refills with a linked payment (%)	65.5	—	60.1
Mean / median payment delay (days)	13.9 / 7	—	9.8 / 6

Notes: Eligible cycles have an observed refill or reach their due date within primary follow-up. A refill is on time if it occurs on or before the due date. The three reminder categories partition eligible cycles. Each cycle is counted once, and reminder lead is measured from the first qualifying contact to the due date. The payment percentage is the share of eligible on-time refills with a successful linked transfer. Payment records must concern refills recorded within primary follow-up; transfers sent afterward are included.

A 1.8. Implementation of the Pre-Analysis Plan

Table A5 maps the registered design and administrative outcomes to their implementation. Detailed measurement rules appear in Appendix A 2.

Table A5: *Implementation of the Registered Design and Administrative Outcomes*

Topic	Registered plan	Current implementation
Assignment and arms	Seven groups; the plan describes individual-level random assignment.	Recruitment-day assignment in seven-working-day blocks. The intended information-plus-stigma arm received information only and is pooled with information; intended assignment is a robustness check.
Calendar controls and inference unit	Enrollment-week and enumerator fixed effects, plus the stated baseline covariates.	Same substantive covariates, education and food-security missingness indicators, enumerator effects, and 17 adjusted analysis strata. Recruitment-day clustered standard errors; original-block controls in robustness.
Medication coverage	MPR $\geq$ 95% over six months; $\geq$ 80%, continuous MPR, and three-month variants. Windows end earlier for transfer, opt-out, death, or LTFU.	Primary outcomes: up to 180 days, capped at 24 March 2020, with earlier death, transfer, or adjusted inactivity endpoints; short-window outcomes cap these at 90 days. Table A7 removes COVID censoring. Fieldwork retained the 180-day graduation schedule.
Appointment attendance	Share of scheduled visits completed on the scheduled date or up to seven days earlier, with the stated truncation rules.	Dossier-based share of enrollment months with a clinical or psychosocial visit. Visits within seven days of an episode's first date are merged; the final partial month is weighted. Uses primary MPR endpoints.
Death / loss to follow-up	No contact at least 90 days after a missed scheduled appointment; transfers and opt-outs excluded and deaths retained.	Composite death/LTFU uses more than 120 days since last recorded contact, with retrospective confirmation. The observable denominator requires at least 120 days of observation or a recorded death, and excludes transfers ($N = 588$).

A 2. Measurement and Survey Instruments

This appendix connects each source to its reported outcomes, then gives the reconciliation, endpoint and scoring rules. Refill-based medication coverage and dossier-based clinic attendance use distinct source streams. Table A6 summarizes survey and referral outcomes and their available samples.

A 2.1. Data Sources and Collection

Refill records. During 2019–20, a research assistant checked the clinic pharmacy's dispensing records daily, recording patient identifiers and doses dispensed. In January–April 2021, we supplemented these real-time histories with retrospective records transcribed from the clinic statistical department's electronic information system. These two streams supply MPR. The real-time log also updated refill deadlines for reminders and on-time financial incentives. Both sources are subject

to missingness or human error, thus we reconcile the records in consultation with Munhava Health Center staff for analysis.

Medical files. After participants' observation windows, we digitized their physical medical dossiers, maintained from initial HIV diagnosis. These contain demographics, laboratory results, ART and other health history, and scheduled and actual clinical and psychosocial visit dates. They supply attendance outcomes and death/LTFU evidence; adjudicated clinic evidence also informs follow-up endpoints.

Baseline survey and referral coupons. An in-person survey immediately after enrollment, conducted privately at the clinic, collected demographic, socioeconomic and health information, HIV knowledge, personal stigma experiences, community stigma beliefs, and initial pill holdings for adherence construction. All arms received advice to follow their prescribed ART regimen and three to five barcoded HIV-test referral coupons for family members or sexual partners. Coupons anonymously linked redemptions to participants. Each was valid for one month and redeemable at Munhava for 100 MT upon proof of testing, without disclosure of the result; participants received another 100 MT if at least one linked coupon was redeemed. Section A 2.4 gives the implemented validity and censoring rules.

Follow-up surveys. The approximately five-minute intermediate phone survey, administered one to two months after enrollment, remeasured HIV knowledge and community stigma beliefs to assess short-term intervention effects. The intermediate survey experienced attrition. Table A6 reports observation rates for the analyzed intermediate outcomes, whose interpretation is limited by response selection.

A post-graduation phone survey in October–December 2020 covered self-reported adherence, pandemic-related care changes, and perceived intervention usefulness. It was not prespecified in the analysis plan. Completed interviews are available for 417 of 821 primary-sample participants (50.8%). Given substantial nonresponse and collection after the primary observation windows, these responses are excluded from reported analyses.

A 2.2. Reconciling the Two Refill Records

We reconcile two sources of refill records: tracker data and retrospective digitization data. Enrollment supply is starting stock, not a subsequent refill. For each

outcome window, eligible refills are strictly after enrollment and on or before the observation endpoint, using source coverage appropriate to that window. An exact participant–date match is one double-confirmed event, retaining both sources' recorded attributes.

Refill dates. Two dates for the same participant are potential representations of the same event if they are no more than 15 calendar days apart, or if their recorded next-refill appointment dates share a nonmissing date on or after both refill dates. The shared-appointment criterion also applies when refill dates are more than 15 days apart.[15] We process double-confirmed dates first, then retrospective-only dates, then tracker-only dates, in ascending date order within each category. All double-confirmed dates are retained. A single-source date is absorbed into the first conflicting retained date in that acceptance order; dates supported solely by the same source do not absorb one another. Otherwise, it is retained. Absorbed dates cannot trigger further absorption, and absorbing a record does not upgrade the retained date's source support. Thus the rule does not chain date clusters or select the nearest date. We impose no participant-level subset exemption, no five-refill cap, and no automatic deletion for overlapping supply.

Dose quantities. Quantities are daily doses under the one-dose-per-day convention. For each source separately, we first use its nonmissing quantity at the retained date; if absent, we use that source's quantity evidence from linked absorbed records. The same direct-record-before-linked-record precedence applies to appointment evidence. Distinct candidates are retained and within-source ambiguity is flagged.

A single distinct candidate quantity is accepted. If neither source supplies a quantity, we assign 30 doses before 24 March 2020 and 90 doses on or after that date, as suggested by clinic technicians. Conflicting candidates are evaluated using recorded appointment dates strictly after the retained refill. We select an appointment-supported quantity only if every valid appointment supports the same candidate. If this unanimity condition fails, we use the unique retrospective candidate; if there is no unique retrospective candidate, the unique tracker candi-

[15] We adopted the 15-day rule in consultation with the Munhava Health Center staff. In practice, intervals of no more than 15 days between visits were rare and occurred only in medical emergencies; refills (each refill is 30 doses minimum) within 15 days would be flagged as suspicious behavior if identified and rarely happened. As a result, we treat refills no more than 15 days apart as possible typos unless they are doubly-confirmed.

date; and otherwise the smallest candidate. Actual subsequent refill dates do not determine quantities.

Medication coverage and follow-up. MPR uses the reconciled daily possession series under one prescribed dose per day, carrying remaining stock into subsequent refill accounting. A day is covered under the positive-stock-or-refill convention. Table A8 separately omits the enrollment-day dose deduction and removes death and loss-to-follow-up censoring while retaining transfer censoring. Its source-only columns reconstruct coverage from each accepted refill source separately, retaining common starting stock and clinical records and rebuilding source-specific inactivity endpoints. Both horizons use days after enrollment through the primary endpoint, with the shorter series capped at 90 days.

Follow-up endpoints. Primary follow-up is up to 180 days after enrollment, capped at 24 March 2020 and shortened by the existing clinically adjudicated death, transfer and adjusted inactivity rules. Last activity is rebuilt from the retained refill history and clinic contact evidence. For loss to follow-up, the endpoint is 120 days after last activity, rather than the last-contact date itself. Later contact can reverse an inactivity classification but does not extend the COVID cutoff. Transfers are excluded from the secondary death/LTFU outcome. The early-enrollment cohort retains its original membership; early endpoints can still shorten individual windows. Clinic attendance uses the rebuilt primary endpoints.

Follow-up without COVID censoring. The full-sample check in columns (4)–(6) of Table A7 removes the COVID cutoff and applies the same reconciliation rule throughout, including the 30/90-dose defaults when quantity is missing. Death, transfer and inactivity endpoints are rebuilt before constructing coverage; starting stock is unchanged.

A 2.3. Clinic Attendance

AAR uses accepted clinical and psychosocial visit dates in the 808 linked medical dossiers during the primary MPR observation window; the 13 unlinked files remain missing. We deduplicate dates across services and combine visits occurring within seven days of the first visit into one episode, without chaining successive seven-day gaps. Each episode is assigned to the enrollment-based month containing its first date (days 1–30, 31–60, and so on), and each month

receives at most one attendance credit. AAR is the share of these months with a visit, weighting a partial final month by its observed fraction.

A 2.4. Survey Items, Scoring, and Referral Outcomes

Knowledge and stigma changes equal the intermediate follow-up index minus its baseline value and require both indices. These respondent-only outcomes do not establish population-wide changes when response differs across arms. Neither survey outcomes nor referral is censored at the primary MPR endpoint, because each measures a different horizon. Table A6 reports outcome-specific availability and levels.

A 2.4.1. Baseline knowledge of the risks of missing ART doses

The baseline covariate and subgroup index counts correct responses to four recruitment-survey items about the consequences of missing ART doses: increased HIV transmission risk, drug resistance and treatment failure, becoming sick with AIDS, and impaired immune function after stopping ART. Positively worded items score one for agreement or strong agreement; negatively worded items score one for disagreement or strong disagreement. Other responses score zero, giving a score from 0 to 4. This index is distinct from the transmission-knowledge index used for the secondary change outcome below.

A 2.4.2. HIV transmission knowledge

The knowledge index counts correct answers to the four comparable questions below (range 0–4); other answers, including don't-know, score zero. The fifth registered item was not re-asked. The immediate seven-question intervention comprehension score is a separate measure.

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**)

A 2.4.3. Beliefs about social stigma

The index is the mean of available oriented responses (range 0–10), rather than the registered five-item sum (0–50). For 44 of 511 paired respondents with partially

missing items at baseline or follow-up, it averages only answered items. Items 1–2 use the guessed number of "Yes" answers; items 3–5 use ten minus that number, so higher scores indicate greater perceived stigma.

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?
2. Do you agree: I would be ashamed if someone in my family had HIV?
3. If a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in school?
4. Do you think that children living with HIV should be able to attend school with children who are HIV negative?
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?

A 2.4.4. Test referral

Referral equals one if someone presents proof of an HIV test and a participant-linked barcode coupon to the clinic study team within the implemented 31-day window after recruitment, and zero otherwise when the full window is observed. Redemption records end on 13 March 2020; the 33 participants without a complete validity window remain missing, giving N=788.

Table A6: *Survey and referral outcomes*

	Primary cohort			Control		
	N	Mean	SD	N	Mean	SD
Successful test referral	788	0.131	0.337	100	0.120	0.327
Change in four-item knowledge	518	0.268	1.253	60	0.350	1.246
Change in stigma belief	511	-1.824	2.396	59	-1.953	2.031

Notes: Outcome-specific samples within the primary cohort of 821. Referral uses the 31-day validity window; incomplete windows are missing. Knowledge change uses four comparable questions; stigma change uses the available-item mean (0–10). Both changes require baseline and follow-up responses. See Appendix A 2.4 for scoring. Across arms, knowledge-change observation ranges from 56.6% to 72.0%; stigma-change observation ranges from 55.7% to 72.0%.

A 2.5. Clinical Motivation and Adherence Benchmarks

How ART works and how it is taken. HIV damages the immune system by infecting CD4 cells. Antiretroviral drugs inhibit viral replication at different stages; combination treatment can suppress viral load and allow immune recovery. Sustained suppression reduces morbidity and mortality and prevents sexual transmission when viral load remains undetectable. ART does not eliminate the infection and must be continued (National Institutes of Health, 2025b). The relevant oral regimens are taken on a regular daily schedule, around the same time each day. Fixed-dose combinations put multiple active drugs into one tablet. Efavirenz-containing combinations are generally taken on an empty stomach, preferably at bedtime, whereas exact instructions depend on the prescribed regimen (National Institutes of Health, 2026a, National Library of Medicine, 2025).

Regimens recorded in this study. The medical-file instrument records initial first-line ART, subsequent visit-level regimens, and free-text dosing instructions. Most nonmissing initial-regimen entries in the cleaned clinical panel specify tenofovir and lamivudine combined with either dolutegravir or efavirenz. In the visit-level dosing fields, common instructions are "1+0+0" or "0+0+1," denoting a tablet in the morning or evening. Three active ingredients can thus be delivered in a single daily tablet; pill burden depends on the formulation prescribed.

Benefits and tolerability. Effective ART can sustain viral suppression and long-term health, but its benefits require continued treatment. Side effects depend on the regimen and patient: common complaints include nausea, diarrhea, headache, fatigue and sleep disturbance. Efavirenz can cause dizziness and abnormal dreams (National Institutes of Health, 2026a); some toxicities, including renal or bone effects with tenofovir disoproxil fumarate, require clinical monitoring. These are general clinical considerations, not measured treatment effects or estimates of side-effect prevalence in our sample (National Institutes of Health, 2026b, 2025a).

Pharmacy-refill measures provide evidence on medication availability and are associated with virologic outcomes (Sangeda et al., 2014). Our prespecified MPR $\geq$ 95% outcome uses an established high-adherence benchmark (Paterson et al., 2000); clinical requirements vary across ART regimens (Bezabhe et al., 2016). We therefore interpret the 95% and 80% thresholds as medication-coverage benchmarks, alongside continuous MPR, rather than as universal clinical cutoffs. MPR does not establish ingestion or viral suppression.

A 3. Supporting Treatment-Effect Evidence

These checks assess sample, assignment and measurement choices, followed by secondary outcomes. Baseline characteristics appear in Table A2.

Table conventions. Unless a column specifies otherwise, Tables A7–A9 use the main covariates: gender, education, food security, travel time, baseline knowledge of the risks of missing ART doses, education and food-security missingness indicators, and enumerator and analysis-stratum fixed effects. Parentheses contain recruitment-day clustered standard errors. Stars indicate unadjusted two-sided tests (*** $p < .01$, ** $p < .05$, * $p < .10$); curly braces contain the family-specific FWER-adjusted p-values described in each table. Component contrasts are outside these FWER families. Square brackets report sharpened q-values calculated jointly across the treatment-effect tests in Section 3 and its associated appendix, including the primary specifications.

A 3.1. Enrollment Cohort and Follow-up Horizon

Table A7 compares longer-window medication coverage in the early-enrollment cohort (columns (1)–(3)) and the full sample without COVID censoring (columns (4)–(6)). The latter applies the two-source reconciliation rule in Appendix A 2.2. Table notes give the sample and endpoint conventions.

Table A7: *Medication coverage: early-enrollment and uncensored follow-up checks*

	Restricted sample			Full sample, no COVID cutoff		
	(1)	(2)	(3)	(4)	(5)	(6)
	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR (continuous)	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR (continuous)
Incentives	0.020	0.094	0.080	0.090	0.092	0.077**
	(0.069)	(0.114)	(0.056)	(0.054)	(0.064)	(0.032)
	[0.557]	[0.450]	[0.271]	[0.239]	[0.269]	[0.105]
	{0.799}			{0.380}		
Reminders	0.079	-0.011	0.075	0.060	0.038	0.060*
	(0.076)	(0.113)	(0.052)	(0.055)	(0.068)	(0.033)
	[0.369]	[0.611]	[0.269]	[0.364]	[0.483]	[0.189]
	{0.580}			{0.441}		
Combination	0.224***	0.136	0.118**	0.160***	0.113*	0.099***
	(0.071)	(0.112)	(0.053)	(0.048)	(0.061)	(0.031)
	[0.029]	[0.318]	[0.121]	[0.016]	[0.185]	[0.020]
	{0.144}			{0.019}		
Information	0.171**	0.099	0.075	0.054	0.010	0.017
	(0.069)	(0.109)	(0.052)	(0.046)	(0.059)	(0.030)
	[0.104]	[0.424]	[0.269]	[0.323]	[0.603]	[0.483]
	{0.205}			{0.441}		
Stigma-relieving	0.202**	0.080	0.094	0.097*	0.041	0.051
	(0.094)	(0.156)	(0.070)	(0.056)	(0.081)	(0.040)
	[0.134]	[0.504]	[0.283]	[0.214]	[0.506]	[0.287]
	{0.328}			{0.380}		
PAP covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	378	378	378	821	821	821
Recruitment-day clusters	50	50	50	137	137	137
Control mean	0.140	0.419	0.630	0.179	0.453	0.640
Combination minus incentives	0.204***	0.042	0.038	0.071	0.021	0.022
	(0.051)	(0.067)	(0.033)	(0.052)	(0.049)	(0.028)
	[0.005]	[0.482]	[0.336]	[0.276]	[0.527]	[0.458]
Combination minus reminders	0.145**	0.147**	0.043	0.100*	0.074	0.039
	(0.065)	(0.066)	(0.028)	(0.054)	(0.054)	(0.028)
	[0.121]	[0.122]	[0.264]	[0.185]	[0.276]	[0.276]
Departure from additivity	0.125	0.053	-0.037	0.011	-0.018	-0.038
	(0.088)	(0.129)	(0.061)	(0.076)	(0.084)	(0.043)
	[0.271]	[0.527]	[0.482]	[0.605]	[0.593]	[0.435]

Notes: Square brackets report Section 3 sharpened q-values. Covariates and inference conventions are defined in Appendix A 3. Columns (1)–(3) retain the primary endpoint rules for participants enrolled by 26 September 2019. Columns (4)–(6) remove COVID censoring. Curly braces report separate five-treatment step-down FWER families for columns (1) and (4). Each two-sided FWER family uses Stata `mhtreg` with 10,000 recruitment-day cluster bootstrap replications (seed 31415).

A 3.2. Coverage, Endpoints, and Design Controls

Table A8 varies the enrollment-day dose deduction, death and loss-to-follow-up censoring, and assignment-block controls in columns (1)–(3). Column (1) retains the full enrollment supply at the start of follow-up the next day. Column (2) retains the primary enrollment-day deduction and transfer censoring, but otherwise follows participants through 180 days or the COVID cutoff, whichever comes first. Coverage continues under the standard stock-accounting rule, without automatically coding days after death or loss to follow-up as uncovered. Column (4) separates the original information and intended information-plus-stigma categories; the latter received information only. Columns (5) and (6) reconstruct medication coverage from the real-time log and retrospective refill source separately, using accepted within-source cleaning without cross-source matching or quantity borrowing. Both retain the primary cohort, starting stock, clinical death and transfer adjudications, and stock-accounting rules; the inactivity endpoint uses activity in the selected source. Each column retains its treatment-versus-control FWER adjustment.

Table A8: *Targeted robustness: MPR* $\geq$ 95%

	(1) No enrollment-day deduction	(2) No death or LTFU censoring	(3) Design blocks	(4) Intended assignments	(5) Real-time log only	(6) Retrospective only
Incentives	0.096*	0.087*	0.081	0.086	0.089*	0.042
	(0.055)	(0.052)	(0.054)	(0.052)	(0.050)	(0.047)
	[0.214]	[0.239]	[0.264]	[0.242]	[0.198]	[0.424]
	{0.203}	{0.225}	{0.297}	{0.314}	{0.297}	{0.393}
Reminders	0.132**	0.100*	0.099*	0.100*	0.051	0.102*
	(0.059)	(0.053)	(0.055)	(0.053)	(0.043)	(0.052)
	[0.121]	[0.177]	[0.194]	[0.180]	[0.323]	[0.164]
	{0.182}	{0.220}	{0.256}	{0.279}	{0.429}	{0.206}
Combination	0.221***	0.226***	0.220***	0.233***	0.150***	0.195***
	(0.045)	(0.044)	(0.046)	(0.044)	(0.044)	(0.043)
	[0.001]	[0.001]	[0.001]	[0.001]	[0.014]	[0.001]
	{< 0.001}	{< 0.001}	{< 0.001}	{< 0.001}	{0.016}	{0.001}
Information (pooled)	0.075	0.075	0.061	—	0.013	0.072*
	(0.049)	(0.047)	(0.047)		(0.045)	(0.043)
	[0.264]	[0.257]	[0.286]		[0.554]	[0.239]
	{0.203}	{0.225}	{0.297}		{0.780}	{0.245}
Stigma-relieving	0.131**	0.141**	0.134**	0.141**	0.072	0.156***
	(0.060)	(0.059)	(0.058)	(0.059)	(0.050)	(0.055)
	[0.121]	[0.104]	[0.110]	[0.104]	[0.269]	[0.048]
	{0.182}	{0.137}	{0.164}	{0.169}	{0.411}	{0.065}
Information (original)	—	—	—	0.081	—	—
				(0.056)		
				[0.269]		
				{0.332}		
Combination 2	—	—	—	0.067	—	—
				(0.057)		
				[0.325]		
				{0.332}		
PAP covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	821	821	821	821	821	821
Recruitment-day clusters	137	137	137	137	137	137
Control mean	0.255	0.245	0.245	0.245	0.189	0.208

Notes: Square brackets report Section 3 sharpened q-values. Covariates and inference conventions are defined in Appendix A 3. Column (1) deducts no enrollment-day dose from starting supply; follow-up still begins the next day, with primary endpoints and subsequent stock accounting unchanged. Column (2) removes death and loss-to-follow-up censoring, retaining transfer censoring and the primary enrollment-day deduction. Otherwise follow-up ends at 180 days or the COVID cutoff, whichever comes first; coverage continues under standard stock accounting, without automatically coding later days as uncovered. Column (3) uses assignment-block fixed effects. Column (4) separates the two original assignments pooled as information, both of which received information only. Columns (5) and (6) use only accepted real-time and retrospective refill records, respectively, with no cross-source matching or quantity borrowing. Starting stock, clinical records, cohort and stock accounting remain common; the inactivity endpoint is rebuilt from the selected source. Missing quantities use the usual 30/90-dose defaults. Curly braces report step-down FWER p-values: separate five-treatment families in columns (1)–(3), (5) and (6), and a six-treatment family in column (4). Each two-sided FWER family uses Stata `mhtreg` with 10,000 recruitment-day cluster bootstrap replications (seed 31415). Adjusted p-values impose the step-down stopping rule by taking cumulative maxima in the order of the unadjusted bootstrap p-values.

A 3.3. Secondary Outcomes and Outcome Observation

Table A9 reports attendance, death/LTFU, referral, knowledge and stigma outcomes. The survey outcomes condition on response; these respondent-only estimates do not establish population-wide changes or mediation. Administrative eligibility for death/LTFU is distinct from survey response.

Table A9: *Effects on secondary outcomes*

	(1) AAR	(2) Death/LTFU	(3) Referral	(4) Knowledge	(5) Stigma belief
Incentives	0.037	0.055	-0.037	0.056	0.471
	(0.031)	(0.036)	(0.040)	(0.211)	(0.331)
	[0.318]	[0.264]	[0.417]	[0.565]	[0.269]
Reminders	0.005	0.042	0.064	-0.404**	0.002
	(0.033)	(0.035)	(0.048)	(0.175)	(0.390)
	[0.603]	[0.318]	[0.283]	[0.110]	[0.622]
Combination	0.076**	0.018	0.048	-0.059	-0.018
	(0.030)	(0.032)	(0.040)	(0.159)	(0.368)
	[0.093]	[0.483]	[0.318]	[0.528]	[0.611]
Information	0.022	0.048*	0.011	0.217	0.468
	(0.028)	(0.029)	(0.037)	(0.157)	(0.356)
	[0.458]	[0.239]	[0.554]	[0.276]	[0.284]
Stigma-relieving	0.083**	0.040	0.019	-0.188	0.223
	(0.033)	(0.038)	(0.041)	(0.178)	(0.318)
	[0.093]	[0.367]	[0.514]	[0.367]	[0.458]
PAP covariates	Yes	Yes	Yes	Yes	Yes
Observations	808	588	788	518	511
Recruitment-day clusters	137	91	129	131	131
Control mean	0.226	0.065	0.120	0.350	-1.953
Combination minus incentives	0.039	-0.037	0.085**	-0.115	-0.489
	(0.028)	(0.036)	(0.037)	(0.184)	(0.328)
	[0.276]	[0.371]	[0.110]	[0.482]	[0.264]
Combination minus reminders	0.071**	-0.024	-0.015	0.345**	-0.020
	(0.030)	(0.037)	(0.047)	(0.152)	(0.368)
	[0.109]	[0.474]	[0.540]	[0.114]	[0.611]
Departure from additivity	0.034	-0.079	0.022	0.289	-0.491
	(0.043)	(0.050)	(0.060)	(0.251)	(0.502)
	[0.458]	[0.257]	[0.528]	[0.336]	[0.404]

Notes: Square brackets report Section 3 sharpened q-values. Covariates and inference conventions are defined in Appendix A 3. AAR is the monthly appointment-attendance share, using linked dossiers and primary MPR endpoints. Death/LTFU includes 5 deaths. Knowledge and stigma changes use paired survey responses.

A 4. Model Derivations and Extensions

The benchmark derivations establish the timing predictions under naive continuation. The extension then allows anticipated future lapses and reports the numerical comparison, including cases in which the combination does not improve the on-time refill share. A final extension allows subsequent reward opportunities under perfect deadline recall and distinguishes individual refill incentives from pooled timing estimates.

A 4.1. Benchmark Derivations

This appendix derives the formal predictions in Section 4.1 under Assumptions 1–3, including naive continuation in every arm. Appendix A 4.2 extends continuation values to anticipated future inattention. Section A 4.1.1 fixes notation and collects auxiliary results on the belief formulas (Lemma 1), the representation of the conditional refill rate as a mixture over pill states (Lemma A.2), and the monotonicity of that mixture (Lemma A.3). Section A 4.1.2 proves Propositions 1–5.

A 4.1.1. Preliminaries

Notation. Days are $t = 1, 2, \ldots$ and pill states are $s \in \{1,2,3,4\}$, as defined in Section 4.1.1. The observable state includes the distinction between the post-exhaustion continuation state and the terminal opportunity; once stock is zero, this distinction is not a pill count alone. Costs c_t are i.i.d. draws independent of attention from the common distribution F_c, which is continuous, strictly increasing on its support, and supported in $[0,\infty)$; $h(2) \geq h(1) > 0$; $\delta \in (0,1)$. The no-incentive value functions $V(s)$ are given by (3). Write $K(s)$ for the payoff to continuing without a refill in state s, the second argument of each maximum in (3). A patient in state s refills if and only if $-c \geq K(s)$, so the probability that an attentive patient in state s refills is

$$\begin{aligned} q(s) \;&=\; F_c\big(-K(s)\big), \qquad \text{where} \\ -K(1) &= -\delta V(2), \qquad\qquad -K(2) = -\delta V(3), \\ -K(3) &= h(1) - \delta V(4), \qquad\qquad -K(4) = h(2). \end{aligned} \tag{A1}$$

In the financial-incentives arm the refill branch in state s carries the perceived bonus

$$\begin{gathered} g(s) = \gamma \cdot \Pr(t \leq 2 \mid s), \\ g(1) = \gamma(1-\theta^2), \quad g(2) = \gamma(1-\theta)^2, \quad g(3) = g(4) = 0. \end{gathered} \tag{A2}$$

by Lemma 1; write $V^F(s)$, $K^F(s)$, and $q^F(s) = F_c\big(g(s) - K^F(s)\big)$ for the corresponding objects. In the combination arm the refill branch on day t carries $\gamma\,\mathbb{I}\{t \le 2\}$ and the patient knows t; write $q^C(s,t)$ for her refill probability in state s on day t. Under Assumption 3, continuation is evaluated with the state advancing by one each day. The no-incentive values do not depend on θ; financial-arm values do depend on it through $g(s)$. Throughout, $\gamma > 0$ and $\theta \in [0,1)$.

Assumption A.1 (Interior thresholds) *Every refill threshold used below ($-K(s)$, $g(s) - K^F(s)$, and the combination-arm thresholds of Lemma A.5) lies in the interior of the support of F_c, so that every refill probability is strictly between 0 and 1.*

The value functions. The no-incentive values satisfy $V(s) \le 0$. Values with a positive incentive need not be nonpositive; no such restriction on those values is used below. We use repeatedly that for any $a, b \in \mathbb{R}$ and $x \ge 0$,

$$\max\{a,b\} \;\le\; \max\{a + x, b\} \;\le\; \max\{a,b\} + x, \tag{A3}$$

because $a \mapsto \max\{a,b\}$ is nondecreasing and 1-Lipschitz.

Lemma A.1 (Refill probabilities rise as pills run out) *$V(2) > V(3)$, and $q(1) < q(2) < q(3)$.*

Proof By Assumption A.1, $q(2) = \Pr(-c \ge \delta V(3)) > 0$, so refilling is optimal with positive probability in state 2 and $V(2) > \delta V(3)$. Since $V(3) \le 0$ and $\delta < 1$, $\delta V(3) \ge V(3)$, so $V(2) > V(3)$. Hence $-\delta V(2) < -\delta V(3)$, and F_c strictly increasing gives $q(1) < q(2)$. For $q(2) < q(3)$, compare the thresholds $-\delta V(3)$ and $h(1) - \delta V(4)$. Since $V(3) \ge -h(1) + \delta V(4)$,

$$\begin{aligned}\big(h(1) - \delta V(4)\big) - \big(-\delta V(3)\big) \;&\ge\; h(1) - \delta V(4) - \delta h(1) + \delta^2 V(4)\\ &=\; (1-\delta)\big(h(1) - \delta V(4)\big) \;>\; 0,\end{aligned}$$

using $h(1) > 0$ and $V(4) \le 0$. □

Proof of Lemma 1 (beliefs about eligibility) Under Assumption 2, the patient's belief that today is day t, given that she is in state s and has not refilled, is proportional to the probability of that event. Let q_1 denote her refill probability on an attentive day in state 1 (this is $q^F(1)$ in the financial-incentives arm; the argument does not depend on its value).

State 1. She holds two pills on day t if and only if she was inattentive on each of the $t-1$ preceding days, which has probability θ^{t-1} for $t \ge 1$. The weights sum to $\sum_{t\ge1} \theta^{t-1} = 1/(1-\theta)$, so $\Pr(t \mid s = 1) = (1-\theta)\theta^{t-1}$ and $\Pr(t \le 2 \mid s = 1) = (1-\theta)(1+\theta) = 1 - \theta^2$.

State 2. She holds one pill on day $t \geq 2$ if and only if exactly one of the $t-1$ preceding days was attentive, she was in state 1 on that day, and she did not refill: probability $(t-1)(1-\theta)\theta^{t-2}(1-q_1)$. The factor $(1-\theta)(1-q_1)$ is common to all t and cancels. Since $\sum_{t\geq 2}(t-1)\theta^{t-2} = 1/(1-\theta)^2$, $\Pr(t \mid s=2) = (t-1)\theta^{t-2}(1-\theta)^2$, and $\Pr(t \leq 2 \mid s=2) = \Pr(t=2 \mid s=2) = (1-\theta)^2$.

States 3 and 4. Reaching a day without pills requires at least two attentive days beforehand, so $t \geq 3$ and $\Pr(t \leq 2 \mid s \geq 3) = 0$. □

Lemma A.2 (The conditional refill rate as a mixture over pill states) *Consider an arm in which the refill probability of an attentive patient depends only on her pill state, $q(s)$, and the daily probability of inattention is θ. Then*

$$R(1) = (1-\theta)\, q(1), \tag{A4}$$

$$R(2) = (1-\theta)\, M_2, \qquad M_2 = \frac{w_1 q(1) + w_2 q(2)}{w_1 + w_2}, \quad w_1 = \theta,\ w_2 = (1-\theta)\big(1-q(1)\big), \tag{A5}$$

$$R(3) = (1-\theta)\, M_3, \qquad M_3 = \frac{w_1 q(1) + w_2 q(2) + w_3 q(3)}{w_1 + w_2 + w_3}, \tag{A6}$$

with, in (A6), $w_1 = \theta^2$, $w_2 = 2\theta(1-\theta)\big(1-q(1)\big)$, *and* $w_3 = (1-\theta)^2\big(1-q(1)\big)\big(1-q(2)\big)$. *In each case w_s is proportional to the probability that a patient is in state s on day t and has not refilled, and M_t is the average refill probability across the patients still in the window; this is the representation described in words in Section 4.1.4.*

Proof On day 1 every patient is in state 1 and refills if and only if she is attentive and draws a low cost: $R(1) = (1-\theta)q(1)$. A patient who has not refilled by the start of day 2 was either inattentive on day 1 (probability θ, state 1) or attentive and drew a high cost (probability $(1-\theta)(1-q(1))$, state 2); she refills on day 2 with probability $(1-\theta)q(s)$ in state s, which gives (A5). A patient who has not refilled by the start of day 3 was inattentive on both days (probability θ^2, state 1); attentive on exactly one of them while in state 1 and did not refill (probability $2\theta(1-\theta)(1-q(1))$, state 2); or attentive on both, in states 1 and then 2, and did not refill on either (probability $(1-\theta)^2(1-q(1))(1-q(2))$, state 3). The terminal state can first be reached on day 4, so no exit other than a refill has occurred by the start of day 3. This gives (A6). □

Lemma A.3 (Monotonicity of the mixtures) *Let $\theta \in (0,1)$ and $q(s) \in (0,1)$.*

(i) M_2 is strictly increasing in $q(1)$ and in $q(2)$.

(ii) M_3 is strictly increasing in $q(1)$, $q(2)$, and $q(3)$.

(iii) If $q(1) \le q(2) \le q(3)$, then M_2 and M_3 are nonincreasing in θ; the same holds at $\theta = 0$, where $M_2 = q(2)$ and $M_3 = q(3)$.

Proof Write $W = \sum_s w_s$ and note that w_j does not depend on $q(j)$, while for $s > j$, w_s contains the factor $(1 - q(j))$, so $\partial w_s / \partial q(j) = -w_s/(1 - q(j))$. Differentiating $M = \sum_s w_s q(s)/W$,

$$\frac{\partial M}{\partial q(j)} = \frac{w_j}{W} + \sum_{s>j} \frac{\partial w_s}{\partial q(j)} \frac{q(s) - M}{W} = \frac{1}{W}\Big[w_j - \frac{1}{1 - q(j)} \sum_{s>j} w_s\big(q(s) - M\big)\Big].$$

Since $\sum_s w_s(q(s) - M) = 0$, $\sum_{s>j} w_s(q(s) - M) = \sum_{s \le j} w_s(M - q(s))$, and therefore

$$\frac{\partial M}{\partial q(j)} = \frac{w_j\big(1 - M\big) - \sum_{s<j} w_s\big(M - q(s)\big)}{\big(1 - q(j)\big)\, W}. \tag{A7}$$

(i) For M_2 there is no state above 2, so $\partial M_2/\partial q(2) = w_2/W > 0$. For $j = 1$, (A7) gives $w_1(1 - M_2)/((1 - q(1))W) > 0$, because $M_2 < 1$.

(ii) For $j = 1$, (A7) gives $w_1(1 - M_3)/((1 - q(1))W) > 0$. For $j = 3$, using $\sum_{s<3} w_s(M_3 - q(s)) = w_3(q(3) - M_3)$, the numerator of (A7) is $w_3(1 - M_3) - w_3(q(3) - M_3) = w_3(1 - q(3)) > 0$. For $j = 2$ the numerator is $w_2(1 - M_3) - w_1(M_3 - q(1))$, which is positive if and only if $(w_1 + w_2)M_3 < w_1 q(1) + w_2$. Since $q(3) < 1$ and $w_3 > 0$, $M_3 < \bar{M} := (w_1 q(1) + w_2 q(2) + w_3)/W$, so it suffices that $(w_1 + w_2)\bar{M} \le w_1 q(1) + w_2$, i.e. that

$$(w_1 + w_2)\big(w_1 q(1) + w_2 q(2) + w_3\big) \;\le\; \big(w_1 q(1) + w_2\big)\big(w_1 + w_2 + w_3\big).$$

Expanding, the right side minus the left side equals $(w_1 + w_2)\, w_2\, (1 - q(2)) - w_3\, w_1\, (1 - q(1))$. Substituting the weights of (A6),

$$\begin{aligned}&(w_1 + w_2)\, w_2\, (1 - q(2)) - w_3\, w_1\, (1 - q(1)) \\ &\quad = \; 2\theta^3(1-\theta)(1 - q(1))(1 - q(2)) + 3\theta^2(1-\theta)^2(1 - q(1))^2(1 - q(2)) \;>\; 0.\end{aligned}$$

(iii) In M_2 the weight on $q(1)$ is $\theta/\big(\theta + (1-\theta)(1 - q(1))\big)$, which is increasing in θ; with $q(1) \le q(2)$, shifting weight toward $q(1)$ weakly lowers M_2. In M_3, $w_s = \binom{2}{s-1}(1-\theta)^{s-1}\theta^{3-s}\phi_s$ with $\phi_s = \prod_{u<s}(1 - q(u))$ independent of θ. For $\theta' < \theta$ the ratio $w_s(\theta')/w_s(\theta) = \big[(1-\theta')/(1-\theta)\big]^{s-1}\big[\theta'/\theta\big]^{3-s}$ is increasing in s, so the normalized weights under θ' dominate those under θ in the likelihood-ratio order and hence in first-order stochastic dominance; with $q(s)$ nondecreasing in s, $M_3(\theta') \ge M_3(\theta)$. At $\theta = 0$ all weight is on the highest state, which carries the largest q, so the inequality extends to $\theta' = 0$. □

Lemma A.4 (Refill thresholds in the financial-incentives arm) $q^F(4) = q(4)$, $q^F(3) = q(3)$, $q^F(2) > q(2)$, *and* $q^F(1) > q(1)$. *Moreover* $0 \le V^F(2) - V(2) \le g(2)$.

Proof Since $g(3) = g(4) = 0$, neither the flow payoffs nor the continuation payoffs in states 3 and 4 involve the incentive: $V^F(4) = V(4)$, $V^F(3) = V(3)$, and hence $q^F(4) = q(4)$ and $q^F(3) = q(3)$. In state 2 the patient compares $g(2) - c$ with $\delta V^F(3) = \delta V(3)$, so $q^F(2) = F_c\big(g(2) - \delta V(3)\big) > F_c\big(-\delta V(3)\big) = q(2)$, because $g(2) = \gamma(1-\theta)^2 > 0$ and F_c is strictly increasing at an interior threshold. Applying (A3) with $x = g(2)$ to $V^F(2) = \mathbb{E}\max\{g(2) - c, \delta V(3)\}$ and $V(2) = \mathbb{E}\max\{-c, \delta V(3)\}$ gives $0 \le V^F(2) - V(2) \le g(2)$. In state 1 the thresholds are $g(1) - \delta V^F(2)$ and $-\delta V(2)$, whose difference is

$$g(1) - \delta\big(V^F(2) - V(2)\big) \;\ge\; g(1) - \delta g(2) \;=\; \gamma\big[(1-\theta^2) - \delta(1-\theta)^2\big] \;>\; 0,$$

because $1 - \theta^2 = (1-\theta)(1+\theta) \ge (1-\theta)^2 > \delta(1-\theta)^2$. Hence $q^F(1) > q(1)$. □

Lemma A.5 (Refill thresholds in the combination arm) *Let $V^C(2,2) = \mathbb{E}\max\{\gamma - c, \delta V(3)\}$ be the value of beginning day 2 in state 2 in the combination arm. Then*

$$\begin{aligned} q^C(1,1) &= F_c\big(\gamma - \delta V^C(2,2)\big), \\ q^C(1,2) &= F_c\big(\gamma - \delta V(2)\big), \\ q^C(2,2) &= F_c\big(\gamma - \delta V(3)\big), \end{aligned}$$

and, when $\theta > 0$: $q^C(2,2) > q^F(2)$, $q^C(1,2) > q^F(1)$, $q^C(1,2) \le q^C(2,2)$, and $q^C(1,1) \le q^C(1,2)$. When $\theta = 0$, $q^C(2,2) = q^F(2)$ and $q^C(1,1) = q^F(1)$.

Proof From day 3 on there is no bonus, so the combination arm's continuation values from any state on day 3 or later are the no-incentive values $V(s)$. On day 2 in state 2 the patient compares $\gamma - c$ with $\delta V(3)$; on day 2 in state 1 (she was inattentive on day 1) she compares $\gamma - c$ with the value of being in state 2 on day 3, $\delta V(2)$; on day 1 she compares $\gamma - c$ with $\delta V^C(2,2)$. This gives the three thresholds.

For $\theta > 0$: $\gamma > g(2)$, so $q^C(2,2) > q^F(2)$. Since $\gamma > g(1)$ and $V^F(2) \ge V(2)$ (Lemma A.4), $\gamma - \delta V(2) > g(1) - \delta V^F(2)$, so $q^C(1,2) > q^F(1)$. Lemma A.1 gives $V(2) > V(3)$, hence $q^C(1,2) < q^C(2,2)$. Finally $V^C(2,2) \ge V(2)$ by (A3), hence $q^C(1,1) \le q^C(1,2)$. At $\theta = 0$, $g(1) = g(2) = \gamma$, $V^F(2) = V^C(2,2)$, and the equalities follow. □

A 4.1.2. Proofs of the propositions

Proof of Proposition 1 With $\theta = 0$ every patient is attentive every day, the pill state equals the day, and $R(t) = q(t)$; by Lemma 1 the perceived bonus equals the true bonus, $g(1) = g(2) = \gamma$. Lemma A.4 then gives $R^F(1) > R(1)$, $R^F(2) > R(2)$,

$R^F(3) = R(3)$, and $R^F(4) = R(4)$. The threshold shifts are exactly γ on day 2 and at least $\gamma(1-\delta)$ on day 1. □

Proof of Proposition 2 In the reminders arm there is no incentive, so the information channel is payoff-irrelevant and the refill probabilities on attentive days are the control arm's $q(s)$; under naive continuation they do not depend on θ. In this benchmark, the only difference from control is the probability of inattention, $\theta^R = \theta + \Delta\theta$. By Lemma A.2, $R^R(t) = (1-\theta^R)M_t(\theta^R)$ and $R(t) = (1-\theta)M_t(\theta)$ for $t \le 3$, with $M_1 \equiv q(1)$. If $\Delta\theta < 0$ then $1-\theta^R > 1-\theta$, and by Lemma A.3(iii), which applies because $q(1) < q(2) < q(3)$ (Lemma A.1), $M_t(\theta^R) \ge M_t(\theta)$. Hence $R^R(t) > R(t)$ for $t = 1,2,3$. If $\theta = 0$, feasibility requires $\Delta\theta = 0$ and the two arms are identical. More generally, the same equality holds whenever $\Delta\theta = 0$. □

Proof of Proposition 3 Both arms have the same θ, so by Lemma A.2 the comparison reduces to $M_t(q^F)$ versus $M_t(q)$, with $q^F(1) > q(1)$, $q^F(2) > q(2)$, and $q^F(3) = q(3)$ by Lemma A.4. Day 1: $R^F(1) = (1-\theta)q^F(1) > (1-\theta)q(1) = R(1)$. Day 2: if $\theta = 0$, $R(2) = q(2)$ and $R^F(2) = q^F(2) > q(2)$; if $\theta > 0$, Lemma A.3(i) gives $M_2(q^F) > M_2(q)$. Day 3: if $\theta = 0$ then $w_1 = w_2 = 0$, $M_3 = q(3) = q^F(3)$, and $R^F(3) = R(3)$; if $\theta > 0$ then $w_1, w_2 > 0$ and Lemma A.3(ii) gives $M_3(q^F) > M_3(q)$, so $R^F(3) > R(3)$. □

Proof of Proposition 4 *(a) Day 2.* Write $\theta^C = \theta + \Delta\theta \le \theta$. By the argument of Lemma A.2, the combination arm's patients still in the window at the start of day 2 are in state 1 with weight θ^C and in state 2 with weight $(1-\theta^C)(1-q^C(1,1))$, and they refill with probabilities $q^C(1,2)$ and $q^C(2,2)$ respectively:

$$R^C(2) = (1-\theta^C)\,M^C, \qquad M^C = \frac{\theta^C\, q^C(1,2) + (1-\theta^C)\big(1-q^C(1,1)\big)\, q^C(2,2)}{\theta^C + (1-\theta^C)\big(1-q^C(1,1)\big)},$$

while $R^F(2) = (1-\theta)M_2\big(q^F(1), q^F(2)\big)$ by Lemma A.2. If $\theta = 0$ then $\theta^C = 0$, $R^C(2) = q^C(2,2) = q^F(2) = R^F(2)$ by Lemma A.5. Suppose $\theta > 0$. Let $a = q^C(1,2)$, $b = q^C(1,1)$, $c = q^C(2,2)$, so that $b \le a \le c$ (Lemma A.5), and write $M(a,b,c;\theta)$ for the weighted average above evaluated at attention θ.

Step 1: replacing b by a. M is a weighted average of a and c with $a \le c$, so $M \le c$. Its derivative in b is $(1-\theta)(M-c)/W \le 0$, so lowering the day-1 refill probability from a to $b \le a$ weakly raises M: $M(a,b,c;\theta) \ge M(a,a,c;\theta)$.

Step 2: monotonicity in a and c. $M(a,a,c;\theta)$ is M_2 of Lemma A.2 with $q(1) = a$ and $q(2) = c$, which by Lemma A.3(i) is strictly increasing in both arguments. Since $a > q^F(1)$ and $c > q^F(2)$, $M(a,a,c;\theta) > M_2\big(q^F(1), q^F(2)\big)$.

Step 3: attention. $M(a,b,c;\theta)$ is a weighted average of a and $c \geq a$ in which the weight on a is increasing in θ, so it is nonincreasing in θ: $M(a,b,c;\theta^C) \geq M(a,b,c;\theta)$. Also $1-\theta^C \geq 1-\theta$.

Combining the three steps, $R^C(2) = (1-\theta^C)M(a,b,c;\theta^C) \geq (1-\theta)M(a,b,c;\theta) \geq (1-\theta)M(a,a,c;\theta) > (1-\theta)M_2\big(q^F(1),q^F(2)\big) = R^F(2)$.

(b) Day 1. $R^C(1) = (1-\theta^C)\,q^C(1,1)$ and $R^F(1) = (1-\theta)\,q^F(1)$. With $\Delta\theta = 0$ the sign of $R^C(1) - R^F(1)$ is the sign of the threshold difference

$$D \;=\; \big(\gamma - \delta V^C(2,2)\big) - \big(g(1) - \delta V^F(2)\big) \;=\; \gamma\theta^2 - \delta\big[\Phi(\gamma) - \Phi\big(g(2)\big)\big],$$

where $\Phi(x) := \mathbb{E}\max\{x-c, \delta V(3)\}$, so that $V^C(2,2) = \Phi(\gamma)$ and $V^F(2) = \Phi(g(2))$. Because F_c is continuous, Φ is differentiable with $\Phi'(x) = \Pr(x - c \geq \delta V(3)) = F_c\big(x - \delta V(3)\big)$, which is nondecreasing in x and equals $q^F(2)$ at $x = g(2)$ and $q^C(2,2)$ at $x = \gamma$. Integrating over $[g(2),\gamma]$, an interval of length $\gamma - g(2) = \gamma(2\theta - \theta^2) = \gamma\theta(2-\theta)$,

$$\gamma\theta(2-\theta)\,q^F(2) \;\leq\; \Phi(\gamma) - \Phi\big(g(2)\big) \;\leq\; \gamma\theta(2-\theta)\,q^C(2,2).$$

Therefore $\gamma\theta\big[\theta - \delta(2-\theta)q^C(2,2)\big] \leq D \leq \gamma\theta\big[\theta - \delta(2-\theta)q^F(2)\big]$. If $\theta < \delta(2-\theta)q^F(2)$ the upper bound is negative, so $D < 0$ and $R^C(1) < R^F(1)$; if $\theta > \delta(2-\theta)q^C(2,2)$ the lower bound is positive, so $R^C(1) > R^F(1)$. With $\Delta\theta < 0$, $R^C(1)$ carries the additional factor $(1-\theta^C)/(1-\theta) > 1$, which raises it; the net sign is not determined without further restrictions. □

Proof of Proposition 5 A patient refills on time if she refills on day 1, or does not and then refills on day 2: $\Pi = R(1) + (1-R(1))R(2) = 1 - (1-R(1))(1-R(2))$, with $\partial\Pi/\partial R(1) = 1 - R(2) > 0$ and $\partial\Pi/\partial R(2) = 1 - R(1) > 0$. Financial incentives raise $R(1)$ and $R(2)$ (Proposition 3), reminders raise both when $\Delta\theta < 0$ (Proposition 2), and, when $\theta > 0$, the combination arm raises $R(2)$ relative to the financial-incentives arm (Proposition 4(a)); whenever its $R(1)$ is also no lower, Π is strictly higher. With $\theta = 0$, feasibility implies $\Delta\theta = 0$ and both rates coincide. More generally, the exact decomposition is

$$\Pi^C - \Pi^F = (R^C(1) - R^F(1))(1 - R^F(2)) + (1 - R^C(1))(R^C(2) - R^F(2)).$$

When $R^C(1) < R^F(1)$, the second term must exceed the magnitude of the first for the aggregate effect to be positive. □

A 4.1.3. Scope and interpretation

The day-1 comparison between the combination and financial-incentives arms depends on the balance between information and attention (Proposition 4(b)). The

combination-minus-incentives day-3 comparison is also unsigned: knowing that the bonus has expired and paying greater attention can act in opposite directions. With frictions, day 4 and subsequent days involve evolving pill states and terminal exit; the preceding mixture formulas establish the results for days 1–3.

The perfectly anticipated day-2 signal isolates knowledge of the deadline. The known-calendar formulas assume that the signal arrives with certainty while the window is open, even on an inattentive day, and that its date is retained afterward. Its known schedule also makes nonarrival on day 1 informative. Field calls instead involved repeated attempts, failed contacts, and cancellation following early refills (Appendix A 1.7). The attention change $\Delta\theta$ is a reduced-form response encompassing salience and self-monitoring. Holding attention fixed, date information has no separate payoff without incentives in this specification; health-related benefits of remembering the date operate through attention.

The on-time share Π summarizes a single window. Empirical cumulative refill counts extend the question to repeated care, where prior visits and quantities dispensed also matter. Neither Π nor those counts are an identity for MPR. The aggregate combination comparison requires the stated condition on early refilling; precise information alone can encourage waiting and need not raise the on-time share. These conclusions follow from the analytic conditions above.

A 4.2. Anticipated Inattention and Numerical Comparisons

This appendix relaxes the naive-continuation requirement in Assumption 3, allowing patients to anticipate future inattention, and examines the combination's effect on the on-time refill share. In the extension, the patient assigns a subjective probability $\eta^a \in [0,1)$ to inattention on each future day, independently across days. This can equal the actual probability θ^a in arm a or differ from it; $\eta^a = 0$ in every arm recovers naive continuation. Accurate anticipation here means that the future lapse rate is correctly perceived. It does not impose fully Bayesian updating about the deadline: the financial-incentives arm resets eligibility beliefs to the rule in Lemma 1 at each future pill state, preserving the state-based eligibility rule while relaxing only the assumption about future attention.

A 4.2.1. Continuation values with anticipated lapses

Let $W^a(s)$ denote the value of a future day before its attention realization, and $A^a(s)$ the value conditional on being attentive. Put $\ell_1 = \ell_2 = 0$ and $\ell_3 = h(1)$. For the control, financial-incentives, and reminders-only arms, the recursion for

$s = 1,2,3$ is

$$A^a(s) = \mathbb{E}\max\{g^a(s) - c, -\ell_s + \delta W^a(s+1)\}, \tag{A8}$$

$$W^a(s) = (1-\eta^a)A^a(s) + \eta^a[-\ell_s + \delta W^a(s)] = \frac{(1-\eta^a)A^a(s) - \eta^a\ell_s}{1-\delta\eta^a}. \tag{A9}$$

Here $g^a(s)$ is zero without incentives and equals equation (A2) in the financial arm. Before state 4, an inattentive day leaves the state unchanged, and a day without medication incurs its health cost. State 4 is terminal: failure to refill, whether from a high cost or inattention, incurs $h(2)$ and ends the window. Thus

$$A^a(4) = \mathbb{E}\max\{-c, -h(2)\}, \qquad W^a(4) = (1-\eta^a)A^a(4) - \eta^a h(2).$$

The attentive refill probability is $F_c(g^a(s) + \ell_s - \delta W^a(s+1))$ for $s < 4$, and $F_c(h(2))$ at the terminal state. Since $1 - \delta\eta^a > 0$, the displayed recursion determines a unique finite value in each state, working backward from state 4. Setting $\eta^a = 0$ recovers the main-text value functions. Actual state transitions and refill rates continue to use θ^a, rather than the subjective η^a.

In the combination arm, let $W_C^0(s)$ denote the no-reward continuation value evaluated at its own subjective rate η^C. On day 2, for either pill-holding state $s = 1,2$,

$$A^C(s,2) = \mathbb{E}\max\{\gamma - c, \delta W_C^0(s+1)\},$$
$$W^C(s,2) = (1-\eta^C)A^C(s,2) + \eta^C\delta W_C^0(s).$$

Both branches recognize that the calendar deadline passes even if the patient is inattentive. The attentive refill probabilities are

$$q^C(1,1) = F_c(\gamma - \delta W^C(2,2)), \qquad q^C(s,2) = F_c(\gamma - \delta W_C^0(s+1)), \quad s = 1,2.$$

Combining these probabilities with actual attention gives $R^C(1)$ and $R^C(2)$ by the same state-weight accounting used in Appendix A 4.1.

Anticipation introduces an additional response to reminders. If patients expect fewer future lapses, they may find it more attractive to wait for a later opportunity. Consequently, neither the reminders-only comparison in Proposition 2 nor the combination day-2 comparison in Proposition 4(a) is guaranteed under arbitrary continuation beliefs. Both proofs use the no-anticipated-lapse restriction. The numerical exercise below therefore evaluates the extended model directly rather than imposing the benchmark signs on it. Partial anticipation alone is not sufficient to guarantee those signs.

For fixed model primitives and positive conditional risk sets, the displayed value functions and refill probabilities are continuous in the subjective lapse probabilities at zero. Any strict benchmark inequality therefore survives sufficiently

small departures from naive continuation. The size of that neighborhood depends on the parameters; this local result does not establish the predictions for every degree of imperfect anticipation.

A 4.2.2. Parameter grid

We consider five equally spaced values of $h(1)$,$h(2)$,$\mu_c \in [1,20]$, $\theta \in [0,.8]$, $\Delta\theta \in [-.8,0]$, and $\gamma \in [1,50]$, subject to $h(2) \geq h(1)$ and $0 \leq \theta + \Delta\theta \leq \theta$. There are 5625 admissible vectors. The discount factor is $\delta = .8$. Refill costs follow a normal distribution with location μ_c and scale 1, truncated below zero to match the nonnegative support in the analytical model. We calculate expectations and state probabilities directly, without simulated patient draws, and retain every admissible vector, including those near zero or unit refill probabilities.

We evaluate three continuation beliefs: $\eta^a = \lambda\theta^a$ with $\lambda = 0,.5,1$. These represent naive continuation, partial anticipation, and accurate anticipation of future lapses within each arm. In particular, $\lambda = .5$ means that patients anticipate half the actual lapse probability. Table A10 reports the sign of the combination-minus-incentives difference in Π, counting differences within 10^{-8} as ties.

When reminders improve attention, the combination raises the on-time share in 3622 of 3750 vectors (96.6%) in the benchmark, 3538 (94.3%) under partial anticipation, and 3500 (93.3%) under accurate anticipation. Thus the positive aggregate comparison extends across most of the illustrated grid when patients anticipate future lapses. The exceptions show why the conditional proposition should not be stated as an unconditional ranking. When attention is unchanged, learning the deadline can instead encourage waiting, and the combination need not improve on incentives alone.

The parameter ranges are illustrative rather than estimated from this experiment; grid frequencies are not probabilities that the comparison is positive in the population. The exercise shows how a positive aggregate effect can arise over a broad range of circumstances. Its outcome is the on-time refill share within one window, not an identity for empirical MPR or cumulative refills across windows.

Table A10: *Numerical comparison of combination and incentives: on-time refill share*

Future inattention	Attention response	Vectors	Higher	Equal	Lower
Not anticipated	Attention improves	3,750	3,622	120	8
Not anticipated	Attention unchanged	1,875	358	898	619
Partly anticipated	Attention improves	3,750	3,538	117	95
Partly anticipated	Attention unchanged	1,875	437	1,184	254
Correctly anticipated	Attention improves	3,750	3,500	107	143
Correctly anticipated	Attention unchanged	1,875	497	1,233	145

Notes: Entries classify the combination-minus-incentives difference in the single-window on-time refill share Π over the parameter grid in Appendix A 4.2. Subjective inattention is $\eta^a = \lambda\theta^a$; the three blocks vary λ. Differences within 10^{-8} count as ties. Grid frequencies are illustrative, not estimated population probabilities.

A 4.3. Future Reward Opportunities with a Known Deadline

Extend Proposition 1 to repeated refills, retaining perfect attention and deadline recall. Let t count days since enrollment, $H = 180$ be the last reward day, and $z = d - t$ measure signed supply relative to due date d, using the empirical stock convention. A refill adds Q days of supply (normally 30). Tomorrow's states are

$$z_R = \max\{z,0\} + Q - 1, \qquad z_W = z - 1.$$

The next deadline is $t + \max\{z,0\} + Q$, preserving stock carryover. The financial arm's current bonus is $\gamma \mathbb{I}\{t \leq H,\, z \geq 0\}$; missing it does not cancel subsequent eligibility.

Let $V_t^j(z)$ be optimal expected utility before the i.i.d. cost draw, with $j = F$ for incentives and $j = 0$ otherwise. Arms share health payoffs and cost distribution F_c. Medication needs continue after rewards end: $V_{H+1}^F(z) = V_{H+1}^0(z) = U(z)$, the ongoing no-reward value.

Write $\Delta V_t = V_t^F - V_t^0$. After expiry ($z < 0$), the current bonus is zero. Common health payoffs cancel when subtracting refill-cost thresholds:

$$c_F^*(t,z) - c_0^*(t,z) = \delta\big[\Delta V_{t+1}(z_R) - \Delta V_{t+1}(z_W)\big]. \tag{A10}$$

If the right-hand side is positive, incentives raise the threshold and, at interior thresholds, the refill probability $F_c(c_j^*)$ despite accurate recall.

For intuition, when a short delay merely postpones subsequent reward opportunities, returning sooner brings future payments forward by a few days. These payments are discounted for their timing and weighted by the probability of earning them. Even with accurate deadline recall, patients cannot be certain that they will refill on time at the next opportunity: costs or other constraints may again prevent timely return. If delaying by k days only postpones an otherwise unchanged reward expected with probability p at delay L, the gain from receiving it sooner is $p\gamma\delta^L(1 - \delta^k)$. This expression isolates the timing benefit; returning sooner can also preserve opportunities that would otherwise be lost before graduation.

This continuation motive operates both before and after the current deadline. It need not generate a positive late-period estimate: if the patients most responsive to future rewards return earlier, those remaining overdue may be less likely to refill. The extension therefore permits post-deadline motivation without mistaken eligibility, but does not establish its magnitude relative to the memory channel. Reminders may also strengthen this motive by making future reward opportunities more salient and helping patients plan and follow through on timely returns.

A 5. Further Evidence on Refill Mechanisms

A 5.1. Timing Definitions and the All-Date Comparison

Refill timing is measured relative to the refill due date defined in Section 2.2. The analysis calculates this date from recorded medication supplies under prescribed daily use and applies the same rule across treatment arms. Let s_{it} denote the pre-refill stock measure, distinct from the model's four-valued pill state. The empirical timing bins are $s \geq 6$, $0 \leq s \leq 5$, $-5 \leq s \leq -1$, and $s \leq -6$; an on-time refill satisfies $s_{it} \geq 0$. Every arm uses the same rule, independent of whether a call or payment is recorded.

Equation (6) uses each period reached before a window closes, controlling for the full PAP covariate set together with period and refill-number fixed effects. Both panels of Table A11 include January due dates. Calling operations were disrupted in January, with some contacts delivered after the intended reminder window: 19 of the 32 successful contacts associated with logged January due dates occurred in a catch-up round on 4 February. In Panel A, the combination-minus-incentives estimate is larger one to five days late (9.6 percentage points) than just before the deadline (6.0 points). This pattern is consistent with the importance of timely reminder delivery, although the comparison does not isolate the effect of delayed calls. Panel A uses the full primary sample; Panel B uses the existing early-enrollment cohort with potential follow-up of 180 days before COVID, retaining earlier individual endpoints. The restricted cohort contains 200 participants in the four core arms. Its near-deadline combination-minus-incentives estimate is 9.0 percentage points ($p = 0.035$), while the incentive estimate one to five days late is 7.5 points ($p = 0.326$). The near-deadline combination increment is significant at the 5% level, while the post-deadline incentive estimate remains imprecise.

Table A11: *Conditional refill timing including all due dates*

	Reminders vs. control	Incentives vs. control	Combination vs. incentives
Panel A. Full primary sample			
At least 6 days early	9.090***	8.490***	-3.663
	(2.707)	(2.333)	(2.540)
0–5 days early	4.810	1.646	5.978
	(4.341)	(4.765)	(3.695)
1–5 days late	1.832	7.072	9.580*
	(5.664)	(4.893)	(5.227)
At least 6 days late	4.636	2.509	1.826
	(6.382)	(7.791)	(9.772)
PAP covariates	Yes	Yes	Yes
Person-period observations	1818	1883	2180
Participants	213	216	245
Recruitment-day clusters	37	37	41
Panel B. Early-enrollment restricted sample			
At least 6 days early	10.310**	8.428***	-4.820
	(4.587)	(2.711)	(3.300)
0–5 days early	4.561	-3.925	8.998**
	(5.905)	(6.482)	(3.848)
1–5 days late	1.811	7.534	4.371
	(7.699)	(7.355)	(7.086)
At least 6 days late	-7.979	2.313	1.243
	(11.194)	(9.967)	(11.568)
PAP covariates	Yes	Yes	Yes
Person-period observations	887	1032	1289
Participants	85	96	115
Recruitment-day clusters	12	13	15

Notes: Includes January 2020 due dates. Panel A uses 1606 windows from 458 participants; Panel B uses 849 windows from 200 participants enrolled by 26 September 2019. Both use the four core arms and the specification in Table 3, including period and refill-number fixed effects and the full PAP control set. Coefficients are percentage points; parentheses contain recruitment-day clustered standard errors. Stars: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A 5.2. Cumulative Refills in a Stable Cohort

Table A12 uses the same 330 participants at every milestone, retaining the four core treatment arms and the covariates used in Table 4. Eligibility depends on administrative follow-up through the fourth milestone, not on making a refill. Non-returners and earlier death, transfer and loss-to-follow-up endpoints remain included, and only documented qualifying refills count. The fourth column therefore coincides with Table 4, while the earlier columns show cumulative outcomes for the same cohort. The combination-minus-control effect is 0.41 larger at the fourth than at the first milestone ($p = 0.002$); the corresponding combination-minus-incentives difference is 0.32 ($p = 0.004$). These contrasts use the change in cumulative counts within each patient, with the same covariates and recruitment-day clustering. They describe accumulated gains over the longer horizon, rather than an increasing effect per refill opportunity.

Table A12: *Cumulative on-time refills in a stable four-arm cohort*

	(1) First milestone	(2) Second milestone	(3) Third milestone	(4) Fourth milestone
Financial incentives	-0.019	-0.076	0.038	0.074
	(0.115)	(0.164)	(0.162)	(0.179)
Reminders	0.009	0.088	0.123	0.240
	(0.087)	(0.137)	(0.159)	(0.180)
Combination	0.002	0.082	0.239	0.414**
	(0.090)	(0.138)	(0.158)	(0.175)
Combination − incentives	0.021	0.158	0.201	0.340**
	(0.108)	(0.142)	(0.133)	(0.145)
Count horizon	B_i	$B_i + 30$	$B_i + 60$	$B_i + 90$
Outcome range	0–1	0–2	0–3	0–4
PAP covariates	Yes	Yes	Yes	Yes
Participants	330	330	330	330
Recruitment-day clusters	52	52	52	52
Control mean (refills)	0.532	0.818	0.948	1.091

Notes: All columns use the same 330 participants eligible through the fourth milestone, with outcomes, comparisons and specifications as in Table 4. The fourth-minus-first change in the combination effect is 0.412 (SE 0.124, $p = 0.002$) relative to control and 0.319 (SE 0.105, $p = 0.004$) relative to incentives. Change regressions account for covariance across horizons. Parentheses contain recruitment-day clustered standard errors. Stars denote nominal two-sided p-values: *** $p < .01$, ** $p < .05$, * $p < .10$.

A 5.3. Refills and Reported Pickup Appointments

Figure A2 compares initial appointments (Panel A) with initial and subsequent appointments (Panel B). Exact-date returns account for 29.2% of initial appointments and 12.9% of the broader set. Neither panel detects differences across treat-

ment arms in exact-date attendance (covariate-adjusted joint $p = 0.713$ and 0.956, respectively). These patterns are descriptive: later appointments depend on prior refilling, and the alignment does not identify the causal effect of appointments.

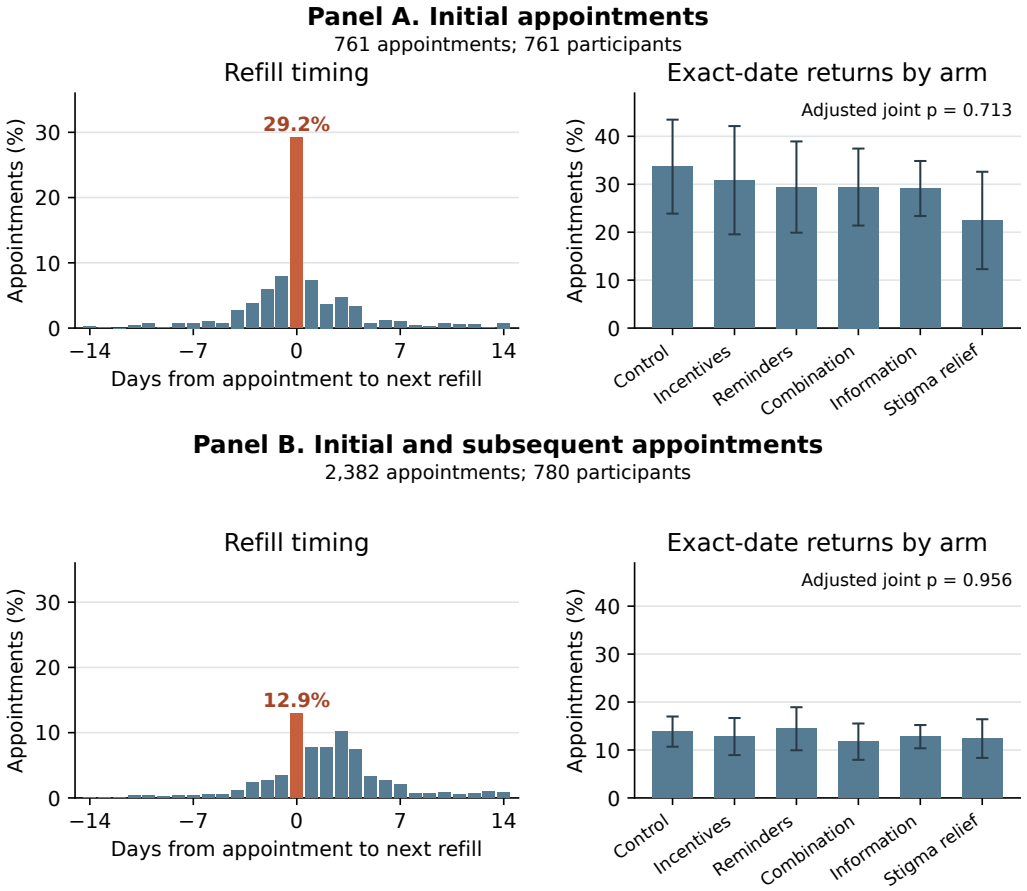


Figure A2: Refills Relative to Initial and Subsequent Pickup Appointments

Notes: Panel A uses enrollment-reported pickup dates; Panel B adds distinct valid subsequent appointments recorded by either source at reconciled refills, counting each participant–date once at first issuance. Eligible appointments have a full ±14-day interval after issuance and within follow-up; non-returners remain included. Left panels show the daily first-refill share. Right panels show exact-date shares with recruitment-day clustered 95% confidence intervals; joint tests adjust for the main covariates. Panel B comparisons are descriptive because later appointments depend on prior refilling.

A 6. Cost Calculations

Unit costs and administrative allowances. We use a benchmark cost of 12.5 MT (approximately US$0.20) per call. The staff position was paid 5,000 MT per month. Assuming half of this position's time supports reminder calls and maintenance of reminder records, and an average workload of 200 calls per month, the implied cost is

$$\frac{0.50 \times 5{,}000}{200} = 12.5 \text{ MT per call.}$$

The time allocation and monthly workload are costing assumptions. The reward is 200 MT (approximately US$3.22), using the same historical dollar benchmark as the intervention description. Our preferred implementation budget allows for 4 calls per qualifying reminder, giving US$0.80, and adds a 50% allowance to each eligible reward for payment processing, materials and administration, giving US$4.83. These conservative budgeting allowances are assumptions, not measured resource use or fees. Payment administration excludes reminder work already included in the calling allowance.

Population and budget quantities. All delivery quantities come from Table A4. Let R_g denote qualifying reminder cycles, E_g eligible on-time refills in reward arms, Q_g eligible reminder cycles and N_g assigned participants in arm g. Quantities are zero in arms without the corresponding intervention. The incentive, reminder and combination arms contain 110, 107 and 135 participants, respectively. We budget a reward for every eligible on-time refill. Average budgets divide by everyone assigned to the arm, including participants receiving no intervention. The preferred budget per participant is

$$C_g = \frac{\$0.80\, R_g + \$4.83\, E_g}{N_g}.$$

This is an assumed implementation budget relative to standard care. Research, routine care, patient costs and costs outside the stated allowances are excluded.

Sensitivity analyses. Panel B of Table 5 doubles the base call price to US$0.40, retaining the assumed calls per qualifying reminder and payment allowance. Panel C instead budgets one call per eligible reminder cycle and one reward per eligible on-time refill at base prices, without administrative allowances:

$$C_g^{\text{simple}} = \frac{\$0.20\, Q_g + \$3.22\, E_g}{N_g}.$$

All scenarios hold observed refill behavior and estimated treatment effects fixed; they illustrate alternative budgets rather than predict outcomes under different implementation.

Cost-effectiveness calculations. If β_g is the Table 1 treatment effect on MPR $\geq$ 95% in probability units, cost per additional high-coverage patient relative to standard care is C_g/β_g. Adding reminders to incentives costs $(C_C - C_F)/(\beta_C - \beta_F)$ per additional high-coverage patient, where C denotes the combination and F incentives alone. The analogous formula compares the combination with reminders. Ratios use unrounded costs and effect estimates. The ordinary effect confidence intervals in Table 5 describe effect uncertainty; the ratios are point estimates, not confidence intervals or tests of a cost-effectiveness ranking.

Table A13: *Inputs and components of the preferred implementation budget*

	Incentives	Reminders	Combination
A. Assigned participants and Table A4 quantities			
Assigned participants	110	107	135
Qualifying reminders	0	120	182
Eligible on-time refills	148	0	193
Eligible reminder cycles	0	315	415
B. Preferred budget per assigned participant (US$)			
Reminder calling allowance	0.00	0.90	1.08
Rewards and payment administration	6.50	0.00	6.91
Total implementation budget	6.50	0.90	7.98

Notes: Delivery quantities are those in Table A4; each qualifying cycle or refill is counted once. The preferred budget assigns US$0.80 per qualifying reminder and US$4.83 per eligible on-time refill. These include assumed allowances for contact effort and payment administration, respectively; they do not measure actual resource use or fees. Average budgets divide by all assigned participants, including those receiving no intervention. Eligible reminder cycles enter only the simple-budget sensitivity in Table 5. All calculations use unrounded inputs.

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Supplemental material for initial submission

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

This supplement documents the experimental design, participant selection and eligibility, participant instructions, and ethics review as analyzed in the paper. It is supplied for editorial and referee review and is not proposed as a published journal appendix.

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Declarations

Conflicts of interest: All co-authors declare that they have no conflicts of interest.

Data and code availability: Upon acceptance, the authors will make de-identified study data, code, and documentation needed to reproduce the results available in accordance with The Economic Journal's data and code availability policy.

1. Experimental design and participant selection

1.1 Setting, eligibility and recruitment

The experiment took place at Munhava Health Center in Beira, Mozambique. The study population was adults living with HIV who had not yet started antiretroviral therapy (ART) or had recently initiated ART.

Eligibility criteria:
1. Age 18 or above.
2. Diagnosed HIV infection.
3. No ART initiation or less than three months on ART.
4. Access to an own or shared family telephone.

Clinic providers referred eligible patients to the study team after their routine visit. An enumerator approached the patient in a private location, explained the study, confirmed eligibility and obtained informed consent before administering the baseline survey. Participation was voluntary; declining participation or withdrawing did not affect usual clinical care. The baseline survey collected demographic and household information, economic circumstances, treatment history, and knowledge and beliefs about HIV and ART.

Surveys were administered in Portuguese using electronic tablets. For people unable to read the consent form, it was read in their native language. Participants not speaking Portuguese could be assisted by a translator or an enumerator able to speak the local language. Participants unable to write could give oral consent, with the enumerator recording a witness's name and signature. The consent materials are reproduced in Section 4.

1.2 Assignment and treatment groups

Planned enrollment was 2,100 participants, with 300 randomly assigned to each of the treatment groups below.

Assigned arm	Intervention
Control	Usual clinical care; no additional study intervention.
Financial incentives	200 meticais for each qualifying on-time medication refill during six months.
Reminders	Telephone reminders of upcoming refills during six months.
Incentives + reminders	The same refill incentive and reminder interventions together.
Information	Information video about HIV, ART, and adherence.
Stigma relief	Feedback about community attitudes when the participant overestimated stigma.
Information + stigma relief	Both information and stigma-relief interventions.

1.3 Intervention timing and follow-up

Financial incentives consisted of 200-metical rewards for on-time medication refills over six months. Telephone reminders were provided three days before upcoming refills. Follow-up and intervention completion were scheduled over 180 days.

Administrative clinic and pharmacy records were linked to survey participants to measure attendance and medication refills. A short telephone follow-up, approximately one month after recruitment, re-elicited beliefs. The primary analysis ends observation at 24 March 2020 as described in the article.

1.4 Other participant interactions

Participants receive an automatic cash transfer of 80 meticais for their time completing the enrollment survey. Participants could also receive barcoded HIV-testing referral coupons, valid for one month. A person presenting a coupon after testing at Munhava could receive 100 meticais; the original participant could receive 100 meticais when one of their coupons was first used, at most once. The referral component was common to the study rather than an additional randomized arm.

2. Ethics approval and informed consent

The main study was approved by the University of Michigan Medical School Institutional Review Board (IRBMED), study HUM00133179, on 30 November 2017. The protocol underwent several amendments. On 12 July 2019, IRBMED approved amendment Ame00083464, adding three intervention arms and increasing planned enrollment. This established the seven treatment groups described in Section 1, including control.

The main analysis in the paper followed the protocol after this amendment and uses data collected under that protocol. This supplement describes the corresponding experimental design and participant interactions. Earlier protocol descriptions and subsequent COVID amendments are excluded.

In Mozambique, approval was granted by the Ministry of Health's Comité Nacional de Bioética para a Saúde (CNBS; IRB registration IRB00002657), under approval-letter reference 43/CNBS/19 dated 22 February 2019.

The survey supplying the community stigma benchmarks was reviewed by the University of Michigan Health Sciences and Behavioral Sciences Institutional Review Board (IRB-HSBS), study HUM00141271. The board issued its exemption determination on 14 February 2018, under the survey/interview exemption category then cited as 45 CFR 46.101(b)(2). This determination is separate from the trial's IRBMED approval.

The consent process explained the study, voluntary participation, the right to decline questions or withdraw without penalty, risks and confidentiality, and use of linked clinic records. Consent was obtained before study participation. The original-language (Portuguese) and English consent versions are included in Section 4. These are blank forms, not participant records.

Record	Approval date and reference
Trial initial approval	30 November 2017; HUM00133179; IRBMED
Seven-arm amendment	12 July 2019; Ame00083464; HUM00133179
Mozambique approval letter	22 February 2019; 43/CNBS/19; CNBS IRB00002657
Stigma-survey exemption	14 February 2018; HUM00141271; IRB-HSBS

3. Recruitment instructions

The Portuguese and English recruitment scripts follow. Eligibility criteria are described in Section 1.1.

3.1 Portuguese

"Meu nome é _____ e o nome do meu colega é _____. Estamos aqui em nome de uma parceria entre a Universidade de Michigan e Centro de Investigação Operacional da Beira, Instituto Nacional de Saúde de Moçambique. Estamos a levar a cabo um estudo para entender melhor como melhorar a adesão ao tratamento anti-retroviral do HIV. Neste estudo, implementaremos e avaliaremos vários métodos para ajudar os pacientes com HIV a melhorar a adesão ao tratamento.

Posso fazer uma pergunta para determinar sua elegibilidade?

(prossiga as seguintes perguntas após o assunto potencial dar resposta afirmativa)

1. Existe um número de telefone para os oficias de saúde entrar em contato com você para assuntos relacionados à saúde?

(se as respostas a ambas as perguntas forem positivas, então prossiga)

Você é elegível para o nosso estudo. Vamos pesquisá-lo sobre suas características gerais, antecedentes familiares, atividades econômicas, saúde, história sexual e crenças. Vamos relacionar a sua informação aos seus registros médicos da Unidade Sanitária. Todas as suas respostas e informações serão tratadas com rigorosa confidencialidade.

Antes de começar a pesquisa, preciso que o/a senhor(a) leia e preencha uma folha de consentimento informado.

(Se a resposta a uma das perguntas de triagem for negativa, O paciente não é elegível)

Obrigado pelo seu interesse, você não é elegível para o estudo.

3.2 English

"My name is _____ and my colleague's name is _____. We are here on behalf of a partnership between the University of Michigan and Beira Operational Research, National Institute of Health, Mozambique. We are conducting a study to better understand how to improve HIV antiretroviral therapy adherence. In this study, we will implement and evaluate several methods to help HIV patients improve medicine adherence.

May I ask you a question to determine your eligibility?

(proceed with the following questions after the potential subject gives affirmative response)

Is there a phone number for the clinics to contact you for health-related matters?

(if the answers to both questions are positive, then proceed to recruit)

You are eligible for our study. We will survey you about your general characteristics, family backgrounds, economic activities, health, sexual history, and beliefs. We will link your information to your medical records from the clinic. All your responses and information will be treated with strict confidentiality.

Before we begin the survey, I need you to read and fill out an informed consent sheet.

(if the answer to either of the screening questions are negative, then this patient is not eligible)

Thanks for your interest, you are not eligible for the study.

4. Informed-consent materials

The Portuguese and English consent forms follow.

4.1 Portuguese consent

Intervenções para Melhorar a Adesão à Medicação Anti-retroviral para o Tratamento do HIV/SIDA na Beira, Moçambique.

Formulário de Informação ao Participante do Inquérito

Investigador Principal: James Riddel IV, M.D.

Co-Investigadores: Dean Yang, Arlete Mahumane, Hang Yu, Vicente Chaúque

Organização: University of Michigan Medical Center

Organização Financiadora: Fundos do Departamento de Economia da Universidade de Michigan

O(a) senhor(a) é convidado(a) a participar neste estudo intitulado Intervenções para Melhorar a Adesão ao Tratamento Anti-retroviral na Beira, Moçambique que terá lugar no Centro de Saúde Urbano da Munhava. Se decidir fazer parte do estudo, deverá assinar este formulário de informação. Por isso é importante que (o) senhor(a) entenda claramente a informação apresentada nesta folha porque esta explica os seus direitos e responsabilidades. Nesta folha estão explicados todos os objectivos, possíveis riscos e benefícios relacionados com o estudo. Este formulário também informa que o(a) senhor(a) tem o direito de não participar ou retirar-se do estudo a qualquer momento. O(a) senhor(a) tem o direito de colocar perguntas a qualquer momento. Este estudo é financiado pelo Fundos do Departamento de Economia da Universidade de Michigan.

A sua participação é voluntária. Tem o direito de conversar sobre a sua participação com outras pessoas como por exemplo amigos, familiares ou médicos para decidir participar ou não no estudo. Caso aceite participar no estudo receberá uma cópia desta folha que deve guardar consigo enquanto o estudo decorrer.

Porque o estudo está a ser feito?

Muitas pessoas que vivem com HIV enfrentam desafios em tomar a sua medicação todos os dias devido a dificuldade em adquiri-los

Objectivo e descrição do estudo

Neste estudo queremos identificar formas de fazer com que as pessoas que vivem com HIV/SIDA tenham um tratamento antirretroviral (TARV) sem interromper.

Como será realizado o estudo

O estudo vai contar com 2100 participantes. Que devem ser adultos com 18 ou mais anos de idade que tenham sido diagnosticados com infeção de HIV e que tenham acesso a um telemóvel.

Se decidir fazer parte neste estudo, a sua participação envolverá:

O/A senhor(a) será alocado ocasionalmente em um dos sete grupos de estudo. Nem você nem o(s) pesquisador(es) poderão escolher o grupo ao qual você será alocado. Dependendo do sorteio, iremos efectuar diferentes intervenções como a lembrança mensal de chamada telefónica, vídeos informativos para ajudá-lo a prosseguir com o seu tratamento anti-retroviral..

Ser-lhe-ão colocadas questões acerca do seu contexto familiar, condição económica, saúde, história sexual e crenças. Todas as suas respostas e informação serão tratadas de forma estritamente confidencial.

Os seus registos médicos e da farmácia serão revistos, e iremos recolher informação acerca da sua contagem de CD4, reabastecimento e adesão aos medicamentos.

Você poderá continuar a receber sua normal medicação fornecida pelo hospital sem nenhuma alteração e cumprir com todas visitas agendadas.

No final do inquérito, ofereceremos cupões para indicar outras pessoas para fazer teste o HIV. O teste é anônimo e, mediante seu consentimento, rastrearemos o uso do cupão.

Após seu consentimento, em um mês a partir de hoje, poderemos estabelecer contacto com o(a) senhor(a) por via de telefone para fazer novamente algumas perguntas que fizemos hoje. O inquérito através do telefone pode durar cerca de 5 minutos.

Enquanto participante deste estudo, o/a senhor(a) tem a responsabilidade de responder ao nosso inquérito.

Este inquérito tomará cerca de 30 minutos do seu tempo. Alguns grupos irão também receber chamadas telefónicas uma vez por mês nos próximos 6 meses. Cada chamada durará menos de 5 minutos.

Para além da duração acima indicada, recolheremos informação dos seus registos médicos por mais 6 meses depois da sua participação. A maioria dos participantes irá concluir a sua participação no estudo dentro de 6 meses. A duração total esperada do estudo é de um ano.

Possíveis riscos e inconvenientes

Que riscos irei correr por participar neste estudo? Que medidas serão tomadas pelos investigadores para me proteger contra estes riscos?

Os riscos conhecidos e esperados são:

A sua participação envolve o risco de perda de privacidade de informação. A probabilidade deste risco acontecer é baixa (i.e., incidência de aproximadamente 1%). Uma vez que este inquérito inclui questões relacionadas com HIV e que os registos médicos incluem testagem e tratamento de HIV, a perda desta informação poderá causar um risco de dano social dado o estigma em relação ao HIV.

Poderemos fazer-lhe algumas perguntas que podem deixá-lo desconfortável devido à sensibilidade das questões.

Por exemplo, vamos perguntar-lhe sobre sua saúde, seu histórico médico e o estado de HIV dos membros da sua família.

Os investigadores irão tentar minimizar estes riscos através de:

As seguintes medidas serão tomadas para assegurar que a confidencialidade dos participantes seja protegida: todo o inquérito será conduzido num ambiente privado. Os registos médicos serão recolhidos pelos membros da equipa de inquérito que receberão formação acerca de como lidar com dados confidenciais. Dados pessoais identificáveis serão armazenados de forma segura em dispositivos protegidos com palavra-passe e encriptados. Apenas investigadores afiliados ao estudo terão acesso a informação pessoal identificável.

Após a conclusão do estudo (e depois de um período de 5 anos de armazenamento), toda a informação pessoal identificável será destruída. De nenhuma forma serão armazenados nomes ou outra informação dos participantes que os possa identificar.

O/A senhor(a) tem a opção de rejeitar qualquer questão que o deixa desconfortável.

Como em qualquer estudo, poderão existir outros riscos que sejam desconhecidos ou inesperados.

O que poderá acontecer se o participante se magoar, ficar doente, ou tiver outros problemas decorrentes da sua participação no estudo?

Os investigadores tomarão medidas para minimizar os riscos deste estudo. Mesmo assim, o/a senhor(a) poderá experienciar problemas ou efeitos secundários, mesmo tendo os investigadores sido cuidadosos para os evitar. Por favor, avise os investigadores indicados como pessoas de contacto caso sinta qualquer efeito secundário, ou outros problemas que possa ter durante o estudo. Deverá também avisar os médicos que o atendem regularmente.Não será feita nenhuma alteração à sua medicação como parte deste estudo.

Se eu participar neste estudo, poderei também participar noutros estudos?

Participar em mais do que um estudo simultaneamente, ou mesmo em momentos diferentes, poderá aumentar os riscos para si. Isto poderá também afectar os resultados destes estudos. O/A senhor(a) não deverá participar em mais do que um estudo sem a aprovação dos investigadores envolvidos neste estudo.

Será que os investigadores me irão informar se obtiverem nova informação que possa mudar a minha vontade de permanecer no estudo?

Sim, os investigadores irão informar se obtiverem informação nova que possa mudar a sua vontade de permanecer no estudo. Se nova informação lhe for fornecida após o começo do estudo, é possível que lhe seja pedido para assinar uma nova folha de consentimento informado que inclua esta informação.

Se eu decidir não participar neste estudo, que outras opções tenho?

Quer o/a senhor(a) decida participar ou não, poderá sempre receber tratamento regular como prescrito pelo seu médico. Este estudo não irá substituir qualquer tratamento fornecido pelo seu médico.

Benefícios

Como poderei me beneficiar se decidir participar neste estudo? Como poderão beneficiar os outros?

O/A senhor(a) poderá ou não obter qualquer benefício pessoal por participar neste estudo. Contudo, algumas das intervenções poderão ajudar alguns participantes a melhorar a sua adesão à medicação do HIV, o que terá um impacto positivo na sua saúde. Este estudo poderá sugerir formas eficazes de melhorar a adesão à medicação para o tratamento do HIV, o que poderá beneficiar outros pacientes no futuro.

Compensações

Quem irá pagar os custos do estudo? Serei eu ou o meu seguro de saúde a cobrir os custos deste estudo?

Não existem custos decorrentes deste estudo.

Ao assinar esta folha, o/a senhor(a) não está a abdicar do seu direito de ser ressarcido caso sofra algum dano resultante da sua participação no estudo.

Será que eu irei ser pago ou me será oferecido algo por participar neste estudo?

Não haverá compensação pela sua participação no estudo, contudo oO/A senhor(a) receberá lembrança da equipa do estudo constituída por uma recarga de 80 Meticais da operadora da rede de telefone móvel da sua preferência como gratificação em fazer parte do estudo.

Ser-lhe-á oferecido algum cupão de referência para o teste do VIH na inscrição. Se alguém fizer um teste de HIV no Munhava Health Center e apresentar um cupom para nossa equipe de pesquisa dentro de um mês, recompensaremos o assunto original em 100 Mts. Cada sujeito original receberá essa recompensa no máximo uma vez.

Quem poderá beneficiar financeiramente dos resultados deste estudo?

Ninguém nem nenhuma organização tem interesse financeiro no resultado deste estudo.

Protecção da privacidade

Como irão os investigadores proteger a minha privacidade?

Serão tomadas as seguintes medidas para garantir que a confidencialidade dos participantes está protegida:

Todas as perguntas do inquérito serão colocadas num ambiente privado. Os registos de saúde serão recolhidos pela equipa de inquérito que irá receber formação sobre como lidar com dados confidenciais. Informação pessoal identificável será armazenada de forma segura em dispositivos protegidos com palavra-passe e encriptados. Apenas investigadores afiliados ao estudo terão acesso a informação pessoa identificável. Após a conclusão do estudo (e depois de um período de armazenamento de 5 anos), toda a informação pessoal identificável será destruída. Não serão mantidos os nomes dos participantes nem outra informação que os possa identificar.

Que informação sobre mim poderá ser vista por outros investigadores ou por outras pessoas? Porquê? Quem poderá vê-la?

Ao assinar este consentimento o/a senhor(a) está a dar a sua permissão para obtermos, usarmos, e partilharmos informação sobre si para este estudo, e este é um dos requisitos para a sua participação. Informação sobre si poderá ser obtida de qualquer hospital, médico, e outro serviço de saúde envolvido no seu tratamento, incluindo:

Resgistos do hostipal/ médico, incluindo resultados de testes médicos (raios-X, análises ao sangue, análises à urina, etc.)

Diagnóstico de HIV/SIDA

Diagnóstico de doenças sexualmente transmissíveis e/ou outras doenças contagiosas

Todos os registos sobre a sua condição de saúde, tratamento que recebeu, e a sua reacção ao tratamento

Informação Farmacêutica

Informação Demográfica

Informação pessoal identificável

Existem várias razões pelas quais a informação sobre si poderá ser usada ou vista pelos investigadores ou outros durante ou após a conclusão do estudo. Exemplos incluem:

Os investigadores podem precisar da informação para garantir que pode participar no estudo.

Os investigadores podem precisar da informação para verificar os resultados dos seus exames médicos ou para identificar efeitos secundários.

University, Food and Drug Administration (FDA) e outras autoridades, auditors e/ou o IRB podem precisar da informação para garantir que o estudo decorre de forma segura e correta.

Os financiadores do projecto, ou as autoridades e comités de monotoria de segurança podem precisar da informação para:

Garantir que o estudo é feiro de forma segura e correta

Aprender mais acerca de efeitos secundários

Analisar os resultados do estudo

Os investigadores podem precisar da informação para criar uma base de dados acerca da sua condição ou sobre o seu tratamento.

O fornecimento de informação às autoridades poderá ser um requisito por lei. Por exemplo, para prevenir que danos sejam causados a si ou a outros, ou que por razões de saúde pública.

Os resultados deste estudo poderão vir a ser publicados num artigo, mas não irão incluir qualquer informação que possa deixar os outros descobrir quem o/a senhor(a) é.

O que poderá acontecer à informação sobre mim após a conclusão do estudo ou se eu cancelar a minha permissão?

Por regra, os investigadores não irão continuar a usar ou divulgar informação sobre si, mas manterão a sua informação armazenada de forma segura até esta ser destruída. Por vezes, poderá ser necessário continuar a usar e divulgar sua informação, mesmo depois de o/a senhor(a) ter cancelado a sua permissão ou o estudo ter acabado.

Algumas razões para que isto tenha de acontecer incluem:

Para evitar perda dos resultados do estudo que já tenham a sua informação incluida

Para fornecer alguma informação para pesquisas, educação ou outras actividades (Esta informação não incluiria o seu nome, número de INSS, ou qualquer outra informação que possa permitir que outros o identifiquem

Para ajudar a universidade e autoridades a garantir que o estudo decorre de acordo com as normas

Quando expira a minha permissão?

A sua permissão expira no fim do estudo, a não ser que o/a senhor(a) decida cancelá-la antes. Poderá cancelar a sua permissão a qualquer momento escrevendo aos investigadores indicados abaixo.

Comité de Ética

Antes de iniciar, este estudo foi aprovado pelo Comité Nacional de Bioética para a Saúde (Instituto Nacional de Saúde, Ministério da Saúde) e pelo IRB da Universidade de Michigan (Institutional Review Board). Este órgão avalia e faz o seguimento do estudo para garantir que este seja realizado da forma mais segura possível.

Participação Voluntária

Se eu quiser terminar a minha participação neste estudo, o que deverei fazer?

O/A senhor(a) é livre para abandonar o estudo a qualquer altura. Se abandonar o estudo antes do seu final, não haverá qualquer penalização. O/A senhor(a) não irá perder quaisquer benefícios aos quais tenha direito. Se decidir informar os investigadores acerca das razões que o levam a abandonar o estudo, estas poderão fazer parte dos registo do estudo. Se desejar abandonar o estudo antes do fim, por favor informe uma das pessoas indicadas abaixo como pessoas de contacto.

Se eu decidir abandonar o estudo antes do seu fim, será que isso me poderá trazer qualquer dano?

Não haverá qualquer dano ou desvantagem em termos de cuidados médicos que está a ter se decidir abandonar o estudo antes deste estar completo.

Será que os investigadores me poderão retirar do estudo mesmo que eu queira continuar a participar?

Sim. Existem várias razões que poderão levar os investigadores a terminar a sua participação no estudo. Alguns exemplos são:

O investigador acredita que participar no estudo não é benéfico para si.

Se se tornar inelegível para participar.

Se a sua condição de saúde mudar e precisar de tratamento que não seja permitido enquanto participa no estudo.

Se não seguir as instruções dos investigadores.

Se o estudo for suspenso ou cancelado.

Pessoa de contacto no caso de ter perguntas/dúvidas

Quem devo contactar para esclarecimentos acerca deste estudo?

Por favor contacte os investigadores indicados abaixo para:

Obter mais informação acerca do estudo

Colocar questões acerca dos procedimentos ou tratamentos usados no estudo

Falar sobre custos para si ou para o seu seguro de saúde decorrentes do estudo

Reportar qualquer lesão, ferimento, ou outro problema (pode também necessitar de falar com o seu médico)

Abandonar o estudo antes de este terminar

Expressar qualquer preocupação acerca do estudo

Investigador Principal: James Riddell IV, M.D.Endereço: 1500 E Medical Center Drive, University Hospital South F4131, Ann Arbor, MI 48109-5226, U.S.A.Telefone: +1 888-229-7409

Coordenador do Estudo: Vicente ChauqueEndereço: Av. Samora Machel nº45

Bairro de Esturro

Cidade da Beira- Moçambique .Telefone: +258 845106300

Poderá também expressar qualquer preocupação acerca do estudo contactando o The Institutional Review Board indicado abaixo.

University of Michigan Medical School Institutional Review Board (IRBMED)2800 Plymouth RoadBuilding 520, Room 3214Ann Arbor, MI 48109-2800, U.S.A.Telephone: 734-763-4768 (For International Studies: US Country Code: 001)Fax: 734-763-1234e-mail: irbmed@umich.edu

Se tem qualquer preocupação acerca da possível violaçã da sua privacidade ou acerac do estudo poderá contactar University of Michigan Health System Compliance Help Line at 1-866-990-0111. Quando telefona ou escreve acerca de um problema, por favor forneça tanta informação quanto possível, incluindo o nome do investigador, número IRBMED (not topo desta pagina), e detalhes sobre o problema. Isto irá ajudar a universidade a resolver o seu problema. Quando reportar um problema, não terá de indicar o seu nome, a menos que o queira fazer.

Comité Nacional de Bioética para a Saúde

Ministério da Saúde

C.Postal 264

Av. Eduardo Mondlane/Salvador Allende

Maputo - Moçambique

Telefone: +258 82 40 66350

Registo de Informação fornecida

Que documentos me irão ser dados?

A sua assinatura na próxima secção significa que o senhor(a) recebeu uma cópia dos seguintes documentos:

Este consentimento informado. (Nota: Para além da cópia que recebeu, cópias deste documento serão armazenadas um ficheiro confidencial.)

O/A senhor(a) receberá uma cópia do consentimento informado assinado e com data.

Formulário de Consentimento Informado

University of Michigan

Intervenções para Melhorar a Adesão à Medicação Anti-retroviral para o Tratamento do HIV/SIDA na província de Sofala em Moçambique.

eResearch ID number: HUM00133179

Consentimento/Assentimento para participar no estudo

Eu compreendo a informação impressa neste formulário. Eu discuti este estudo, os riscos e potenciais benefícios, e as minha opções com _____. As questões por mim colocadas até agora foram esclarecidas. Eu compreendo que se me surgirem novas questões ou preocupações acerca do estudo ou da minha participação, eu poderei contactar uma das pessoas indicadas como pessoas de contacto. Eu compreendo que eu receberei uma cópia deste formulário no momento em que o assinar e mais tarde sempre que a requisitar. Eu compreendo que se a minha capacidade de dar consentimento ou assentimento mudar, eu ou o meu representante legal poderemos ser chamados a dar novamente o nosso consentimento para a continuação da minha participação no estudo.

Nome Legal: _____

Assinatura: _____

Data da Assinatura (dd/mm/aaaa): _____

Data de Nascimento (dd/mm/aaaa): _____

Número de NID: _____

Investigador Principal ou outro Membro da equipa de Pesquisa

Eu forneci a este participante e ao(s) seu(s)/sua(s) representante(s) legal toda a informação acerca do estudo que eu acredito ser completa e correcta. Este participante e o/a seu(s)/sua(s) representante(s) legal indicaram que compreenderam a natureza do estudo, incluindo riscos e benefícios resultantes da participação.

Nome Legal: _____

Título: _____

Assinatura: _____

Data da Assinatura (dd/mm/aaaa): _____

Testemunha Independente

Eu observei que o participante (ou o(s)/a(s) seu(s)/sua(s) representante(s) legais, se aplicável) assinaram este consentimento.

Nome Legal: _____

Título: _____

Assinatura: _____

Data da Assinatura (dd/mm/aaaa): _____

Nota: O PARTICIPANTE DEVERÁ RECEBER UMA CÓPIA DO CONSENTIMENTO INFORMADO PARA O SEU ARQUIVO.

O CONSENTIMENTO DEVE SER ASSINADO EM DUPLICADO, UMA CÓPIA PARA O PARTICIPANTE E OUTRA PARA O ARQUIVO DO ESTUDO.

4.2 English consent

University of Michigan

Consent To Be Part Of A Research Study

Information About tHIS form

You may be eligible to take part in a research study. This form gives you important information about the study. It describes the purpose of the study, and the risks and possible benefits of participating in the study.

Please take time to review this information carefully. After you have finished, you should talk to the researchers about the study and ask them any questions you have. You may also wish to talk to others (for example, your friends, family, or other doctors) about your participation in this study. If you decide to take part in the study, you will be asked to sign this form. Before you sign this form, be sure you understand what the study is about, including the risks and possible benefits to you.

1. General Information About This Study AND the RESEARCHERS

1.1 Study title:

Interventions to improve HIV antiretroviral therapy adherence in Sofala province Mozambique

1.2 Company or agency sponsoring the study:

Michigan Institute for Teaching and Research in Economics (MITRE) Faculty Research Fund

1.3 Names, degrees, and affiliations of the researchers conducting the study:

James Riddell IV, MD Clinical Professor of Internal Medicine, Michigan Medicine, University of Michigan.

Dean Yang, PhD, Professor, Department of Economics and the Ford School of Public Policy, University of Michigan.

Hang Yu, PhD Candidate, Department of Economics, University of Michigan.

2. PURPOSE OF THis STUDY

2.1 Study purpose:

Antiretroviral Therapy (HIV medication also known as “ART”) is the recommended treatment for all people living with HIV by World Health Organization (WHO). For HIV medication to work effectively, it must be taken every day without interruption. Many people living with HIV fail to adhere to their medication correctly because of limited knowledge about the ART process, high cost of obtaining the medication, forgetting of medical appointment or concerns about a stigmatizing public attitude. In this study, we will explore ways to improve antiretroviral therapy (ART) medication adherence among HIV patients by ways such as sending reminders or providing information about HIV stigma. We also will be examining the impact of HIV on overall health as well as household economic outcomes.

3. Information About STUDY participants (SUBJECTS)

Taking part in this study is completely voluntary. You do not have to participate if you don't want to. You may also leave the study at any time. If you leave the study before it is finished, there will be no penalty to you, and you will not lose any benefits to which you are otherwise entitled. This study will have no impact on the medical care that you are currently receiving. No changes in your HIV medicine will occur as part of this study.

3.1 Who can take part in this study?

Adults 18 years and older who have been diagnosed with HIV infection and are taking HIV medication and have access to a cell phone.

3.2 How many people (subjects) are expected to take part in this study?

2100 subjects are expected to participate.

4. information about study participation

4.1 What will happen to me in this study?

If you decide to participate in this study the following will occur:

You will be assigned by chance to a study group. Your chance of being assigned to a group is 1 in 7. Neither you nor the researcher(s) can choose the group to which you will be assigned. We will offer different interventions, such as monthly reminder phone calls, a one-time information session about ART or about the public attitudes toward HIV, to help you adhere to your ART depending on the random draw. In an ART information session, we will show you a video that explains how anti-retroviral therapy may help you stay healthy and provide some tips to help you adhere to it. In a public attitude information session, we will provide data on the attitudes of people living in this area toward HIV. The public attitudes are assessed from a survey conducted by our research team.

We will perform a survey and ask questions about your family background, wealth, health, sexual history, and beliefs. All your responses and information will be treated with strict confidentiality.

We will review your medical record and pharmacy record and collect information about your CD4 cell count and medication refills and medication adherence.

At the end of the survey, we will offer you coupons to refer others to test for HIV and track their redemption.

In the time window of 15-30 days from today, we will contact you over the phone to re-ask a few questions we asked today. The phone call is expected to last for 5 minutes.

You can opt out of any one or more components of this study described above at any time without penalty.

You should continue to take your HIV medication without change as prescribed by your health care provider and attend regularly scheduled appointments. There are no extra visits as part of this study.

4.2 How much of my time will be needed to take part in this study?

This survey will take about 30 minutes of your time. In the time window of 15-30 days from today, we will contact you over the phone to re-ask a few questions we asked today. The phone call is expected to last for 5 minutes. Certain groups will also receive phone calls once a month in the next 6 months. Each phone call will be less than 5 minutes long.

4.3 When will my participation in the study be over?

In addition to the time above, we will collect information from your medical records for another 6 months after your participation. Most subjects will complete their part in the study within 6 months. The entire study is expected to last about 1 year.

5. information about RISKS and benefits

5.1 What risks will I face by taking part in the study? What will the researchers do to protect me against these risks?

The known or expected risks are:

By taking part of this study, you will face the risk of loss of private information. The likelihood of this risk is rare (i.e., approximate incidence < 1%). Because the survey will include questions related to HIV status and health records will include HIV testing and treatment, loss of these information could pose a serious social/reputational risk due to the stigma attached to HIV.

We will ask you some questions that may make you uncomfortable due to the sensitive nature of these questions. For example, we will ask you about your health conditions, your medical history and family members' HIV status. You may skip any question that makes you feel uncomfortable or that you simply choose not to answer.

The researchers will try to minimize these risks by:

We will take the following steps to ensure participants confidentiality is protected: The entire survey will be conducted in a private setting. Health records will be collected by survey staff who will receive extensive training on handling confidential data. Personally-identifiable data will be securely stored on password protected devices and encrypted. Only researchers affiliated with the study will have access to this personally identifiable data.

After the conclusion of the study (following up to a five-year storage period), all personally identifiable data will be destroyed. We will not store your name or other information that could identify you.

As with any research study, there may be additional risks that are unknown or unexpected.

5.2 What happens if I get hurt, become sick, or have other problems as a result of this research?

The researchers have taken steps to minimize the risks of this study. Even so, you may still have problems or side effects, even when the researchers are careful to avoid them. Please tell the researchers listed in Section 10 about any side effects, or other problems that you have during this study. You should also tell your regular doctors. No change in your medications will occur as part of this study.

5.3 If I take part in this study, can I also participate in other studies?

Being in more than one research study at the same time, or even at different times, may increase the risks to you. It may also affect the results of the studies. You should not take part in more than one study without approval from the researchers involved in each study.

5.4 How could I benefit if I take part in this study? How could others benefit?

You may or may not receive any personal benefits from being in this study. However, some of our interventions may help some subjects improve their HIV medication adherence rate which will have a beneficial impact on health. This research may lead to the development of effective ways to improve HIV medication adherence which can benefit other patients in the future.

5.5 Will the researchers tell me if they learn of new information that could change my willingness to stay in this study?

Yes, the researchers will tell you if they learn of important new information that may change your willingness to stay in this study. If new information is provided to you after you have joined the study, it is possible that you may be asked to sign a new consent form that includes the new information.

6. Other options

6.1 If I decide not to take part in this study, what other options do I have?

Whether you decide to take part in this study, you can always choose to receive the regular treatment as prescribed by your health care provider. This study will not replace any treatment provided by your health care provider.

7. ENDING THE STUDY

7.1 If I want to stop participating in the study, what should I do?

You are free to leave the study at any time. If you leave the study before it is finished, there will be no penalty to you. You will not lose any benefits to which you may otherwise be entitled. If you choose to tell the researchers why you are leaving the study, your reasons for leaving may be kept as part of the study record. If you decide to leave the study before it is finished, please tell one of the persons listed in Section 10 “Contact Information” (below).

7.2 Could there be any harm to me if I decide to leave the study before it is finished?

There is not harm to you if you decided to leave the study before it is finished.

7.3 Could the researchers take me out of the study even if I want to continue to participate?

Yes. There are many reasons why the researchers may need to end your participation in the study. Some examples are:

The researcher believes that it is not in your best interest to stay in the study.

You become ineligible to participate.

Your condition changes and you need treatment that is not allowed while you are taking part in the study.

You do not follow instructions from the researchers.

The study is suspended or canceled.

8. Financial Information

8.1 Who will pay for the costs of the study? Will I or my health plan be billed for any costs of the study?

There are no costs or billing for this study

By signing this form, you do not give up your right to seek payment if you are harmed as a result of being in this study.

8.2 Will I be paid or given anything for taking part in this study?

You will receive a small amount of compensation (80 Mts or a gift of equivalent value) for initially completing the survey at enrollment.

You will be offered some HIV-test-referral coupons at enrollment. If someone gets an HIV test in Munhava Health Center and present a coupon to our research team there within one month, we will reward the original subject 100 Mts (about $1.8). Each original subject will receive this reward at most once.

8.3 Who could profit or financially benefit from the study results?

No person or organization has a financial interest in the outcome of the study.

9. confidentiality of subject records and authorization to release your protected health information

The information below describes how your privacy and the confidentiality of your research records will be protected in this study.

9.1 How will the researchers protect my privacy?

We will take the following steps to ensure participants confidentiality is protected:

All survey questions will be asked in private. Health records will be collected by survey staff who will receive extensive training on handling confidential data. Personally-identifiable data will be securely stored on password protected devices and encrypted. Only researchers affiliated with the study will have

access to this personally identifiable data. After the conclusion of the study (following up to a five-year storage period), all personally identifiable data will be destroyed. We will not store your name or other information that could identify you.

9.2 What information about me could be seen by the researchers or by other people? Why? Who might see it?

Signing this form gives the researchers your permission to obtain, use, and share information about you for this study, and is required in order for you to take part in the study. Information about you may be obtained from any hospital, doctor, and other health care provider involved in your care, including:

Hospital/doctor's office records, including test results (X-rays, blood tests, urine tests, etc.)

HIV/AIDS status

Sexually transmitted disease and/or other communicable disease status

All records relating to your condition, the treatment you have received, and your response to the treatment

Pharmacy information

Demographic information

Personal identifiers

There are many reasons why information about you may be used or seen by the researchers or others during or after this study. Examples include:

The researchers may need the information to make sure you can take part in the study.

The researchers may need the information to check your test results or look for side effects.

University, Food and Drug Administration (FDA) and/or other government officials, auditors, and/or the IRB may need the information to make sure that the study is done in a safe and proper manner.

Study sponsors or funders, or safety monitors or committees, may need the information to:

Make sure the study is done safely and properly

Learn more about side effects

Analyze the results of the study

The researchers may need to use the information to create a databank of information about your condition or its treatment.

Federal or State law may require the study team to give information to government agencies. For example, to prevent harm to you or others, or for public health reasons.

The results of this study could be published in an article, but would not include any information that would let others know who you are.

9.3 What happens to information about me after the study is over or if I cancel my permission?

As a rule, the researchers will not continue to use or disclose information about you, but will keep it secure until it is destroyed. Sometimes, it may be necessary for information about you to continue to be used or disclosed, even after you have canceled your permission or the study is over.

Examples of reasons for this include:

To avoid losing study results that have already included your information

To provide limited information for research, education, or other activities (This information would not include your name, social security number, or anything else that could let others know who you are.)

To help University and government officials make sure that the study was conducted properly

9.4 When does my permission expire?

Your permission expires at the end of the study, unless you cancel it sooner. You may cancel your permission at any time by writing to the researchers listed in Section 10 "Contact Information" (below).

10. Contact Information

10.1 Who can I contact about this study?

Please contact the researchers listed below to:

Obtain more information about the study

Ask a question about the study procedures or treatments

Talk about study-related costs to you or your health plan

Report an illness, injury, or other problem (you may also need to tell your regular doctors)

Leave the study before it is finished

Express a concern about the study

Principal Investigator: James Riddell IV, M.D.Mailing Address: 1500 E Medical Center Drive, University Hospital South F4131, Ann Arbor, MI 48109-5226, U.S.A.Telephone: +1 888-229-7409

Study Coordinator: Vicente ChauqueMailing Address:

Bairro George Dimitrov,

Block 78, House, 65

Maputo / MozambiqueTelephone: +258 84 5106300

You may also express a concern about a study by contacting the Institutional Review Board listed below.

University of Michigan Medical School Institutional Review Board (IRBMED)2800 Plymouth RoadBuilding 520, Room 3214Ann Arbor, MI 48109-2800, U.S.A.Telephone: 734-763-4768 (For International Studies: US Country Code: 001)Fax: 734-763-1234e-mail: irbmed@umich.edu

If you are concerned about a possible violation of your privacy or concerned about a study you may contact the University of Michigan Health System Compliance Help Line at 1-866-990-0111.

When you call or write about a concern, please provide as much information as possible, including the name of the researcher, the IRBMED number (at the top of this form), and details about the problem. This will help University officials to look into your concern. When reporting a concern, you do not have to give your name unless you want to.

11. record of Information provided

11.1 What documents will be given to me?

Your signature in the next section means that you have received copies of all of the following documents:

This "Consent to be Part of a Research Study" document. (Note: In addition to the copy you receive, copies of this document will be stored in a separate confidential research file.)

You will receive a copy of the signed and dated informed consent document.

Impartial Witness

I observed the above subject (or his/her legally authorized representative, if applicable) sign this consent document.

Legal Name: _____

Title: _____

Signature: _____

Date of Signature (mm/dd/yy): _____

Principal Investigator or Designee

I have provided this participant and/or his/her legally authorized representative(s) with information about this study that I believe to be accurate and complete. The participant and/or his/her legally authorized representative(s) indicated that he or she understands the nature of the study, including risks and benefits of participating.

Legal Name: _____

Title: _____

Signature: _____

Date of Signature (mm/dd/yy): _____

Consent/Assent to Participate in the Research Study

I understand the information printed on this form. I have discussed this study, its risks and potential benefits, and my other choices. My questions so far have been answered. I understand that if I have more questions or concerns about the study or my participation as a research subject, I may contact one of the people listed in Section 10 (above). I understand that I will receive a copy of this form at the time I sign it and later upon request. I understand that if my ability to consent or assent for myself changes, either I or my legal representative may be asked to re-consent prior to my continued participation in this study.

Legal Name: _____

Signature: _____

Date of Signature (mm/dd/yy): _____

Date of Birth (mm/dd/yy): _____

NID Number: _____

5. Treatment instructions

5.1 Arm assignment and initial explanation

At the end of the enrollment survey, enumerators explain the following information according to the participant's assigned group. Financial incentives, reminders, and their combination each have a separate message. The information, stigma-relief, information-plus-stigma-relief, and control groups receive a common closing message.

Financial incentives

Portuguese

Você está no grupo de incentivo financeiro. Nos próximos 6 meses, toda vez que você reabastecer sua medicação TARV no prazo, enviaremos uma recompensa de 200Mt pelo seu número de telefone através do Mpesa, "A tempo" significa que você faz o levantamento antes da data prevista. Por exemplo, se você levantar 30 doses hoje, você deve reabastecer dentro de 30 dias. Tomar a medicação diariamente é essencial para mantê-lo saudável. Por favor, note que, às vezes,o hospital pode agendar sua data de recarga muito tarde que você já terá esgotado a medicação até essa data. Nesse caso, não espere até a data agendada para recarregar. Você deve sempre reabastecer antes de ficar sem medicamentos em casa. Se você reabastecer a tempo, nós enviaremos a recompensa no prazo de 3 dias após o seu reabastecimento. Por favor, note que se você não levantar na hora, você não receberá nenhuma recompensa por esse período. Mas você ainda é elegível para receber recompensas no futuro, se você prosseguir de acordo. Você receberá, no máximo, a recompensa uma vez por mês pelos próximos 6 meses. Este dinheiro destina-se a ajudá-lo a reabastecer o seu medicamento ART a tempo e levá-lo diariamente. Pode ser usado para pagar transporte ou qualquer outra coisa que o ajude a aderir ao medicamento.

English

You are in the financial incentive group. In the next 6 months, every time you refill your ART medication on time, we will send you a 200Mt reward for your phone number through the Mpesa, "On time" means that you refill before the due day. For example, if you pick up 30 doses today, you are supposed to refill within 30 days. Taking the medication daily is essential to keep you healthy. Please note that, sometimes, the clinic may schedule your refill date too late that you will have already run out of medication by that date. In that case, please don't wait until the scheduled date to refill. You should always refill before you run out of medications at home. If you refill in time, we will send the reward within 3 days after your refilling. Please note that if you do not refill on time, you will not receive any rewards for that period. But you are still eligible to receive rewards in the future if you proceed accordingly. You will, at most, receive the reward once a month for the next 6 months. This money is intended to help you refill your ART medication on time and take it daily. It can be used to pay for transportation or anything else that helps you to adhere to the medication.

Reminders

Portuguese

Você está no grupo de lembretes. Nos próximos 6 meses, cada vez antes de ficar sem medicação, ligaremos para você através do número de telefone que você nos forneceu para lembrá-lo de levantar a tempo. "A tempo" significa que você faz o levantamento antes da data prevista. Por exemplo, se você levantar 30 doses hoje, você deve reabastecer dentro de 30 dias. Tomar a medicação diariamente é essencial para mantê-lo saudável. Por favor, note que, às vezes, o hospital pode agendar sua data de levantamento muito tarde que você já terá esgotado a medicação até essa data. Nesse caso, não espere até a data agendada para recarregar. Você deve sempre reabastecer antes de ficar sem medicamentos em casa. Todos os meses, ligaremos para você três dias antes da data do levantamento esperado se esgote. Se você não responder à nossa chamada de lembrete, tentaremos ligar para você duas vezes por dia, antes da data do levantamento. Nós protegeremos sua privacidade. Na ligação telefônica, confirmaremos sua identidade antes de falarmos do seu levantamento.

English

You are in the reminder group. In the next 6 months, each time before you run out of medication, we will call you through the phone number you provided us to remind you to refill on time. "On time" means that you refill before the due day. For example, if you pick up 30 doses today, you are supposed to refill within 30 days. Taking the medication daily is essential to keep you healthy. Please note that, sometimes, the clinic may schedule your refill date too late that you will have already run out of medication by that date. In that case, please don't wait until the scheduled date to refill. You should always refill before you run out of medications at home. Every month, we will call you three days before the expected refill date. If you do not answer our reminder call, we will try to call you twice more each day, prior to the refill date. We will protect your privacy. In the phone call, we will confirm your identity before we discuss your refilling.

Incentives and reminders

Portuguese

Você está no grupo de lembretes e incentivos financeiros. Nos próximos 6 meses, antes de ficar sem medicação, ligaremos para você através do número de telefone que você nos forneceu para lembrá-lo de fazer levantamento da medicação a tempo. E sempre que você reabastecer sua medicação ART a tempo, nós lhe enviaremos uma recompensa de 200Mt pelo seu número de telefone através do Mpesa, "A tempo" significa que você faz o levantamento antes da data prevista. Por exemplo, se você levantar 30 doses hoje, você deve reabastecer dentro de 30 dias. Tomar a medicação diariamente é essencial para mantê-lo saudável. Por favor, note que, às vezes, o Hospital pode agendar sua data de levantamento muito tarde que você já terá esgotado a medicação até essa data. Nesse caso, não espere até a data agendada para levantar. Você deve sempre reabastecer antes de ficar sem medicamentos em casa. Todos os meses, ligamos para você três dias antes de acabar com as suas doses. Se você não responder à nossa chamada de lembrete, tentaremos ligar para você duas vezes por dia, antes da data de recarga. Nós protegeremos sua privacidade. Na ligação telefônica, confirmaremos sua identidade antes de falarmos sobre o seu levantamento. Se você reabastecer a tempo, enviaremos a recompensa no prazo de 3 dias após o reabastecimento. Por favor, note que se você não levantar a tempo, você não receberá nenhuma recompensa por esse período. Mas você ainda é elegível para receber recompensas no futuro, se você prosseguir de acordo. Você receberá, no máximo, a recompensa uma vez por mês pelos próximos 6 meses. Este dinheiro destina-se a ajudá-lo a reabastecer o seu medicamentoTARV a tempo e tomá-lo diariamente. Pode ser usado para pagar transporte ou qualquer outra coisa que o ajude a aderir ao medicamento.

English

You are in the group of reminders and financial incentives. In the next 6 months, each time before you run out of medication, we will call you through the phone number you provided us to remind you to refill on time. And that every time you refill your ART medication on time, we will send you a 200Mt reward for your phone number through the Mpesa, "On time" means that you refill before the due day. For example, if you pick up 30 doses today, you are supposed to refill within 30 days. Taking the medication daily is essential to keep you healthy. Please note that, sometimes, the clinic may schedule your refill date too late that you will have already run out of medication by that date. In that case, please don't wait until the scheduled date to refill. You should always refill before you run out of medications at home. Every month, we will call you three days before the expected refill date. If you do not answer our reminder call, we will try to call you twice more each day, prior to the refill date. We will protect your privacy. In the phone call, we will confirm your identity before we discuss your refilling. If you refill on time, we will send the reward within 3 days after your refilling. Please note that if you do not refill on time, you will not receive any rewards for that period. But you are still eligible to receive rewards in the future if you proceed accordingly. You will, at most, receive the reward once a month for the next 6 months. This money is intended to help you refill your ART medication on time and take it daily. It can be used to pay for transportation or anything else that helps you to adhere to the medication.

Information, stigma relief, information plus stigma relief, and control

Portuguese

Nós rastrearemos seus reabastecimentos pelos próximos 6 meses. Gostaríamos de salientar que tomar a medicação diariamente é essencial para mantê-lo saudável. Então, por favor, sempre recarregue seus medicamentos a tempo. "A tempo" significa que você reabastece antes que o levantamento mais recente esgote. Por exemplo, se você levantar 30 doses hoje, você deve reabastecer dentro de 30 dias. Por favor, note que, às vezes, o hospital pode agendar sua data de levantamento muito tarde que você já terá esgotado a medicação até essa data. Nesse caso, não espere até a data agendada para recarregar. Você deve sempre reabastecer antes de ficar sem medicamentos em casa. Desejamos-lhe o melhor.

English

We will track your refillings for the next 6 months. We would like to stress that taking the medication daily is essential to keep you healthy. So, please always refill your medications on time. "On time" means that you refill before the most recent refill is expected to run out. For example, if you pick up 30 doses today, you are supposed to refill within 30 days. Please note that, sometimes, the clinic may schedule your refill date too late that you will have already run out of medication by that date. In that case, please don't wait until the scheduled date to refill. You should always refill before you run out of medications at home. We wish you the best.

Contact arrangements

Participants assigned to reminders or incentives plus reminders are asked for their preferred calling time. All participants are asked to confirm their telephone number; if it differs from the number provided earlier, the enumerator requests confirmation.

Portuguese

Qual hora do dia é a melhor para lhe telefonar?

8:00am - 12:00pm; 12:00pm - 4:00pm; 4:00pm - 8:00pm; qualquer hora.

É muito importante ter o número de telefone correto. Por favor, repita o seu número de telefone.

O número de telefone que você acabou de inserir é diferente daquele que você forneceu no início da entrevista.

English

What time of day is the best time to call you?

8:00am - 12:00pm; 12:00pm - 4:00pm; 4:00pm - 8:00pm; any time.

It is very important to have the correct phone number. Please repeat your phone number.

The phone number you just entered is different from the one you provided at the beginning of the interview.

5.2 Reminder-only and combined-arm calls

Portuguese

Guião de chamada telefónica para o grupo de tratamento com lembretes:

"Olá, posso falar com o Sr./Sra._____ (nome do paciente)? (Após a identidade confirmada). Estou a telefonar em nome da equipe de pesquisa "Intervenções para melhorar a adesão ao tratamento anti-retroviral do HIV". Gostaríamos de lembrá-lo de que sua próxima consulta da Unidade Sanitária / reabastecimento de medicamentos TARV está agendada para ____ (data). Por favor, procure visitar a Unidade Sanitária / farmácia a tempo. A visita antepada/ reabastecimento de medicamentos TARV é essencial para o seu tratamento. Obrigado pela sua atenção. "

Guiao de chamada telefónica para o grupo de tratamento de incentivos e lembretes financeiros:

"Olá, posso falar com o Sr./Ms._____ (nome do paciente)? (Após a identidade confirmada). Telefono em nome da equipe de pesquisa "Intervenções para melhorar a adesão ao tratamento anti-retroviral do HIV". Gostaríamos de recordá-lo que a sua próxima consulta a Unidade Sanitária / reabastecimento de medicamentos ART está marcada para ____ (data). Por favor, procure visitar a Unidade Sanitária / farmácia a tempo. Uma recompensa de 200 Mts será depositada em sua conta bancária / M-Pesa se você estiver a tempo para sua próxima visita clínica / recarga de medicamentos TARV. A visita antepada / reabastecimento de medicamentos TARV é essencial para o seu tratamento. Obrigado pela sua atenção. "

English

Phone call script for the reminders treatment group:

“Hello, can I talk to Mr./Ms._____(subject’ name)? (After confirmed identity). I am calling on behalf of the “Interventions to improve HIV antiretroviral therapy adherence” research team. We would like to remind you that your next clinic visit/ ART medication refill is scheduled on ____ (date). Please plan to visit the clinic/pharmacy on time. On-time clinic visit/ ART medication refill is essential for your treatment. Thanks for your attention.”

Phone call script for the financial incentive and reminders treatment group:

“Hello, can I talk to Mr./Ms._____(subject’ name)? (After confirmed identity). I am calling on behalf of the “Interventions to improve HIV antiretroviral therapy adherence” research team. We would like to remind you that your next clinic visit/ ART medication refill is scheduled on ____ (date). Please plan to visit the clinic/pharmacy on time. A reward of 200 Mts will be deposited into your bank account/ M-Pesa account if you are on time for your next clinic visit/ ART medication refill. On-time clinic visit/ ART medication refill is essential for your treatment. Thanks for your attention.”

5.3 Information intervention

Participants in the information and information-plus-stigma-relief groups are shown the information video. The Portuguese introduction and transcript are followed by the English introduction and script.

Portuguese introduction

Agora vou te mostrar um vídeo. Por favor, observe atentamente, depois que o vídeo for reproduzido, farei algumas perguntas sobre o conteúdo do vídeo.

Qual idioma você prefere?

Portuguese transcript

O que é o HIV?

A sigla HIV significa vírus da imunodeficiência humana. Quando este vírus infecta alguém, ele ataca e por vezes destrói o sistema imunitário por vários anos. O sistema imunitário é uma parte do teu corpo que te protege de doenças. Muitas pessoas com HIV sentem-se normais no início até à altura em que o seu sistema imunitário fica destruído e desenvolvem infecções graves e cancros que podem ser fatais.

O HIV não é feitiçaria ou força sobrenatural. O HIV é transmitido de uma pessoa para outra, através do sémen, fluido vaginal, sangue e leite de amamentar. O HIV não é transmitido através do beijo, do aperto de mão ou da partilha de louça.

Por que não devo deixar o meu HIV sem tratamento?

Se deixado sem tratamento, o HIV vai multiplicar-se muito rapidamente no corpo e com o tempo destruirá as defesas do teu corpo contra infecções e cancros. Estas infecções e cancros são muitas vezes fatais.

Sem o tratamento, uma pessoa infectada desenvolve o SIDA em 10 anos em média. Sintomas comuns do SIDA são perda de peso, febres recorrentes, fadiga extrema, diarreias prolongadas, inchaços dos gânglios linfáticos, manchas na pele ou na boca, nariz ou pálpebras e perda de memória. Sem o tratamento, alguém com o SIDA geralmente sobrevive cerca de 3 anos antes de morrer. Além disso, sem o tratamento, tens muitas chances de transmitir o HIV aos teus parceiros sexuais.

Como é que o tratamento pode me ajudar?

O tratamento antirretroviral, também conhecido por TARV, é a medicação que trava a progressão do HIV onde se encontra. Ele impede o HIV de destruir o sistema imunitário para te manter saudável. Ele não elimina o vírus do teu corpo, mas impede que ele te prejudique e que você o transmita aos outros. Toda a pessoa com HIV deve iniciar imediatamente o tratamento. Se tomares correctamente os medicamentos antirretrovirais, tens muitas chances de te manter saudável, ter uma vida tão longa quanto a de pessoas não infectadas e impedir a propagação do HIV para os outros.

Como é que o TARV funciona?

O TARV é completamente gratuito neste centro de saúde. Não precisas de pagar nada para receberes o tratamento. Para tornar o tratamento mais eficaz, precisas de fazê-lo continuamente uma vez iniciado. Abandonar o TARV depois de iniciar é mau para a tua saúde e tornará o teu futuro tratamento mais difícil.

É normal ter alguns efeitos secundários ao iniciar o TARV pela primeira vez. Portanto, não fiques assustado se tiveres uma perda ou aumento de apetite, fadiga ou diarreia. Se os teus efeitos secundários forem graves, venha à unidade sanitária. Os profissionais da saúde podem ajudar-lhe a lidar com eles ou a mudar para um regime diferente.

Tomar os medicamentos todos os dias e não falhar nenhuma dose. O TARV pode parar eficazmente o vírus somente se tu tomas os medicamentos em noventa por cento das vezes, falhando não mais de uma ou

duas vezes por mês. Por isso, não se preocupe demasiado se falhares acidentalmente uma dose, mas procure sempre manter o horário da toma.

Aqui estão algumas dicas que podem ajudar o teu TARV a funcionar bem:

Tenha sempre uma boa reserva de ARVs.

Faça o levantamento regular dos ARVs na unidade sanitária antes de ter uma rotura de stock.

Tenha sempre contigo alguns comprimidos.

Compareça sempre às consultas marcadas programadas com regularidade.

English introduction

Now I will show you a video about ART. Please watch carefully. After playing, I will ask you some question about the video.

Which language do you prefer?

English script

What is the 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 ninety percent 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

5.4 Stigma relief: elicitation and conditional feedback

Participants were asked how many of ten people in the neighboring community would answer yes to each attitude question. Feedback was provided only when their answers implied more stigma than the community benchmark. For the two negatively framed items, a larger yes share indicates more stigma; for the three supportive items, a smaller yes share indicates more stigma.

Attitude item	Stigmatizing answer	Community proportion
Separate dishware	Yes	0.186
Shame if a family member has HIV	Yes	0.147
Teacher with HIV should continue teaching	No	0.062
Children with HIV should attend school with other children	No	0.073
Willingness to care for a family member with AIDS	No	0.019

The introductory message and five item-specific feedback paragraphs follow. Each message includes the participant's own estimate.

Portuguese version

Num inquérito à população realizado numa comunidade vizinha, fizemos perguntas às pessoas sobre as suas atitudes em relação ao HIV/SIDA. Gostaríamos de partilhar consigo como as pessoas responderam.

No inquérito que acabámos de realizar, estimou que [o programa do inquérito insere automaticamente a proporção estimada pelo(a) inquirido(a)] das pessoas responderiam "sim" à pergunta: "Acha que as pessoas que vivem com HIV devem sempre usar loiça separada quando partilham comida com outras pessoas, para proteger a saúde dessas pessoas?". Fizemos esta pergunta no inquérito à população em Munhava. As pessoas responderam "sim" a esta pergunta menos vezes do que estimou. Os nossos dados mostram que menos de 2 em cada 10 pessoas (ou 18,6%) responderam "sim", o que indica que a maioria das pessoas que vivem neste bairro apoia as pessoas que vivem com HIV.

No inquérito que acabámos de realizar, estimou que [o programa do inquérito insere automaticamente a proporção estimada pelo(a) inquirido(a)] das pessoas responderiam "sim" à pergunta: "Concorda com a seguinte afirmação? Eu teria vergonha se alguém da minha família tivesse HIV.". Fizemos esta pergunta no inquérito à população em Munhava. As pessoas responderam "sim" a esta pergunta menos vezes do que estimou. Os nossos dados mostram que apenas cerca de 1 em cada 10 pessoas (ou 14,7%) responderam "sim", o que indica que a maioria das pessoas que vivem neste bairro apoia as pessoas que vivem com HIV.

No inquérito que acabámos de realizar, estimou que [o programa do inquérito insere automaticamente a proporção estimada pelo(a) inquirido(a)] das pessoas responderiam "sim" à pergunta: "Na sua opinião, se um professor tem HIV mas não está doente, deve ser autorizado a continuar a ensinar na escola?". Fizemos esta pergunta no inquérito à população em Munhava. As pessoas responderam "sim" a esta pergunta mais vezes do que estimou. Os nossos dados mostram que mais de 9 em cada 10 pessoas (ou 93,8%) responderam "sim", o que indica que a maioria das pessoas que vivem neste bairro apoia as pessoas que vivem com HIV.

No inquérito que acabámos de realizar, estimou que [o programa do inquérito insere automaticamente a proporção estimada pelo(a) inquirido(a)] das pessoas responderiam "sim" à pergunta: "Acha que as crianças que vivem com HIV devem poder frequentar a escola com crianças que não têm HIV?". Fizemos esta pergunta no inquérito à população em Munhava. As pessoas responderam "sim" a esta pergunta mais vezes do que estimou. Os nossos dados mostram que mais de 9 em cada 10 pessoas (ou 92,7%) responderam "sim", o que indica que a maioria das pessoas que vivem neste bairro apoia as pessoas que vivem com HIV.

No inquérito que acabámos de realizar, estimou que [o programa do inquérito insere automaticamente a proporção estimada pelo(a) inquirido(a)] das pessoas responderiam "sim" à pergunta: "Se um membro da sua família ficasse doente com SIDA, estaria disposto(a) a cuidar dele na sua própria casa?". Fizemos esta pergunta no inquérito à população em Munhava. As pessoas responderam "sim" a esta pergunta mais vezes do que estimou. Os nossos dados mostram que quase todas as pessoas (ou 98,1%) responderam "sim", o que indica que a maioria das pessoas que vivem neste bairro apoia as pessoas que vivem com HIV.

English version

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 [the survey software automatically inserts the respondent's guessed share] 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.

In the survey we just finished, you guessed that [the survey software automatically inserts the respondent's guessed share] would answer "yes" to the question, "Do you agree? I would be ashamed if someone in my family had HIV". 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 only about 1 out of 10 people (or 14.7%) answered "yes", indicating that the majority of the people living in this neighborhood are supportive of people living with HIV.

In the survey we just finished, you guessed that [the survey software automatically inserts the respondent's guessed share] 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?". We did ask this question in the population survey in Munhava. People answered "yes" to this question more often than you think they would. Our data show that more than 9 out of 10 people (or 93.8%) answered "yes", indicating that the majority of the people living in this neighborhood are supportive of people living with HIV.

In the survey we just finished, you guessed that [the survey software automatically inserts the respondent's guessed share] 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?". We did ask this question in the population survey in Munhava. People answered "yes" to this question more often than you think they would. Our data show that more than 9 out of 10 people (or 92.7%) answered "yes", indicating that the majority of the people living in this neighborhood are supportive of people living with HIV.

In the survey we just finished, you guessed that [the survey software automatically inserts the respondent's guessed share] people 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?". We did ask this question in the population survey in Munhava. People answered "yes" to this question more often than you think they would. Our data show that almost all people (or 98.1%) answered "yes", indicating that the majority of the people living in this neighborhood are supportive of people living with HIV.

5.5 HIV-testing referral coupons

The referral message is given to participants in all groups. Participants reporting more than three sexual partners in the preceding 12 months receive five coupons; others receive three. The assigned number is inserted into the message. Enumerators write the expiry date, one month after enrollment, on each coupon and record its barcode before distribution.

Portuguese

As pessoas devem fazer o teste de HIV para saber regularmente da sua situação se ainda não tiverem resultado positivo. Esperamos que você possa indicar outras pessoas para fazer o teste.

Oferecemos-lhe [número de cupões] cupões de teste para o HIV para encorajar te a encaminhar-nos mais pessoas. Você pode compartilhar esses cupons com qualquer pessoa que não saiba do seu estado de HIV. É altamente recomendável que você compartilhe com seus parceiros sexuais primeiro se eles não tiverem sido testados recentemente.

Cada cupão tem um valor de 100 Mts. Para resgatar um cupão de teste, uma pessoa deve ir ao Centro de Saude da Munhava para fazer o teste de HIV dentro de um mês. Nossa equipa pagará a você e as pessoas que voce for a encaminhar para aqui um valor de 100Mt cada. Não vamos pedir ver o resultado do teste e não vamos levar nenhuma informação sobre identidade da pessoa que esgata o cupão. Esperamos que cada pessoa tenha que dedicar cerca de 30 minutos do seu tempo no hospital para testar e receber a sua recompensa.

O seu pagamento será feito através da MPesa no dia em que pelo menos uma pessoa fizer o teste com um dos cupons que eu dou hoje. Nós agradeceríamos se você pudesse indicar quantas pessoas fossem necessárias para fazer os testes, mas você será pago no máximo uma vez (100Mts). Cada pessoa que fizer o teste com um cupão receberá 100Mt através da MPesa, recarga de celular ou pacotes de presentes (mas não daremos dinheiro) imediatamente após os testes.

English

People should take HIV tests to learn their status regularly if they have not already tested positive. We hope you can refer other people to get tested.

We will offer you [number of coupons] HIV testing coupons to encourage refering. You can share these coupons to anyone who do not know their status to take tests. We strongly encourage you to share to your sex partners first if they have not been tested recently.

Each coupon has a value of 100 Mts. To redeem a test coupon, a person must come to Munhava Health Center to take an HIV test within one month. Our staff will pay you and the test taker 100Mt each. We will not ask for the result of the test and will not collect any personally-identifying information about any person redeeming a coupon. We expect that each person will have to devote about 30 minutes of their time at the clinic for testing and to receive payment.

The payment to you will be made through MPesa on the day when a person get tested with one of the coupon I give you today. We would appreciate it if you could refer as many people as necessary to take tests, but you will be paid at most once (100Mts). Each person who get tested with a coupon will be paid 100Mt through MPesa, cellphone recharge, or gift packages (but we do not give out cash) immediately after their tests.

6. Brief follow-up and intervention completion

A brief telephone survey approximately one month after enrollment re-elicits beliefs. The questions are provided below in Portuguese and English, followed by the calls announcing completion of the six-month intervention.

6.1 Portuguese brief follow-up

1. Se eu perguntar a 10 pessoas do seu bairro a seguinte pergunta, quantas delas, você esperaria que respondessem “sim”?

"Você acha que as pessoas que vivem com o HIV devem sempre usar louça separada ao compartilhar alimentos com outras pessoas para proteger a saúde dos outros?"

2. Se eu perguntar a 10 pessoas do seu bairro, quantas delas, você esperaria que concordariam com a seguinte declaração?

"Eu ficaria envergonhado se alguém da minha família tivesse HIV."

3. Se eu perguntar a 10 pessoas do seu bairro a seguinte pergunta, quantas delas, você esperaria que respondessem “sim”?

“Se uma professora tem o vírus de HIV/Sida, mas não está doente, deveria continuar ensinando na escola?”

4. Se eu perguntar a 10 pessoas do seu bairro a seguinte pergunta, quantas delas, você esperaria que respondessem “sim”?

Você acha que as crianças que vivem com HIV devem frequentar a escola com crianças que são HIV negativas?

5. Se eu perguntar a 10 pessoas do seu bairro a seguinte pergunta, quantas delas, você esperaria que respondessem “sim”?

"Se um membro da sua família ficou doente com o vírus de HIV, você estaria disposto a cuidar dele em sua casa?"

6.2 English brief follow-up

Section G: If, for each of the following questions, I ask 10 people living in the neighboring community, to each question, how many of the people I surveyed, do you think, will answer "Yes"? (Choose from 0, 1, 2, …, 10 for each questions)

Do you think that people living with HIV should always use separate dishware when sharing food with others to protect other's health?

Do you agree: I would be ashamed if someone in my family had HIV

If a female teacher has the AIDS virus but is not sick, should she be allowed to continue teaching in school?

Do you think that children living with HIV should be able to attend school with children who are HIV negative?

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?

6.3 Completion of incentives and reminders

At the end of 180 days, callers use the shared introduction, the message for the participant's treatment group, and the shared closing instruction below.

Portuguese version

Introdução comum

Bom dia / boa tarde. O meu nome é [membro da equipa de pesquisa]. Estou a ligar em nome da Universidade de Michigan e do Centro de Investigação Operacional da Beira (CIOB), que estão a realizar um estudo chamado "Intervenções para melhorar a adesão à terapia antirretroviral do HIV na Beira."

Incentivos

Queremos informar que a sua participação no nosso estudo terminou. Isto significa que deixaremos de lhe enviar o valor de 200 meticais correspondente ao incentivo pelo levantamento dos seus medicamentos dentro do prazo. A equipa de pesquisa agradece a sua participação.

Lembretes

Queremos informar que a sua participação no nosso estudo terminou. Isto significa que deixaremos de lhe ligar para recordar a data do levantamento dos seus medicamentos. A equipa de pesquisa agradece a sua participação.

Combinação

Queremos informar que a sua participação no nosso estudo terminou. Isto significa que deixaremos de lhe ligar para recordar a data do seu próximo levantamento de medicamentos e deixaremos de lhe enviar o valor de 200 meticais correspondente ao incentivo pelo levantamento dos seus medicamentos dentro do prazo. A equipa de pesquisa agradece a sua participação.

Instrução final comum

Deve continuar a comparecer às consultas no centro de saúde, levantar os seus medicamentos e tomá-los diariamente, conforme as orientações dos médicos ou dos profissionais de saúde.

English version

Shared introduction

Good morning / good afternoon. My name is [research team member]. I call on behalf of the University of Michigan and the Beira Operational Research Center (CIOB), who are conducting a study called "Interventions to improve adherence to HIV antiretroviral therapy in Beira."

Incentives

We want to inform you that your participation in our study has ended. This means that we will no longer continue to send you the 200 meticais amount corresponding to the incentive for your on-time refill of the medication. The research team thanks you for your participation.

Reminders

We want to inform you that your participation in our study has ended. This means that we will no longer continue to call you to remind you about your medication refill date. The research team thanks you for your participation.

Combination

We want to inform you that your participation in our study has ended. This means that we will no longer call you to remind you about your next refill date, and we will not continue to send you the 200 meticais amount corresponding to the incentive for your on-time refill of medication. The research team thanks you for your participation.

Shared closing instruction

You must continue to attend appointments at the health center, pick up your medications and take them daily, as instructed by doctors or health clinic staff.

Populated Pre-Analysis Plan

ART Adherence Trial in Mozambique

AEARCTR-0003184 NCT03618511

1 Study, data and implementation

This document reports analyses corresponding to the 14 August 2019 pre-analysis plan (PAP) for the ART adherence trial at Munhava Health Center, Beira, Mozambique, and recruitment-sample robustness checks. The companion paper is *When Incentives Aren't Enough: Evidence on Inattention and Imperfect Memory from HIV Medication Adherence* (5 November 2026). Tables attributed to the paper report the same coefficient estimates and standard errors; its appendices give the detailed design and measurement rules.

The primary sample contains 821 participants recruited in person by 21 February 2020. Recruitment days were assigned to financial incentives, reminders, both together, information, stigma relief, or control. A form error delivered information alone to the intended information-plus-stigma arm, which is pooled with information. Section 3 adds later recruits, keeping the corrected combined intervention distinct, and analyzes telephone recruitment separately.

Measurement. Medication possession ratio (MPR) measures medication availability, not ingestion. The six outcomes are MPR at least 95%, at least 80%, and continuous MPR, each over up to 180 or 90 days. We follow the paper's refill reconciliation, accepted quantities, carryover of unused stock, enrollment-day dose deduction and exact covered-day thresholds. Primary follow-up ends at 180 days, an earlier clinical/inactivity endpoint, or 24 March 2020. The extensions remove the COVID cutoff; their source and observation limits are specified below. See the paper's Appendix A 2 for construction details.

Specification and departures. OLS controls for sex, education, food security, travel time, missed-dose knowledge, education/food-security missingness, enumerators and analysis strata, with recruitment-day clustered standard errors. Departures from the registered plan are day-level assignment, merged strata instead of enrollment weeks, the primary COVID cutoff, monthly documented attendance instead of scheduled-date attendance, adjusted 120-day inactivity instead of scheduled-date 90-day LTFU, four comparable knowledge items instead of five, and available-item mean stigma instead of a five-item sum. Additional tables report the registered pooled-component model and complete five-item stigma sum. Registration followed the start of recruitment.

Inference. Table 1 retains the five-treatment primary family-wise error rate (FWER) correction and directional supplement. Table 6 applies a separate six-treatment, two-sided FWER correction to the expanded in-person sample's primary outcome. Other outcomes and contrasts report unadjusted two-sided exploratory inference. Significance stars use unadjusted p-values.

2 Results corresponding to the registered analyses

2.1 Medication coverage and complementarity

Table 1 reports the estimates from Table 1 of the main paper. It covers the primary outcome and the five registered alternative MPR definitions. It also reports the incentive–reminder additivity contrast and the two component-dominance comparisons. The combination increases primary high coverage by 23.3 percentage points.

Table 1: *Effects on medication coverage*

	Primary window (up to 180 days)			Up to 90 days		
	(1)	(2)	(3)	(4)	(5)	(6)
	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR (continuous)	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR (continuous)
Incentives	0.086	0.128*	0.073**	0.102	0.081	0.048
	(0.052)	(0.065)	(0.032)	(0.079)	(0.077)	(0.029)
	{0.230}					
Reminders	0.100*	0.086	0.058*	0.119*	0.088	0.040
	(0.053)	(0.063)	(0.031)	(0.066)	(0.064)	(0.028)
	{0.224}					
Combination	0.233***	0.203***	0.121***	0.197***	0.138**	0.067**
	(0.044)	(0.058)	(0.029)	(0.064)	(0.062)	(0.026)
	{0.000}					
Information	0.075	0.087	0.037	0.046	0.045	0.005
	(0.048)	(0.057)	(0.029)	(0.064)	(0.062)	(0.026)
	{0.230}					
Stigma-relieving	0.141**	0.113	0.060	0.062	0.022	0.004
	(0.059)	(0.084)	(0.039)	(0.084)	(0.082)	(0.036)
	{0.137}					
PAP covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	821	821	821	821	821	821
Recruitment-day clusters	137	137	137	137	137	137
Control mean	0.245	0.453	0.691	0.472	0.613	0.809
Combination minus incentives	0.147***	0.075	0.048*	0.095	0.057	0.019
	(0.050)	(0.052)	(0.025)	(0.069)	(0.060)	(0.024)
Combination minus reminders	0.133***	0.117**	0.063**	0.078	0.050	0.027
	(0.049)	(0.050)	(0.024)	(0.052)	(0.044)	(0.021)
Departure from additivity	0.047	-0.011	-0.010	-0.024	-0.031	-0.021
	(0.072)	(0.083)	(0.041)	(0.097)	(0.091)	(0.037)

Notes: Reports the estimates from Table 1 of the main paper. Covariates are listed in Section 1. Parentheses contain recruitment-day clustered standard errors. Stars indicate unadjusted two-sided tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Curly braces report two-sided List–Shaikh–Xu FWER-adjusted p-values from `mhtreg` (Steinmayr, 2020) with 10,000 recruitment-day cluster bootstrap replications (seed 31415) for the five treatment-versus-control hypotheses in column (1), with cumulative maxima in bootstrap-p order. Both horizons use the same 821 participants; individual endpoints may precede 90 days. As a separately computed supplement for the prespecified positive alternatives in column (1), the one-sided List–Shaikh–Xu step-down FWER-adjusted p-values are 0.1070 (incentives), 0.0834 (reminders), 0.0001 (combination), 0.1070 (information), and 0.0522 (stigma-relieving), using the same 10,000 recruitment-day bootstrap draws and the cumulative-maximum sequential convention.

2.2 Other registered outcomes

Table 2 reports the estimates from Table A9 of the paper's appendix. It reports all five treatment effects on death/LTFU, attendance, referral, knowledge and stigma under the implemented definitions. Outcome-specific sample sizes reflect linkage and survey availability; missing survey outcomes are not coded zero. Survey-response selection limits extrapolation to the full recruitment sample. Further scoring and availability details appear in the paper's measurement appendix.

Table 2: *Effects on secondary outcomes*

	(1) AAR	(2) Death/LTFU	(3) Referral	(4) Knowledge	(5) Stigma belief
Incentives	0.037	0.055	-0.037	0.056	0.471
	(0.031)	(0.036)	(0.040)	(0.211)	(0.331)
Reminders	0.005	0.042	0.064	-0.404**	0.002
	(0.033)	(0.035)	(0.048)	(0.175)	(0.390)
Combination	0.076**	0.018	0.048	-0.059	-0.018
	(0.030)	(0.032)	(0.040)	(0.159)	(0.368)
Information	0.022	0.048*	0.011	0.217	0.468
	(0.028)	(0.029)	(0.037)	(0.157)	(0.356)
Stigma-relieving	0.083**	0.040	0.019	-0.188	0.223
	(0.033)	(0.038)	(0.041)	(0.178)	(0.318)
PAP covariates	Yes	Yes	Yes	Yes	Yes
Observations	808	588	788	518	511
Recruitment-day clusters	137	91	129	131	131
Control mean	0.226	0.065	0.120	0.350	-1.953
Combination minus incentives	0.039	-0.037	0.085**	-0.115	-0.489
	(0.028)	(0.036)	(0.037)	(0.184)	(0.328)
Combination minus reminders	0.071**	-0.024	-0.015	0.345**	-0.020
	(0.030)	(0.037)	(0.047)	(0.152)	(0.368)
Departure from additivity	0.034	-0.079	0.022	0.289	-0.491
	(0.043)	(0.050)	(0.060)	(0.251)	(0.502)

Notes: Reports the estimates from Table A9 of the main paper. Covariates and inference are summarized in Section 1. AAR is the monthly appointment-attendance share, using linked dossiers and primary MPR endpoints. Death/LTFU includes 5 deaths. Knowledge and stigma changes use paired survey responses.

2.3 Prespecified subgroup comparisons

Table 3 reports the estimates from Table 2 of the main paper. It presents the five registered subgroup splits, all 50 treatment effects and all 25 equality tests. Each regression omits the split-defining control; missing education or food security is excluded from the corresponding split. The equality tests, rather than differences in within-subgroup significance, assess heterogeneity.

Table 3: *Prespecified treatment heterogeneity: MPR ≥ 95%*

	Gender		Education		Food security		Travel time		ART missed-dose knowledge	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Men	Women	0–6 years	7+ years	Insecure	Secure	<30 min.	≥30 min.	0–3 correct	4 correct
Incentives	0.125*	0.087	0.212**	0.010	0.100	0.042	0.004	0.106	-0.022	0.159**
	(0.070)	(0.078)	(0.083)	(0.064)	(0.081)	(0.057)	(0.071)	(0.073)	(0.091)	(0.064)
Reminders	0.201**	0.029	0.169*	0.090	0.025	0.111*	0.038	0.118*	0.047	0.109
	(0.084)	(0.061)	(0.091)	(0.072)	(0.087)	(0.061)	(0.079)	(0.064)	(0.089)	(0.070)
Combination	0.366***	0.145**	0.292***	0.216***	0.284***	0.207***	0.228***	0.224***	0.164**	0.283***
	(0.079)	(0.063)	(0.080)	(0.059)	(0.067)	(0.062)	(0.081)	(0.065)	(0.071)	(0.065)
Information	0.099	0.062	0.206***	0.008	0.137*	0.010	-0.018	0.119**	0.009	0.120**
	(0.076)	(0.059)	(0.076)	(0.055)	(0.072)	(0.055)	(0.067)	(0.056)	(0.077)	(0.059)
Stigma-relieving	0.213***	0.116	0.246***	0.125*	0.158*	0.118	0.029	0.201***	0.176**	0.089
	(0.076)	(0.080)	(0.082)	(0.065)	(0.086)	(0.077)	(0.076)	(0.064)	(0.084)	(0.074)
PAP covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	335	486	304	511	361	449	344	477	385	436
Recruitment-day clusters	120	131	125	132	124	134	123	130	121	133
Control mean	0.195	0.277	0.158	0.288	0.222	0.259	0.279	0.222	0.304	0.180
Difference between subgroups (second minus first)										
Incentives	-0.038		-0.202**		-0.057		0.102		0.181	
	(0.101)		(0.100)		(0.084)		(0.096)		(0.112)	
Reminders	-0.173*		-0.079		0.086		0.081		0.062	
	(0.097)		(0.114)		(0.105)		(0.101)		(0.115)	
Combination	-0.221**		-0.077		-0.077		-0.005		0.119	
	(0.102)		(0.103)		(0.094)		(0.109)		(0.102)	
Information	-0.037		-0.198**		-0.127		0.138*		0.110	
	(0.087)		(0.087)		(0.083)		(0.078)		(0.096)	
Stigma-relieving	-0.096		-0.122		-0.040		0.172**		-0.087	
	(0.107)		(0.085)		(0.109)		(0.086)		(0.105)	

Notes: Reports the estimates from Table 2 of the main paper. Parentheses contain recruitment-day clustered standard errors. Stars use unadjusted two-sided tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Each regression omits the split-defining covariate and its missingness indicator. Education and food-security splits exclude missing values; knowledge uses the four-item index. Bottom-panel differences use joint recruitment-day clustered covariance.

2.4 Pooled treatment components

The PAP also specifies a joint regression on four indicators for receiving the incentive, reminder, information and stigma-relief components, whether alone or in combination:

$$Y_i = \alpha + \beta_I I_i + \beta_R R_i + \beta_F F_i + \beta_S S_i + X_i'\gamma + \lambda_{w(i)} + \mu_{e(i)} + \varepsilon_i.$$

Here X_i contains the baseline controls, and $\lambda_{w(i)}$ and $\mu_{e(i)}$ are enrollment-week and enumerator effects. Both I_i and R_i equal one in the incentive–reminder arm. Information assignments affected by the form error enter F_i alone. This model imposes additivity; its component coefficients differ from the separate-arm effects in Table 1. Tables 4 and 5 report this specification across the observed outcomes.

Table 4: *Pooled treatment components: medication outcomes*

	180d ≥ 95%	180d ≥ 80%	180d MPR	90d ≥ 95%	90d ≥ 80%	90d MPR
Incentive component	0.082*	0.105**	0.063***	0.062	0.059	0.034*
	(0.042)	(0.048)	(0.022)	(0.051)	(0.048)	(0.019)
Reminder component	0.131***	0.070	0.049**	0.114**	0.070	0.030
	(0.040)	(0.044)	(0.021)	(0.051)	(0.045)	(0.019)
Information component	0.099**	0.090*	0.042	0.045	0.050	0.008
	(0.047)	(0.051)	(0.027)	(0.062)	(0.059)	(0.025)
Stigma-relief component	0.141**	0.078	0.042	0.016	-0.029	-0.019
	(0.061)	(0.078)	(0.038)	(0.085)	(0.082)	(0.036)
Observations	821	821	821	821	821	821
Recruitment-day clusters	137	137	137	137	137	137
Control mean	0.245	0.453	0.691	0.472	0.613	0.809

Notes: All regressions include the baseline controls in Section 1, enumerator effects and enrollment-week effects. Parentheses contain recruitment-day clustered standard errors. Stars report unadjusted two-sided exploratory tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The incentive and reminder indicators each include the combined-treatment arm; the four component indicators enter jointly, imposing additivity. Information includes the intended information-plus-stigma assignments that received information alone. The six medication outcomes and primary sample are identical to Table 1; the primary follow-up retains the COVID cutoff.

Table 5: *Pooled treatment components: other outcomes and registered stigma sum*

	Attendance	Death/LTFU	Referral	Knowledge	Stigma mean	Stigma sum
Incentive component	0.031	0.003	-0.037	0.265*	-0.154	-0.893
	(0.022)	(0.026)	(0.031)	(0.142)	(0.275)	(1.287)
Reminder component	0.032	0.028	0.069**	-0.321**	-0.038	-0.422
	(0.021)	(0.028)	(0.031)	(0.147)	(0.334)	(1.549)
Information component	0.011	0.032	0.006	0.255	0.331	1.540
	(0.026)	(0.033)	(0.037)	(0.165)	(0.318)	(1.613)
Stigma-relief component	0.062*	0.029	0.021	-0.190	0.040	0.930
	(0.034)	(0.040)	(0.041)	(0.188)	(0.317)	(1.589)
Observations	808	588	788	518	511	467
Recruitment-day clusters	137	91	129	131	131	127
Control mean	0.226	0.065	0.120	0.350	-1.953	-9.113

Notes: All regressions include the baseline controls in Section 1, enumerator effects and enrollment-week effects. Parentheses contain recruitment-day clustered standard errors. Stars report unadjusted two-sided exploratory tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The incentive and reminder indicators each include the combined-treatment arm; the four component indicators enter jointly, imposing additivity. Information includes the intended information-plus-stigma assignments that received information alone. Columns 1–5 use the main paper's outcome definitions and outcome-specific samples. Column 6 is follow-up minus baseline stigma on a 0–50 sum scale, requiring all five correctly oriented items at both interviews; no missing item is replaced by zero.

3 Robustness to extending the recruitment sample

3.1 All eligible in-person recruitment, without a COVID cutoff

Table 6 repeats the six medication regressions for 898 participants: the original 821 plus all 77 recruits from 24 February through 13 March 2020. Outcomes remove the COVID cutoff, retaining earlier clinical/inactivity endpoints. Table A7 of the paper gives the original-cohort no-COVID benchmark; source details for the extension appear in the table notes.

The 16 corrected information-plus-stigma assignments remain separate, with only six contemporaneous controls. Their coefficient and the information–stigma additivity contrast rely on common component effects across recruitment periods and warrant caution. The incentive–reminder combination raises high coverage by 17.8 percentage points (FWER-adjusted p=0.009).

Table 6: *Expanded in-person sample: medication coverage without a COVID cutoff*

	Up to 180 days			Up to 90 days		
	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR
Incentives	0.097*	0.104*	0.083***	0.094	0.094	0.058**
	(0.052)	(0.061)	(0.031)	(0.075)	(0.071)	(0.028)
	{0.276}					
Reminders	0.063	0.041	0.068**	0.144**	0.120*	0.064**
	(0.052)	(0.064)	(0.031)	(0.066)	(0.064)	(0.029)
	{0.579}					
Combination	0.178***	0.135**	0.113***	0.167***	0.116*	0.069**
	(0.047)	(0.059)	(0.031)	(0.059)	(0.061)	(0.027)
	{0.009}					
Information	0.055	0.013	0.019	0.028	0.019	-0.003
	(0.045)	(0.057)	(0.029)	(0.059)	(0.063)	(0.027)
	{0.579}					
Stigma-relieving	0.075	0.022	0.038	0.023	-0.012	-0.012
	(0.054)	(0.076)	(0.038)	(0.077)	(0.078)	(0.035)
	{0.579}					
Information plus stigma	0.363**	0.289*	0.139*	0.167*	0.123	0.071
	(0.147)	(0.149)	(0.077)	(0.097)	(0.100)	(0.053)
	{0.579}					
Observations	898	898	898	898	898	898
Control mean	0.188	0.446	0.639	0.464	0.589	0.790
Recruitment-day clusters	152	152	152	152	152	152
Combination minus incentives	0.081	0.031	0.030	0.073	0.023	0.011
	(0.050)	(0.048)	(0.027)	(0.066)	(0.056)	(0.024)
Combination minus reminders	0.115**	0.094*	0.046	0.022	-0.004	0.006
	(0.052)	(0.053)	(0.028)	(0.055)	(0.049)	(0.024)
Departure from additivity	0.018	-0.010	-0.037	-0.071	-0.098	-0.053
	(0.072)	(0.081)	(0.042)	(0.096)	(0.087)	(0.038)
Information–stigma additivity	0.233	0.254*	0.082	0.117	0.116	0.086
	(0.150)	(0.153)	(0.079)	(0.110)	(0.113)	(0.057)

Notes: Coefficients are differences relative to control. Parentheses contain standard errors. All stars use unadjusted two-sided tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The combination is incentives plus reminders. Curly braces in column 1 report two-sided List–Shaikh–Xu FWER-adjusted p-values for six treatment-versus-control tests, using 10,000 recruitment-day cluster bootstrap replications. Other columns and contrasts are unadjusted. The sample adds all 77 later recruits to the original 821, retaining 16 corrected information-plus-stigma assignments and six late controls. Follow-up removes the COVID cutoff but retains earlier clinical/inactivity endpoints. Controls match the main specification, with distinct late design-block effects; standard errors cluster by recruitment day. Added recruits use real-time records; the original cohort retains two-source outcomes. Information–stigma additivity subtracts the two component coefficients from the corrected combined-arm coefficient and relies on the pooled cross-period specification.

3.2 Telephone recruitment cohort

Table 7 analyzes 243 of 298 enrollment records recruited exclusively by telephone from 16 April through 5 August 2020. Individual assignment was limited to incentives, reminders, their combination, stigma relief or control: the information video could not be shown over the phone. This cohort entered under a 90-day refill protocol. Longer initial supplies reduced the need to return to the clinic and limited the scope for variation in medication coverage within six months. The 53 records without tracking links and two with undated death reports are excluded.

The six medication outcomes use real-time records and the same baseline controls, with enrollment-week and enumerator effects. The documented tracking-snapshot ceiling of 18 December 2020 leaves 128 complete 180-day windows and 50 shorter windows ending at that ceiling; other windows end at earlier exits/inactivity. Missing linkage is not medication non-possession. Canonical enrollment records are retained; shared clinic identifiers alone do not establish duplicate people.

The combination coefficient on high coverage is -2.9 percentage points (unadjusted p=0.793; 95% interval -24.3 to 18.6). This is imprecise evidence, not evidence of zero effect. Differences across cohorts do not identify a causal effect of dispensing policy.

Table 7: *Telephone recruitment cohort: medication coverage*

	Up to 180 days			Up to 90 days		
	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR	MPR $\geq$ 95%	MPR $\geq$ 80%	MPR
Incentives	-0.104	-0.246**	-0.092	-0.076	-0.029	-0.036
	(0.112)	(0.105)	(0.063)	(0.094)	(0.093)	(0.060)
Reminders	0.032	-0.050	-0.011	-0.014	0.028	0.010
	(0.099)	(0.096)	(0.056)	(0.082)	(0.078)	(0.050)
Combination	-0.029	-0.086	0.006	0.022	0.076	0.025
	(0.109)	(0.102)	(0.055)	(0.093)	(0.084)	(0.052)
Stigma-relieving	-0.016	-0.091	-0.006	-0.047	-0.014	0.011
	(0.109)	(0.108)	(0.063)	(0.096)	(0.093)	(0.060)
Observations	243	243	243	243	243	243
Control mean	0.482	0.661	0.782	0.768	0.786	0.853
Combination minus incentives	0.075	0.161	0.098*	0.098	0.105	0.062
	(0.111)	(0.103)	(0.058)	(0.095)	(0.090)	(0.056)
Combination minus reminders	-0.060	-0.035	0.017	0.037	0.049	0.015
	(0.103)	(0.097)	(0.049)	(0.085)	(0.074)	(0.045)
Departure from additivity	0.043	0.211	0.109	0.113	0.077	0.051
	(0.151)	(0.141)	(0.080)	(0.127)	(0.119)	(0.075)

Notes: Coefficients are differences relative to control. Parentheses contain standard errors. All stars use unadjusted two-sided tests: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The combination is incentives plus reminders. The sample contains 243 of 298 randomized enrollment records: 53 have no linked tracking record and two have a death report without a dated endpoint. Controls are sex, education, food security, travel time, missed-dose knowledge, education/food-security missingness indicators, enrollment-week and enumerator effects. Standard errors are heteroskedasticity-robust. Follow-up uses real-time records and ends at 180 days, an earlier dated exit/inactivity endpoint, or the documented tracking-snapshot date of 18 December 2020. The 90-day outcomes cap the same window at 90 days.

Sources. Registered PAP, 14 August 2019 (AEARCTR-0003184; NCT03618511), and the companion paper identified in Section 1. Inference follows the paper's implementation of List, Shaikh and Xu (2019) and Steinmayr (2020).