Designing Incentives for Impatient People: An RCT Promoting Exercise to Manage Diabetes

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Abstract

Many people are impatient. We test a prediction for how to make incentives work particularly well when people are impatient over effort: implement *time-bundled* contracts that make the payment for future effort increase in current effort. We find empirical support for this prediction using a randomized evaluation of an incentive program for exercise (walking) among diabetics in India. On average, a time-bundled contract generates as much effort as a time-separable linear contract, yet at a 15% lower cost. Moreover, time-bundled contracts perform roughly 30% better among individuals with above-median impatience over effort than those with below-median. Pooled across contracts, incentives increase daily steps by roughly 20% and improve blood sugar control relative to a control group.

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1 Introduction

Policymakers are increasingly using incentives to encourage behaviors that have immediate costs but yield benefits in the future, such as saving, exercising, and studying (e.g., Gertler et al., 2019; Carrera et al., 2020; Fryer, 2011). A key motivation for these incentives is to offset underinvestment due to impatience or high discounting of the future, a common trait (e.g., Mahajan et al., 2020; Augenblick and Rabin, 2019; O'Donoghue and Rabin, 1999a). Given this motivation, it is critical that incentive contracts perform well when people are impatient.

This paper proposes and validates a novel strategy for increasing the performance of incentives in the face of impatience: implement *time-bundled* contracts in which the payment for future effort increases with current effort. Notably, this approach is designed to be effective for those who discount effort highly, an important consideration given empirical findings that discount rates can be domain-specific and higher over effort than payment (e.g., Augenblick et al., 2015). We use a randomized controlled trial (RCT) to compare time-bundled contracts to a more standard time-separable contract (in which the payment for current effort depends only on current effort). We find that time-bundled contracts significantly improve contract performance for individuals who are impatient over effort and hence are an effective way to adapt incentives for impatience. In contrast, we test a more traditional strategy (more frequent payments) that should be effective if people are impatient over payment and find no evidence of its effectiveness. Our RCT, which randomizes these contracts among participants in an incentive program for exercise among diabetics and prediabetics, also shows that the program could be a powerful tool in the global fight against chronic disease.

We begin by introducing separate discount rates for effort and payment to a standard contracting model. With this addition, we show that under many conditions, relative to time-separable contracts, time-bundled contracts are more effective when individuals discount their future effort costs more. To illustrate the intuition, imagine you need a worker to perform two days of work. Consider first a time-bundled *threshold* contract that pays the worker a lump sum on day two if and only if she worked on both days. For the contract to induce two days of work, the total payment must exceed the worker's present discounted cost of effort.[1] For example, if her daily cost of effort is \$10, and she discounts future effort by 50%, the payment only needs to be \$15: \$10 for the first day plus a discounted \$5 for the second. In contrast, if you pay her linearly on day two for each day of work, a larger minimum payment of \$20 is required to induce two days of work: \$10 per day. Time-bundled contracts exploit the fact that, when individuals have high effort discount rates, it is "cheaper" to buy their future (discounted) effort than their current effort. However, theory also suggests that time-bundled contracts do not perform better than time-separable under all conditions—even with substantial effort impatience—making it

[1]This example assumes no short-run discounting of payments for simplicity.

crucial to empirically test their performance.

The second (and less novel) strategy we consider is to increase the frequency of payment, which should be effective if individuals are impatient over *payments*. Scholars have long theorized that, because people are impatient, "the more frequent the reward, the better" (Cutler and Everett, 2010). Indeed, DellaVigna and Pope (2018) describes more frequent payment as the main way to adjust incentives for present bias. However, they also acknowledge that increasing payment frequency should only be effective if people heavily discount payments, which even those who heavily discount effort may not do (Augenblick et al., 2015).

We assess the effectiveness of time-bundled contracts and increased payment frequency using an incentive program designed to encourage exercise for diabetics and prediabetics—an important policy goal. Our incentive program monitored participants' walking for 3 months using pedometers and provided financial incentives in the form of mobile phone credits for achieving a daily step target of 10,000 steps. Among participants randomly selected to receive incentives, we randomly varied the contract. In the "base case" contract, payment was a time-separable (in particular, linear) function of the number of days the participant complied with the step target, with payments made weekly. To evaluate time-bundling, we randomized some participants to receive time-bundled threshold contracts (which we also refer to more simply as threshold contracts). These contracts only rewarded compliance with the step target if the step target was met a minimum number of days that week. We used two threshold levels: four and five days. Both contracts paid at the end of the week, like the Base Case. To explore payment frequency, we then randomized two additional linear contracts that paid daily and monthly.

Our primary contribution is to validate time-bundled contracts as a strategy for tailoring incentives for impatience over effort, and we present three main empirical findings. First, on average across the full sample, the time-bundled threshold contracts perform better than the time-separable linear contract—they achieve the same sample-average level of compliance, but do so at a lower cost to the principal. For example, the 5-day threshold contract pays out nearly 15% less in incentives than the linear contract for the same level of compliance because it does not pay out for every day of compliance like the linear contract does. This improves performance from the perspective of a policymaker who wants to maximize the benefits of compliance net of the incentive costs.[2]

The second finding is that high levels of impatience over effort in our sample are an important mechanism driving the effectiveness of time-bundled threshold contracts, as the contracts are significantly more effective for those with higher impatience over effort. Specifically, heterogeneity analysis using a measure of impatience over effort shows that, relative to the time-separable contract, the time-bundled threshold contracts increase compliance with the step target by 6

[2]This assumes linear benefits from compliance, which are appropriate in many contexts.

percentage points (pp) more for those with above-median impatience than for those with below-median impatience. This difference is large, equivalent to roughly 30% of the sample-average effect of either contract (20 pp). The 6 pp estimate represents the difference between a 3 pp positive effect among those with above-median impatience and a 3 pp negative effect among those with below-median impatience. In addition to their effects on compliance, the thresholds also improve cost-effectiveness (i.e., decrease the payout per day of compliance) among both less and more impatient populations. The thresholds thus clearly improve performance among those with greater impatience, while having an ambiguous effect for those with lower impatience. Although our analysis exploits non-random variation in impatience across the population, we provide evidence suggesting that confounding factors do not drive our results.

The better relative performance of the time-bundled threshold among effort-impatient people suggests that a policymaker can improve incentive performance by customizing thresholds based on impatience over effort. Incentives could be customized at the population level by using threshold contracts for populations that are particularly impatient, such as those with chronic diseases. Customization could also occur at the individual level (Andreoni et al., 2023). Individual-level customization can be challenging to implement since impatience is often not observable; however, we provide evidence suggesting that such personalization would be feasible, for example by showing that a principal could use more easily observed characteristics to proxy for impatience.

Finally, we find that increasing the frequency of payment has no discernable impact in our setting. Although this result is somewhat imprecise, additional evidence suggests that participants have low discount rates over the contract payments (mobile phone credits).[3] The low discount rates over payment and lack of impact of high-frequency payments in our setting make it important to identify other methods to adjust incentives for impatience and highlight the significance of our finding that time-bundled contracts are one such method.

A second contribution of our evaluation is to demonstrate that incentives for exercise are a useful tool that could help decrease the burden of chronic disease in India and beyond. Chronic lifestyle diseases such as diabetes represent a severe threat to health and development in low and middle income countries (LMICs). The cost of diabetes alone is estimated to be 1.8% of GDP annually in LMICs (Bommer et al., 2017), with 12% of adults estimated to have the disease (International Diabetes Federation, 2019). Although there is widespread agreement that the key to addressing the burden is to promote lifestyle changes such as better exercise (World Health Organization, 2009), the existing evidence-based interventions promoting such changes in this population are prohibitively expensive (Howells et al., 2016). Governments are thus eager for scalable interventions to promote lifestyle change among diabetics. Our RCT was

[3]While it is possible that people would be more impatient over payments delivered with a different modality, limited impatience over payments is not rare (e.g., Augenblick et al., 2015; Tanaka et al., 2010).

funded by the Government of Tamil Nadu, one of the most populous states in India, who sought an intervention to scale up across their state to address their exploding diabetes epidemic.

Pooling across our incentive contracts, we show that, relative to a control group, our relatively low-cost incentives program substantially increases exercise and moderately improves health among a diabetic population. Average daily steps increase by roughly 20 percent during the intervention, and approximately half of this treatment effect persists after the intervention ends. Incentives also improve blood sugar control, providing the first experimental evidence that an incentive program can improve blood sugar control for diabetes.[4] Because few evidence-based lifestyle-change interventions exist for diabetics, our scalable program offers a promising strategy to combat diabetes in LMICs. Reassuringly, the results appear to replicate: a subsequent study finds a comparable relationship between steps and blood sugar after a similar step target incentive intervention, though it did not prespecify any primary or secondary health outcomes (Dizon-Ross and Zucker, 2025).[5]

1.1 Contributions to the Literature

This paper's main contributions are to theoretically investigate and empirically validate time-bundled contracts as a novel strategy for motivating a wide range of people with high discount rates over effort. In doing so, we connect a classic literature on dynamic incentives (e.g., Lazear, 1981; Lambert, 1983) with a newer literature on domain-specific time preferences and high discount rates over effort (e.g., Augenblick et al., 2015), providing the first examination of how domain-specific discounting affects the design of dynamic incentive contracts. We describe how our work contributes to the dynamic incentives and time-preferences literatures in turn.

Dynamic Incentives We contribute to the literature on dynamic incentives, specifically on contracts that "defer compensation" to the future (as time-bundled contracts do).[6] This primarily theoretical literature explores reasons that time-bundled deferred compensation contracts may be better or worse than time-separable ones (e.g., Lazear, 1979; Rogerson, 1985).

Our first contribution is empirical: we conduct, to our knowledge, the first rigorous empirical comparison of the two types of contracts.[7] Our comparison explores their distributional effects and the mechanisms driving their relative performance. This comparison is valuable because,

[4]Prior work on incentives for diabetics have targeted non-exercise outcomes with limited success. Long (2012) finds no impact from incentivizing blood sugar control. VanEpps et al. (2019) and Desai et al. (2020) report mixed effects on weight loss from incentives for health program attendance and weight loss among prediabetics.

[5]Dizon-Ross and Zucker (2025) was conducted among a similar population of adults with chronic disease. While it also studied step target incentives, its goal was distinct from this study's: it evaluated how to personalize the step targets used in time-separable contracts using choice menus and observable characteristics.

[6]The literature refers to time-bundling as deferring compensation or backloading. Both terms have various meanings, including making payment functions non-separable over time and changing payment timing. We introduce the term time-bundling to clarify that our focus is on non-separability, not payment timing.

[7]In a lab experiment where college students acted as workers and as firms without exerting any real effort, Huck et al. (2011) compares deferred compensation contracts to one another but not to separable contracts.

while both contract types are popular, their relative performance is theoretically ambiguous.

Our second contribution, which has both an empirical and theoretical aspect, is to introduce and test a new theoretical channel for the effectiveness of time-bundled contracts: domain-specific agent discounting of *effort*. Previous dynamic contracting papers use the same discount rate for effort and payments. In such models, time bundling can be effective when barriers, such as unobservable effort, prevent the principal from compensating agents for their exact effort cost at the end of each period (e.g., Lazear, 1979, 1981).[8] Building on evidence that people discount utility and payments differently (e.g., Chapman, 1996; Ubfal, 2016; Augenblick et al., 2015), we introduce domain-specific discounting, which strengthens the case for time-bundled contracts when agents substantially discount future effort in two ways. First, high domain-specific agent discount rates over effort can make time-bundled contracts more effective than time-separable even without traditional barriers to direct end-of-period compensation. Second, with domain-specific discounting, compliance in time-bundled contracts increases relative to time-separable as the discount rate for effort grows—a clean comparative static that does not hold in models with a single discount rate.[9]

Time Preferences Our finding that time-bundled contracts effectively adapt incentives for impatience adds to a small literature proposing incentive designs to motivate impatient agents. As with our contribution to the dynamic incentives literature, we depart from previous work by allowing for domain-specific discounting and targeting impatience over effort in particular. This focus leads naturally to time-bundled contracts, a different solution than generally studied in the impatience literature. For example, DellaVigna and Pope (2018) shows that reducing payment delays—an approach effective for impatience over *payment*—does not significantly increase effort. O'Donoghue and Rabin (1999b) show theoretically that using an increasing punishment for delay over time can help time-inconsistent procrastinators to quickly complete single-period tasks; this is a setting where time-bundling cannot be used since it requires effort in multiple periods. Carrera et al. (2020) examines whether larger time-separable incentives upfront help time-inconsistent people overcome startup costs but finds no evidence of this.

Finally, researchers have also motivated impatient agents with commitment devices (e.g., Royer et al., 2015; Schilbach, 2019). Commitment is a useful tool, but it is not a panacea. Take-up of commitment devices is typically modest (Laibson, 2015), undermining their use as a broad policy solution. Moreover, commitment devices are only effective for sophisticated time-inconsistents, and can even be harmful for naifs (e.g., Bai et al., 2021; John, 2020). In contrast,

[8]This is relevant for us as effort costs vary by period, making it hard to pay the exact effort cost each period.

[9]This comparative static is unambiguous for time-bundled threshold contracts that require effort in all periods. Our exploration of impatience and incentives relates to Jain (2012), which assumes identical discount rates for effort and money and shows that with barriers to immediate payment and quasi-hyperbolic discounting, firms can increase profits by offering two-period quotas. Our model with domain-specific discounting yields stronger and more general results and, unlike Jain (2012), we also confirm our insights empirically.

our theory and empirics show that time-bundled thresholds are effective for all types of people with high discount rates over effort, including both sophisticates and naifs. The effectiveness among naifs is valuable since naive time inconsistency is common (Mahajan et al., 2020). Our work thus broadens the arsenal by identifying an approach that can succeed in settings with naivete or without commitment demand.

The paper proceeds as follows. Section 2 presents our theoretical predictions. Sections 3 and 4 discuss the study setting and design. Sections 5 and 6 present our results on incentive design and our program evaluation of incentives, respectively. Section 7 concludes.

2 Theoretical Predictions

This section describes our theoretical result that, under a broad range of assumptions, time-bundled contracts are particularly effective when individuals have high discount rates over effort. We first summarize the setup of the model, which departs from related literature in modeling separate discount factors in the domains of effort and money. We then describe our main theoretical results and the intuition behind them, with the full model and proofs detailed in Appendix B. Finally, we show that high-frequency payments can address impatience over payment but not effort.

2.1 Set-Up

We study a setting where, each day, an individual chooses whether to comply (i.e., complete a binary action. The principal designs contracts to incentivize compliance over a sequence of T days (the payment period), with payments m_T delivered on day T that depend on the entire sequence of compliance from day 1 to T.

The principal aims to maximize *effectiveness*, defined as the expected daily benefit to the principal from compliance less the expected daily payment to agents. We also define cost-effectiveness as the ratio of the expected daily compliance rate to the expected daily payment. Assuming for simplicity that the benefits to the principal of compliance are linear,[10] one contract is more *effective* than another if it has strictly larger compliance and weakly larger cost-effectiveness, or weakly larger compliance and strictly larger cost-effectiveness.

On day j, the individual chooses compliance (denoted w_j) to maximize expected discounted payments net of expected discounted effort costs. The individual discounts payments and effort with different discount factors. We denote $\delta^{(t)}$ and $d^{(t)}$ as the discount factors over effort and payments t days in the future, respectively; both functions are weakly decreasing in t but not necessarily exponential.[11] From the time j perspective, discounted period i effort costs are

[10]This simplifying assumption is reasonable in our empirical setting since the estimated marginal health benefit of days of exercise is approximately linear (Warburton et al., 2006; Banach et al., 2023).

[11]This model excludes the long-term health benefit of walking. Appendix B.7 discusses why we think this is a reasonable simplification and assesses robustness to adding such a benefit to the model.

$\delta^{(i-j)} w_i e_i$, with e_i being the effort cost of complying on day i. Discounted period T payments are $d^{(T-j)} m_T$.

2.2 Time-Separable Linear Contracts (the Base Case)

We first consider a time-separable, linear contract, paying m per day of compliance, all paid on day T: $m_T = m \sum_{t=1}^{T} w_t$.

Participants comply on day j if the discounted payment outweighs the effort cost:

$$e_j < d^{(T-j)} m. \tag{1}$$

Importantly, this decision rule is independent of $\delta^{(t)}$, making compliance independent of $\delta^{(t)}$. Moreover, since cost-effectiveness is simply $\frac{1}{m}$ for any linear contract with positive compliance, cost-effectiveness and effectiveness are also independent of $\delta^{(t)}$.

2.3 Time-Bundled Thresholds and Impatience Over Effort

We focus on time-bundled *threshold* contracts that pay only if a threshold level of compliance is reached. Formally, with K denoting the threshold level, the payment m_T on day T equals $m' \sum_{i=1}^{T} w_i$ if the individual complies on at least K days in the payment period ($\sum_{i=1}^{T} w_i \geq K$) and 0 otherwise. We present two testable predictions comparing time-separable linear contracts and time-bundled contracts (where the payment for future effort increases in current effort).

Prediction 1 (Comparative Static in $\delta^{(t)}$ of Time-Bundled Threshold versus Time-Separable Effectiveness)**.** *Holding all else equal, under a broad range of reasonable conditions, compliance and effectiveness in time-bundled threshold contracts relative to time-separable contracts decrease in the discount factor over effort, $\delta^{(t)}$.*

Prediction 2 (The Level of Time-Bundled Threshold versus Time-Separable Linear Effectiveness by δ, $T = 2$)**.** *Holding all else equal, under certain conditions:*
(a) When δ is sufficiently low, threshold contracts are more effective than linear contracts that offer the same payment amount per day. When δ is sufficiently high, the reverse is true.
(b) When δ is sufficiently low, the most effective contract is a threshold contract. When δ is sufficiently high, the most effective contract is linear.

Predictions 1 and 2 are each based on a series of formal mathematical results, presented in Appendices B.4 and B.5 respectively, which we label propositions. Both predictions hold under various types of impatience, including time-inconsistent sophistication, time-inconsistent naivete, and time consistency. While it is possible to find specific parameter values that are exceptions, Prediction 1 holds in many typical and empirically relevant cases. Prediction 2 strengthens the first by speaking to the overall effectiveness of threshold and linear contracts rather than just heterogeneity, but holds in fewer cases.

Compliance in Time-Bundled Threshold Contracts To illustrate why time-bundled thresholds behave differently with respect to impatience over effort, we consider a simple example: a threshold contract with $K = T$ (i.e., that pays a fixed m_T if the individual complies every day and 0 otherwise). We assume no payment discounting ($d^{(t)} = 1$), a fixed discount factor for future effort ($\delta^{(t)} = \delta$ for all $t > 0$), and effort costs that are positive, constant across periods, and known from day 1. The full model that we used to derive our predictions, presented in Appendix B, does not make these simplifying assumptions, and our analysis there also explores other threshold levels.

With constant costs, if the participant complies on any given day, they will continue to comply on all subsequent days, since they sink effort costs as they go. On day 1, the participant complies if the present effort cost plus the discounted future effort costs on each of the remaining $(T-1)$ days is less than the payment:

$$e + \delta[(T-1)e] < m_T. \tag{2}$$

On any later day j where the participant has complied on all previous days, the compliance condition compares the remaining present discounted effort costs with the payment:

$$e + \delta[(T-j)e] < m_T. \tag{3}$$

Since the present discounted effort cost $\delta[(T-j)e]$ strictly decreases over time, equation 3 will hold whenever equation 2 is satisfied. Moreover, if the participant does not comply on day 1, she never will (because costs are assumed to be positive). Thus, compliance is 100% if equation 2 holds, and 0% otherwise.

This example illustrates both of our predictions. Threshold compliance decreases as δ increases (Prediction 1) since the present discounted value of future effort costs (the term $\delta[(T-1)e]$ in equation 2) increases. (In Appendix B, we show that effectiveness patterns follow closely from compliance.) Moreover, the time-bundled threshold could achieve full compliance with a payment of just $e + \delta(T-1)e$, whereas a linear contract would require a payment of eT, making thresholds more cost-effective for reaching a given compliance level—and therefore more effective—when δ is low (Prediction 2).

Both predictions rest on the foundation that, in time-separable linear contracts, compliance and effectiveness are independent of $\delta^{(t)}$, while in time-bundled thresholds, they tend to decrease with $\delta^{(t)}$.

2.4 Payment Frequency and Impatience over Payment

Returning to the compliance condition in the separable linear contract (equation 1), it is intuitive to see that increasing payment frequency (i.e., reducing T) will increase compliance if people are impatient over payment. In Appendix B.6, we prove the following prediction:

Prediction 3 (Frequency)**.** *If agents are impatient over the receipt of financial payments (i.e., if $d^{(t)} < 1$ for $t > 0$ and is weakly decreasing in t), then the compliance and effectiveness of the base case linear contract are weakly increasing in the payment frequency. If agents are patient over the receipt of financial payments ($d^{(t)} = 1$), then payment frequency does not affect compliance or effectiveness.*

2.5 Empirical Tests

Our theoretical analysis informed the design of our experiment. Among participants who receive incentives in our experiment, we randomly vary whether the contract is linear or is a threshold contract offering the same payment per day as the linear ($m' = m$). To assess the empirical relevance of Prediction 2—that, under certain conditions, the threshold contract has higher effectiveness than the linear when discount factors over effort are low—we compare the effectiveness of the two contracts in the full sample. To assess our more general Prediction 1 and investigate whether impatience is a contributor to the effectiveness of thresholds, we test for heterogeneity in the effect of the threshold relative to the linear contract based on a measure of impatience over effort. Finally, to shed light on the role of payment frequency and the discount rate over payments (per Prediction 3), we randomize the frequency of payments.[12]

3 Experimental Design

3.1 Sample Selection and Pre-Intervention Period

We conducted our experiment in the South Indian city of Coimbatore, Tamil Nadu. India is facing a diabetes epidemic, and prevalence is highest in urban areas of southern states (Anjana et al., 2011). We selected our sample through public screening camps held across the city in hospitals, markets, religious institutions, parks, and other locations in order to recruit a diverse socioeconomic group. During the camps, surveyors took health measurements, discussed each individual's risk for diabetes and hypertension, and conducted an eligibility survey. To be eligible for the study, individuals needed to have a diabetes diagnosis or elevated blood sugar, have low risk of injury from regular walking, be capable with a mobile phone, and be able to receive payments in the form of mobile recharges.[13] After screening, eligible individuals were

[12] Before launching our experiment, we drafted a pre-analysis plan that guided our design and power calculations. While we did not polish it for public posting at the time, we have since posted the draft (last modified before we launched endline data collection) in our AEA registry to demonstrate that our key subsample and heterogeneity analyses were conceived *ex-ante*.

[13] The full list of eligibility criteria was: must be diabetic or have elevated random blood sugar (> 150 if has eaten in previous two hours, > 130 otherwise); be 30–65 years old, physically capable of walking 30 minutes, literate in Tamil, and not pregnant or on insulin; have a prepaid mobile number used solely by them, without unlimited calling; reside in Coimbatore; not have blindness, kidney disease, type 1 diabetes, or foot ulcers; not have had major medical events such as stroke or heart attack.

invited by phone to participate in a program encouraging walking.

Surveyors visited the participants at their homes or workplaces for a pre-intervention visit to conduct a baseline health survey, deliver lifestyle modification advice, and enroll them in a one-week phase-in period to familiarize them with our procedures and collect baseline walking data. Surveyors gave participants pedometers for the duration of the program and collected step data by syncing the pedometers with a central database. Because syncing requires an internet connection, which most participants did not have, pedometer step data were not available in real time. Thus, we also asked participants to report their daily step count to an automated calling system which called every evening and prompted them to enter the step count recorded on their pedometer. During the pre-intervention visit, surveyors demonstrated how to wear a pedometer, report steps, and check text messages from our reporting system. Surveyors asked participants to wear the pedometer and report their steps each day of the phase-in period.

At the end of the phase-in period, surveyors visited respondents to sync the data from the pedometers and conduct a baseline time-preference survey. After all baseline data were collected, surveyors described to participants their randomly assigned treatment group by guiding them through a contract describing the intervention period.[14] We exclude from the sample all participants who withdrew or were found ineligible prior to receiving their contracts, leaving a final experimental sample of 3,192 individuals. The sample represents 41% of the screened, eligible population.[15] We began screening in October 2016 and enrolled participants on a rolling basis, roughly in order of screening date, from February–November 2017. Endline data collection launched in May 2017 as participants completed their intervention period.

3.2 Experimental Design and Contract Launch

Our interventions encouraged participants to walk at least 10,000 steps a day. We chose this daily step target to match exercise recommendations for diabetics; it is also a widely quoted target among health advocates and a common benchmark in health studies.

We randomized participants into the incentive group or one of two comparison groups.

1. **Incentive:** Receive a pedometer and incentives to reach a daily target of 10,000 steps.

2. **Monitoring:** Receive a pedometer but receive no incentive contract.

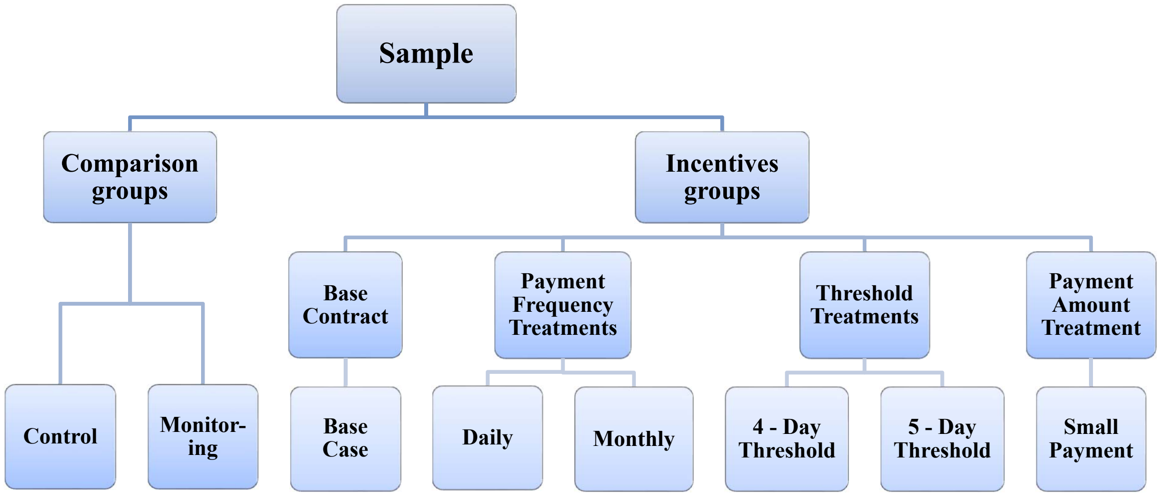
3. **Control:** Receive neither a pedometer nor an incentive contract.

Within the incentive group, we randomized participants to receive one of six incentive contracts

[14] All participants who completed the baseline survey were randomly assigned to treatment prior to this visit. The randomization was stratified by baseline HbA1c (a measure of blood sugar control) and a simple one-question proxy for impatience using a randomization list generated in Stata.

[15] As described in Section 4.2, we also exclude forty participants who are assigned to receive a contract or payment based on real-stakes preference elicitations that would interfere with the randomly assigned contract.

for walking, as shown in Figure 1.



Pedometers	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incentives	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Incentive Details								
Frequency	N/A	N/A	Weekly	Daily	Monthly	Weekly	Weekly	Weekly
Threshold	N/A	N/A	None	None	None	4 Days	5 Days	None
Amount (INR)	N/A	N/A	20	20	20	20	20	10
Sample Sizes	585	203	902	166	164	794	312	66

Figure 1: Experimental Design

3.2.1 Incentive Sub-Groups

All incentive sub-groups received payments for accurately reporting steps above the daily 10,000-step target through the automated step-reporting system. We delivered all incentive payments as mobile recharges (credits to the participant's mobile phone account).[16] After reporting steps, participants immediately received text-message confirmations of their step report, payment earned, and the payment date. We also sent participants weekly text messages summarizing their walking behavior and total payments earned.

Each of the six incentive subgroups received a different incentive contract with three dimensions of variation: time-separability, payment frequency, and payment amount.

[16]The relevant payment discount rate is therefore over mobile recharges, which could be higher, lower, or the same as that over cash (e.g., it could be the same for people whose baseline daily mobile usage is higher than the payment amount: payment would decrease money spent on recharges and increase cash on hand).

The Base Case Participants in *Base Case* received a time-separable, linear contract paying 20 INR (0.3 USD) per day of compliance with the 10,000-step target. Payments were made at a weekly frequency.

We call this the base case contract because it differs from all other contracts in exactly one dimension: time-separability, payment frequency, or payment amount. We can compare any other group to the Base Case to assess the effect of changing a single contract dimension.

Threshold Contracts Participants in the threshold groups received contracts that differ from the base case contract only in time-separability: the threshold contracts use time-bundled threshold payment functions. Participants in the *4-Day Threshold* received 20 INR for each day of compliance only if they met the target at least four days in the weeklong payment period. So, a 4-Day Threshold participant who met the step target on only three days in a payment period would receive no payment, while one who met it on five days would receive $5 \times 20 = 100$ INR. Similarly, participants in the *5-Day Threshold* received 20 INR for each day of compliance if they met the target at least five days in the week.

The threshold contracts implicitly gave participants a goal of how many days to walk per week. To control for goal effects, surveyors verbally encouraged all incentive sub-groups to walk at least four or five days per week when initially explaining the contracts.[17] To maximize statistical power, we pool the 4- and 5-Day Threshold for our main analyses. We show results for the two threshold groups separately in some exploratory analyses.[18]

Payment Frequency The contracts for two groups, *Daily* and *Monthly*, differ from the base case contract only in the payment frequency. In Daily, recharges were delivered at 1:00 am the same night participants reported their steps. In Monthly, recharges were delivered every four weeks for all days of compliance in the previous four weeks.

Higher payment frequency could increase both the salience of compliance and trust in the payment system. To hold these factors constant, all incentive sub-groups received daily feedback on their compliance and a test payment of 10 INR the night before their contract launched.

Payment Amount Participants in our final incentive group, *Small Payment*, received contracts that differ from the base case only by the amount of incentive paid. This group received 10 INR, instead of 20 INR, for each day of compliance. We included this group to learn about the distribution of walking costs and to benchmark the size of our other treatments effects.

[17]For the threshold groups, the target days-per-week was the same as their assigned threshold level. For the other groups, it was randomized between 4 and 5 in the same proportion as in the threshold groups.

[18]We prespecified in our AEA registry that we would pool the threshold groups (see the power calculations section). We included the two threshold levels, with the *ex ante* intention to pool them, to reduce the risk that compliance would be so high or so low (because the threshold was very easy or hard to reach) that we would not have statistical power to test the prediction that relative compliance would increase with effort impatience.

We allocated more of our sample to the threshold groups than the payment frequency groups for two reasons. First, we regard our insights about time-bundled thresholds as more novel than our insights about frequency. Second, we need to understand heterogeneous threshold treatment effects to test Prediction 1, while the main effects of frequency suffice to test Prediction 3.

3.2.2 Comparison Groups

The incentive program could affect behavior because it provides financial incentives or simply because it monitors walking behavior. We include two control groups in our experiment, a monitoring group and a pure control, to allow us to isolate the effects of financial incentives on steps while also testing whether the full program impacts health relative to pure control.

Monitoring Monitoring participants were treated identically to the incentive groups except that they did not receive incentives. They received pedometers and were encouraged to wear the pedometers and report their steps every day. They also received daily step report confirmation texts and weekly text message summaries, as in the incentive groups. Finally, during the upfront explanation of the contract, surveyors delivered the same verbal step target of 10,000 daily steps and the same encouragement to walk at least four or five days per week.

Pure Control Control participants received neither pedometers nor incentives during the intervention period (they returned their pedometers at the end of the phase-in period). Because most incentive programs bundle the "monitoring" effect of a pedometer with the effect of incentives, the pure control group is a useful benchmark from a policy perspective.[19]

3.2.3 Contract Understanding

To ensure participants understood their contracts, a few days after each participant was assigned their contract, a surveyor called them to ask several questions testing their understanding of their contract. If participants got an answer wrong, the surveyor would explain the correct response. The responses indicate that a vast majority of participants did indeed understand their assigned contract (Online Appendix Table F.1).

3.3 The Intervention Period and After

During the 12-week intervention period, participants received incentives, which were based on both their assigned contracts and their reported steps. To verify the reports, we visited participants every two to three weeks to manually sync their pedometers, cross-check the pedometer data against the reported data, and discuss any discrepancies. Anyone found to be chronically overreporting was suspended from the program. All empirical analysis is based on

[19] At the request of our government partners, we tested an additional intervention: weekly text message reminders promoting healthy behaviors (the "SMS treatment"). Ten percent of the sample, cross-randomized with all treatments, received the messages, which we control for in our regressions.

the synced pedometer data, not the reported data.[20]

At these visits, we also conducted short surveys to collect biometric data (we conducted these visits even with pure control group participants who did not have a pedometer in order to hold survey visits constant across participants). At the end of the 12-week intervention period, we conducted an endline survey. Figure A.1 shows the intervention timeline.

Finally, to assess the persistence of our treatment effects on exercise, we gave pedometers to the final 1,254 participants enrolled in our experiment (including Control participants) for 12 weeks after the intervention period had ended. We refer to this period as the post-intervention period. Participants no longer reported steps daily or received incentive payments, but surveyors still returned every four weeks to sync their pedometers.

4 Data and Outcomes

This section first describes our measures of baseline information—including health, walking and time-preferences—and presents summary statistics. Next, it describes our two sources of outcomes data: pedometer data and a health survey.

4.1 Baseline Data: Demographics, Health and Walking

The baseline health survey, conducted at the first household visit, contains information on respondent demographics, health, fitness, and lifestyle. Health measures include HbA1c, a measure of blood sugar control over three months; random blood sugar (RBS), a measure of more immediate blood sugar control; body mass index (BMI) and waist circumference, two measures of obesity; blood pressure (BP), a measure of hypertension; and a short mental health assessment. During the phase-in period (between the baseline health survey and randomization), we collected one week of baseline pedometer data.

4.2 Time Preferences Data

Impatience over Effort Following the phase-in period, we conducted a baseline time-preference survey to measure impatience over effort. As highlighted in Kremer et al. (2019), "time preferences [over effort and consumption] are difficult to measure, and the literature has not converged on a broadly accepted and easily implementable approach." Notably, our sample was elderly and had limited education, and had difficulty with the screen-based convex time budget (CTB) measure of Andreoni and Sprenger (2012a); although we implemented a CTB module, the data are of such poor quality that we do not use them for analysis.[21] Our heterogeneity analyses instead leverage four other measures of impatience over effort collected during

[20]Online Appendix G contains detailed statistics on misreporting. Misreporting rates are similar across monitoring and incentive groups, suggesting misreports were primarily accidental.

[21]Respondents did not understand the CTB method well, and we have an order of magnitude more law-of-demand violations than lab-based studies with college students. Moreover, as described in Online Appendix I.3,

the time-preference survey, with relatively consistent results.

Impatience Index and Predicted Impatience Index: Our preferred measure of impatience over effort is an index of responses to simple survey questions from the psychology literature on impatience and procrastination. The questions, listed in Panel A of Table A.1, are a subset of the Tuckman (1991) and Lay (1986) scales chosen *ex ante* by our field team as translating well to our setting. Each question asks respondents to respond on a Likert scale of agreement with a statement such as "I'm continually saying 'I'll do it tomorrow'." We construct the index (hereafter: the impatience index) by averaging the standardized question responses.

The Tuckman and Lay scales are validated predictors of real behaviors such as poor academic performance (Kim and Seo, 2015). The impatience index also predicts behavior in our sample: those with higher index values walk less and have worse diets at baseline (Table A.1). We further validate the impatience index by showing that it predicts an incentivized measure of effort impatience. After the completion of our experiment, we elicited incentivized choices from a separate sample of similar participants (n=71) regarding the number of effort tasks they wanted to complete on different days for different piece rates following the methodology of Augenblick (2018) (we were unaware of the Augenblick (2018) methodology when we began our experiment in 2016.) Reassuringly, Appendix C.1.3 shows that those with higher impatience index also make more effort-impatient choices, choosing relatively more tasks in the future than the present.[22]

We began collecting the impatience index partway through the experiment,[23] so it is only available for the latter 54% of the sample. The available sample yields sufficient power to conduct heterogeneity analyses. That said, to check the robustness of our results in the full sample, we fit a "predicted index" using a LASSO model with three survey questions on self-control asked of all participants. Panel B of Table A.1 lists the questions and shows that the predicted index also predicts behavior in our sample.

Simple CTB Questions: Our third impatience measure uses two simplified questions that follow the CTB paradigm of selecting intertemporal effort allocations. However, instead of allowing participants to allocate steps from a continuous convex walking budget, these questions require respondents to select a preferred allocation between just two discrete points from such a

our CTB estimates do not converge for 44% of the sample, they do not correlate in the expected direction with any behaviors we measure, and respondents did not follow through with their chosen allocations. These issues make the CTB estimates unusable for analysis.

[22]Specifically, Figure C.1(a) shows the gap between tasks chosen for the future versus the present is more than twice as large for those with above- than below-median impatience index. Table C.1 also shows that a structurally estimated effort discount factor is large and statistically indistinguishable from 1 among people with below-median impatience, but significantly smaller than 1 among those with above-median impatience.

[23]We introduced the impatience index in response to the challenges we encountered with the full CTB module.

budget.[24] For example, one question asked the respondent whether they would rather walk (A) 30 minutes today and 60 minutes one week from today, or (B) 60 minutes today and 20 minutes one week from today, both in exchange for the same large payment. Our impatience measure is the average of the indicators for choosing the option with more walking later (e.g., option (A) above), but our findings are robust to different ways of combining the answers (Online Appendix Table F.2). Table A.1 shows this measure correlates in the expected direction with baseline exercise: people who are more impatient according to this measure exercise less.

Demand for Commitment: Our final measure relies on participants' demand for contracts that are financially dominated but increase incentives for future effort, a common (but coarse) indicator of sophisticated present bias in the literature (e.g., Ashraf et al. 2006; Kaur et al. 2015).[25] We presented participants with two choices, each between the base case contract and one of the financially-dominated contracts, either the 4-day or 5-day threshold. Our measure is the simple average of the two indicators for choosing the threshold contracts (Online Appendix Table F.2 shows that our findings are robust to different ways of aggregating).

The elicitation was incentive-compatible, as we assigned a very small fraction of the sample to their selected contract for each choice.[26] To ensure understanding, we provided visual-aid based explanations of payment in both contracts, emphasizing the dominated payment schedule in the threshold contracts, followed by quizzes to test understanding.[27] Demand for commitment is relatively high in our setting, with 51% and 46% of the sample preferring the 4-day and 5-day thresholds to the linear contract, respectively (Table A.1).

Impatience Over Payments Our theory predicts that impatience over effort affects the performance of time-bundled thresholds, and so we focus on measuring impatience over effort for heterogeneity analysis. However, we also collected several measures of impatience over payments to better understand our population and for use in robustness checks.

[24]These questions were asked as a "warm-up" for the (unsuccessful) full CTB module. The Simple CTB seems to have performed better than the full version, as described in more detail in Online Appendix I.2.

[25]While Carrera et al. (2022) shows that this measure is biased upwards due to measurement error caused by participants misunderstanding their utility under the contracts, the authors argue it is still "useful as one imperfect measure of awareness [sophistication] of time-inconsistency." While we prefer our primary measure because it is can detect naive impatience and because it is less coarse, we believe the demand for commitment measure is useful both to show robustness to an incentive-compatible measure and because it provides a proxy of sophistication that we can use to disentangle behavior between impatient sophisticates and impatient naifs.

[26]We used a random lottery to make preference elicitations incentive-compatible. Respondents answered a number of "lucky choice" questions—including the two commitment questions, preferred payment frequency, a risk aversion question, and others—and were told that one would be randomly selected for implementation. The risk aversion question (which allocated either a certain or a lottery-based participation incentive) was selected for 98% of respondents. One of the other questions was selected for 2% of survey respondents, with one of the two commitment choices selected for 0.25% of survey respondents. This 2% is excluded from all analyses.

[27]The preference elicitation is in our Time Preference survey ("04_Time Preference Survey.pdf"), available at https://faculty.chicagobooth.edu/rebecca-dizon-ross/survey-instruments with our other surveys.

We collected three proxies for impatience over recharges at baseline: a real-stakes measure of demand for more frequent payment, recharge balances, and recharge usage.[28]

We complement these proxies with more direct measures of impatience over payments that we collected after randomization. We use these measures to shed light on the overall levels of impatience over payments in our sample (Appendix C.2) and to show evidence that impatience over effort and over payments vary independently (Appendix C.3).

The first more direct measure uses a series of seven real-stakes *Simple CTB* questions in the recharge domain that we collected at the second Fitbit sync visit for a subset of participants.[29] (Since our focus is impatience over effort, the term "Simple CTB" without further specification hereafter refers to the Simple CTB over effort measure.)

Participants were asked to choose between four allocations of recharges between an earlier and a later date (selected from a discretized CTB budget set), and were told upfront that a randomly selected fraction of them would receive their choice from a randomly selected question. The second measure harnesses *Paycycle Effects*—the degree to which participants' compliance increases as the payday approaches—following the methodology of Kaur et al. (2015).

Impatience Over Effort versus Payments Our theoretical predictions rely on there being a distinction between discount rates over effort (δ) and payment (d), as we take comparative statics with respect to one holding the other constant. Appendix C provides two pieces of evidence that δ and d are distinct. First, Section C.2 shows that, in our setting, population-level estimates of δ and d are significantly different, with $\delta < d$. Second, Section C.3 shows that, at the individual level, there is no correlation between our measures of impatience over effort and our measures of impatience over payment.

4.3 Summary Statistics

The first column of Table 1 displays the baseline characteristics of our sample. The sample is, on average, 50 years old. The average monthly household income is approximately 16,000 INR (about 240 USD) per month, close to the median for an urban household in India (Ministry of Labour and Unemployment, 2016). Panel B shows that our sample is at high risk for diabetes and its complications: 67% of the sample has been diagnosed with diabetes by a doctor, 82% have HbA1c levels that indicate diabetes, and the RBS measures show poor blood sugar control. The sample also has high rates of comorbidities: 49% have hypertension and 61% are overweight. Panel C shows that, on average, participants walked 7,000 steps per day in the phase-in period,

[28] Demand for more frequent payment is an incentive-compatible measure gathered by asking participants' preferences among the daily, base case, and monthly contracts. Higher balances and/or usage indicate a person's recharge purchases are less constrained, and thus, their discount rate over recharges is more likely to be low.

[29] We began collecting these data after problems with the full CTB surfaced. Rather than further bog down the lengthy time-preference survey, we chose to add these simpler questions to a later encounter with participants.

comparable to average daily steps in many developed countries (Bassett et al., 2010). Panels D and E show our measures of impatience over effort and impatience over payment.

Baseline measures are balanced across treatment groups. Columns 3 and 6 show means for the control and base case groups, while columns 4–5 and 7–10 show differences in means relative to Control and Base Case, respectively. To explore balance, we jointly test the equality of all characteristics relative to Control for Incentives and Monitoring or the Base Case for all other incentives sub-groups. All tests fail to reject the null that all differences are zero. Covariates are also balanced in the subsample with post-intervention period data.

4.4 Outcomes: Exercise

We measure exercise using a time-series dataset of daily steps walked by each participant with a pedometer during the intervention period and (for a subset of the sample) the 12-week period after that. We do not have daily steps for the control group during the intervention period because they did not have pedometers. All analyses use *pedometer* steps as the outcome; however, payments to participants were based on *reported* steps.[30]

Our primary preregistered outcome for assessing how contract design impacts performance is compliance with the 10,000 daily step target, as this is the action we reward.[31] Compliance with a moderate daily step target is also a health-relevant outcome among people with diabetes, as walking leads muscles to increase glucose uptake for 24–48 hours, impacting short-term blood sugar management. Exercise recommendations for diabetics are thus focused on frequency (ideally daily, or at least 5-6 days per week) rather than quantity alone (Colberg et al., 2016). However, because the quantity of activity is likely to have independent health benefits, we include the average number of steps taken per day as a secondary exercise outcome.

4.4.1 Data Quality Controls

A potential issue with the daily step data is that we only observe steps taken while participants wear the pedometer. Because participants in the incentive groups are rewarded for taking 10,000 steps in a day with the pedometer, they have an additional incentive to wear the pedometer. This could lead to a potential selection issue if the incentive group participants wear their pedometers more than the monitoring group.

To minimize selective pedometer-wearing in the intervention period, we incentivized participants to wear their pedometers. We offered a cash bonus of 200 INR ($\approx$ 3 USD) if participants wore their pedometer (i.e., had positive steps) on at least 80% of days. As a result, pedometer

[30] Although incentives were delivered for reported steps, we cross-checked reports with actual pedometer data after every pedometer sync. Anyone who was overreporting was initially warned, then suspended, and eventually terminated from the program if the behavior continued. Online Appendix G provides more detail.

[31] We preregistered this outcome in our AEA registry.

Table 1: Baseline Summary Statistics and Balance Across Treatment Groups

			Groups			Incentives sub-groups				
	Full Sample		Control	Incentives	Monitoring	Base Case	Threshold	Daily	Monthly	Small Payment
	Mean	SD	Mean	Coef	Coef	Mean	Coef	Coef	Coef	Coef
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Demographics										
Age	49.56	8.51	49.78	-0.33	0.50	49.60	-0.20	-0.03	-0.80	-0.50
				(0.38)	(0.48)		(0.61)	(0.96)	(0.29)	(0.62)
Female (=1)	0.42	0.49	0.46	-0.04	-0.02	0.41	-0.01	0.03	-0.04	0.07
				(0.06)	(0.57)		(0.79)	(0.55)	(0.38)	(0.27)
Labor force participation (=1)	0.74	0.44	0.73	0.02	-0.01	0.74	0.01	0.01	0.07	-0.04
				(0.24)	(0.84)		(0.62)	(0.76)	(0.04)	(0.45)
Per capita income (INR/month)	4465	3641	4488	-41	132	4477	-16	-410	122	-136
				(0.84)	(0.66)		(0.92)	(0.11)	(0.67)	(0.72)
Household size	3.91	1.62	3.94	-0.03	-0.11	3.89	0.07	0.03	-0.14	-0.31
				(0.68)	(0.36)		(0.34)	(0.79)	(0.29)	(0.06)
B. Health										
Diagnosed diabetic (=1)	0.67	0.47	0.67	0.00	0.01	0.68	-0.01	-0.06	-0.06	-0.09
				(0.90)	(0.73)		(0.62)	(0.15)	(0.12)	(0.16)
Blood sugar index	0.01	0.92	0.01	0.00	0.04	0.02	-0.01	-0.02	-0.01	-0.17
				(0.91)	(0.59)		(0.78)	(0.82)	(0.88)	(0.11)
HbA1c (mmol/mol)	8.68	2.33	8.67	0.01	0.09	8.72	-0.04	-0.15	-0.06	-0.38
				(0.92)	(0.65)		(0.71)	(0.46)	(0.77)	(0.17)
Random blood sugar (mmol/L)	192.52	89.44	191.32	1.18	4.75	193.26	-1.03	2.32	0.05	-15.88
				(0.77)	(0.50)		(0.80)	(0.76)	(0.99)	(0.11)
Systolic BP (mmHg)	133.38	19.16	133.33	0.01	0.73	133.27	-0.44	1.98	0.90	2.35
				(0.99)	(0.63)		(0.60)	(0.27)	(0.58)	(0.38)
Diastolic BP (mmHg)	88.48	11.10	88.54	-0.08	-0.01	88.19	0.26	1.11	0.41	1.81
				(0.89)	(0.99)		(0.60)	(0.29)	(0.64)	(0.27)
HbA1c: Diabetic (=1)	0.82	0.38	0.82	0.00	-0.01	0.84	-0.03	-0.07	-0.05	-0.07
				(0.88)	(0.77)		(0.08)	(0.04)	(0.13)	(0.18)
BP: Hypertensive (=1)	0.49	0.50	0.46	0.04	0.05	0.49	0.00	0.04	0.02	-0.03
				(0.12)	(0.21)		(0.98)	(0.33)	(0.69)	(0.59)
Overweight (=1)	0.61	0.49	0.62	-0.02	0.04	0.60	0.00	-0.04	-0.03	0.06
				(0.32)	(0.35)		(0.98)	(0.36)	(0.51)	(0.30)
BMI	26.42	4.34	26.52	-0.12	-0.05	26.47	-0.17	-0.06	-0.08	0.52
				(0.55)	(0.88)		(0.37)	(0.89)	(0.84)	(0.32)
C. Walking - phase-in										
Exceeded step target (=1)	0.25	0.32	0.25	0.00	-0.01	0.23	0.03	0.02	0.04	0.04
				(0.90)	(0.58)		(0.04)	(0.47)	(0.13)	(0.32)
Average daily steps	7004	3981	7066	-68	-174	6810	268	236	639	208
				(0.71)	(0.57)		(0.14)	(0.50)	(0.05)	(0.69)
D. Impatience over effort										
Impatience index (SD's)	0.09	0.99	0.00	0.12	0.05	0.14	-0.05	-0.10	0.05	0.12
				(0.06)	(0.62)		(0.47)	(0.36)	(0.67)	(0.44)
Predicted index (SD's)	-0.05	1.00	0.00	-0.06	-0.15	-0.02	-0.06	-0.07	0.00	-0.09
				(0.22)	(0.05)		(0.17)	(0.43)	(0.97)	(0.44)
E. Mobile recharges										
Current mobile balance (INR)	29.34	49.59	30.80	-1.83	-1.33	29.69	-1.25	-1.09	-1.14	0.36
				(0.42)	(0.74)		(0.58)	(0.75)	(0.83)	(0.94)
Yesterday's talk time (INR)	6.58	8.76	7.22	-0.78	-0.75	6.58	-0.26	-0.72	1.09	-1.64
				(0.09)	(0.33)		(0.50)	(0.22)	(0.16)	(0.04)
Prefers daily (=1)	0.17	0.38	0.18	0.00	-0.02	0.17	0.01	0.03	0.04	0.02
				(0.79)	(0.47)		(0.58)	(0.31)	(0.28)	(0.73)
Prefers monthly (=-1)	0.24	0.43	0.25	-0.01	0.02	0.24	0.00	0.03	-0.01	0.02
				(0.59)	(0.50)		(0.90)	(0.36)	(0.74)	(0.71)
F-tests for joint orthogonality										
p-value	.	.	.	0.35	0.70	.	0.83	0.63	0.74	0.20
Sample size										
Number of individuals	3,192		585	2,404	203	902	1,106	166	164	66
Percent of sample	100.0		18.3	75.3	6.4	28.3	34.6	5.2	5.1	2.1
Number of ind. with ped. data	2,559		0	2,359	200	890	1,079	163	163	64

Notes: "Mean" is group mean; "Coef" columns show coefficients from regressing the variable on a treatment indicator among the treatment group and its main comparison group (for Incentives and Monitoring, the main comparison group is Control; for Threshold, Daily, Monthly, and Small Payment, it is Base Case). p-values are in parentheses. BMI is body mass index, BP is blood pressure, overweight is BMI>25, hypertensive is systolic BP>140 or diastolic BP>90. Current mobile balance is the available phone credit on the respondent's phone, and yesterday's talk time is the monetary equivalence of minutes used the day before the baseline survey. Threshold pools 4- and 5-day threshold groups. In Incentives and Monitoring, the total number of individuals is larger than the number with pedometer data as some participants withdrew immediately. The F-tests are from separate regressions for each treatment group.

wearing rates are high, and the difference between treatments is small: 85% in Monitoring versus 88% in Incentives. However, the difference is statistically significant (Table A.2, column 2). To address this imbalance, we show the robustness of our results to Lee (2009) bounds accounting for missing step data due to not wearing pedometers.[32] Our primary specifications do not condition on wearing the pedometer (instead we set steps and compliance to 0 on days when the pedometer was not worn), but we show that our results are robust to conditioning on wearing.

We also assess whether the incentive group wore their pedometers for more minutes per day, conditional on wearing. To do so, we use data recorded by pedometers on the times that the participant put it on and took it off. Reassuringly, these times are balanced across arms (Online Appendix Table F.4, Panel B).

To encourage participants to wear their pedometers in the post-intervention period, we provided all participants with a small incentive for wearing their pedometers on a sufficiently high fraction of days. While average pedometer-wearing rates declined somewhat to 69% (from 87% in the intervention period), the rates are balanced across arms.

Another concern is that participants might give their pedometers to someone else. Our data suggest that this concern is limited. First, we performed 835 unannounced audit visits to participants' homes. In 99.6% of visits, participants were not sharing their pedometers. Second, we check if participants' minute-wise step counts exceed age-based expectations. This is very rare, and balanced across Incentives and Monitoring (Online App. Table F.4).

4.5 Outcomes: Health

The second outcomes dataset, the endline survey, gathered health, fitness, and lifestyle information similar to the baseline health survey. The completion rate is 97% in each treatment group (Control, Monitoring, and Incentive; p-value for equality 0.99).

Our primary health outcome is blood sugar, the main clinical marker of diabetes. Our preferred measure of blood sugar control is a standardized index of two measures: HbA1c (longer-term blood sugar control) and RBS (short-term blood sugar control), which have independent predictive power.[33] We also present the measures separately. While our AEA registry prespecified HbA1c as our blood sugar measure, accurately measuring HbA1c in the field proved challenging.[34] It was easier to measure RBS, another blood sugar indicator

[32] We do not have participant pedometer data (e.g., because the pedometer broke or the sync was unsuccessful) on 6% of days. Missing pedometer data are balanced across Incentives and Monitoring (column 3, Table A.2). While our main specifications drop days with missing pedometer data, Online Appendix Table F.3 shows robustness to alternate specifications and Lee bounds. While missing data are balanced overall, one specific source of missing data (mid-intervention withdrawals) is imbalanced (column 6 of Table A.2). Results are robust to Lee bounds accounting specifically for that source (column 5 of Online App Table F.3).

[33] Online Appendix Table F.5 shows that baseline RBS strongly predicts endline HbA1c in Control even conditional on baseline HbA1c, and vice versa.

[34] The only available measurement tool (the SD A1cCare analyzer from SD Biosensor) was temperature-sensitive and error prone, and in a validation subsample it did not align with gold-standard lab measurements.

strongly associated with diabetes severity(Bowen et al., 2015).[35]

Since exercise is also associated with improvements in hypertension and cardiovascular health, we measured blood pressure, BMI, and waist circumference as secondary health outcomes. We combine these three measures with the two blood sugar measures to construct a standardized "health risk index".

We also gathered information on two secondary health outcomes: mental health and anaerobic fitness. We measure mental health using seven questions from RAND's 36-Item Short Form Survey. Anaerobic fitness is measured via two fitness tests (time to complete five stands from a seated position, and time to walk four meters). Following Kling et al. (2007), we impute missing components of all indices as the mean within an individual's group (Control, Monitoring, or Incentive) for individuals who have at least one nonmissing index component.

5 Empirical Results: Incentive Design

This section empirically examines the implications of impatience for incentive design. We first show that our incentive program increases compliance with the step target, making this a good setting to explore our contract variations. Second, we show that adding a time-bundled threshold increases effectiveness. Third, we show that the threshold is particularly effective for those with higher impatience over effort, in line with our theoretical prediction that impatience over effort is a mechanism for its effectiveness. Finally, we find that higher-frequency payments do not increase effectiveness, suggesting limited impatience over payments.

5.1 Incentives Increase Exercise

We first test whether providing financial incentives increases steps and compliance with the 10,000-step target during the intervention period. To do so, we compare outcomes in the pooled incentive groups with the monitoring group, thus isolating the impact of the financial incentives alone. We estimate regressions of the following form:

$$y_{it} = \alpha + \beta Incentives_i + \boldsymbol{X}_i'\gamma + \boldsymbol{X}_{it}'\lambda + \varepsilon_{it}, \tag{4}$$

where y_{it} is either individual i's steps on day t during the intervention period or an indicator for individual i surpassing the 10,000-step target on day t; $Incentives_i$ is an indicator for being in the incentive group; and $\boldsymbol{X}_i$ and $\boldsymbol{X}_{it}$ are vectors of individual- and day-level controls, respectively, described in the notes to Table 2. We exclude the control group, for whom we have no pedometer data. We cluster the standard errors at the individual level. The coefficient of interest, β, is the average treatment effect of Incentives relative to Monitoring only. Panel A of Table 2 shows the results.

Incentives have large impacts on walking, increasing the share of days that participants reach their 10,000-step target by 20 pp or roughly 70 percent (column 1 of Table 2). This

[35] While RBS is problematic as a diagnostic because it is sensitive to recent activity such as eating, it yields a good measure of average glycemic control in a sample (Dandona, 2017).

Table 2: Incentives Increase Average Walking

Dependent variable:	Exceeded step target	Daily steps	Daily steps (if > 0)	Earned payment when target met
	(1)	(2)	(3)	(4)
A. Pooled incentives				
Incentives	0.200***	1266.0***	1161.5***	0.952***
	[0.0186]	[208.7]	[188.5]	[0.00305]
B. Unpooled incentives				
Base Case	0.211***	1388.4***	1203.1***	1.006***
	[0.0201]	[222.1]	[199.9]	[0.00262]
Threshold	0.198***	1216.3***	1142.6***	0.892***
	[0.0199]	[220.9]	[198.5]	[0.00546]
Daily	0.201***	1122.5***	1283.1***	1.003***
	[0.0303]	[331.5]	[277.9]	[0.00362]
Monthly	0.177***	1274.2***	1179.4***	1.002***
	[0.0288]	[307.4]	[271.1]	[0.00325]
Small Payment	0.137***	731.5*	552.9*	1.000***
	[0.0383]	[386.2]	[335.0]	[0.00479]
p-*value for Base Case vs*				
Daily	0.708	0.348	0.726	0.359
Monthly	0.185	0.652	0.913	0.185
Threshold	0.356	0.211	0.610	<0.001
Small Payment	0.038	0.057	0.028	0.182
Monitoring mean	0.294	6,774	7,986	0
# Individuals	2,559	2,559	2,557	2,394
# Observations	205,732	205,732	180,018	99,406

Notes: This table shows the treatment effect of incentives on walking (relative to Monitoring). Incentive groups are pooled in Panel A and considered separately in Panel B. The columns show estimates of coefficients from equation (4) (Panel A) and (5) (Panel B) using intervention-period pedometer data at the individual-day level. "Exceeded step target" is an indicator for whether the individual exceeded their step target and "Earned payment when target met" is an indicator for receiving payment on a given day, conditional on meeting the step target. All regressions control for gender, an indicator for being in the cross-randomized text message group, average daily steps in the phase-in period (before randomization), second order polynomials of age, weight, and height, and year-month and day-of-week fixed effects. Online Appendix Table F.6 shows robustness to excluding controls, adding stratum fixed effects, or selecting controls by double-LASSO. The sample includes the incentive and monitoring groups. The omitted category is Monitoring. Threshold pools the 4- and 5-day threshold groups. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

effect does not simply reflect participants shifting steps from one day to another: column 2 of Table 2 shows that incentives increase walking by 1,266 steps per day, roughly a 20 percent increase that is equivalent to approximately 13 minutes of extra brisk walking each day. This treatment effect is at the high end of effect sizes for pedometer incentives (found in nondiabetic populations in developed countries), which range from only 1.5 steps in Bachireddy et al. (2019) to 1,050 steps in Finkelstein et al. (2016).

The estimated effects of incentives on exercise are robust to accounting for missing data from failure to wear pedometers. Column 3 of Table 2 reports impacts on daily steps treating days with no steps recorded as missing (which gives an unbiased estimate if participants randomly choose not to wear pedometers), and Online Appendix Table F.3 reports Lee bounds which account for the non-random patterns of missing data, with similar results.

5.2 Time-bundled Threshold Contracts Increase Average Effectiveness

We begin our analysis of time-bundled thresholds by comparing the average performance of the threshold and the (linear) base case contracts. Prediction 2 suggests that when the effort discount rate is high, as it appears to be in our sample (Appendix C.2), time-bundled threshold contracts tend to be more effective overall than linear contracts. In order to establish that the thresholds are effective on average, we can show that they result in weakly more compliance and weakly higher cost-effectiveness than the base case contract in the full sample, with one inequality strict, as described in Section 2. We thus examine compliance and cost-effectiveness in turn.

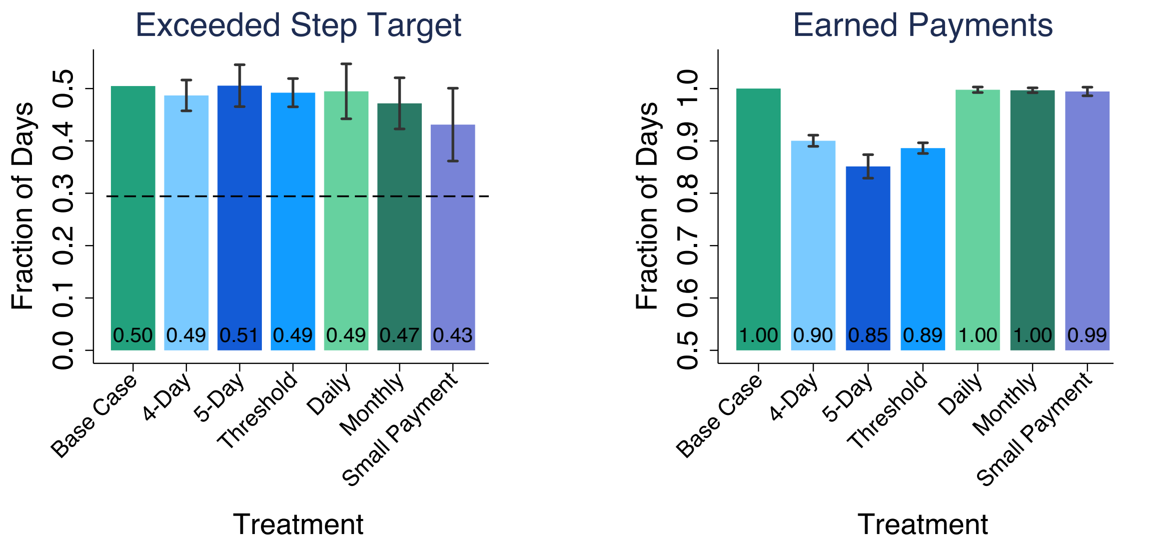
Compliance We find that adding a time-bundled threshold does not change average compliance relative to the Base Case. Specifically, to test for differences across the incentive treatment groups, we estimate regressions of the following form:

$$y_{it} = \alpha + \sum_j \beta_j \times \left(incentives^j\right)_i + \boldsymbol{X}_i'\gamma + \boldsymbol{X}_{it}'\theta + \varepsilon_{it}, \tag{5}$$

where y_{it} are daily walking outcomes and $\left(incentives^j\right)_i$ is an indicator for whether individual i is enrolled in incentive treatment group $j \in$ (Daily, Base Case, Monthly, Threshold, Small Payment). The β_j coefficients capture the average effect of each incentive treatment group relative to Monitoring. Panel B of Table 2 displays the results.

The effect of the threshold contract on compliance is very similar to the effect of the base case contract, with the estimates within 1.3 pp of each other and the difference not statistically significant (p-value=0.356). Figure 2(a) displays the result graphically. It also shows the 4- and 5-Day Threshold separately—neither has meaningfully different compliance than Base Case.

Cost-Effectiveness and Overall Effectiveness While compliance is similar, the threshold contracts are more cost-effective than the base case contract. Individuals in the threshold groups only receive payment for exceeding the step target if they do so on at least four or five days in a given week; when they comply on fewer days, they are not rewarded. As shown in Figure 2(b), we find that the 4-day and 5-day threshold groups are paid on only 90% and 85% of the days they achieve the step target, respectively, as opposed to the 100% of days that the base case group (by definition) receives payment. This difference, which is



(a) Probability Exceeded Target

(b) Earned Payment When Step Target Met

Figure 2: Thresholds Do Not Affect Average Walking But Increase Cost-Effectiveness

Notes: The figure compares the base case treatment with all other incentive treatments during the intervention period. Panel (a) shows the average probability of exceeding the daily step target, with the dashed line representing the Monitoring mean. Panel (b) shows the fraction of days on which the participants received payments, conditional on meeting the step target (the Monitoring mean here is 0). In both Panels, the confidence intervals represent tests of equality between Base Case and each treatment group using the same controls as in Table 2. Data are at the individual-day level. Threshold pools the 4- and 5-day threshold groups.

mathematically equivalent to a difference in cost-effectiveness since the groups receive the same daily payment rate, is significant at the 1% level, as shown in Table 2 column 4.[36] As a result, the cost-effectiveness of the threshold contracts are 11% and 17% higher than that of the base case contract (Table A.3).[37]

Because the threshold contracts have the same compliance and are more cost-effective than Base Case, they are more effective overall. For comparison, the small payment treatment is also more cost-effective than Base Case (it pays half as much per day complied), but this comes at the cost of reduced compliance, as shown in Panel B of Table 2. The fact that the threshold contracts achieve the same compliance as the base case contract for lower cost implies that a budget-neutral threshold (i.e., a threshold contract with the same average cost

[36]For all groups but Small Payment, we can test for equal cost-effectiveness by testing for equality in the Figure 2(b) outcome of *fraction of days on which earned payment when step target met*. To see the equivalence, express cost-effectiveness as

$$\frac{C}{P} = \frac{C}{C \times \textit{daily payment rate} \times \textit{fraction of days on which earned payment when step target met}}$$

and note that the C cancels out, and all but Small Payment have the same *daily payment rate* of 20 INR.

[37]While our contracts do not explicitly target total steps, the relative cost-effectiveness of thresholds for generating steps is of policy interest. Our estimates suggest that thresholds are slightly more cost-effective than the Base Case in the step domain as well (1.24 INR/1000 steps vs 1.10 INR/1000 steps).

as Base Case) would have higher compliance than Base Case.

Distributional Impacts and Effectiveness in Other Settings Equal compliance and higher cost-effectiveness only necessarily imply higher effectiveness if the benefits of compliance are linear. While the estimated health benefits of compliance are approximately linear in our setting (Warburton et al., 2006; Banach et al., 2023), many settings have nonlinear benefits. In those settings, effectiveness depends not just on average compliance but also on its distribution and variance.

Theory suggests that thresholds could increase the variance of compliance by decreasing intermediate effort just below the threshold (Grant and Green, 2013). This could decrease the effectiveness of thresholds for principals who particularly value compliance improvements among those with low average compliance (i.e., principals with concave benefits to compliance). Appendix D assesses the effect of thresholds on the distribution of compliance (e.g., Figure D.3). While the thresholds have some impact, the magnitude of the impact is small, implying that thresholds would be preferred even by many principals with concave benefit functions, provided their benefit functions are not too concave.

5.3 Mechanisms: Effort Impatience Contributes to Threshold Effectiveness

Our theory indicates that high discount rates over future effort may be an important contributor to the effectiveness of threshold contracts. This section presents empirical evidence supporting this theoretical prediction, as we show that the threshold is more effective for more impatient individuals. Specifically, relative to the base case contract, the threshold contract generates significantly more compliance from more impatient individuals without any loss in cost-effectiveness. Since Predictions 1 and 2 regard heterogeneity in the threshold effect *holding all else constant*, this heterogeneity analysis is a direct test of the theory only if impatience is not correlated with other variables that influence the effectiveness of the threshold. We show that the estimated heterogeneity is robust to controlling for many covariates interacted with Threshold, suggesting that this condition holds.

Compliance We use a regression of the following form to test for heterogeneity in the effect of the time-bundled threshold on compliance by impatience:

$$y_{it} = \alpha + \beta_1 Impatience_i \times Thresh_i + \beta_2 Thresh_i + \beta_3 Impatience_i + \boldsymbol{X}_i'\pi + \boldsymbol{X}_{it}'\theta + \varepsilon_{it}, \quad (6)$$

where y_{it} is an indicator for whether individual i exceeded the 10,000-step target on day t and $Thresh_i$ is an indicator for being in a threshold group. Measures of individual impatience are denoted by $Impatience_i$. For the predicted impatience measure, which is estimated, we use a bootstrap procedure to construct 95% confidence intervals.

We restrict the sample to the base case and threshold groups, so the only difference between groups is whether their contract has a time-bundled threshold. The key coefficient of interest is β_1, which captures how the effect of Threshold (relative to Base Case) varies with impatience. Our prediction is that $\beta_1 > 0$.

Table 3: Time-Bundled Thresholds Increase Compliance More for the Effort-Impatient

Dependent variable:	Exceeded step target (×100)					
	(1)	(2)	(3)	(4)	(5)	(6)
Impatience × Threshold	3.80**	6.50*	3.12**	5.94**	6.06**	4.70*
	[0.07, 7.53]	[-0.90,13.89]	[0.54, 5.93]	[1.21, 13.10]	[0.18, 11.94]	[-0.84, 10.25]
Threshold	-1.30	-4.13	-1.18	-3.41**	-4.29**	-3.78*
	[-4.99, 2.38]	[-9.30,1.03]	[-3.98, 1.73]	[-7.14, -0.20]	[-8.42, -0.15]	[-7.73, 0.18]
Impatience	-2.97**	-5.03*	-2.38**	-5.3***	-2.37	-2.67
	[-5.46, -0.47]	[-10.46,0.39]	[-4.51, -0.55]	[-10.84, -2.00]	[-6.65, 1.91]	[-6.72, 1.38]
Impatience measure:	Impatience index	Above-median impatience index	Predicted impatience index	Above-median predicted index	Chose commitment	Simple CTB
Sample:	Late	Late	Full	Full	Full	Full
Base Case mean	50.4	50.4	50.2	50.2	49.9	50.2
# Individuals	1,075	1,075	1,969	1,969	1,798	1,967
# Observations	86,215	86,215	157,946	157,946	144,099	157,799

Notes: This table shows heterogeneity by impatience over effort in the effect of the threshold contracts relative to the linear Base Case. The sample includes the base case and threshold groups only. The impatience measure changes across columns; its units in columns 1 and 3 are standard deviations. "Chose commitment" is the average of indicators for preferring the 4- and 5-day threshold to the base case contract. "Simple CTB" is an average of two indicators for impatient walking choices. See Online Appendix Table F.2 for robustness to different ways of combining the Chose commitment and Simple CTB choices. Column 5 has fewer observations because the questions used to construct the "Chose commitment" measure had a "no preference" response option which we treat as missing. Online Appendix Table F.8 shows that the results are similar when we instead assume "no preference" responses indicate a preference for either option. The "Late" sample includes only participants who were enrolled after we started measuring the impatience index; the Full sample includes everyone. Threshold pools the 4- and 5-day threshold groups. See Online Appendix Table F.9 Panel B for results with the threshold groups disaggregated. Data are at the individual-day level. 95% confidence intervals are shown in brackets. For columns 1–2 and 5–6, confidence intervals are based on standard errors clustered at the individual level. For columns 3–4, which use the predicted impatience index, confidence intervals are constructed using bootstrap, with bootstrap draws clustered at the individual level. Within each bootstrap sample, we conduct three steps: 1) run the LASSO prediction model, 2) create the predicted impatience index using that sample's LASSO coefficients, thus accounting for the error in constructing the index itself, and 3) estimate equation (6). Controls are the same as in Table 2. Significance levels: * 10%, ** 5%, *** 1%.

Table 3 shows that, consistent with the theory, relative to the Base Case, thresholds generate meaningfully more compliance among those with higher impatience over effort. Column 1 uses the impatience index as the measure of impatience. Having a one standard deviation higher value of the impatience index increases compliance in Threshold relative to Base Case by 4 pp (statistically significant at the 5% level). Column 2 uses a dummy for having an above-median value of the impatience index. While this estimate leverages less of the variation available in the data and hence has lower power, it is easier to interpret. Relative to Base Case, Threshold generates 6.5 pp higher compliance for those with above-median impatience than those with below-median (p-value<0.10). This represents a large increase, equal to over 30% of the sample-average effect of either contract (20 pp). Recall that we only have the impatience index for the sample enrolled later in the experiment; to verify the results in the full sample, columns 3 and 4 use the predicted impatience index, which is available for the full sample. We find very similar (and more precise) results, with p-values <0.05 and <0.05 in columns 3 and 4, respectively.

The point estimates in columns 2 and 4 imply that, relative to the linear contract, the threshold contract increases compliance among the more impatient (by 2–3 pp), while decreasing it among the less patient (by 3–4 pp).

Columns 5 and 6 of Table 3 show that these heterogeneity results are robust to using our alternative measures of impatience: incentivized demand for commitment, and the simple CTB measure (p-values <0.05 and <0.10, respectively). Although the impatience index is our preferred measure, given our *ex ante* intention that it be primary, we find the robustness across multiple types of measures reassuring. The consistently large magnitudes of heterogeneity are also notable, especially given the noise in measurement of impatience (Kremer et al., 2019), which could bias our heterogeneity results toward zero.

The heterogeneity based on the demand for commitment measure suggests that the threshold contracts work well among sophisticated impatient people in particular. To shed light on whether the thresholds also work well for impatient naifs, we re-estimate equation (6) but exclude from the sample the participants who demanded commitment. Since this restriction should exclude most sophisticated impatient people for whom the threshold will increase compliance, the $Impatience \times Threshold$ coefficient should be primarily identified by naifs.

The results, shown in Table A.4, suggest that threshold contracts are also relatively more effective among naifs. The $Impatience \times Threshold$ remains positive and relatively large across all impatience measures, although significance is lower than in the full sample due to the smaller samples (except for the Simple CTB measure). Thus, consistent with our theory, both impatient naifs and impatient sophisticates appear to have higher compliance with threshold contracts, and it is only patient people who appear to do more poorly in the threshold groups (as evidenced by the negative main effect of Threshold in Table A.4).

Cost-Effectiveness and Effectiveness Prediction 1 suggested that, in addition to increasing compliance more among people who are impatient over effort, threshold contracts should also increase *effectiveness* more. Since we have already established the compliance result, to demonstrate the effectiveness result it is sufficient to show that, relative to Base Case, the threshold contracts do not decrease cost-effectiveness more among the impatient than the patient. Table A.5 shows that this is true.[38] Paired with the compliance result, this implies that the threshold increases effectiveness more for those with higher impatience over effort than lower impatience over effort.

In addition to caring about the threshold's relative effects among more and less impatient people, a policymaker may also care whether the threshold has higher effectiveness than the linear contract for each group in our specific context. For those with above-median impatience, the threshold increases both compliance and cost-effectiveness and is thus more effective overall than the linear contract. This important finding is consistent with Prediction 2 and implies that principals could increase effectiveness by using thresholds for impatient populations. For those with below-median impatience, the answer is more ambiguous. Relative to Base Case, Threshold decreases compliance but increases cost-effectiveness. Whether a principal would prefer it for this population thus depends on the principal's specific value of compliance (λ from Section 2).

Robustness of the Compliance Heterogeneity by Impatience Impatience over effort is correlated with other factors, such as baseline exercise levels, that may also independently influence the performance of thresholds. For example, if impatient people are more likely to also have counterfactual walking that is right below the threshold level (as opposed to above or far below), that could independently cause them to respond more to the threshold. To shed light on whether this type of factor plays a role in the heterogeneity we see, Figure 3 examines the robustness of the Table 3 estimates to controlling for other baseline covariates and their interactions with Threshold, such as the mean of baseline steps (a proxy for the mean of the walking cost distribution), the standard deviation of baseline steps (a proxy for the variance of the walking cost distribution), and fixed effects for the number of days the individual walked at least 10,000 steps in the baseline period (a proxy for how close to the threshold the person's counterfactual walking is). We also add controls for risk aversion, "scheduling uncertainty" (the stated frequency with which unexpected events arise), proxies for impatience over payment, and other characteristics that could influence the performance of threshold contracts (and all of their interactions with Threshold).

The coefficients on the interaction of impatience and Threshold remain stable as we add these additional controls. Panels A and B of Figure 3 show this for the actual and predicted impatience index, respectively, and Online Appendix Figure F.1 shows this for our other impatience measures. The coefficient stability suggests that it is impatience itself (and not its correlates) driving the estimated relationships.

[38] We do see that the threshold is slightly less cost-effective when the impatience measure is demand for commitment. While the significance of this coefficient at the 10% level (out of six specifications) may be due to chance, demand for commitment (which is equivalent to a preference for the threshold contract over the base case) requires both (sophisticated) impatience and a perception that the participant will regularly receive payment under the threshold contract (e.g., because of low walking costs). This second factor may underlie the heterogeneity in cost-effectiveness.

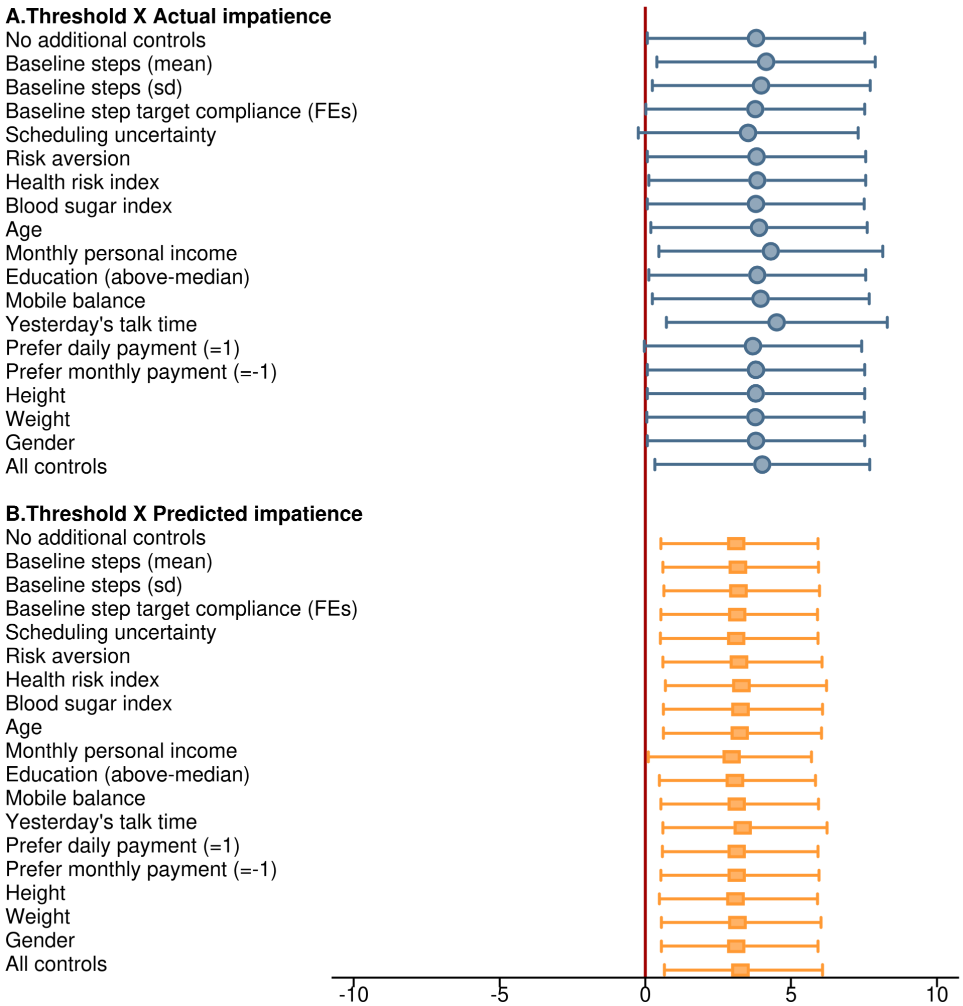


Figure 3: Threshold Heterogeneity by Impatience is Robust to a Variety of Controls

Notes: Panel A displays robustness of the $Threshold \times Impatience$ coefficient from column 1 of Table 3 (actual impatience index) to including various additional controls, interacted with $Threshold$, in the regression. As a reference, the "No additional controls" row just displays the $Threshold \times Impatience$ coefficient and 95% confidence interval from column 1 of Table 3. The next 17 rows show estimates of the $Threshold \times Impatience$ coefficient with two additional controls: the main effect of the covariate listed and the covariate interacted with $Threshold$. The final "All controls" row shows estimates of the $Threshold \times Impatience$ coefficient from a regression where we control simultaneously for all covariates included in the previous 17 rows (both main effects and interactions with $Threshold$). Panel B is analogous but based on column 3 of Table 3 (predicted impatience index). Baseline steps (mean) and baseline steps (sd) represent the mean and standard deviation of the baseline steps distribution. Baseline step target compliance (FEs) are fixed effects for the number of days the individual walked at least 10,000 steps in the baseline period. Scheduling uncertainty represents the individual's stated frequency of facing unexpected events that would prevent them from walking for 30 minutes in a given day. Risk aversion is an incentivized measure from a multiple price list. The health risk index is an index created by taking the average of endline HbA1c, RBS, mean arterial BP, waist circumference, and BMI, standardized by the control group mean and standard deviation. The blood sugar index is constructed by taking the mean of endline HbA1c and RBS standardized by their average and standard deviation in the control group. Mobile balance and Yesterday's talk time are as in Table 1. Data are at the respondent-day level and include the threshold and base case groups only. Confidence intervals in Panel A are based on standard errors clustered at the individual level. Confidence intervals in Panel B are constructed using bootstrap, with bootstrap draws clustered at the individual level; see the notes to Table 3 for a description of the bootstrap procedure. See Online Appendix Figure F.1 for a version with our alternate impatience measures.

Even if omitted variables were affecting the heterogeneity estimates in Table 3, the estimates are still relevant for policy. Policymakers want to customize contract thresholds based on how their efficacy varies with observed participant impatience, irrespective of whether it is impatience itself (as opposed to the correlates of impatience) that generates the heterogeneity.

5.3.1 Policy Implications of Time-Bundled Thresholds Results

We find that, in the full sample, time-bundled thresholds increase effectiveness by increasing cost-effectiveness without decreasing compliance. Moreover, consistent with theory, we provide evidence that one of the mechanisms for the effectiveness of thresholds is impatience over future effort. Specifically, we show that time-bundled thresholds generate meaningfully greater compliance and effectiveness among the impatient than the patient.

Our findings suggest that time-bundled thresholds are a useful policy tool for adapting incentives to address impatience over effort. Policymakers could tailor time-bundled thresholds at the population level, using them for groups known for greater impatience, such as younger people (Read and Read, 2004).

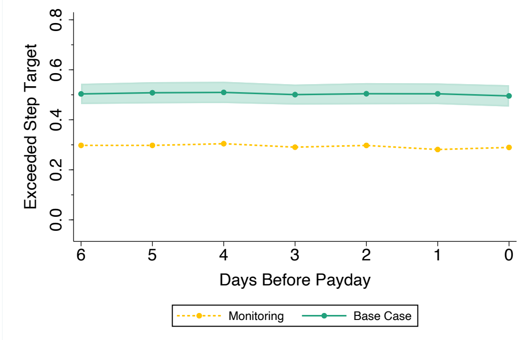
Alternatively, policymakers could personalize the assignment of time-bundled thresholds within a population. One approach would be to personalize contracts by measuring impatience, which Andreoni et al. (2023) finds is feasible.[39] Although participants might misreport their discount rates, evidence suggests that misreporting to avoid assignment to dominated contracts is limited in the context of incentives for behavior change.[40] Another approach would be to personalize on predictors of impatience that are harder to manipulate. Appendix E demonstrates that a prediction of impatience based on such characteristics (e.g., gender and BMI) predicts heterogeneity in the Threshold effect.

5.4 Payment Frequency Does Not Meaningfully Change Effectiveness

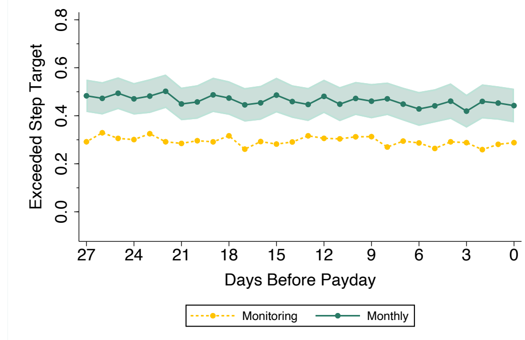
We now explore the roles of payment frequency and the discount rate over financial payments in incentive design. To do so, we compare average compliance in the daily, base case (weekly), and monthly groups. Columns 1–3 of Panel B of Table 2 and Figure 2(a) show that the three payment frequency treatments have similar effects on walking; compliance and steps walked are statistically indistinguishable across the three treatments. The point estimates also do not increase monotonically with frequency, as would be expected if differences reflected discounting instead of statistical noise. The lack of between-treatment frequency effects implies that the discount rate over our financial payments is small. However, our precision here is somewhat low. To gain precision, we also examine how compliance changes

[39] Andreoni et al. (2023) customized the parameters of a contract for 2-day vaccination drives to equalize worker effort across both days, using discount rates measured in a simple effort allocation experiment. They succeeded: customized contracts resulted in more equal effort than randomized contracts.

[40] Dizon-Ross and Zucker (2025) explores an incentive program for steps in the same setting as this study. While they do not implement any time-bundled contracts, they find that participants do not manipulate their observable characteristics to avoid assignment to a financially dominated contract with a higher step target but the same payment.



(a) Weekly Payment Cycle



(b) Monthly Payment Cycle

Figure 4: The Probability of Exceeding the Step Target Is Stable Over the Payment Cycle

Notes: The figures show the probability of exceeding the daily 10,000-step target among individuals receiving the base case incentive (Panel (a)) and a monthly incentive (Panel (b)) relative to the monitoring group, according to days remaining until payday. Effects control for payday day-of-week fixed effects, day-of-week fixed effects, day-of-week relative to survey day-of-week fixed effects, and the same controls as in Table 2. The shaded area represents a collection of confidence intervals from tests of equality within each daily period between the incentive and monitoring groups from regressions with the same controls as in Table 2. Panel (a) includes the monitoring and base case groups; Panel (b) includes the monitoring and monthly groups. Data are at the individual-day level.

as the payday approaches in the base case and monthly groups. If people are impatient over payments, compliance should increase as the payday approaches (as shown in both Kaur et al. 2015 and Prediction 4 in Appendix B.6). Yet, Figure 4 shows that walking behavior is remarkably steady across the payment cycle. The estimates here are more precise, allowing us to rule out even small effects of decreasing the lag until payment on compliance.[41]

The limited effect of increased payment frequency theoretically hinges on the discount factor over our contract payments, which Appendix C.2 shows are close to 1. While this estimate is specific to our sample and payment modality, limited impatience over payments is not rare (e.g., Augenblick et al., 2015; Andreoni and Sprenger, 2012a). Thus, we expect that lackluster payment frequency effects may be common. Indeed, DellaVigna and Pope (2018) also finds limited impact of randomizing the payment lag among US participants on mTurk. Note that the effects of payment frequency are relevant for considering time-bundled contracts as well: if higher-frequency payments were more effective, it could present challenges for time-bundled contracts, which require a delay between effort and payment.

[41]Specifically, Online Appendix Table F.10 shows estimates of the change in compliance as the payment date approaches within the base case and monthly groups, conditional on day-of-week fixed effects. The estimates are precise and near zero, allowing us to rule out even small effects of more immediate payment. For example, if we assume linearity of compliance in lag to payment, then the confidence interval around the slope in the base case treatment rules out the possibility that, because of monetary discounting, daily payments would generate a mere 0.3 pp more compliance than Base Case.

6 Empirical Results: Program Evaluation

The impacts of incentive programs on health and behavior are of policy interest, especially among populations at high risk of complications from chronic disease. This section examines the exercise impacts over time and provides evidence that the program improved health.

6.1 The Impacts of Incentives Persist During and After the Intervention Period

Chronic disease management requires lasting lifestyle changes, underscoring the need for programs that yield sustained improvements in exercise. We show the treatment effects of our incentives intervention on exercise over time, first during the intervention period and then after the intervention ends. Figure A.2 estimates equation (4) separately by week of the intervention for walking outcomes. After an initial spike at week 1, the effect of incentives on walking remains stable for the full intervention period. This suggests that longer-term (and even permanent) programs have the potential to promote sustained exercise improvements, an encouraging finding as insurers and governments are increasingly rolling out such programs.

Do the effects of incentives persist after the payments stop? Studies of similar exercise programs find mixed results (e.g., Royer et al., 2015; Charness and Gneezy, 2009). To examine persistence, we estimate equation (4) using the pedometer data from the 12 weeks after the intervention ended.[42]

Table A.6 shows that the incentive group continues to walk significantly more then the comparison groups after incentives end. The treatment effect on steps is statistically significant and large: around 10% of the comparison group mean (columns 2 and 3), or roughly 60% of the size of the treatment effect of incentives on steps during the intervention period (which was 15% of the comparison group mean).[43] Figure A.3 shows that effects persist until the end of the 12-week post-intervention period. Our short-run incentive program may thus induce habit formation, resulting in long-term impacts.

6.2 Incentives Moderately Improve Health

We now examine whether the incentives program measurably improves health. We powered our RCT to detect the difference in health outcomes (which are relatively noisy) between the pooled incentive groups and Control. (While we did not power it to compare Incentives with Monitoring, we include this comparison alongside our comparison with Control for completeness.) Table 4 reports results from regressions of the following form:

$$y_i = \alpha + \beta_1 Incentives_i + \beta_2 Monitoring_i + \boldsymbol{X}_i' \gamma + \varepsilon_i, \tag{7}$$

[42] While we have pedometer data from Control during this period, sample size is limited: we collected post-intervention period data from only a third of participants. We thus pool Control and Monitoring, so the *Incentives* coefficient represents the effect of incentives relative to this pooled comparison group. Results are similar when we compare Incentives with Control alone; with only 70 people post-intervention, Monitoring is too small to analyze alone.

[43] Since we compare the effect of Incentives relative to Control in the post-intervention period with the effect of Incentives relative to *Monitoring* in the intervention period, we will overestimate persistence if Monitoring alone increases steps. However, Online Appendix J suggests that monitoring does not affect steps.

where y_i is an endline health outcome for individual i and $\boldsymbol{X}_i$ is a vector of controls (shown in the table notes). β_1 represents the overall effect of the incentive program.

The results suggest that the program moderately improves blood sugar and cardiovascular health. Column 1 presents the treatment effect on our preferred blood sugar measure, the index incorporating both HbA1c and RBS. Incentives improve the index by 0.05 standard deviations, significant at the 10% level. Columns 2 and 3 display HbA1c and RBS separately. Column 4 shows that incentives improve the health risk index by 0.05 standard deviations, significant at the 10% level.

Table 4: Incentives Moderately Improve Blood Sugar and Cardiovascular Health

Dependent variable:	Blood sugar index	HbA1c	Random blood sugar	Health risk index	Blood sugar index	HbA1c	Random blood sugar	Health risk index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentives	-0.05*	-0.07	-6.1*	-0.05*	-0.10**	-0.1	-12.6**	-0.08**
	[0.03]	[0.07]	[3.5]	[0.03]	[0.05]	[0.1]	[5.9]	[0.04]
Monitoring	-0.02	-0.1	1.8	0.02	-0.06	-0.3	1.5	-0.05
	[0.05]	[0.1]	[6.6]	[0.04]	[0.08]	[0.2]	[10.5]	[0.07]
p-value: I = M	0.491	0.534	0.188	0.119	0.574	0.294	0.133	0.556
Sample	Full	Full	Full	Full	Above-median blood sugar	Above-median blood sugar	Above-median blood sugar	Above-median blood sugar
Control mean	0	8.44	193.83	0	.64	10.09	248.26	.45
# Individuals	3,067	3,066	3,067	3,068	1,530	1,529	1,530	1,531

Notes: p-value I = M is Incentives = Monitoring. Columns 1–4 report results estimated in the full sample while columns 5–8 report results estimated in the sample with above-median blood sugar index. Observations are at the individual-level. HbA1c is the average plasma glucose concentration (%). Random blood sugar (RBS) is the blood glucose level (mg/dL). The blood sugar index is constructed by taking the mean of endline HbA1c and RBS standardized by their average and standard deviation in the control group. The health risk index is an index created by taking the average of endline HbA1c, RBS, mean arterial BP, waist circumference, and BMI, standardized by the control group mean and standard deviation. See Online Appendix Table F.11 for treatment effects on the index components not shown here. We follow World Health Organization guidelines to trim biologically implausible physical health outcomes and index components (i.e., z-scores < -4 or > 4). All specifications control for the baseline value of the dependent variable (or index components for indices), the baseline value of the dependent variable squared (or index components squared for indices), a dummy for the SMS treatment, and the following controls: age, weight, height, gender, and their second-order polynomials, as well as endline completion date, hour of endline completion, and dummy for late completion. Online Appendix Table F.12 shows that the estimates are similar but less precise when we omit the control variables, add stratum fixed effects, or use controls selected by double-LASSO. Robust standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Since health outcomes among those with more severe diabetes are likely to be more responsive to exercise, we separately assess health impacts among those with higher blood sugar in columns 5–8 of Table 4.[44] As expected, the estimated health improvements are larger

[44]While this subsample comparison was not formally pre-registered, our registry mentions that we stratified randomization by baseline HbA1c, a step we took to maximize statistical power for this subsample comparison. Our Analysis Plan, discussed in footnote 12, also outlines this analysis (see the first bullet of Hypothesis 1).

among those with above-median blood sugar. Incentives decrease the blood sugar index by 0.10 standard deviations and decrease RBS by 13 mg/DL, both significant at the 5% level.

Online Appendix Table F.13 examines whether the intervention had coincident impacts on mental health or fitness. Incentives improve the mental health index by 0.10 SD. We find no effects on physical fitness, however, perhaps because we measure higher-intensity fitness while our intervention motivated lower-intensity exercise. Finally, we do not find impacts on diet or addictive good consumption (Online Appendix Table F.14).

6.3 Incentives and Chronic Disease: Results Summary and Discussion

Overall, these results show that incentives for exercise are a scalable, effective intervention to decrease the burden of diabetes in resource-poor settings. Exercise has important long-run health benefits for diabetics (e.g., Qiu et al., 2014), and our incentives substantially increase exercise during and after the intervention.

We also find clinically meaningful treatment effects on blood sugar: the estimated program impact of lowering RBS by 6 mg/dl would bring someone near the diabetes threshold a quarter of the way to healthy blood sugar levels.[45] In addition, an exploratory analysis shows that the treatment effects on RBS grow over the intervention period (Figure A.4), and hence might continue to amplify after the program given that exercise habits persist.

Our findings contribute to the health literature by providing the first experimental evidence of the impact of a pedometer-based intervention on blood sugar control. This is particularly significant given the scalability of such interventions in resource-poor settings. Previous interventions shown to improve health outcomes among diabetics require highly trained staff for frequent, personalized interactions (Aziz et al., 2015; Qiu et al., 2014).[46] In contrast, our intervention is scalable, low-cost, and induces lasting behavior change, with the potential to generate health savings that exceed program costs.[47] As a result, programs like ours could be essential tools in mitigating the global impacts of chronic disease.

7 Conclusion

This paper provide new insights into how to adjust incentives for impatience. We show both theoretically and empirically that, relative to time-separable contracts, the performance of time-bundled contracts is significantly higher among participants who are more impatient over effort. One useful feature of this prediction is that it holds regardless of whether agents

[45] For RBS measured in the morning, values less than 100 mg/dl are normal, 100-125 mg/dl indicate prediabetes, while above 126 mg/dl indicate diabetes.

[46] Other incentive interventions for diabetics have targeted non-exercise outcomes and have found limited success and face similar scalability concerns (e.g., Finkelstein et al., 2017; Desai et al., 2020).

[47] The per-person incentive program cost is 1,700 INR (26 USD), which is only 7% of the estimated annual direct cost of care for a diabetic in Tamil Nadu, or 28% of the direct cost of care during the 3-month intervention period (Tharkar et al., 2010). Interventions generating similar short-run levels of exercise among diabetics in other contexts have produced cost savings of this order of magnitude (Nguyen et al., 2008).

are time-consistent or time-inconsistent, sophisticated or naive, thus broadening the arsenal for motivating impatient individuals. The intuition behind the prediction is that time-bundled contracts enable the principal to purchase future effort from participants instead of current effort, which is advantageous when participants discount their future effort and are willing to effectively sell it "at a discount." The success of time-bundled contracts in adjusting incentives for impatience is particularly striking when compared to the failure of higher-frequency payments in our sample. More frequent payments only work if individuals are impatient over *payment*, which may not be the case even for those with high discount rates over utility. In contrast, time-bundled contracts succeed by leveraging impatience over effort.

We explore time-bundled contracts using an experiment evaluating incentives for behavior change. This is a particularly apt setting for exploring the relationship between incentives and impatience, as a key rationale for incentivizing behavior change (e.g., savings, preventive health behaviors) is to mitigate underinvestment due to present bias and impatience. Adapting these types of incentives for impatience may thus be particularly impactful.

Our particular empirical setting also allows us to make a second contribution: we show that an incentive program for walking increases exercise and health in a diabetic population. In doing so, we provide some of the first evidence of a scalable, low-cost intervention with the potential to decrease the large and growing burden of chronic disease worldwide.

Our insight that impatience increases the value of time-bundling for the principal in principal-agent relationships could have broad applicability. Dynamic incentives are widespread, and we find that high discount rates over effort may be a potential explanation. A common dynamic incentive is a labor contract where an individual could be fired if they do not exert enough effort today, so effort today increases their future payoff to effort. While standard models show one reason such contracts enhance effort is the high stakes of job loss in the presence of imperfect information, our work suggests that these contracts have extra bite if the agent discounts their future effort.

Our empirical findings regarding time-bundling are promising for policy and open up new research directions. One question for future research is how to optimize the specific features of time-bundled contracts such as the payment period length and threshold level. Future research can also probe external validity, exploring whether time-bundled contracts are indeed more effective than time-separable contracts in other populations with high discount rates for effort. Future work could also go further in exploring how to personalize time-bundled contracts at scale at the individual level, evaluating the options we explore in this paper (e.g., targeting via observables). Together, the answers to these questions will allow policymakers to effectively employ time-bundled contracts to motivate impatient people.

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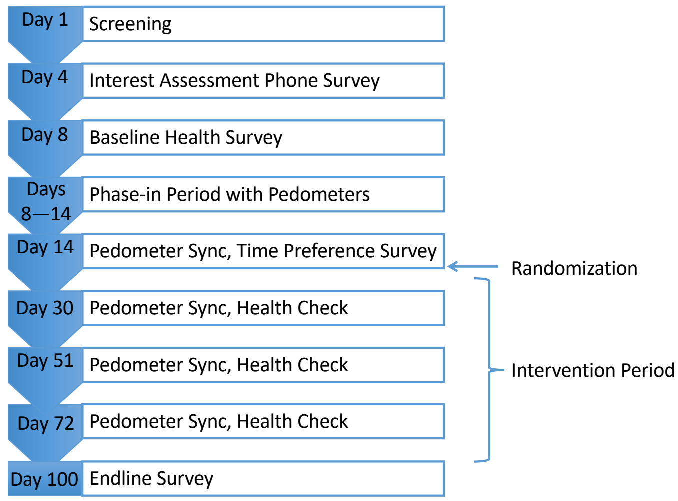
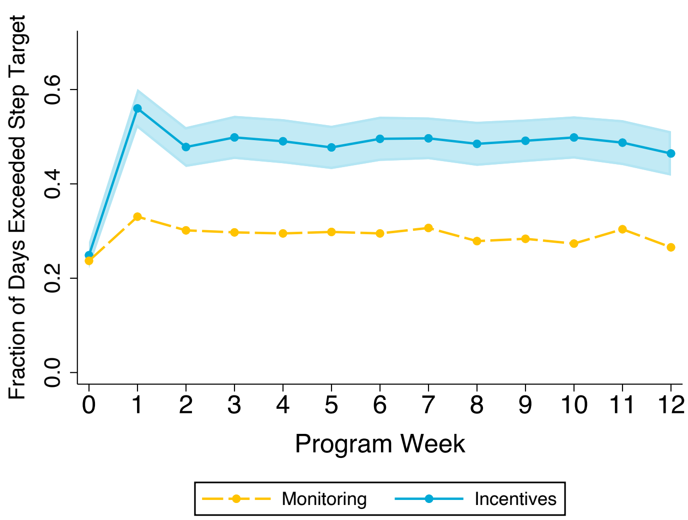
Appendices

This section contains all appendix tables and appendix figures labeled with the prefix "A" (e.g., Table A.1, Figure A.1). It also contains Appendices B - E. The Online Appendix contains Appendices F - K and is available at: faculty.chicagobooth.edu/-/media/faculty/rebecca-dizon-ross/research/incentivedesignapp.pdf

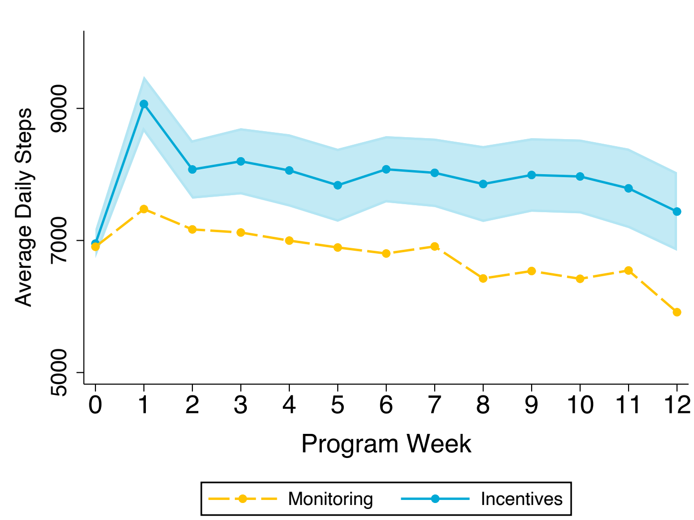


Appendix Figure A.1: Experimental Timeline for Sample Participant

Notes: This figure shows an experimental timeline for a participant. Visits were scheduled according to the participants' availability. We introduced variation in the timing of incentive delivery by delaying the start of the intervention period by one day for randomly selected participants. The intervention period was exactly 12 weeks for all participants.



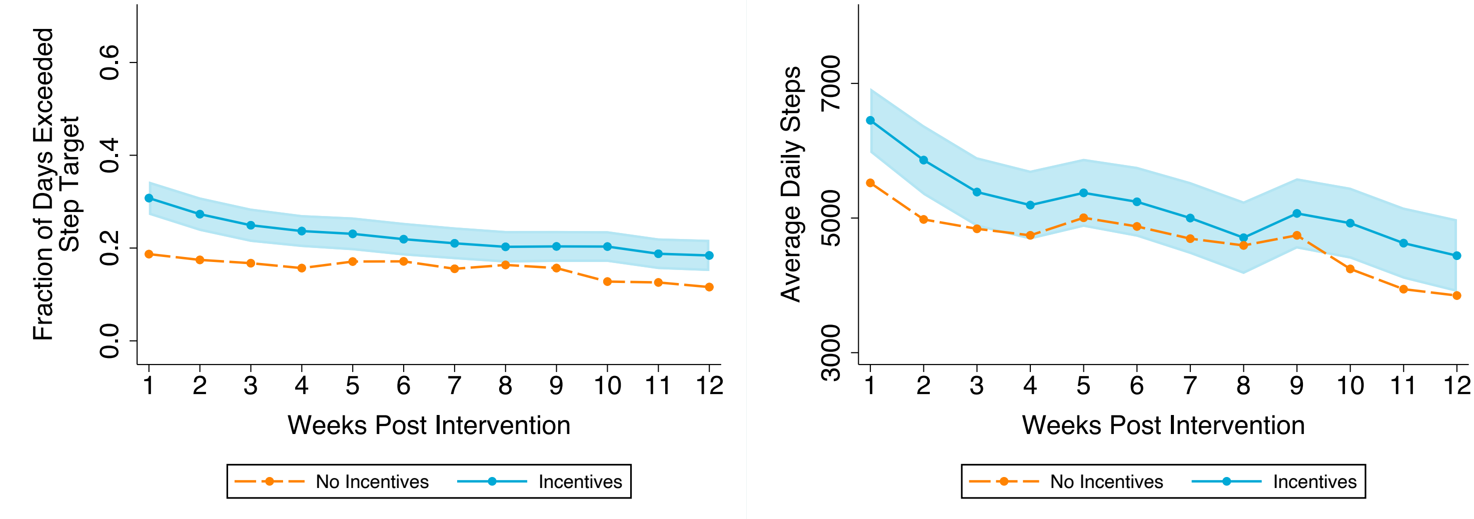
(a) Step-Target Compliance



(b) Daily Steps Walked

Appendix Figure A.2: Incentive Effects are Steady through the 12-Week Program

Notes: Panel (a) shows the average probability of exceeding the step target and Panel (b) shows the average daily steps walked, both during the intervention period. Week 0 is the phase-in period (before randomization). The shaded areas represent a collection of confidence intervals from tests of equality within each weekly period between the incentive and monitoring groups from regressions with the same controls as in Table 2. Data are at the individual-week level. Both graphs are unconditional on wearing the pedometer. Graphs look similar when condition on wearing the pedometer except that, in both groups, there is less downward trend over time.

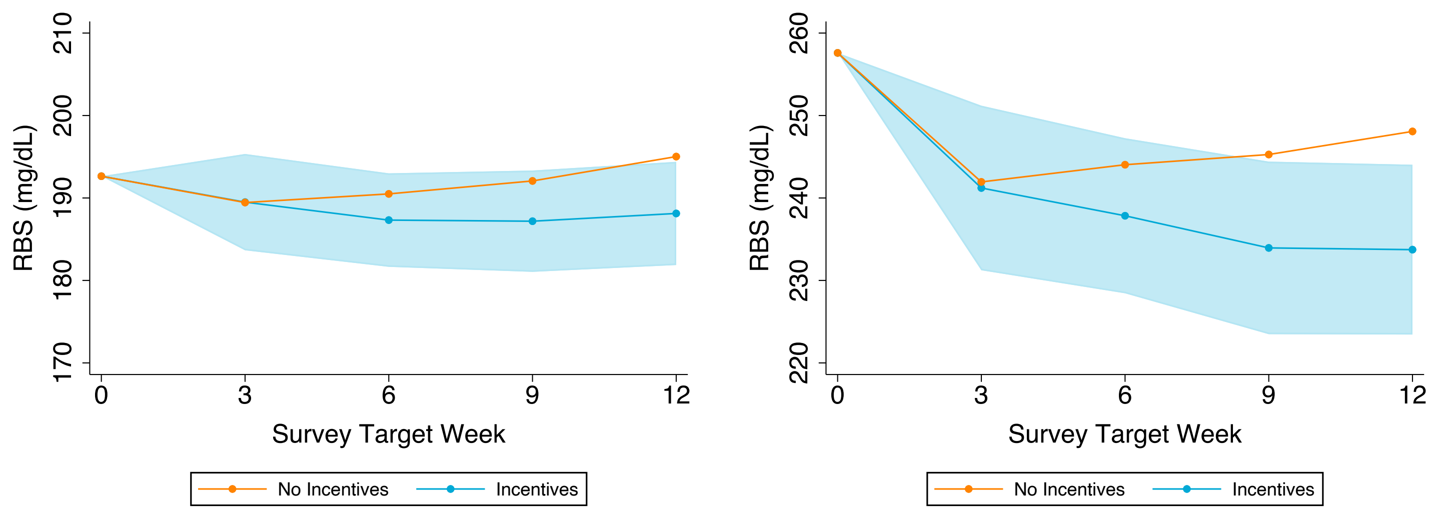


(a) Step-Target Compliance

(b) Daily Steps Walked

Appendix Figure A.3: Incentive Effects Persist After the 12-Week Program

Notes: Panel (a) shows the average probability of exceeding the step target and Panel (b) shows the average daily steps walked, both in the 12 weeks following the intervention. "No incentives" represents the pooled monitoring and control groups; the Panels look very similar when we compare with the control group only. The shaded areas represent a collection of confidence intervals from tests of equality within each weekly period between the incentive and no incentive groups from regressions with the same controls as in Table 2. All graphs are unconditional on wearing the pedometer. Data are at the individual-week level. Graphs look similar when condition on wearing the pedometer except that, in both groups, there is less downward trend over time.



(a) Full Sample

(b) Above-Median Blood Sugar Sample

Appendix Figure A.4: Blood Sugar Treatment Effects Grow Over Time

Notes: Figures show how the impact of incentives on random blood sugar (RBS) evolves over time by presenting the treatment effect of incentives on RBS separately for each time RBS was measured. Panel A shows the full sample and Panel B restricts to those with above-median baseline values of the blood sugar index. Survey week 0 was the baseline survey measurement; survey week 12 was the endline survey measurement; and survey weeks 3, 6, and 9 were the measurements at the pedometer sync visits held every three weeks during the intervention period. Observations are at the individual level. The "No incentives" group represents the pooled monitoring and control groups. As in our other graphs of trends over time, we pool the two comparison groups (control and monitoring) for power. Results are similar but slightly less precise if we compare incentives with control alone. For each survey, we regress random blood sugar on the incentives dummy and control for the same controls as in the random blood sugar specification in Table 4. The shaded areas represent a collection of 95% confidence intervals from those regressions. The p-values for the significance of the increase over time are .05 and .02 for the Panels A and B, respectively.

Appendix Table A.1: Measures of Effort Impatience Correlate with Baseline Exercise, Health, and Behavior

		Correlation with					
		Baseline exercise		Baseline indices			
	Mean	Daily steps	Daily exercise (min)	Negative health risk index	Negative vices index	Healthy diet index	# Individuals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Impatience index measures							
Impatience index	0.092	-0.080***	-0.070***	-0.016	-0.052	-0.181***	1,740
1. I'm always saying: I'll do it tomorrow	2.217	-0.059	-0.101***	-0.010	-0.031	-0.147***	1,740
2. I usually accomplish all the things I plan to do in a day	0.643	-0.054	-0.052	-0.012	-0.043*	-0.149***	1,740
3. I postpone starting on things I dislike to do	3.967	-0.042*	0.004	0.004	-0.052	0.050	1,740
4. I'm on time for appointments	0.468	-0.054	0.006	-0.021	0.008	-0.097***	1,740
5. I often start things at the last minute and find it difficult to complete them on time	2.506	-0.039	-0.069***	-0.009	-0.043*	-0.207***	1,740
B. Predicted index measures							
Predicted index	-0.052	0.000	-0.036	-0.064***	0.021	0.004	3,192
1. In the past week, how many times have you found yourself exercising less than you had planned?	0.526	0.015	-0.006	-0.064***	0.007	0.026	3,192
2. In the past 24 hours, how many times have you found yourself eating foods you had planned to avoid?	0.208	-0.001	0.050***	-0.058***	0.015	0.034*	3,192
3. Do you worry that if you kept a higher balance on your phone, you would spend more on talk time?	0.131	-0.027	-0.062***	-0.018	0.031*	-0.038	3,192
C. Simple CTB							
Simple CTB index	0.532	-0.120***	-0.028	-0.003	-0.018	-0.020	3,190
1. Chose 30 minutes today and 60 minutes in one week	0.508	-0.115***	-0.018	-0.006	-0.020	-0.021	3,190
2. Chose 20 minutes today and 60 minutes in one week	0.555	-0.120***	-0.037	0.000	-0.015	-0.019	3,190
D. Demand for commitment							
Chose commitment index	0.485	0.045	-0.005	-0.027	0.011	0.015	2,871
1. Chose 4-day threshold	0.511	0.027	-0.010	-0.021	0.017	0.017	2,881
2. Chose 5-day threshold	0.461	0.057***	-0.003	-0.030	0.004	0.015	2,889

Notes: This table displays the correlations between impatience measures and baseline behavior and health. Each coefficient represents results from a separate regression. We normalize variables such that a higher impatience measure value corresponds to greater impatience, and a higher health or behavior measure value corresponds to healthier behavior. Panel A shows the impatience index and its five components. Panel B shows the predicted index and its three components. Panel C shows the Simple CTB index and its two components. The Simple CTB index is the average of preferences for option (A) in the following two scenarios: 1. In exchange for 500 INR in 8 days, walk (A) 30 minutes today and 60 minutes in one week, or (B) 60 minutes today and 20 minutes in one week; 2. In exchange for 500 INR in 8 days, walk (A) 20 minutes today and 60 minutes in one week, or (B) 60 minutes today and 20 minutes in one week. Panel D shows the chose commitment index and its two components. The chose commitment index is defined as the average of preferring the Time Bundled contract in each of the following questions: "Which program would you prefer: The Weekly Recharge Program with a condition of 5 days, or the Basic Weekly Recharge Program with no condition?" and "Which program would you prefer: the Program with a minimum weekly condition of 4 days, or the Basic Weekly Program with no condition?"

Daily steps are from the phase-in period pedometer data. Daily exercise is self-reported. The health index is as in Table 4. The vices index includes an individual's daily cigarette, alcohol, and areca nut usage. The healthy diet index includes an individual's daily number of wheat, vegetable, and rice; spoonfuls of sugar; fruit, junk food, and sweets intake; and whether one avoids unhealthy foods. Data are at the individual level and include the full sample. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.2: Missing Pedometer Data During the Intervention Period

Dep. variable:	No Steps data	**Reason** no steps data		**Reason** no data from Fitbit			
		Did not wear Fitbit	No data from Fitbit	Lost data entire period	Immediate withdrawal	Mid-intervention withdrawal	Other reasons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Incentives	-0.0140	-0.0287**	0.0155	-0.00203	0.00571	0.0166**	-0.00471
	[0.0174]	[0.0142]	[0.0124]	[0.00511]	[0.00731]	[0.00694]	[0.00594]
Monitoring mean	.19	.15	.047	.0049	.0099	.012	.02
# Individuals	2,607	2,559	2,607	2,607	2,607	2,607	2,607
# Observations	218,988	205,732	218,988	218,988	218,988	218,988	218,988

Notes: Each observation is an individual × day. The sample includes Incentives and Monitoring. Missing data have two sources: pedometer non-wearing (i.e., steps = 0) (column 2) or failure to retrieve pedometer data (column 3). Columns 2 + 3 = column 1 except column 2 conditions on there not being missing data (for consistency with our main step analyses, results are similar without this restriction), while columns 1 and 3 do not. Columns 4–7 summarize the reasons pedometer data in column 3 were missing. Controls are the same as in Table 2. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.3: Threshold Treatments Increase Cost-Effectiveness Relative to Base Case, With Similar Increases Among Those Who Are More and Less Impatient

		Sample defined by impatience indices			
	Full sample	Below-median (actual)	Above-median (actual)	Below-median (predicted)	Above-median (predicted)
Treatment group	(1)	(2)	(3)	(4)	(5)
Base Case	0.050	0.050	0.050	0.050	0.050
Threshold	0.056	0.056	0.057	0.057	0.056
4-Day Threshold	0.055	0.055	0.056	0.056	0.055
5-Day Threshold	0.059	0.059	0.059	0.059	0.058

Notes: The table displays the cost-effectiveness of different treatment groups (in rows) and different samples (in columns). Cost-effectiveness equals average compliance divided by the average payment per day, in units of days complied per INR. The sample includes Base Case and Threshold (Threshold pools the 4- and 5-day Threshold). We test for differences in cost-effectiveness using a mathematically equivalent test for differences in the fraction of days complied on which participants earned payment, shown in column 4 of Table 2 and Figure 2b.

Appendix Table A.4: Threshold Heterogeneity Results are Similar Among Naive Individuals

Dependent variable:	Exceeded step target (×100)				
	(1)	(2)	(3)	(4)	(5)
Impatience × Threshold	2.99	3.94	2.76	6.8	8.07**
	[-2.56, 8.55]	[-7.09,14.97]	[-1.31, 6.76]	[-1.31, 14.88]	[0.14, 16.01]
Threshold	-5.29*	-6.79*	-3.97*	-6.5**	-8.50***
	[-10.89, 0.32]	[-14.72,1.14]	[-7.98, 0.07]	[-11.14, -1.37]	[-14.19, -2.80]
Impatience	-3.11*	-6.70	-1.56	-5.15*	-4.06
	[-6.69, 0.47]	[-14.84,1.44]	[-4.51, 1.52]	[-11.18, 0.17]	[-9.73, 1.62]
Impatience measure:	Impatience index	Above-median impatience index	Predicted impatience index	Above-median predicted index	Simple CTB
Sample:	Late	Late	Full	Full	Full
Base Case mean	51.7	51.7	50.6	50.6	50.6
# Individuals	496	496	977	977	977
# Observations	39,562	39,562	78,096	78,096	78,096

Notes: This table is the same as Table 3 but limited to the subsample of participants who did not demand commitment (that is they did not prefer both the 4-day and 5-day threshold contract relative to the base case contract). Controls are the same as in Table 2. 95% confidence intervals are shown in brackets. For columns 1, 2, and 5, confidence intervals are based on standard errors clustered at the individual level. For columns 3 and 4, confidence intervals are constructed using bootstrap, with bootstrap draws clustered at the individual level; see the notes to Table 3 for a detailed description of the bootstrap procedure. Data are at the individual × day level. The sample includes Base Case and Threshold. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.5: Thresholds Are Similarly Cost-Effective Among Those with Higher Impatience

Dependent variable:	Earned payment when exceeded target					
	(1)	(2)	(3)	(4)	(5)	(6)
Impatience × Threshold	-0.00625	-0.0109	0.00386	0.0115	0.0202*	-6.96e-05
	[-0.02, 0.01]	[-0.04,0.02]	[-0.01, 0.01]	[-0.01, 0.03]	[-0.00, 0.04]	[-0.02, 0.02]
Threshold	-0.114***	-0.109***	-0.115***	-0.119***	-0.127***	-0.115***
	[-0.13, -0.10]	[-0.13,-0.09]	[-0.13, -0.10]	[-0.13, -0.10]	[-0.14, -0.11]	[-0.13, -0.10]
Impatience	0.00208	0.00544	-0.000834	-0.00275	-0.00233	0.00483*
	[-0.00, 0.01]	[-0.00,0.01]	[-0.00, 0.00]	[-0.01, 0.00]	[-0.01, 0.00]	[-0.00, 0.01]
Impatience measure:	Impatience index	Above-median impatience index	Predicted impatience index	Above-median predicted index	Chose commitment	Simple CTB
Sample:	Late	Late	Full	Full	Full	Full
Base Case mean	1	1	1	1	1	1
# Individuals	1,007	1,007	1,846	1,846	1,681	1,844
# Observations	42,830	42,830	79,248	79,248	71,525	79,150

Notes: This table shows heterogeneity in the impact of Threshold on the fraction of days on which participants received payment, conditional on meeting the step target, by different measures of impatience. A higher level of this outcome indicates lower cost-effectiveness among treatment groups that received the same payment per day (all groups except Small Payment). The impatience measure changes across columns. Controls are the same as in Table 2. 95% confidence intervals are shown in brackets. For columns 1–2 and 5–6, confidence intervals are based on standard errors clustered at the individual level. For columns 3 and 4, confidence intervals are constructed using bootstrap, with bootstrap draws clustered at the individual level; see the notes to Table 3 for a detailed description of the bootstrap procedure. Data are at the individual × day level. The sample includes Base Case and Threshold. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.6: The Effects of Incentives Persist After the Intervention Ends

	Post-intervention		
Dependent variable:	Exceeded step target	Daily steps	Daily steps (if > 0)
	(1)	(2)	(3)
Incentives	0.071***	537.2**	648.3***
	[0.01]	[220.90]	[195.82]
No incentives mean	0.156	4,674	6,773
# Individuals	1,122	1,122	1,122
# Observations	91,756	91,756	62,858

Note: This table shows the average treatment effect of Incentives relative to Control and Monitoring (pooled) during the "post-intervention period" (i.e., the 12 weeks after the intervention ended). Each observation is a person-day. Columns 1 and 2 include all days, and column 3 only includes days where the participant wore the pedometer (i.e., had step count > 0). Controls are the same as in Table 2. The number of individuals differs from the total number recruited for the post-intervention period because roughly 11% of participants withdrew immediately. The likelihood of immediate withdrawal is not significantly different between the incentive and comparison groups. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

B Theoretical Predictions Appendix

We begin by presenting the formal model setup and assumptions in Section B.1. In Sections B.2 and B.3, we describe behavior under time-separable linear contracts and time-bundled contracts, respectively. Sections B.4 and B.5 present the formal mathematical results (labeled propositions) underlying our two key testable predictions regarding behavior in time-bundled relative to linear contracts. Section B.6 briefly analyzes the effects of payment frequency. Finally, Section B.7 considers the implications of adding a discounted health benefit to the model.

B.1 Full Model SetUp

Each day, an individual chooses whether to complete a binary action. Define w_t as an indicator for whether the individual *complies* (i.e., completes the action) on day t.

Incentive Contract Structure and Compliance We consider a principal who designs contracts to incentivize individuals for compliance over a sequence of T days. We call this sequence of days the payment period and index its days $t = 1, ..., T$.

Let m_t be the payment made by the principal to the individual on day t. Within each payment period, payments are delivered on day T only and depend on the individual's compliance decisions from day 1 through T of the payment period.

Define *compliance*, the expected fraction of days on which the individual complies, as $C = \frac{1}{T}\mathbb{E}[\sum_{t=1}^{T} w_t]$ and the expected per-day *payment* as $P = \frac{1}{T}\mathbb{E}[m_T]$. Define *cost-effectiveness* as compliance divided by expected per-day payment, C/P.

The Principal's Objective: Effectiveness We assume that the principal aims to maximize *effectiveness*, defined as the expected per-day benefit to the principal from compliance less the expected payment to agents. Maximizing effectiveness is analogous to the standard contract theory approach of maximizing output net of wage payments.[48] For the definition to be operable, we need to take a stand on the expected benefit function. We assume the expected benefit is linear in compliance, equal to λC for some $\lambda > 0$. This simplifying assumption is reasonable in our empirical setting since the estimated marginal health benefit of days of exercise is approximately linear (Warburton et al., 2006; Banach et al., 2023). With linear benefits, effectiveness becomes $\lambda C - P$.

We want to compare the effectiveness of different contracts even when we do not know λ. Rewriting effectiveness as $C\left(\lambda - \frac{1}{(C/P)}\right)$ shows that (assuming effectiveness is positive) one contract is more *effective* than another if it has strictly larger compliance and weakly larger cost-effectiveness, or weakly larger compliance and strictly larger cost-effectiveness.

Agent Utility Agent utility depends on the payments they receive from the principal and the cost of the effort of complying (if they comply), as captured by the following reduced-form utility function:

$$U = \mathbb{E}\left[\sum_{t=0}^{\infty} d^{(t)} m_t - \delta^{(t)} w_t e_t\right], \tag{8}$$

[48]This objective is often used in practice. For example, health policymakers and insurance companies often want to maximize the total health benefits of a program relative to its costs.

where e_t is the effort cost of complying on day t, $\delta^{(t)}$ is the discount factor over effort t days in the future, and $d^{(t)}$ is the discount factor over payments received t days in the future (for notational simplicity, we denote $\delta^{(1)}$ as δ and $d^{(1)}$ as d). Both $\delta^{(t)} \leq 1$ and $d^{(t)} \leq 1$, with $\delta^{(0)} = d^{(0)} = 1$. Neither $\delta^{(t)}$ nor $d^{(t)}$ are necessarily exponential functions of t; we assume only that they are weakly decreasing in t. We assume utility is linear in payments, which is likely a good approximation in our setting, as payments are small relative to overall consumption.

Importantly, this reduced-form utility function differentiates the discount factor over payments, $d^{(t)}$, from the discount factor over effort, $\delta^{(t)}$. The specification is consistent with a standard model of utility with a single structural discount factor over consumption and effort (e.g., Augenblick et al., 2015). In that case, $\delta^{(t)}$ is the structural discount factor, while $d^{(t)}$ depends on the availability of borrowing and savings. For example, in perfect credit markets, individuals should discount future payments at the interest rate r, and so $d^{(t)} = \left(\frac{1}{1+r}\right)^t$.

Time-Inconsistency and Sophistication Individuals will have time-inconsistent preferences either if $\delta^{(t)}$ or $d^{(t)}$ are non-exponential functions of t, or if $d^{(t)} \neq \delta^{(t)}$. Among time-inconsistent agents, we follow O'Donoghue and Rabin (1999a) in distinguishing sophisticates, who are aware of their discount factors (over both effort and money), from naifs, who "believe [their] future selves' preferences will be identical to [their] current self's." Thus, letting $w_{t,j}$ be the agent's prediction on day j about her compliance on day $t > j$, sophisticates accurately predict how their future selves will behave ($w_{t,j} = w_t$), while naifs may not.[49]

Effort Costs Let e_t be identically (but not necessarily independently) distributed across days, with the marginal distribution of e_t given by continuous cumulative distribution function (CDF) $F(\cdot)$. Individuals know the joint distribution of effort costs in advance but do not observe the realization of e_t until day t. e_t can be negative, as agents may comply without payment.

Agent Problem Given the notation and assumptions above, we can express the agent's problem as follows. On day t, the agent chooses compliance, w_t, to maximize expected discounted payments net of effort costs:

$$\max_{w_t \in \{0,1\}} \mathbb{E}\left[d^{(T-t)} m_T - \sum_{j=t+1}^{T} \delta^{(j-t)} w_{j,t} e_j \,\middle|\, e_1, .., e_t, w_1, .., w_t \right] - w_t e_t, \tag{9}$$

where the expectation over future discounted payment and future discounted effort depends on the history of effort costs $(e_1, .., e_t)$ and compliance decisions $(w_1, .., w_t)$ through time t, and where $w_{j,t}$ represents the agent's prediction on day t about her compliance on day j.

Denoting $\mathbb{E}\left[d^{(T-t)} m_T - \sum_{j=t+1}^{T} \delta^{(j-t)} w_{j,t} e_j \,\middle|\, e_1, .., e_t, w_1, .., w_t \right]$ as $V_t(w_t)$, the agent will thus choose to set $w_t = 1$ (i.e., comply on day t) if the following holds:

$$V_t(0) < V_t(1) - e_t \tag{10}$$

That is, on day t, the agent complies if the continuation value of complying net of the effort cost

[49] With domain-specific discounting, naivete can stem from misunderstanding how the future self will either (a) value current effort relative to money, or (b) discount effort or money further in the future.

is greater than the continuation value of not complying.

B.2 Time-Separable Linear Contracts (the Base Case)

We now solve for compliance and effectiveness under the base case contract. The contract is linear, paying m per day of compliance:

$$m_T^{\text{Base Case}} = m \sum_{t=1}^{T} w_t. \tag{11}$$

Agents comply on day t if the discounted payment outweighs the effort cost:

$$e_t < d^{(T-t)} m. \tag{12}$$

Expected payment per period P is then mC. As a result, effectiveness is $(\lambda - m)C$. Cost-effectiveness, C/P, is simply $\frac{1}{m}$ for any linear contract with positive compliance.

Observation 1. In a time-separable contract, holding all else constant, neither compliance, cost-effectiveness, nor effectiveness depend on $\delta^{(t)}$.[50]

B.3 Time-Bundled Contracts

Time-bundled contracts contain at least one period in which the payment for future compliance is increasing in current compliance. We focus on a *threshold* time-bundled contract, where there is a minimum threshold level of compliance K.[51] In a threshold contract, if the participant complies on fewer than K days in the payment period, no incentive is received. If they comply on at least K days, payment is a linear function of the number of days of compliance, with a rate of m' per day. Total payment in the threshold contract is thus:

$$m_T^{\text{Threshold}} = \begin{cases} m' \sum_{t=1}^{T} w_t & \text{if } (\sum_{t=1}^{T} w_t \geq K) \\ 0 & \text{otherwise.} \end{cases} \tag{13}$$

In the following two subsections, we theoretically examine the effect, relative to the Base Case, of adding a threshold while maintaining the same payment period length. Our results rest on the fact that, unlike in the Base Case, compliance, cost-effectiveness, and effectiveness in threshold contracts depends critically upon the discount factor over effort.

B.4 Thresholds versus Linear: Comparative Statics in the Effort Discount Rate

In this section, we present a series of propositions that provide the theoretical underpinning for Prediction 1 from Section 2.3. The prediction is that the lower is $\delta^{(t)}$, the higher are compliance and effectiveness in a threshold relative to time-separable contract. We have already seen that in time-separable contracts, compliance and effectiveness are flat in $\delta^{(t)}$ (Observation 1). The propositions demonstrate that in contrast, both compliance and effectiveness in time-bundled threshold contracts tend to decrease in $\delta^{(t)}$.

Specifically, Proposition 1 examines threshold contracts with $K = T$ (i.e., where one must comply on all days to receive payment). It shows that, for all T, *regardless of the effort cost distribution,* compliance is weakly decreasing in $\delta^{(t)}$.

[50] In linear contracts, compliance is $\frac{1}{T}\mathbb{E}\left[\sum_{t=1}^{T} w_t\right] = \frac{1}{T}\sum_{t=1}^{T} F(d^{(T-t)}m)$, which is not directly related to $\delta^{(t)}$.

[51] Our predictions hold for other types of time-bundled contracts in many circumstances.

To gain tractability to examine threshold effectiveness and threshold contracts with $K < T$, we then make assumptions about the effort cost distribution. Proposition 2 examines effectiveness when $K = T = 2$ and shows that, under relatively general conditions, effectiveness in the threshold contract is weakly decreasing in δ. Proposition 3 shows that, if costs are perfectly positively correlated over time, both compliance and effectiveness under the threshold are decreasing in $\delta^{(t)}$ for any $K \leq T$ and any T. Finally, Proposition 4 examines a simplified model where costs are binary and known from day 1, $K = 2$ and $T = 3$. We show that compliance and effectiveness are higher when $\delta^{(t)}$ is lower.

The propositions together suggest that Prediction 1 holds in many empirically-relevant conditions, including when either (a) K is high relative to T,[52] or (b) costs are positively correlated across periods. Both (a) and (b) hold in our empirical setting: our experiment uses relatively high levels of K relative to T, and costs are positively correlated across days.

Proposition 1 ($T = K$, Threshold Compliance and Impatience Over Effort)**.** *Let $T > 1$. Fix all parameters other than $\delta^{(t)}$. Take any threshold contract with threshold level $K = T$; denote the threshold payment M. Compliance in the threshold contract is weakly decreasing in $\delta^{(t)}$ for all $t \leq T - 1$.*

Proof. We provide the proof here for $T = 2$. The proof for $T > 2$ is in Online Appendix H.1.

Recall that the condition for complying on day 1 is to comply if $e_1 < V_1(1) - V_1(0)$ (equation (10)). Let $w_{t,j}$ be the agent's prediction on day j about her compliance on day $t > j$. With the threshold contract, we have that:

$$V_1(1) - V_1(0) = \mathbb{E}\left[(dM - \delta e_2)w_{2,1}|e_1, w_1 = 1\right] - \mathbb{E}\left[-\delta e_2 w_{2,1}|e_1, w_1 = 0\right] \tag{14}$$

We examine this expression separately for sophisticates and naifs.

For sophisticates, who accurately predict their own future behavior, $w_{2,1}|^{w_1=1} = \mathbb{1}\{e_2 < M\}$ and $w_{2,1}|^{w_1=0} = \mathbb{1}\{e_2 < 0\}$. Thus:

$$\begin{aligned} V_1(1) - V_1(0) &= \mathbb{E}\left[(dM - \delta e_2)w_{2,1}|e_1, w_1 = 1\right] - \mathbb{E}\left[-\delta e_2 w_{2,1}|e_1, w_1 = 0\right] \\ &= \mathbb{E}\left[(dM - \delta e_2)\mathbb{1}\{e_2 < M\} + \delta e_2 \mathbb{1}\{e_2 < 0\}|e_1\right] \end{aligned} \tag{15}$$

We show that this is weakly decreasing in δ by showing that the argument $(dM - \delta e_2)\mathbb{1}\{e_2 < M\} + \delta e_2 \mathbb{1}\{e_2 < 0\}$ is weakly decreasing in δ for all values of e_2. There are two cases:

1. $e_2 > 0$: In this case, $(dM - \delta e_2)\mathbb{1}\{e_2 < M\} + \delta e_2 \mathbb{1}\{e_2 < 0\} = (dM - \delta e_2)\mathbb{1}\{e_2 < M\}$, which is weakly decreasing in δ.
2. $e_2 \leq 0$: In this case, $(dM - \delta e_2)\mathbb{1}\{e_2 < M\} + \delta e_2 \mathbb{1}\{e_2 < 0\} = (dM - \delta e_2) + \delta e_2 = dM$, which is invariant to δ.

Since equation (15) is weakly decreasing in δ, day 1 compliance is decreasing in δ. The same is true for day 2 compliance, since $w_2 = 1$ if both $w_1 = 1$ and $e_2 < M$ (or if $e_2 < 0$), and

[52] Thresholds where K/T is very low may not always be better for impatient naifs than patient people because they include more days where current and future effort are substitutes, which can cause naifs to procrastinate.

w_1 is weakly decreasing in δ. Thus, compliance in the threshold contract is decreasing in δ for sophisticates.

We now turn to naifs. For naifs, who think their day 2 selves will share their day 1 preferences, $w_{2,1}|^{w_1=1} = \mathbb{1}\{\delta e_2 < dM\}$ and $w_{2,1}|^{w_1=0} = \mathbb{1}\{\delta e_2 < 0\}$. Thus:

$$\begin{aligned} V_1(1) - V_1(0) &= \mathbb{E}\left[(dM - \delta e_2)w_{2,1}|e_1, w_1 = 1\right] - \mathbb{E}\left[-\delta e_2 w_{2,1}|e_1, w_1 = 0\right] \\ &= \mathbb{E}\left[(dM - \delta e_2)\mathbb{1}\{\delta e_2 < dM\} + \delta e_2 \mathbb{1}\{\delta e_2 < 0\}|e_1\right] \\ &= \mathbb{E}\left[\max\{dM - \delta e_2, 0\} + \delta e_2 \mathbb{1}\{e_2 < 0\}|e_1\right] \end{aligned} \tag{16}$$

Again, we show that this is decreasing in δ by showing that the argument, $\max\{dM - \delta e_2, 0\} + \delta e_2 \mathbb{1}\{e_2 < 0\}$, is weakly decreasing in δ for all values of e_2. There are two cases:

1. $e_2 > 0$: In this case, $\max\{dM - \delta e_2, 0\} + \delta e_2 \mathbb{1}\{e_2 < 0\} = \max\{dM - \delta e_2, 0\}$, which is weakly decreasing in δ.
2. $e_2 \leq 0$: In this case, for $u = -e_2 \geq 0$, we have $\max\{dM - \delta e_2, 0\} + \delta e_2 \mathbb{1}\{e_2 < 0\} = \max\{dM + \delta u, 0\} - \delta u = (dM + \delta u) - \delta u = dM$ which is invariant to δ.

Since equation (16) is weakly decreasing in δ, day 1 compliance (and hence day 2 and total compliance) are also decreasing in δ for naifs. □

We now examine effectiveness when $T = K$. We examine the case where $T = 2$ and, to gain tractability, make a reasonable assumption on the cost function, assuming that e_2 is weakly increasing in e_1, in a first order stochastic dominance sense.[53] This assumption flexibly accommodates the range from IID to perfect positive correlation, just ruling out negative correlation. Under this assumption, we show that effectiveness is weakly decreasing in δ as long as there is not "too much" inframarginal behavior. When there is too much inframarginal behavior, not only will the effectiveness prediction not hold but incentives cease to be a cost-effective approach.

Proposition 2 ($T = 2, K = 2$, Threshold Effectiveness and Impatience Over Effort)**.** *Let $T = 2$. Let e_2 be weakly increasing in e_1, in a first order stochastic dominance sense. Fix all parameters other than $\delta^{(t)}$. Take any threshold contract with threshold level $K = 2$; denote the threshold payment M. As long as there is not "too much" inframarginal behavior,[54] the effectiveness of the threshold contract is weakly decreasing in δ.*

Proof. We first show that, if costs are positive, cost-effectiveness in the threshold is not increasing in δ. Because Proposition 1 showed that compliance is decreasing in δ, this establishes that effectiveness is decreasing in δ when costs are positive. We then show sufficient conditions for threshold effectiveness to decrease in δ when costs can be negative.

[53] $F_{e_2|e_1}(x)$ is weakly decreasing in e_1 for all x, with $F_{e_t|e_{t'}}(x)$ the conditional CDF of e_t given $e_{t'}$.

[54] See equation (20) for the exact condition. The intuition for why high levels of inframarginal behavior (combined with low $\frac{\lambda}{M}$) can flip the effectiveness prediction is as follows. If there is inframarginal behavior, then the principal effectively gets "free" compliance if people comply on day 2 only and not day 1. As we will show, lower δ increases compliance by making people more likely to comply on day 1. The benefit is extra compliance and the cost is extra payment. The cost will be particularly large if there is a lot of inframarginal behavior on day 2, because now the principal has to pay out for all of the day 2's on which day 1 compliance was induced, which the principal used to get for free.

To simplify notation, let e^* be the agent's cutoff value for complying in period 1, such that agents comply in period 1 if $e_1 < e^*$. From equations (15) and (16), we know that the value of e^* will depend on the agent's sophistication and, importantly, decrease in δ.

With our new notation, we can write the compliance decisions as:

$$\begin{aligned} w_1 &= \mathbb{1}\{e_1 < e^*\} \\ w_2 &= w_1 \mathbb{1}\{e_2 < M\} + (1 - w_1)\mathbb{1}\{e_2 < 0\} \\ &= w_1 \mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\} \end{aligned}$$

A Special Case: Positive Costs We first examine the restricted case where $e_1 > 0$ and $e_2 > 0$ and show that, in that case, C/P is not increasing in δ. In that case, $w_2 = w_1 w_2$. Therefore we have:

$$\begin{aligned} C/P &= \frac{1}{M}\frac{\mathbb{E}[w_1 + w_2]}{\mathbb{E}[w_1 w_2]} = \frac{1}{M}\frac{\mathbb{E}[w_1 + w_1 w_2]}{\mathbb{E}[w_1 w_2]} = \frac{1}{M}\left(\frac{\mathbb{E}[w_1]}{\mathbb{E}[w_1 w_2]} + 1\right) = \frac{1}{M}\left(\frac{\mathbb{E}[w_1]}{\mathbb{E}[w_1]\mathbb{E}[w_2|w_1 = 1]} + 1\right) \\ &= \frac{1}{M}\left(\frac{1}{\mathbb{E}[w_2|w_1 = 1]} + 1\right) \end{aligned} \tag{17}$$

Consider the first term, $\frac{1}{\mathbb{E}[w_2|w_1=1]}$. To show this is not increasing in δ, we show that $\mathbb{E}[w_2|w_1 = 1] = \mathbb{E}[\mathbb{1}\{e_2 < M\}|w_1 = 1]$ is weakly increasing in δ. Call this expression p_2^*. If costs were IID, then $p_2^* = F(M)$, which is independent of δ. To see that p_2^* is also weakly increasing in δ under our more general assumption that e_2 is weakly increasing in e_1, note that higher δ means that $w_1 = 1$ will be associated with lower values of e_1 (since e^* is decreasing in δ). This implies lower values of e_2 conditional on $w_1 = 1$, since we assume that e_2 is weakly increasing in e_1. Lower values of e_2 then mean that $p_2^* = E[w_2|w_1 = 1]$ will be weakly higher. Hence, p_2^* is weakly increasing in δ and the first term is weakly decreasing in δ. Thus, we have shown that, with positive costs, C/P is weakly decreasing in δ.

General Case Instead of using cost-effectiveness as a means to prove the result for effectiveness, we turn to the expression for effectiveness directly: $\lambda C - P$. We show the conditions under which it is weakly increasing in e^*, and hence weakly decreasing in δ.

First, we rewrite the expression for effectiveness under the threshold given what we know about C and P. (For notational simplicity, we examine $2(\lambda C - P)$ instead of $\lambda C - P$.)

$$\begin{aligned} 2(\lambda C - P) &= \lambda \mathbb{E}[w_1 + w_2] - M\mathbb{E}[w_1 w_2] \\ &= \lambda\left(F(e^*) + \mathbb{E}[w_1 \mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}]\right) - M\mathbb{E}[w_1 \mathbb{1}\{e_2 < M\}] \\ &= \lambda\left(F(e^*) + \mathbb{E}[\mathbb{1}\{e_1 < e^*\}\mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}]\right) - M\mathbb{E}[\mathbb{1}\{e_1 < e^*\}\mathbb{1}\{e_2 < M\}] \\ &= \lambda\left(F(e^*) + Prob(e_1 < e^*, 0 < e_2 < M) + Prob(e_2 < 0)\right) - MProb(e_1 < e^*, e_2 < M). \end{aligned} \tag{18}$$

We now take a derivative with respect to e^*. Let $g(e^*) = Prob(e_1 \leq e^*, e_2 \in S)$, where S is some set. It is straightforward to show that $g'(e^*) = f(e^*)\,Prob(e_2 \in S|e_1 = e^*)$.[55] Thus, we have

$$\frac{d}{de^*}[2\,(\lambda C - P)] = \lambda[f(e^*) + f(e^*)Prob(0 < e_2 < M|e_1 = e^*)] - Mf(e^*)Prob(e_2 < M|e_1 = e^*)$$

Hence, a sufficient condition for effectiveness to increase in e^* (and decrease in δ) is:

$$\lambda(1 + Prob(0 < e_2 < M|e_1 = e^*)) \geq M Prob(e_2 < M|e_1 = e^*) \tag{19}$$

or

$$\frac{\lambda}{M}(1 + Prob(0 < e_2 < M|e_1 = e^*)) \geq Prob(e_2 < 0|e_1 = e^*) + Prob(0 < e_2 < M|e_1 = e^*)$$

or

$$Prob(e_2 < 0|e_1 = e^*) \leq \frac{\lambda}{M} + \left(\frac{\lambda}{M} - 1\right) Prob(0 < e_2 < M|e_1 = e^*)). \tag{20}$$

If $\lambda > M$, condition (20) will always hold. More broadly, the condition will be more likely to hold the greater λ relative to M. The condition essentially guarantees that there not be "too much" inframarginal behavior, which generally decreases the efficacy of incentives. For example, when $\lambda > M/2$, which is a reasonable condition as it guarantees that the payment to the agent for two days of compliance is less than the benefits to the principal, a sufficient condition is

$$Prob(e_2 < 0|e_1 = e^*) < Prob(e_2 > M|e_1 = e^*).$$

We have thus showed that, as long as there is not "too much" inframarginal behavior (i.e, as long as equation (20) holds), the effectiveness of a threshold contract is decreasing in δ. □

We now turn to examine threshold contracts with $K < T$. To gain tractability, we begin with the case where costs are perfectly correlated across periods, showing that both compliance and effectiveness under the threshold are increasing in impatience for any threshold level $K \leq T$.

Proposition 3 (Perfect Correlation, Threshold Effectiveness and Impatience over Effort)**.** *Let there be perfect correlation in costs across periods ($e_t = e_{t'} \equiv e$ for all t, t'). For simplicity, if $\delta^{(t)} < 1$ for any t, let $\delta^{(t)} < 1$ for all $t > 0$. Fix all parameters other than $\delta^{(t)}$ for some $t \leq T-1$. Take any threshold contract with threshold level $K \leq T$. Compliance and effectiveness in the threshold contract will be weakly decreasing in $\delta^{(t)}$.*
Proof. See Online Appendix H.1. □

To make the problem more tractable when costs are not perfectly correlated, we now consider a simplified model where $T = 3$, $K = 2$, costs take on only two values (high or low), discount factors are exponential, and agents observe all future cost realizations on day 1. Again, threshold compliance and effectiveness are higher among those who are more impatient over effort.

[55] To show this, note that

$$\begin{aligned} g(e^* + \epsilon) - g(e^*) &= Prob(e^* < e_1 \leq e^* + \epsilon, e_2 \in S) = Prob(e^* < e_1 < e^* + \epsilon)Prob(e_2 \in S|e^* < e_1 \leq e^* + \epsilon) \\ &= (F(e^* + \epsilon) - F(e^*))\,Prob(e_2 \in S|e^* < e_1 \leq e^* + \epsilon). \end{aligned}$$

Dividing by ϵ gives us: $\frac{g(e^*+\epsilon)-g(e^*)}{\epsilon} = \frac{(F(e^*+\epsilon)-F(e^*))}{\epsilon} Prob(e_2 \in S|e^* < e_1 \leq e^* + \epsilon)$. Letting ϵ go to 0 and using the definition of the derivative gives that $g'(e^*) = f(e^*)\,Prob(e_2 \in S|e_1 = e^*)$.

Proposition 4. *Let $T = 3$. Let the cost of effort on each day be binary, taking on either a "high value" (e_H) or a "low value" (e_L), with $e_H \geq e_L$. Let agents observe the full sequence of costs e_1, e_2, e_3 on day 1. Let $\delta^{(t)} = \delta^t$ (i.e., let the discount factor over effort be exponential) and let $d^{(t)} = 1$. Fix all parameters other than δ. Consider a threshold contract with $K = 2$, where the agent must thus comply on at least 2 days in order to receive payment. Compliance and effectiveness in the threshold contract are weakly higher for someone with a discount factor $\delta < 1$ than for someone with discount factor $\delta = 1$.*

Proof. See Online Appendix H.1. □

For sophisticates, we can also show a stronger result. In simulations with most realistic cost distributions, this stronger result goes through for naifs as well.

Proposition 5. *Let $T = 3$. Let costs be weakly positive and let agents observe the full sequence of costs e_1, e_2, e_3 on day 1. Let $\delta^{(t)} = \delta^t$ (i.e., let the discount factor over effort be exponential) and let $d^{(t)} = 1$. Fix all parameters other than δ. Consider a threshold contract with $K = 2$, where the agent must thus comply on at least 2 days in order to receive payment. For sophisticates, compliance and effectiveness in the threshold contract are weakly decreasing in the discount factor δ.*

Proof. See Online Appendix H.1. □

B.5 Overall Effectiveness of Thresholds versus the Base Case

While Prediction 1 speaks to the heterogeneity in the performance of threshold relative to separable contracts by $\delta^{(t)}$, it is also important from a policy perspective to understand which type of contract performs better for any given level of $\delta^{(t)}$. The propositions in this section provide the theoretical underpinning for Prediction 2, which, while less general than Prediction 1, addresses this question. Specifically, Prediction 2 says that, under certain conditions, the most effective time-bundled threshold contract is more effective than the most effective linear contract if the discount factor over effort is sufficiently low, and less effective if the discount factor over effort is high.

Making some additional assumptions for tractability, we compare both optimized threshold and separable linear contracts, and threshold and linear contracts offering the same payment per day (as in our experiment),[56] paying particular attention to how the relative effectiveness of thresholds depends on δ. For simplicity, we assume that $T = 2$ and that $K = 2$ and denote the threshold payment as M (i.e., $M = 2m'$) throughout the section.

Our first proposition (Proposition 6) examines the relative performance of the contracts in the limit as δ goes to 0 under very general assumptions. It shows that, for sufficiently low δ, for any linear contract, there exists a threshold contract that achieves substantially higher cost-effectiveness with relatively little—and potentially even no—loss in compliance. In contrast, for any linear contract, one can always construct another *linear* contract with substantially

[56] In many empirical applications, constructing the optimal contract is not feasible as it requires knowledge of both the discount rate and the distribution of costs.

higher cost-effectiveness by decreasing the payment amount, but the loss in compliance may be arbitrarily large.

The next four propositions (Propositions 7a–8b) examine the full range of δ, not just the case where δ is sufficiently low. While we make additional assumptions on the effort cost distributions for tractability, the propositions demonstrate that thresholds can be effective for those who are impatient over effort in the two limiting cases of perfectly correlated and IID effort costs. IID effort costs is a common assumption in the literature (e.g., Garon et al., 2015). In each case, we begin with a testable comparison between threshold and linear contracts that offer the same payment per day before moving to more abstract comparisons that teach us about whether the optimal threshold contract or the optimal linear contract is more effective (and how that relationship depends on δ).[57]

Proposition 6. *Let $d = 1$ and $T = 2$. Fix all parameters other than δ, and take a linear contract that induces compliance $C > 0$.*
(a) If agents are naive and e_2 is weakly increasing in e_1, in a first order stochastic dominance sense,then for sufficiently small δ, there exists a threshold contract with $K = 2$ that has at least two times higher cost-effectiveness (and $1 + \frac{1}{C}$ times higher cost-effectiveness if costs are IID) and that generates compliance $\frac{1+C}{2}$ of the linear contract.
(b) If agents are sophisticated and costs are IID, then for sufficiently small δ, there exists a threshold contract with $K = 2$ that has at least $1 + C$ times higher cost-effectiveness and that generates compliance at least $\frac{1+C}{2}$ of the linear contract.

Proof. See Online Appendix H.2. □

The potential improvements from threshold contracts demonstrated by Proposition 6 are quantitatively large. For example, when costs are IID and agents are naive with sufficiently low δ, for a linear contract that generates $C = .9$, there exists a threshold contract that generates 95% as much compliance but for less than half the cost.

Proposition 7a (Perfect Correlation, $M = 2m$)**.** *Let $T = 2$. Fix all parameters other than δ. Consider a linear contract with payment m and a threshold contract with payment $2m$. Then, regardless of agent type, the threshold contract is more effective than the linear contract if $\delta < 2d - 1$. If $\delta \geq 2d - 1$, then the linear contract may be more effective.*

Proof. See Online Appendix H.2. □

Proposition 7b (Perfect Correlation, Optimal Contracts)**.** *Let $T = 2$. Fix all parameters other than δ, and take any linear contract that induces compliance $C > 0$. Let there be perfect correlation in costs across days ($e_1 = e_2$). Then, regardless of agent type, there exists a threshold contract that induces compliance of at least C and that has approximately $2\frac{d}{1+\delta}$ times greater cost-effectiveness than the linear contract. Hence, if $\delta < 2d - 1$, the most effective contract will always be a threshold contract.*

[57] Predictions about optimal contracts are hard to test since most policymakers do not have sufficient information about the cost function and δ to solve for the optimal contracts.

Proof. See Online Appendix H.2. □

Proposition 8a (IID Uniform, $M = 2m$)**.** *Let $d = 1$. Fix all parameters other than δ. Let costs be independently drawn each day from a uniform[0,1] distribution. Take any threshold contract paying $M < 2$ and compare it with the linear contract paying $m = \frac{M}{2}$.*
(a) If $M < 1$, the threshold contract is always more cost-effective, but whether it has higher compliance (and hence whether it is more effective) depends on δ. Define $\frac{2M^2}{1+M}$ as the cutoff value *for naifs and $2 - \frac{2}{M+M^2}$ as the* cutoff value *for sophisticates. If δ is less than the cutoff value for a given type, then the threshold contract is more effective, as it generates greater compliance.*
(b) If $1 \leq M < 2$,[58] *then the threshold contract is more effective.*

Proof. See Online Appendix H.2. □

Proposition 8b (IID Uniform, Optimal Contracts)**.** *Let $d = 1$. Fix all parameters other than δ. Let costs be independently drawn each day from a uniform[0,1] distribution. Whether the most effective threshold contract is more effective than the most effective linear contract depends on δ as well as λ, the principal's marginal return to compliance. For a wide and plausible range of values of λ,*[59] *there exists a* cutoff *value of δ such that the threshold contract is more effective when δ is below the cutoff, and the linear contract is more effective when δ is above the cutoff. For the remaining values of λ, either the threshold contract is always more effective, or the linear contract is always more effective, but in either case the effectiveness of the threshold relative to linear is decreasing in δ.*

Proof. See Online Appendix H.2. □

B.6 Payment Frequency

In this subsection, we first prove Prediction 3 from Section 2.4. Next, we present and prove a related prediction (Prediction 4) that follows Kaur et al. (2015) in showing an additional way to use empirical data to make inferences about the discount factor over payments, which we use in Section 5.4.

Before showing its proof, recall that Prediction 3 is the following: If agents are impatient over financial payments ($d^{(t)} < 1$), then the compliance and effectiveness of the base case linear contract are weakly increasing in the payment frequency. If agents are patient over financial payments ($d^{(t)} = 1$), then payment frequency does not affect compliance or effectiveness.

Proof. Equation (12) implies that, in a linear contract, $C = \frac{1}{T}\sum_{t=1}^{T} F(d^{(T-t)}m)$. Compliance is thus increasing in the discount factor over payment $d^{(T-t)}$. If agents are "impatient," then $d^{(T-t)}$ is weakly decreasing in the delay to payment $T-t$. Increasing payment frequency then decreases the average delay to payment, which weakly increases compliance. If agents are patient, then the discount factor is 1 irrespective of the delay to payment and increasing payment frequency has no effect on compliance. Effectiveness follows the same pattern as compliance since cost-effectiveness is invariant to payment frequency (it is always $\frac{1}{m}$). □

[58] Note that the principal would never pay $M > 2$ since $M = 2$ achieves 100% compliance regardless of δ.
[59] See proof in Online Appendix H.2 for specific ranges for both naifs and sophisticates.

Prediction 4 (Payday Effects)**.** *If the discount factor over payments $d^{(t)}$ is decreasing in t, then the probability of complying in the base case linear contract increases as the payday approaches. If the discount factor over payments $d^{(t)}$ is constant in t, then the probability of complying is constant as the payday approaches.*

Proof. Recall that, on day t, agents comply if $e_t < d^{(T-t)}m$. As the payment date approaches, the time to payment $T-t$ decreases. If $d^{(T-t)}$ is decreasing, this increases $d^{(T-t)}$ and hence increases the likelihood that $e_t < d^{(T-t)}m$. If $d^{(T-t)}$ is flat, then the likelihood that $e_t < d^{(T-t)}m$ remains constant. □

B.7 Modeling a Health Benefit to Compliance

Our model excludes a long-term health benefit of walking. In this section, we show that our theoretical predictions are robust to this change under a range of reasonable conditions: (1) with domain-specific discount factors over health and effort, or—even with a single discount factor for health and effort—(2) a small discounted health benefit relative to the discounted daily incentive payment, or (3) more periods of effort required to meet the threshold under the contract.

These conditions appear reasonable in our setting. (1) Empirical work suggests that domain specific discount factors for health and effort are likely. People tend to discount health more than money (Chapman and Elstein, 1995; Chapman, 1996; Hardisty and Weber, 2009), and people discount health gains more sharply than both losses in the health domain and gains in other domains (Hardisty and Weber, 2009; Chapman, 1996). (2) Even if people do discount health and effort with the same discount factor, the health benefit is much further in the future than the effort and payment we model, and so its discounted value is likely small. (3) The 4-day and 5-day thresholds used in our contracts are much higher than the 2-day thresholds that we examine in depth theoretically.

We first show how adding a future health benefit of compliance, b, impacts our predictions about behavior in linear vs. threshold contracts in a simple case where people discount health and effort with a single constant discount factor. We then briefly discuss how alternative, and arguably more realistic, specifications of the discount factor over health will dampen (or even eliminate) the impact of the health benefit to compliance on our predictions.

B.7.1 Predictions with One Discount Factor for Health and Effort

For simplicity, we restrict attention to the case where b is discounted with a simple quasi-hyperbolic discount factor that is identical to the discount factor over effort, $\delta^{(t)} = \delta$ if $t > 0$. We further restrict the discount factor over money, $d^{(t)}$, to be 1.

Compliance in Linear Contract Participants now comply on day j if the discounted payment *and* discounted benefit outweigh the effort cost:

$$e_j < m + \delta b. \tag{21}$$

Compliance is $F(m + \delta b)$, with $F(\cdot)$ the effort cost CDF. Compliance in the linear contract, and compliance without a contract, are thus no longer independent of δ: they increase in δ.

Relative Compliance in Threshold Contract The relationship between $\delta^{(t)}$ and behavior under threshold contracts is now more complex. We discuss the implications of adding b for Prediction 1 and then Prediction 2 in turn. Specifically, Prediction 1—that compliance and effectiveness in threshold relative to linear contracts are decreasing in the discount factor over effort—should still hold provided that: (a) the threshold payment m' is large relative to the future benefit of compliance b, or (b) a large number of periods of effort are required to meet the threshold (i.e., large K, which requires large T). Similarly, Prediction 2 can still hold: the discount factor over effort can still be pivotal to the relative effectiveness of threshold relative to linear contracts. Online Appendix H.3 presents the formal mathematical propositions underlying this discussion.

Prediction 1 with Discounted Benefit Prediction 1 is that compliance and effectiveness in threshold relative to linear contracts tend to decrease in $\delta(t)$. Adding b complicates this prediction and its underlying propositions (for simplicity, we focus here on the compliance implications). For example, without b, compliance in a threshold contract with $K = T$ is weakly decreasing in $\delta^{(t)}$ regardless of the cost distribution or other parameters (Proposition 1). With b, whether threshold compliance is weakly decreasing in $\delta^{(t)}$ now depends on parameters such as the cost distribution, the threshold payment m', and the threshold level K.

Simulation results show that two factors increase the likelihood that Prediction 1 holds: (a) a high threshold payment m' relative to the benefit b, and (b) a large number of periods until the threshold is reached K. We demonstrate these ideas more rigorously in the propositions presented in Online Appendix H.3, which for tractability assume perfect correlation in costs across periods.

First, Proposition 9 shows that, for a threshold contract with threshold level $T = K$, the sign of the derivative of compliance with respect to δ depends on the value of the daily threshold payment m' relative to $\frac{b}{K-1}$. When $m' \geq \frac{b}{K-1}$, compliance in the threshold contract decreases in δ, as it does in the model without b. This implies that compliance in the threshold relative to linear contract also decreases in δ (i.e., that Prediction 1 holds) since compliance in the linear contract is increasing in δ. The expression $m' \geq \frac{b}{K-1}$ is more likely to hold (a) the larger is m' relative to b, and (b) the larger is K, demonstrating the importance of these two factors.

In contrast, when $m' < \frac{b}{K-1}$, the derivative of threshold compliance with respect to δ is positive—making the derivative of *relative* compliance (compliance in the threshold relative to linear contract) ambiguous, as the derivative of linear compliance is also positive. Which derivative is more positive will depend on parameter values. To provide some results in this case, Proposition 10 makes further assumptions about the cost distribution (e.g., uniform costs across people), and shows that for high enough δ, relative compliance again tends to decrease in δ.[60] Simulation results support the findings from this simplified model.

Prediction 2 with Discounted Benefit Prediction 2 concerns the *level* of threshold relative to linear compliance, not just their comparative static in δ. Namely it states that δ can be

[60]Relative compliance also tends to decrease in δ for a wider range of δ among naifs than sophisticates—a finding backed up by simulation results as well.

pivotal to the relative effectiveness of threshold and linear contracts: when δ is sufficiently low, threshold contracts can be more effective than linear, whereas when δ is sufficiently high, linear can be more effective. Simulation results suggest that, even after adding a b term, under parameter assumptions that support Prediction 1, δ can also be pivotal to the relative effectiveness of threshold and linear contracts in models. To demonstrate this idea more rigorously, Proposition 11 shows that, in a simplified model with perfect correlation in costs and $T = 2$, a threshold contract offering the same per-day payment as a linear has the same compliance and effectiveness as the linear when $\delta = 1$, but weakly *higher* compliance and effectiveness when $\delta < 1$.

B.7.2 Alternative Health Discounting Models

While we cannot speak to all potential models, several other reasonable models for discounted benefits reduce the impact of b on our predictions. In particular, the assumption that $\delta^{(t)} = \delta$ produces a particularly large impact for three reasons: it assumes perfect correlation between discounting over the short- and long-run, it applies the same level of discounting in both the short- and long-run, and it applies the same discount rate to both effort costs and health benefits. Relaxing any of these assumptions mitigates the impact of b on our predictions.

Domain-Specific Discount Factors If discount factors are domain-specific across effort and health, then adding b does not change the results in Section 2. Specifically, if b is discounted by a health-specific discount factor other than $\delta^{(t)}$, the addition of b would leave our predictions unchanged.

More Flexible Quasi-Hyperbolic Discounting In practice, while our contracts incentivize effort in the near future, health benefits of compliance are realized far in the future (e.g., years rather than days). This is a critical distinction under a quasi-hyperbolic or "beta-delta" discount factor, where $\delta^{(t)} = \beta\delta^t$ for some $\delta < 1$. These conditions mitigate the impact of b for two reasons.

First, the magnitude of the discounted benefit of compliance will fade if it is further in the future: $\delta^{(t)}b = \beta\delta^t b$ approaches 0 for large enough t. As demonstrated above, the discounted benefit has a smaller impact on our predictions if its value is smaller. Second, while discounting over near-term effort would be primarily driven by the quasi-hyperbolic β term, discounting over future health benefits would depend more on the exponential δ term. This separation brings the comparative statics with respect to the short-run effort discount factor (holding all else constant) closer to the model without b.

More generally, the more people discount events far in the future (conditional on their short-run discount rates) and/or the lower the correlation between short and long-run discount rates, the smaller the impact of b on the comparative statics with respect to the short-run effort discount factor (holding all else constant). At the extremes, if short and long-run discount factors are uncorrelated or if discounted benefits of compliance approach zero, the situation resembles the domain-specific case above, leaving our predictions unchanged.

C Measuring the Effort and Payment Discount Factors

This section provides additional detail on measurements of impatience in our sample. We first describe how we validate the impatience index—our primary measure of effort discounting—using an incentivized effort task. We then present multiple estimates of the discount factors over effort and payment from our experimental context, showing substantial discounting of effort but not of payment. Finally, we show that there is limited correlation between the discount factors over effort and payment.

C.1 Validating the Impatience Index

We begin by describing the incentivized effort task data used for the validation exercise, along with other data collected. Next, we describe two effort discount rate measures obtained from these data. Third, we use these measures to validate our impatience index.

C.1.1 Data Collection in the Validation Sample

We validate our impatience index using a separate sample of 71 people who are very similar to our experimental sample (hereafter: the "validation sample").[61] The validation sample was randomly selected from a later evaluation of a similar incentive program for exercise (Dizon-Ross and Zucker, 2025) with nearly identical recruitment criteria,[62] and observable characteristics are balanced across the validation sample and experimental sample: walking levels, demographic characteristics, BMI, etc., are statistically indistinguishable (Online Appendix Table F.15).

In the validation sample, we collected the same *impatience index* described in this study and incentivized two tasks to measure impatience over effort and recharges, respectively.

Effort Task Respondents were incentivized to perform an effort task, which we call the "Effort Choice by Date" task, following the methodology of Augenblick (2018) and Augenblick and Rabin (2019), which John and Orkin (2022) previously adapted to a field setting. The task was to call into a toll-free automated phone line, listen to a useless 30-second recording, and answer a simple question to confirm that they listened. On the survey date (day 0), individuals chose how many calls to complete at time t for a piece rate w, where t is 0 (i.e., the same day), 1, 7, or 8 days from the time of the decision, and the piece rate is INR 10, 6, 2, or 0.[63] One choice was then randomly selected for implementation, and respondents received both the piece rate for the implemented choice as well as an additional 100 INR if they completed all the tasks they chose (in addition to one "mandatory task"). We refer to the measures we construct from these data as *effort impatience* measures.

Patterns in the data indicate that respondents understood the exercise. For example, the average number of tasks chosen increases with the piece rate, with respondents choosing an average of 5.6, 7.1, 7.6, and 8.0 tasks when the piece rates were 0, 2, 6, and 10 INR, respectively. Our field team also reported limited respondent misunderstanding.

[61] The sample size is comparable to the number of people who completed choices in the two seminal papers measuring impatience with effort tasks: 99 in Augenblick (2018) and 100 in Augenblick and Rabin (2019).

[62] Both studies targeted participants from Coimbatore, Tamil Nadu, using public screening camps as the primary recruitment tool, and both focused on individuals aged 30-65 who were literate, comfortable using mobile phones, capable of receiving mobile recharge payments, and had or were at high risk of lifestyle disease. However, the later study enrolled participants with high blood pressure in addition to high blood sugar.

[63] We include a 0 INR piece rate following guidance from John and Orkin (2022) that it helped their model converge. However, our structural model does not converge with the 0 INR piece rate choice, so we exclude it when estimating the structural parameters.

Recharge Choices A secondary goal for the validation sample was to assess the relationship of the impatience index with recharge impatience. To do so, we measure impatience over recharges with a multiple price list (MPL) (Andreoni and Sprenger, 2012a; John and Orkin, 2022). Participants made 10 choices between receiving a recharge today and a later date (either 7 and 14 days from today). For simplicity, the recharge today was always 50 INR, and the later recharges were larger whole numbers: 60, 70, 90, 100, and 150 INR. One choice from the MPL was also randomly selected for implementation.

The MPL choices are not ideal for estimating a structural recharge discount factor: the later payment amounts are all meaningfully larger than the earlier payment (we cannot distinguish between one-week discount factors in the range from $\frac{50}{60} = 0.83$ to 1), and, as with all MPLs, any mistrust in receiving the payment will push participants toward earlier payment and bias implied discount factors downwards (Halevy, 2008; Andreoni and Sprenger, 2012b). Instead, we construct a reduced-form *recharge impatience measure* as the proportion of choices where the individual chose the smaller recharge on the sooner date.

C.1.2 Structural and Reduced-Form Effort Impatience Measures

The data from the effort task are consistent with positive discounting of future effort with some present bias. Consistent with positive discounting, the number of tasks chosen on days with $t > 0$ are all significantly greater than on $t = 0$. (Specifically, participants chose 7.4, 7.0 and 7.5 tasks on days 1, 7, and 8, respectively, and only 6.4 tasks on day 0.) Consistent with present bias, the biggest jump in task allocations appears between "today" and "tomorrow".

We thus parameterize a constant discount factor for all future days: $\delta^{(t)} = \delta_{QH}$ if $t > 0$. This is equivalent to a $\beta - \delta$ model in which $\delta = 1$. We use the effort task data to construct two measures, one structural and one reduced-form, for this parameter.

Structural Measure and Evidence Our structural estimation follows John and Orkin (2022).[64] (The estimating equation is in the notes to Table C.1.) We structurally estimate δ_{QH} at the group level. As in John and Orkin (2022), individual-level structural estimates converge for less than half of our sample.

Column 1 of Table C.1 shows that, in the full validation sample, we estimate a δ_{QH} of 0.572, which is significantly different from 1 and suggests a high degree of effort impatience. In column 2, we follow Augenblick and Rabin (2019) and remove "problematic" individuals with limited effort choice variation or effort choices that are not primarily monotonic in wage offers.[65] The discount factor estimate is similar and still significantly different from 1.

Reduced-Form Measure and Evidence Our reduced-form measure is based on the excess number of tasks chosen on future dates relative to day 0 at a given piece rate, following Augenblick (2018) and Augenblick and Rabin (2019). Specifically, for all task allocations made on future days ($t > 0$) at piece rate w, we construct a measure at the individual $\times$ choice level equal to the tasks allocated on day t minus the tasks allocated on day 0 at the same piece rate w. People who are more impatient (lower δ_{QH}) will choose more tasks on future days than today, and thus have higher average values of this measure.

[64] John and Orkin (2022) assumes quasilinear utility and a power effort cost function following Augenblick (2018), and includes a non-monetary per-task reward s in addition to the piece rate following DellaVigna and Pope (2018).

[65] We remove 28 of 71 respondents in a field setting; Augenblick and Rabin (2019) remove 28 of 100 in a lab setting for the same reasons. Our removal rates are not significantly different for those with below- vs. above-median impatience index.

Appendix Table C.1: Structural Estimates of the Effort Discount Factor

	Full validation sample		Below-median impatience sample		Above-median impatience sample	
	(1)	(2)	(3)	(4)	(5)	(6)
δ_{QH}	0.572	0.556	0.996	0.996	0.176	0.367
	[0.132]	[0.153]	[0.009]	[0.007]	[0.156]	[0.208]
P-value: $\delta_{QH} = 1$	0.001	0.004	0.674	0.597	<0.001	0.002
P-value: $\delta_{QH} = \delta_{QH}^{\text{Below}}$	0.001	0.004			<0.001	0.002
P-value: $\delta_{QH} = \delta_{QH}^{\text{Above}}$	<0.001	0.063	<0.001	0.002		
Sample	All	Changers + Monotone	All	Changers + Monotone	All	Changers + Monotone
# Individuals	71	43	32	24	39	19
# Observations	852	516	384	228	468	228

Notes: This table displays structural estimates of the effort discount factor, δ_{QH}, in the validation sample, estimated using data from the Effort Choice by Date task of Augenblick (2018) using an estimation approach similar to John and Orkin (2022). The optimal allocation of effort is given by: $e^* = argmax(s + d^{(11)} \cdot \phi \cdot w) \cdot e - \delta^{(t)}(\frac{1}{\gamma}e^{\gamma})$, where t is the time of effort provision, γ captures the convex cost of effort, s is a parameter that captures the non-monetary reward for each task, w is the monetary piece rate, $d^{(11)}$ captures the monetary discounting of the payment in 11 days, and ϕ is a slope parameter. We parametrize $\delta^{(t)} = \delta_{QH}$ (equivalent to a quasihyperbolic model with $\delta = 1$) and $d^{(11)} = 1$ and estimate s, ϕ, δ_{QH}, and γ. We present results using the full validation sample and the subsamples with below- and above-median impatience index, with or without inclusion restrictions from choice patterns. Columns 1, 3, and 5 have no inclusion restriction; columns 2, 4, and 6 restrict to individuals who changed their effort choice at least once and had at most 1 choice non-monotonicity in payment levels.

Overall, participants chose to complete 13% fewer tasks in the present than the future, suggesting meaningful effort discounting. The result is similar if we again remove problematic individuals: the restricted sample allocates 15% fewer tasks in the present than the future. Our results mimic Augenblick (2018) and Augenblick and Rabin (2019) which find that participants choose to complete 16% and 10-12% fewer tasks in the present than the future, respectively.

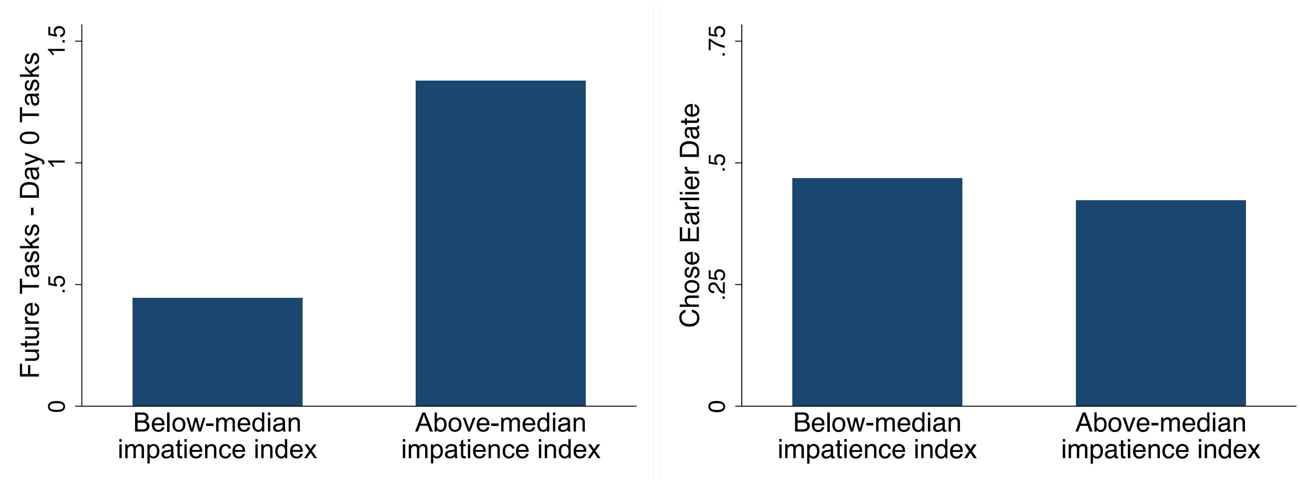
C.1.3 The Impatience Index Correlates with Effort Impatience Measures

In this section, we show that our impatience index correlates with the incentivized effort impatience measures in the validation sample. In contrast, it does not correlate with recharge impatience. Overall, this provides evidence that the impatience index proxies for impatience in the effort, but not payment, domain.

Correlation with Effort Impatience Measures Columns 3–6 of Table C.1 show that structural estimates of δ_{QH} are substantially higher among those with lower impatience index. Specifically, among individuals with below-median impatience index, our estimate of δ_{QH} is 0.996 and statistically indistinguishable from 1 (column 3). In contrast, we estimate that δ_{QH} is 0.176 for those with above-median impatience (column 5). We can reject equality of this estimate with 1 and with the corresponding estimate of δ_{QH} for those with below-median impatience. Columns 2, 4, and 6 show similar results after removing problematic respondents: our estimates of δ_{QH} are again significantly different for those with above- and below-median impatience index.

We summarize the reduced-form effort impatience measure separately for those with above- and below-median impatience index in Figure C.1(a). The above-median impatience sample has substantially higher average values of the reduced-form effort impatience measure: they allocate an average of 1.3 more tasks to future dates than today across piece rates, while those with below-median impatience index allocate only 0.4 more tasks to future days.

Appendix Figure C.1: Higher Impatience Index Predicts Higher Effort Impatience but Not Higher Recharge Impatience



(a) Effort Impatience (b) Recharge Impatience

Notes: Data come from the validation sample and are at the individual level. Panel (a) displays the average difference between the number of tasks chosen on all future dates minus the number of tasks chosen on the survey day (for the same payment amount) separately for the below- and above-median impatience index samples. In Panel (b), we display the average proportion of recharge MPL choices where the individual chose to get a smaller recharge today rather than a larger recharge in the future separately for the below- and above-median impatience index samples.

To test the significance of this difference, we estimate the following regression:

$$EffortImpatience_{itw} = \beta_0 + \beta_1 ImpatienceIndex_i + \beta_2 y_{i0w} + \tau_w + \tau_t + \varepsilon_{itw} \tag{22}$$

where $EffortImpatience_{itw}$ is the reduced-form effort impatience measure for individual i allocating tasks on day t at piece rate w, $ImpatienceIndex_i$ is either the impatience index or an indicator for having an above-median impatience index, and y_{i0w} is the number of tasks chosen by individual i at piece rate w on day 0; controlling for this allows the effort impatience measure to vary with the overall number of chosen tasks and improves precision.[66] τ_w and τ_t are fixed effects for the piece rate and task day, respectively. The coefficient of interest is β_1.

Consistent with Figure C.1, Column 1 of Table C.2 shows that the difference in reduced-form effort impatience between those with above- and below-median impatience index is roughly 1.0 task, significant at the 10% level. Column 2 shows that the relationship is even stronger excluding

[66] Define y_{itw} as the number of tasks chosen by individual i on day t for piece rate w. Since $EffortImpatience_{itw} = y_{itw} - y_{i0w}$, the coefficients from this regression are exactly equivalent to a regression with y_{itw} as the dependent variable that includes the same controls. The specification in equation (22) allows the mean value of the dependent variable to be comparable to Figure C.1.

problematic individuals: the gap is 1.7 tasks, significant at the 5% level. Columns 3 and 4 show qualitatively similar but less precise patterns with the impatience index as the regressor.

Appendix Table C.2: Impatience Index Correlates With Effort (But Not Recharge) Impatience

	Effort impatience				Recharge impatience			
	Future tasks - day 0 tasks				Chose earlier date			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Above-median impatience index	1.000*	1.708**			-0.0457	-0.0397		
	[0.513]	[0.798]			[0.102]	[0.109]		
Impatience index			0.763	2.666*			-0.0425	-0.0559
			[0.629]	[1.490]			[0.0911]	[0.114]
P-value: Impatience	0.055	0.038	0.229	0.081	0.655	0.716	0.642	0.625
Sample	All	Changers + Monotone	All	Changers + Monotone	All	No violations	All	No violations
Dep. var. mean (below-median impatience)	0.445	0.596	0.445	0.596	0.469	0.455	0.469	0.455
Correlation (dep var, Impatience index)	0.15	0.19	0.13	0.25	-0.05	-0.05	-0.05	-0.06
# Individuals	71	43	71	43	71	64	71	64
# Observations	852	516	852	516	710	640	710	640

Notes: This table shows the relationship between the effort and recharge impatience measures and the impatience index in the validation sample. Each observation is an individual $\times$ effort or recharge choice. The dependent variable in columns 1–4 is the difference between the tasks allocated in the choice and the tasks allocated on day 0 (the survey date) for the same piece rate; controls include fixed effects for the piece rate and task day, as well as the number of tasks chosen for that same piece rate on day 0. The dependent variable in columns 5–8 is an indicator for choosing recharges today rather than in the future; controls include fixed effects for how many weeks in the future the individual will be paid for the later recharge option (either 1 or 2 weeks) and for the relevant payment amount. The "Changers + Monotone" sample restricts to individuals who changed their effort choice at least once and had fewer than two choice non-monotonicities in payment levels. The "No violations" sample represents people who do not switch multiple times on either price list. The regressor in columns 1, 2, 5 and 6 is an above-median impatience index dummy, while in columns 3, 4, 7 and 8 the regressor is the continuous index. Correlations shown at the bottom of each column are between the individual-level average of the dependent variable and the version of the impatience index used in that column. Significance levels: * 10%, ** 5%, *** 1%.

Excluding problematic individuals, the magnitudes of the correlations between effort impatience and the impatience index are relatively high for the (noisy) domain of effort impatience—0.3 and 0.2 for the continuous and binary indices, respectively. In comparison, Augenblick et al. (2015) and Augenblick (2018) find correlations of 0.2 and 0-0.2 between effort impatience estimates and demand for commitment or qualitative discounting questions, respectively.

Lack of Correlation with Recharge Impatience Measure Figure C.1(b) summarizes the recharge impatience measure separately for those with above- and below-median impatience index. Recharge impatience (i.e., choosing a smaller, sooner recharge over a larger, later recharge) is very similar across the subsamples; in fact, those with above-median impatience index have slightly lower recharge impatience. Columns 5 and 7 of Table C.2 confirm that there is no meaningful or significant relationship between recharge impatience and the impatience index using regression analysis. Columns 6 and 8 replicate the results without problematic respondents.

C.2 Additional Estimates of the Discount Factors Over Effort and Payment

In this section, we present two estimates of the discount factor over payment (recharges), and one additional estimate of the discount factor over effort, all from our main experimental sample. We then summarize these estimates alongside the effort discount factor estimated in the validation sample (the Section C.1.2 estimate based on the Effort Choice by Date data). While both estimates of the discount factor over effort are meaningfully below 1, both payment discount factor estimates are close to 1 and significantly higher than either effort discount factor estimate. We begin by describing the additional estimation procedures.

"Simple CTB" Estimates of the Discount Factors Over Effort and Payment Following Augenblick et al. (2015), we estimate the discount factors for effort and money using the "Simple CTB" choices in each domain described in Section 4.2. Our primary specifications parametrize each discount factor as a single quasihyperbolic discount factor on future events (e.g., $\delta^{(t)} = \delta_{QH}$) but we estimate exponential parameterizations for robustness (e.g., $\delta^{(t)} = \delta^{t}_{Exp}$).[67]

Paycycle Estimates of the Discount Factor Over Payment Since impatience over payment will lead effort to increase as the payday approaches, one can use the pattern of effort over the pay cycle to estimate the payment discount factor. We follow Kaur et al. (2015), which calculates the discount factor using the elasticity of walking to payment and the pattern of effort as the payday approaches. We calculate the payment discount factor with the equation $\frac{1}{d_{QH}} - 1 = \frac{1}{\varepsilon}\frac{w_T - w_{t<T}}{w_{t<T}}$. where ε is the elasticity of walking to payment, w_t is compliance in period t, day T is payday, and days $t < T$ all occur before payday. We calculate the percentage increase in compliance on payday, $\frac{w_T - w_{t<T}}{w_{t<T}}$ from the estimated "payday spike" in the base case group (column 1 of Online Appendix Table F.10), and we estimate ε from the compliance response to the payment variation between the small payment and base case groups.

Comparing the Discount Factors over Effort and Payment Figure C.2 shows the payment discount factor estimates from both the Simple Recharge CTB and the paycycle effects, as well as the effort discount factors estimated from the Simple Effort CTB and the Effort Choice by Date. In all cases, the figure presents the estimates with the discount factors parametrized as a single discount factor (δ_{QH}) applied to all future periods.

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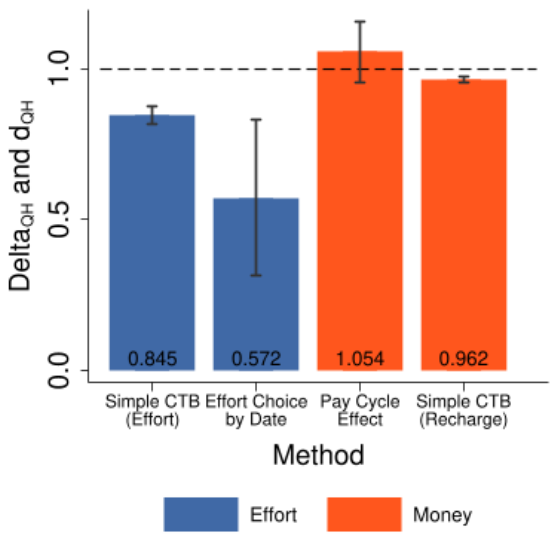
[67] The quasihyperbolic CTB discount factor over recharges is estimated with the equation $\ln\left(\frac{c_t+\omega_1}{c_{t+k}+\omega_2}\right) = \frac{\ln(d_{QH})}{\alpha-1}\mathbb{1}_{t=0} + \frac{1}{\alpha-1}(1+r)$ where c_t is money in the earlier period, c_{t+k} is money in the later period, ω_1 and ω_2 captures background consumption, and r is the interest rate for each choice. The estimating equation for the discount factor over effort is similar: c_t and c_{t+k} are replaced by e_t and e_{t+k} (minutes of walking on days t and $t+k$), ω_1 and ω_2 are background walking effort (10 minutes), and $1+r$ captures the marginal rate of substitution between sooner and later effort. Following Augenblick et al. (2015), we choose $\omega_1 = \omega_2 = \omega$, as a function of the base recharge consumption or base walking effort (we set the ω's at 50% of the base level for recharges and walking, so ω =50 INR and ω =10 minutes of walking, respectively), but the results are robust to a range of values from 25% to 200% of the base level.

The estimates of the payment discount factor are both near 1, with the payday effect estimate greater than (but not statistically significantly different from) 1, and the CTB estimate close to 1 (0.962) but significantly different from it. In contrast, both estimates of the effort discount factor are substantially smaller, at 0.572 from the validation sample and 0.845 from the Simple CTB in our main sample. Both are significantly less than either estimate of the payment discount factor (p-values for tests of equality are in the notes for Figure C.2.)

Appendix Figure C.2: The Discount Factors Over Effort Are Significantly Lower Than the Discount Factors Over Money



Notes: This figure presents four structural estimates of the discount factors over effort (blue bars) and payment (orange bars). From left to right, the estimates come from the Simple Effort CTB data from the experimental sample, the Effort Choice by Date data from the validation sample, the pay cycle method in the experimental sample, and the Simple Recharge CTB data from the experimental sample. The discount factor is parameterized as a single quasihyperbolic discount factor on the future ($\delta^{(t)} = \delta_{QH}$ or $d^{(t)} = d_{QH}$). The p-values for tests of equality between the effort discount factor (δ_{QH}) from the Effort Choice by Date methodology and the two monetary discount factors (d_{QH}) estimated via the Simple Recharge CTB and payday effects are 0.041 and 0.051, respectively. The p-values for tests of equality between the effort discount factor (δ_{QH}) from the Simple Effort CTB and the two monetary discount factors (d_{QH}) estimated via Simple Recharge CTB and payday effects are both $<$0.001. The respective samples for bars 1, 2, 3, and 4 include 852 choices of 71 individuals, 6,380 choices of 3,190 individuals, 71,672 days of 890 individuals, and 16,146 choices of 2,307 individuals.

Results are similar if we estimate exponential discount factors. We estimate daily exponential effort discount factors of 0.976 and 0.950 using Simple Effort CTB and Effort Choice by Date, respectively. Both are significantly less than 1 and significantly less than either estimate of the exponential payment discount factor (1.009 and 0.992 for Pay Cycle and Simple Recharge CTB estimates, respectively).

C.3 Measures of Effort and Recharge Impatience Are Uncorrelated

This section summarizes two types of evidence from our setting suggesting that discount factors over effort and recharge are relatively uncorrelated. First, survey measures of effort and recharge impatience are uncorrelated. Second, measures of effort impatience do not correlate with pay cycle effects.

Appendix Table C.3: No Correlation Between Measures of Impatience over Effort and Recharges

	Direct measure	Proxies for recharge impatience				
	Simple CTB (Recharge)	Negative mobile balance	Negative yesterday's talk time	Prefers daily (=1)	Prefers monthly (=-1)	# Individuals
	(1)	(2)	(3)	(4)	(5)	(6)
Impatience index	0.004	0.032	-0.068	-0.038	0.034	1740
Predicted impatience Index	0.000	0.021	-0.014	-0.005	-0.003	3192
Chose commitment	-0.006	0.009	-0.001	0.005	0.010	2871
Simple CTB	0.006	-0.011	-0.037	0.001	0.041	3190

Notes: This table displays the correlations in our experimental sample between our various measures of impatience in the effort domain (in the rows) and measures and proxies for impatience in the recharge domain (in columns). The "Simple CTB (Recharge)" measure is the average of the share of money allocated to today from the questions used in the Simple Recharge CTB. Proxies for recharge impatience in columns 2–5 were all measured at baseline. For columns 4 and 5: we asked participants whether they preferred daily, weekly, or monthly payments, and "Prefers Daily" ("Prefers Monthly") is an indicator that their most preferred frequency was daily (monthly). We normalize all impatience variables so that a higher value corresponds to greater impatience. Data are at the individual level. The sample in each row is the subset of participants we have each impatience measure for. Significance levels: * 10%, ** 5%, *** 1%.

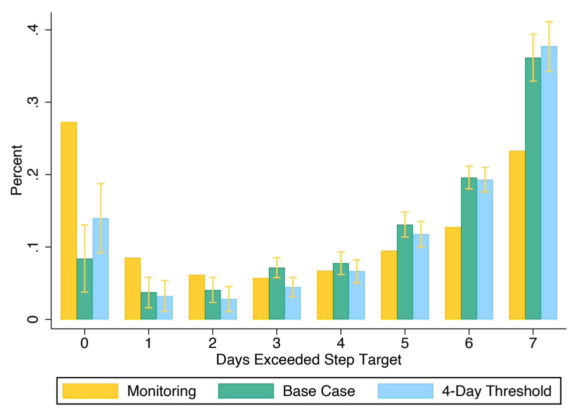
Table C.3 shows that there is no significant or meaningful correlation between any of the measures of impatience over effort and impatience over payment collected in the experimental sample. Similarly, we find that the correlation of the individual-level averages of the recharge impatience and effort impatience measures in the validation sample is only -0.05, which is statistically indistinguishable from 0 (p-value = 0.66).

As discussed in Section C.2, pay cycle effects also measure impatience over payments. Thus, we can test whether participants' impatience over payment relates to our measures of impatience over effort by testing whether effort impatience measures predict pay cycle effects.

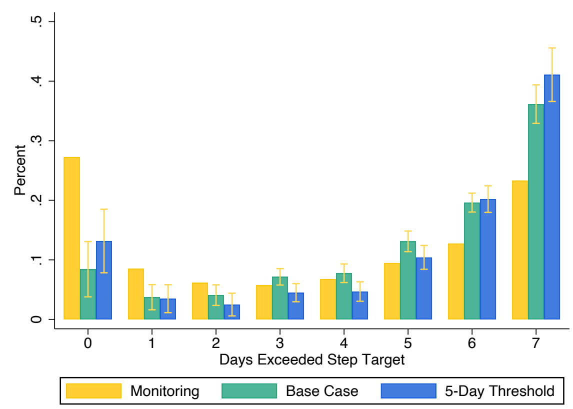
Panel A of Online Appendix Figure F.2 shows that there are no meaningful payday spikes even among those with above-median impatience index. Moreover, the patterns across the pay cycle are very similar for those with below-median impatience, depicted in Panel B. Results are similar for the other measures of effort impatience (i.e., the predicted impatience index, demand for commitment, and simple CTB). Regression analysis confirms that there are no large or significant differences in pay cycle effects across any measure of effort impatience.

D Distributional Impacts of Thresholds

This section assesses the effect of thresholds on the distributions of weekly and intervention-average compliance. We first assess whether thresholds decrease intermediate effort just below the threshold. Panels (a) and (b) of Figure D.1 show histograms at the individual × week level of the number of days the individual met their step target in that week, for the 4-day or 5-day threshold group, respectively, relative to Base Case and Monitoring (confidence intervals are relative to Monitoring). Indeed, the threshold contracts do modestly decrease effort just below the threshold: the prevalence of walking 3 or 4 days is lower in 5-Day Threshold than either Base Case (p-value <0.001) or Monitoring (p-value $= 0.008$), and the prevalence of walking 2 or 3 days is lower in 4-Day Threshold than either reference group (p-values <0.001 for both Base Case and Monitoring).[68] Figure D.2 shows similar patterns for the subsets of people with above- and below-median impatience, showing that the overall distributional patterns we see are not predominantly explained by impatience.



(a) 4-Day Threshold



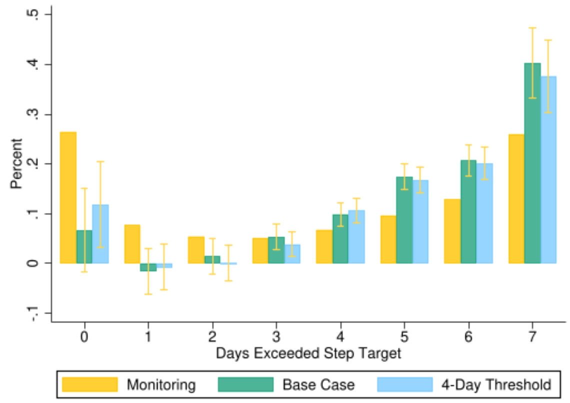
(b) 5-Day Threshold

Appendix Figure D.1: Thresholds Modestly Decrease Compliance Right Below the Threshold

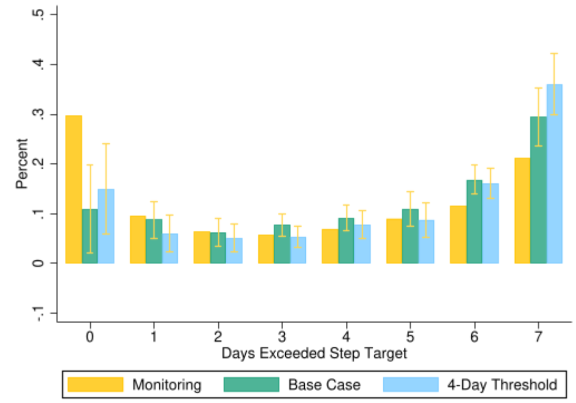
Notes: Figures show histograms of the number of days a participant exceeded the step target each week during the intervention period in the Base Case, 4-Day or 5-Day Threshold, and Monitoring. Data are at the respondent-week level. Confidence intervals represent a test of equality between Monitoring and each other group from regressions with the same controls as Table 2 except for day-of-week fixed effects (because data are weekly).

However, the magnitude of these differences are relatively small (especially compared to the differences from Monitoring), leading to only slight differences between Base Case and Threshold in the overall distribution of weekly compliance. Specifically, Panels (a) and (b) of Figure D.3 show the cumulative distribution functions (CDFs) of weekly compliance in 4-Day and 5-Day Threshold, respectively, relative to Base Case and Monitoring. While the distributions of weekly compliance in Base Case and both threshold groups all differ markedly from the distribution in Monitoring, the differences between Base Case and the threshold groups are small. Panels (c) and (d) of Figure D.3 shows similar results for the distribution of individual-level (instead of individual

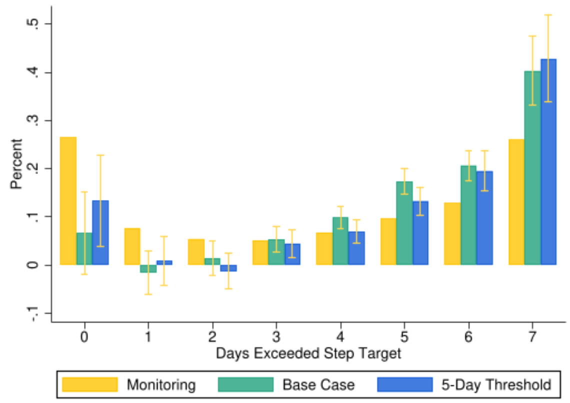
[68]Notably, neither threshold increases the likelihood of walking exactly the threshold number of days. Our model suggests this may reflect that the contracts pay for above-threshold compliance (e.g., the 4-day threshold pays for the 5th day of compliance). Additional explanations outside of the model include habit formation or that it is easier to schedule walking every day in a given week than on a subset of days.



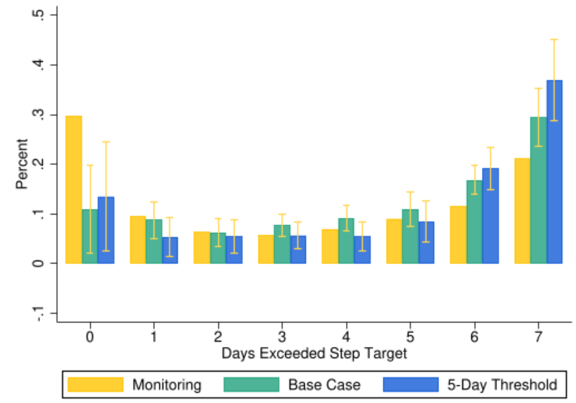
(a) 4-Day Threshold, Below-median Impatience



(b) 4-Day Threshold, Above-median Impatience



(c) 5-Day Threshold, Below-median Impatience

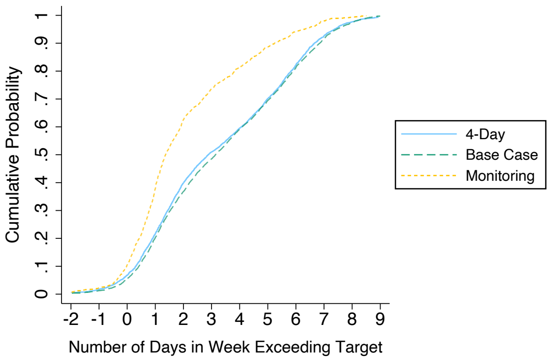


(d) 5-Day Threshold, Above-median Impatience

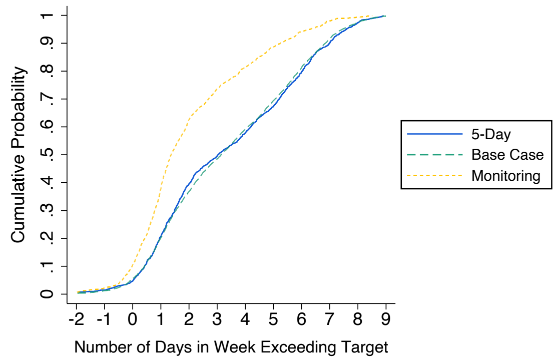
Appendix Figure D.2: Thresholds Modestly Decrease Compliance Right Below the Threshold

Notes: As in Figure D.1, Figures show histograms of the number of days a participant exceeded the step target each week during the intervention period in the Base Case, 4-Day or 5-Day Threshold, and Monitoring, but here we split the sample into below-median impatience index in Panels (a) and (c), and above-median impatience index in Panels (b) and (d). Data are at the respondent-week level. Confidence intervals represent a test of equality between Monitoring and each other group from regressions with the same controls as Table 2 except for day-of-week fixed effects (because data are weekly).

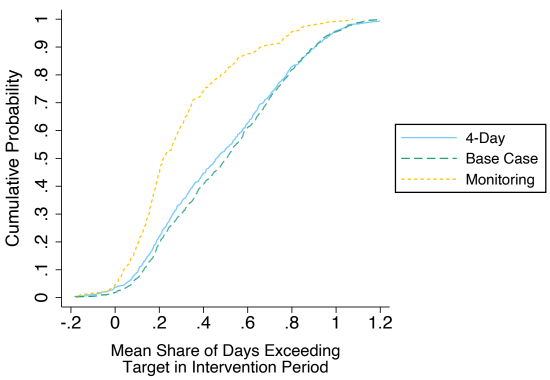
× week-level) compliance. Quantile regressions reveal no significant differences between the threshold groups and Base Case in the 25th, 50th, or 75th percentiles of the distributions of either individual × week-level or individual-level compliance (see Online Appendix Table F.7). Kolmogorov-Smirnov (KS) tests for the equivalence of the individual-level distributions also fail to reject the null of equal distributions (p-values 0.238 and 0.852 for the 4- and 5-Day Threshold, respectively, relative to Base Case).



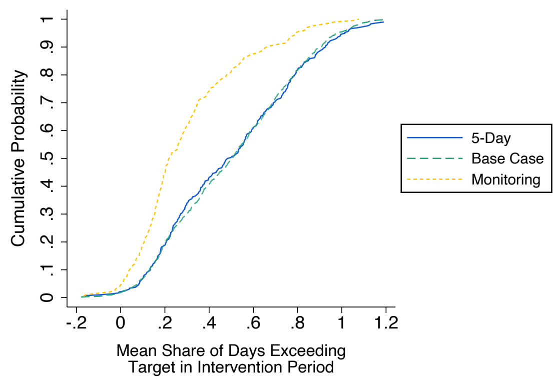
(a) 4-Day Threshold, Weekly Compliance



(b) 5-Day Threshold, Weekly Compliance



(c) 4-Day Threshold, Overall Compliance



(d) 5-Day Threshold, Overall Compliance

Appendix Figure D.3: Threshold and Base Case Have Similar Compliance Distributions

Notes: This figure shows the cumulative distribution functions (CDFs) of the distributions of weekly compliance (i.e., the number of days the individual exceeded the step target in a week) in Panels (a) and (b), and intervention-average compliance (i.e., the percentage of days the individual exceeded the step target during the intervention period) in Panels (c) and (d). All CDFs are plotted separately by treatment group for the monitoring, base case, 4-day (Panels (a) and (c)), and 5-day (Panels (b) and (d)) threshold groups. For Panels (a) and (b), data are at the individual × week level, limited to weeks where the individual has at least 4 days of data. For Panels (c) and (d), data are at the individual level, limited to individuals who had at least 21 days of data over the 12-week intervention period. Both weekly and intervention-average compliance are residualized using the same controls as in Table 2 except that we do not include day-of-week fixed effects because data are not at the day level.

E Predicting Impatience with Policy Variables

This appendix provides proof of concept that a policymaker could use hard-to-manipulate observable characteristics to predict impatience and effectively target the threshold contract.

Our Section 5.3 results suggest that a policymaker could improve our program's effectiveness by targeting threshold contracts only to more impatient individuals. However, impatience is challenging to observe; even were policymakers to field surveys on impatience, participants might game their responses to avoid a specific contract—especially a financially dominated one.

To address this concern, we construct a "policy prediction" of impatience: a prediction of the impatience index using demographics (e.g., age, labor force participation) and medical information (e.g., HbA1c, fatigue) that health policymakers would likely have access to. We show that there is significant heterogeneity in the effect of the threshold by the policy prediction. Hence, the policy prediction could be used to personalize contract assignment.

To prevent overfitting, we use a split sample approach. First, in a randomly-selected training sample, we fit a LASSO model to predict the impatience index with the variables listed in the Table E.1 notes. We then use the model to predict impatience out of sample for all other participants (the "regression sample"). Finally, in the regression sample, we estimate the heterogeneity in Threshold performance by the policy prediction using equation (6). To sufficiently power this regression, we allocate 2/3 of participants to the regression sample.

The results, in Table E.1, are similar to Table 3: Threshold has a higher treatment effect among people with higher predicted impatience. This suggests that personalizing thresholds using a policy prediction could significantly improve the effectiveness of incentives at scale.

Appendix Table E.1: Threshold Effect Varies with Policy Prediction of Impatience

Dependent variable:	Exceeded step target	
	(1)	(2)
Impatience × Threshold	0.03**	0.06**
	[0.00, 0.06]	[0.00, 0.12]
Threshold	-0.01	-0.03**
	[-0.04, 0.02]	[-0.07, -0.00]
Impatience	-0.02**	-0.05**
	[-0.04, -0.00]	[-0.09, -0.01]
Impatience measure:	Policy prediction	Above-median policy prediction
Base Case mean	.502	.502
# Individuals	1,969	1,969
# Observations	157,946	157,946

Notes: This table replicates Table 3 with an impatience index predicted out-of-sample with the following variables (and their interactions with above-median age, gender, and individual and household income): age; gender; labor participation; personal and household monthly income; household size; HbA1c; RBS; systolic and diastolic BP; BMI; waist circumference; walking speed; diagnosed diabetic or hypertensive; overweight; owns home; number of rooms and running water in home; has a bank account; hired help; number of scooters, cars, computers, smartphones, and mobile phones; mobile balance; hours of work on a weekday; consumes alcohol and cigarettes/bidis; has foot ulcer, rapid deterioration in eyesight, and pain or numbness in legs or feet. Significance levels: * 10%, ** 5%, *** 1%.