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

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Abstract

Many people are impatient. We develop 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 test and 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 in the face of impatience over effort, 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 showing theoretically that, 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 minumum 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.

Time-bundled threshold contracts should induce extra effort from all types of people with high discount rates over effort: time-consistent or time-inconsistent and, among time-inconsistent, sophisticated or naive (i.e., aware or unaware) about their own present bias. The contracts'

[1]This example assumes zero short-run discounting of payments for simplicity.

potential for effectiveness among naifs is particularly valuable, since naive time inconsistency is common (Mahajan et al., 2020) but naifs are difficult to motivate (Bai et al., 2021). Time-bundled thresholds work for naifs because they leverage current discount rates, which even naifs understand. That is, even naifs discount their future effort and will sell it at a discount today.

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).

An exercise incentive program for diabetics and prediabetics provides an apt context to assess the effectiveness of time-bundled contracts and increased payment frequency in tailoring incentives for impatience. The healthy behaviors (like exercise) that help prevent and manage diabetes and other lifestyle diseases feature short-run utility costs (such as effort costs) but only long-run benefits, making people with high discount rates over utility less likely to invest in them (Hunter et al., 2018). Indeed, evidence suggests that people with early-onset diabetes or high BMI have higher discount rates over utility than the general population (e.g., Courtemanche et al., 2014; Cobb-Clark et al., 2022), making them well-suited for assessing incentive strategies for mitigating impatience.

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 empirical 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 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. Customized incentives could be leveraged for a range of policy goals, such as savings, preventive health, and school attendance (e.g., Beaman et al., 2014; Breza and Chandrasekhar, 2019; Barrera-Osorio et al., 2011). 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 impact in our setting. While the finding is somewhat imprecise, we present multiple other pieces of evidence indicating 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

[2]We discuss other objectives (e.g., welfare maximization) later in the paper. This statement assumes linear benefits from compliance, which are likely in many contexts, including exercise (e.g., Banach et al., 2023).

[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).

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 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 our diabetic population. Average daily steps increase by roughly 20 percent during the intervention. Roughly 50% of the treatment effect on steps persists even after the intervention and payments end. Incentives also improve blood sugar control. Given the dearth of evidence-based lifestyle-change interventions for diabetics, our scalable program offers a promising strategy to combat diabetes in LMICs.

1.1 Contributions to the Literature

This paper's primary contribution is 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 time-preferences and dynamic incentives literatures in turn.

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. O'Donoghue and Rabin (1999b) theoretically explores incentive schemes to motivate time-inconsistent procrastinators to more quickly complete a task which requires effort only in a single period.[4] In contrast, our solution of time-bundled contracts hinges on the agent incurring effort in multiple periods. Carrera et al. (2020) examines whether larger time-separable

[4]They show that optimal single-period task incentives feature an increasing punishment for delay over time.

incentives upfront can help time-inconsistent people overcome startup costs, but finds no empirical evidence that they do. DellaVigna and Pope (2018) shows that decreasing the lag until payment does not significantly increase effort — an approach targeting impatience over *payment*, while time-bundled contracts target impatience over *effort*.

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, unlike time-bundled thresholds, commitment devices are only effective for sophisticated time-inconsistents, and can even be harmful for naifs (e.g., Bai et al., 2021; John, 2020). Our work broadens the arsenal by considering an approach that can succeed in settings with naivete or without commitment demand.

Dynamic Incentives We contribute to the literature on dynamic incentives, specifically on contracts that "defer compensation" to the future (as time-bundled contracts do).[5] This literature has been primarily theoretical, exploring reasons that time-bundled, deferred compensation contracts may be better or worse than time-separable (e.g., Lazear, 1979; Rogerson, 1985).

Our first contribution is empirical: despite the extensive theoretical literature, we conduct, to our knowledge, the first rigorous empirical comparison of the two types of contracts.[6] Our comparison explores their distributional impacts and the mechanisms driving their relative performance. This comparison is valuable because, while both contract types are popular, their relative performance is theoretically ambiguous.

Our second contribution is to introduce and test a new theoretical channel for the effectiveness of time-bundled contracts: 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).[7] Building on evidence that people discount utility and payments differently (e.g., Chapman and Elstein, 1995; Augenblick et al., 2015), we introduce domain-specific discounting and show that high agent discount rates over effort give principals an additional reason to prefer time-bundled contracts, even without traditional barriers to direct end-of-period compensation, thus broadening

[5]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.

[6]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.

[7]This is relevant for us as effort costs vary by period, making it hard to pay the exact effort cost each period. Under this theory, time-bundling's effectiveness arises because pay in early periods only motivates early effort, while later time-bundled pay can motivate effort in all periods. Another theory is that time-bundling manages marginal utility under concavity (Rogerson, 1985), which is less relevant here due to the small incentive amounts.

the applicability of these contracts beyond the commonly cited scenarios.[8]

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 examines the effectiveness of time-bundled contracts and shows that, under a broad range of assumptions, they are particularly effective when individuals have high discount rates over effort. We first specify the individual's problem and define the principal's goal: contract effectiveness. We then solve for effectiveness under a simple *base case* incentive contract which is linear across days, and therefore time-separable. Next, we examine the impact of a time-bundled contract, where the payment for future effort increases in current effort, focusing on a time-bundled *threshold* contract that pays only if a threshold level of effort is reached.

We present two key results applicable to various types of impatience, including time-inconsistent sophistication, time-inconsistent naivete, and time consistency. The first result is that the effectiveness of the time-bundled threshold contract relative to the linear contract is increasing in the discount rate over effort. While it is possible to find specific parameter values that are exceptions, this result holds in many typical and empirically relevant cases. Our second result is that the most effective time-bundled threshold contract is more effective than the most effective linear contract if the discount rate over effort is sufficiently high, and less effective if the discount rate over effort is low. While this result strengthens the first by speaking to the overall effectiveness of threshold and linear contracts rather than just heterogeneity, it requires more specific conditions such as assumptions about the effort cost distribution. Finally, we briefly explore high-frequency payments as a strategy to adjust incentives for impatience over payment rather than effort, demonstrating their effectiveness when payment discounting is high.

2.1 Set-Up

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. Define m_t as the payment made by the principal to the individual on day t; m_t depends on the individual's compliance decisions through time t.

To solve for compliance, we assume that individual choices on day t maximize the following reduced-form utility function:

[8] Our exploration of impatience and incentives relates to Jain (2012), which assumes identical discount rates for effort and money, showing that with quasi-hyperbolic discounting and barriers to immediate payment, firms can increase profits by offering two-period quotas. In contrast, our model with domain-specific discounting offers stronger and more general results: time-bundled contracts are more effective for agents with high effort discount rates, even without barriers to direct end-of-period compensation or quasi-hyperbolic discounting. Moreover, while Jain (2012) is purely theoretical, we empirically test and confirm our insights.

$$U = \mathbb{E}\left[\sum_{t=0}^{\infty} d^{(t)} m_t - \delta^{(t)} w_t e_t\right], \tag{1}$$

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, sophisticates accurately predict how their future selves will behave, while naifs may not.[9]

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.

Incentive Contract Structure and Compliance The contracts we consider pay individuals based on compliance over a sequence of T days. We call this sequence of days the payment period and index its days $t = 1, ..., T$. Payments are delivered on day T only.

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 $P = \frac{1}{T}\mathbb{E}[m_T]$.

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 P. Maximizing effectiveness is analogous to the standard contract theory approach of maximizing output net of wage payments subject to incentive compatibility

[9]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.

constraints.[10] 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. To do so, define *cost-effectiveness* as compliance divided by expected per-day payment, C/P. One can then easily show that 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.[11]

2.2 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. Total payment is therefore:

$$m_T^{\text{Base Case}} = m \sum_{t=1}^{T} w_t. \tag{2}$$

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

$$e_t < d^{(T-t)} m. \tag{3}$$

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 $\delta^{(t)}$.[12]

We will see that this observation does not hold for time-bundled contracts.

2.3 Time-Bundled Contracts and Impatience over Effort

We now examine the effect, relative to the Base Case, of making the contract time-bundled while maintaining the same payment period length. We pay particular attention to the relationship between the effectiveness of time-bundled contracts and the discount factor over effort. We first use a simple case to show the intuition for why time-bundled contracts are more effective when the discount factor is small. We then present key testable implications, which we label predictions. Appendix B presents our formal mathematical results, which we label propositions.

[10]This is a distinct objective from maximizing welfare, but is often used in practice. For example, in health, policymakers and insurance companies often want to maximize the total health benefits of a program relative to the program costs. We discuss the appropriateness of this objective in Section 5.5.

[11]This is true assuming effectiveness is positive. To see this, rewrite effectiveness as $C\left(\lambda - \frac{1}{(C/P)}\right)$.

[12]In the linear case, 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 $\delta^{(t)}$.

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; however, our testable predictions hold for other types of time-bundled contracts as well. In a threshold contract, if compliance is below K, no incentive is received. If compliance is above K, 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:

$$m_T^{\text{Threshold}} = \begin{cases} m' \sum_{t=1}^{T} w_t & \text{if } (\sum_{t=1}^{T} w_t \geq K) \\ 0 & \text{otherwise.} \end{cases} \tag{4}$$

Intuition: Threshold Effectiveness Decreases in δ We first consider a simplified case with $d = 1$, $T = 2$, $K = 2$, and effort costs that are weakly positive and known from day 1. On day 1 of the threshold contract, the individual complies if both the discounted (by $d = 1$) payment outweighs the present discounted cost of effort on days 1 and 2, and she expects to comply on day 2 if she complies on day 1. Day 1 compliance is hence:

$$w_1 = \begin{cases} 1 & \text{if } (e_1 + \delta e_2 < 2m') \text{ and (believe } w_2 \text{ will be 1 if } w_1 = 1) \\ 0 & \text{otherwise.} \end{cases} \tag{5}$$

Importantly, because future effort costs are discounted, the present discounted effort cost is lower — and day 1 compliance higher — the lower is δ. This holds for both sophisticates and naifs, as outlined in more detail in Appendix B.1.

Impatient people's higher compliance on day 1 underlies the threshold's greater effectiveness for them. On day 2, individuals comply if $w_1 = 1$ and the payment exceeds their effort costs:

$$w_2 = \begin{cases} 1 & \text{if } e_2 < 2m' \text{ and } w_1 = 1 \\ 0 & \text{otherwise.} \end{cases} \tag{6}$$

Impatient people's higher day 1 compliance thus leads to higher day 2 compliance as well. Their greater total compliance makes the contract more effective.[13]

Testable Predictions We next return to the full model with d unrestricted, $T \geq 2$ and $K \leq T$, and extend the intuition from the 2-period model to make two testable predictions in more general models. We first ask how the effectiveness of threshold contracts depends on impatience over effort. Appendix B.2 presents a series of propositions investigating this comparative static, which we summarize in the following prediction:

[13]Effectiveness follows from compliance since an increase in compliance without a decrease in cost-effectiveness implies higher effectiveness, and the Appendix B.2 propositions show that, depending on the cost distribution, threshold cost-effectiveness tends to be flat or decreasing with $\delta^{(t)}$.

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

Holding all else equal, compliance and effectiveness in time-bundled threshold contracts decrease in $\delta^{(t)}$ under a broad range of assumptions. In contrast, in time-separable contracts, both compliance and effectiveness are flat in $\delta^{(t)}$ (Observation 1). Thus, the lower $\delta^{(t)}$ is, the higher compliance and effectiveness are in a threshold relative to time-separable contract.

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 $\delta^{(t)}$. Making some additional assumptions for tractability, Appendix B.3 presents a series of propositions comparing both optimized threshold and separable (in particular, linear) contracts, and threshold and linear contracts offering the same payment per day (as in our experiment).[14] These are summarized in the following prediction:

Prediction 2 (The Level of Time-Bundled Threshold versus Time-Separable Linear Effectiveness by δ, $T = 2$)**.** *Holding all else equal, under many 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.

While the assumptions underlying Prediction 2 are more restrictive than those underlying Prediction 1, the prediction suggests that principals will often prefer threshold to linear contracts when individuals are sufficiently impatient over effort.

2.4 Payment Frequency and Impatience over Payment

We now briefly explore a strategy for improving the performance of incentives if people are impatient over payment rather than effort: increasing payment frequency. We return to the base case separable linear contract from equation (2) and analyze compliance under different payment frequencies (i.e., different T's). Appendix B.4 contains the proof.

Prediction 3 (Frequency)**.** *If agents are impatient over 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 financial payments ($d^{(t)} = 1$), then payment frequency does not affect compliance or effectiveness.*

[14] 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.

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 assumptions, 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.[15]

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 in various locations including government hospitals, markets, religious institutions, and parks, in order to recruit a diverse socioeconomic group. During the camps, trained 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.[16] After screening, eligible individuals were 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; we gathered step data by syncing the pedometers with a central database. Because syncing requires an internet

[15] 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*.

[16] 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.

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.[17] 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. We began screening in October 2016 and enrolled participants on a rolling basis from February–November 2017, with endline data collection launched in May 2017.

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.

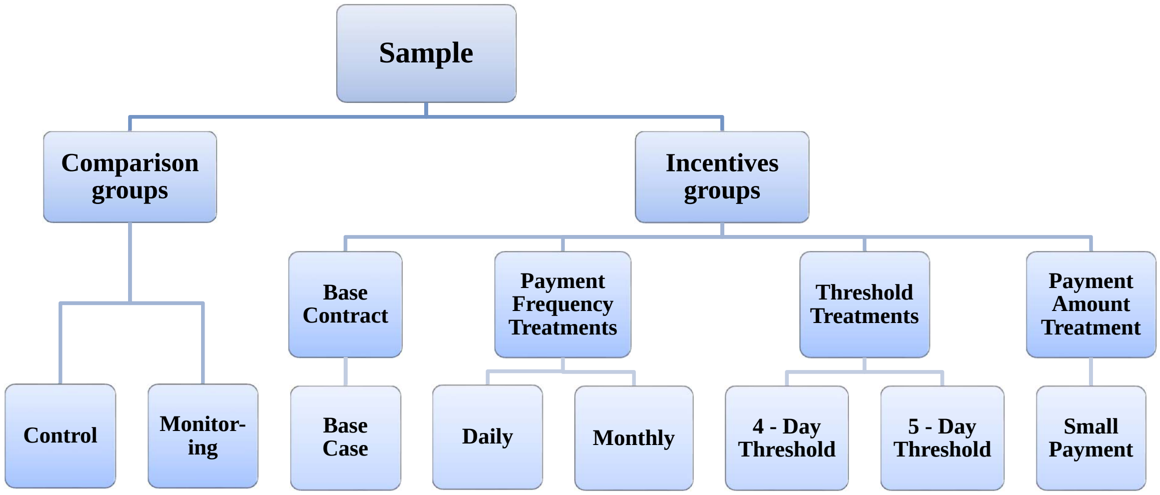
Within the incentive group, we randomized participants to receive one of six incentive contracts for walking, as shown in Figure 1.

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).[18] After reporting steps, participants immediately received text-message confirmations of their step

[17]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.

[18]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).



	Control	Monitoring	Base Case	Daily	Monthly	4 - Day Threshold	5 - Day Threshold	Small Payment
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

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.

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.[19] 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.[20]

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.

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 a heterogeneity analysis to test Prediction 1 about thresholds, but only a main effects analysis to test Prediction 3 about payment frequency.

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.

[19] 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.

[20] We pre-specified 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 was too high or too low (because the threshold was very easy or hard to reach) to have statistical power to test our prediction about heterogeneity by impatience.

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.[21]

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 the synced pedometer data, not the reported data.[22]

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 hereafter refer to this period as the post-intervention period. Participants no longer reported steps daily or received incentive payments,

[21]To accommodate a request from our government partners, we tested one additional intervention: weekly text message reminders to engage in 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.

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

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 full CTB module, the data were of such poor quality that we do not use them for analysis.[23] Our heterogeneity analyses instead leverage four other measures of impatience over effort collected during the time-preference survey, with relatively consistent results.

Although impatience measures tend to be noisy (Kremer et al., 2019), and ours may be particularly so, measurement error will bias our heterogeneity results toward zero. Thus, true heterogeneity across time preferences is likely even larger than what we measure.

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

[23]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 J.3, 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.

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 our impatience index by showing that it predicts an incentivized measure of effort impatience in Appendix C. 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 conducted 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.[24]

We began collecting the impatience index partway through the experiment,[25] so it is only available for the latter 55% 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 correlates in the expected direction with measures of impatient behavior.

Demand for Commitment: Our third impatience 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).[26] 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).

[24]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.

[25]We initially planned to only use measures estimated from the full CTB. We added the impatience index after challenges surfaced in our data collection which made the full CTB estimates unusable for analysis.

[26]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 distentangle behavior between impatient sophisticates and impatient naives.

The elicitation was incentive-compatible, as we assigned a small fraction of the sample to their selected contract for each choice.[27] 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. 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.

Simple CTB Questions: Our final 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 required respondents to select a preferred allocation between just two discrete points from such a budget.[28] 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.

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 setting and for use in robustness checks.

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.[29]

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 partici-

[27]This subsample is excluded from all analyses. To make this and related preference elicitations incentive-compatible, we used the strategy method: participants were presented with 11 different choices and informed that one of their choices would be randomly selected for implementation.

[28]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 J.2.

[29]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.

pants.[30] (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: 65% of the sample has been diagnosed with diabetes by a doctor, 81% 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, 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.

[30]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.

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
				(0.39)	(0.46)		(0.61)	(0.96)	(0.26)	(0.64)
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
				(0.23)	(0.84)		(0.62)	(0.76)	(0.06)	(0.42)
Per capita income (INR/month)	4465	3641	4488	-41	132	4477	-16	-410	122	-136
				(0.82)	(0.71)		(0.92)	(0.17)	(0.69)	(0.78)
Household size	3.91	1.62	3.94	-0.03	-0.11	3.89	0.07	0.03	-0.14	-0.31
				(0.69)	(0.37)		(0.34)	(0.81)	(0.31)	(0.14)
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)	(0.14)	(0.11)	(0.14)
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)	(0.87)	(0.14)
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)	(0.64)		(0.71)	(0.45)	(0.76)	(0.19)
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)	(0.51)		(0.80)	(0.76)	(0.99)	(0.15)
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)	(0.65)		(0.60)	(0.23)	(0.58)	(0.34)
Diastolic BP (mmHg)	88.48	11.10	88.54	-0.08	-0.01	88.19	0.26	1.11	0.41	1.81
				(0.88)	(0.99)		(0.60)	(0.24)	(0.65)	(0.20)
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)	(0.76)		(0.08)	(0.02)	(0.10)	(0.13)
BP: Hypertensive (=1)	0.49	0.50	0.46	0.04	0.05	0.49	0.00	0.04	0.02	-0.03
				(0.13)	(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
				(0.33)	(0.36)		(0.98)	(0.36)	(0.51)	(0.32)
BMI	26.42	4.34	26.52	-0.12	-0.05	26.47	-0.17	-0.06	-0.08	0.52
				(0.56)	(0.89)		(0.37)	(0.88)	(0.84)	(0.37)
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)	(0.46)	(0.11)	(0.27)
Average daily steps	7004	3981	7066	-68	-174	6810	268	236	639	208
				(0.71)	(0.58)		(0.14)	(0.49)	(0.06)	(0.68)
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)	(0.64)		(0.46)	(0.40)	(0.70)	(0.50)
Predicted index (SD's)	-0.05	1.00	0.00	-0.06	-0.15	-0.02	-0.06	-0.07	0.00	-0.09
				(0.23)	(0.05)		(0.17)	(0.42)	(0.97)	(0.46)
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)	(0.80)	(0.80)	(0.96)
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
				(0.06)	(0.35)		(0.50)	(0.33)	(0.15)	(0.15)
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)	(0.48)		(0.58)	(0.29)	(0.26)	(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
				(0.58)	(0.49)		(0.90)	(0.35)	(0.75)	(0.71)
F-tests for joint orthogonality										
p-value	.	.	.	0.11	0.85	.	0.89	0.56	0.79	0.44
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" columns show group means; "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.

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.[31]

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 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 the imbalance, we show robustness 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

[31] 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.

[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).

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 outcome variable for blood sugar is a standardized index of two measures: HbA1c (longer-term blood sugar control) and RBS (short-term blood sugar control). While our AEA registry prespecified HbA1c as our blood sugar measure, accurately measuring HbA1c in the field proved challenging.[33] Thus, we also measure RBS, another blood sugar measure strongly associated with diabetes severity (Bowen et al., 2015)[34] that is easier to accurately measure. Our measures of RBS and HbA1c have independent predictive power for blood sugar control,[35] so our preferred measure incorporates both, but we also present the measures separately.

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 impa-

[33] 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.

[34] RBS is sensitive to recent activity such as eating and is therefore problematic as a clinical measure, but yields a good measure of average glycemic control in a sample.

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

tience 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:

$$y_{it} = \alpha + \beta Incentives_i + \boldsymbol{X}_i' \gamma + \boldsymbol{X}_{it}' \lambda + \varepsilon_{it}, \tag{7}$$

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 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 non-diabetic 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

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	0.710	0.350	0.730	0.360
Monthly	0.180	0.650	0.910	0.190
Threshold	0.360	0.210	0.610	<0.001
Small Payment	0.040	0.060	0.030	0.180
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 (7) (Panel A) and (8) (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%.

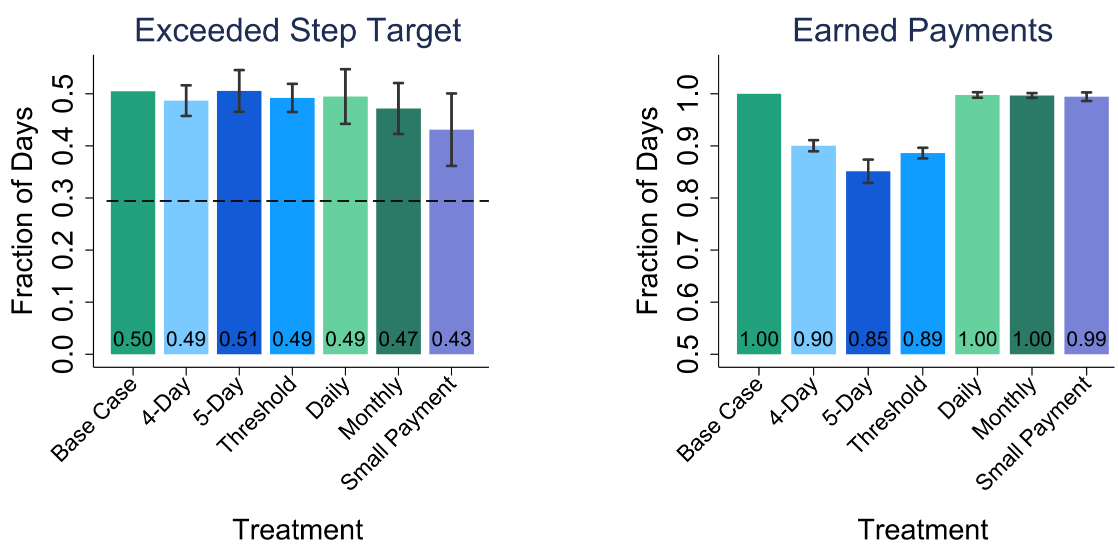
and cost-effectiveness in turn.

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:

$$y_{it} = \alpha + \sum_j \beta_j \times \left(incentives^j\right)_i + \boldsymbol{X}_i'\gamma + \boldsymbol{X}_{it}'\theta + \varepsilon_{it}, \tag{8}$$

where y_{it} are daily walking outcomes and $(incentives^j)_i$ 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=0.360). 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.



(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.

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 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).

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 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.4). 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

[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.

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. We also use machine learning to demonstrate the importance of impatience for predicting the Threshold effect.

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:

$$y_{it} = \alpha + \beta_1 Impatience_i \times Thresh_i + \beta_2 Thresh_i + \beta_3 Impatience_i + \boldsymbol{X}_i' \pi + \boldsymbol{X}_{it}' \theta + \varepsilon_{it}, \quad (9)$$

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 $Impatience_i$. Because some of the measures are estimated, we present bootstrap confidence intervals in the table as well as Gaussian standard errors and p-values in table notes when available.

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 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<0.05). 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 <0.01 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

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*
	[0.22, 7.38]	[0.39,12.60]	[1.00, 5.17]	[0.28, 10.01]	[0.42, 11.71]	[-0.70, 10.11]
Threshold	-1.30	-4.13*	-1.18	-3.41**	-4.29**	-3.78**
	[-5.24, 2.64]	[-8.80,0.54]	[-3.36, 0.93]	[-5.92, -0.47]	[-8.55, -0.02]	[-7.51, -0.05]
Impatience	-2.97**	-5.03*	-2.38***	-5.3**	-2.37	-2.67
	[-5.54, -0.39]	[-10.55,0.49]	[-3.88, -0.85]	[-8.13, -0.69]	[-7.74, 3.00]	[-6.05, 0.71]
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. Bootstrapped 95% confidence intervals are in brackets. Bootstrap draws were clustered at the individual level. For regressions using the predicted impatience index we conduct three steps in each bootstrap sample to construct the 95% confidence interval: 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 (9). The Gaussian standard errors and p-values for the column 1 $Impatience \times Threshold$ coefficient are 1.9 and 0.046, respectively; for column 2, 3.77 and 0.076; for column 5, 3 and 0.044; for column 6, 2.83 and 0.097. Controls are the same as in Table 2. Significance levels: * 10%, ** 5%, *** 1%.

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 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 (9) 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, and maintains at least 10% significance except when the sample size is small (columns 1 and 2). 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.[37] 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

[37]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.

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 control for risk aversion and "scheduling uncertainty" (the stated frequency with which unexpected events arise) and our proxies for impatience over payment (and all of their interactions with Threshold), which could all influence the performance of threshold contracts, among other controls.

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.

A potential confound that was difficult to measure at baseline (and hence which we do not control for) is the propensity for habit formation. However, we measure the propensity for forming habits at endline by assessing how much of the treatment effect of incentives persists after payments stop. Table F.10 in the Online Appendix reassuringly shows that impatience does not predict persistence, suggesting that the propensity to form habits is not correlated with impatience in our setting.

Even if omitted variables were affecting our Table 3 heterogeneity estimates, 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.

Machine learning also confirms the importance of impatience for driving the treatment effect of thresholds. First, using a causal forest (Athey et al., 2019), we find that the impatience index has a particularly strong signal in predicting the threshold effect, as it exhibits the highest variable "importance" among all covariates (Figure A.2). Second, Figure A.3 shows that the Chernozhukov et al. (2018) approach, which addresses multiple hypothesis testing and overfitting concerns, also identifies impatience as a statistically significant predictor of threshold effectiveness. While it is not necessary for us to use the Chernozhukov et al. (2018) approach (since we planned *ex ante* to analyze heterogeneity based on impatience), we find it reassuring that this robust method identifies impatience as a significant predictor.

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

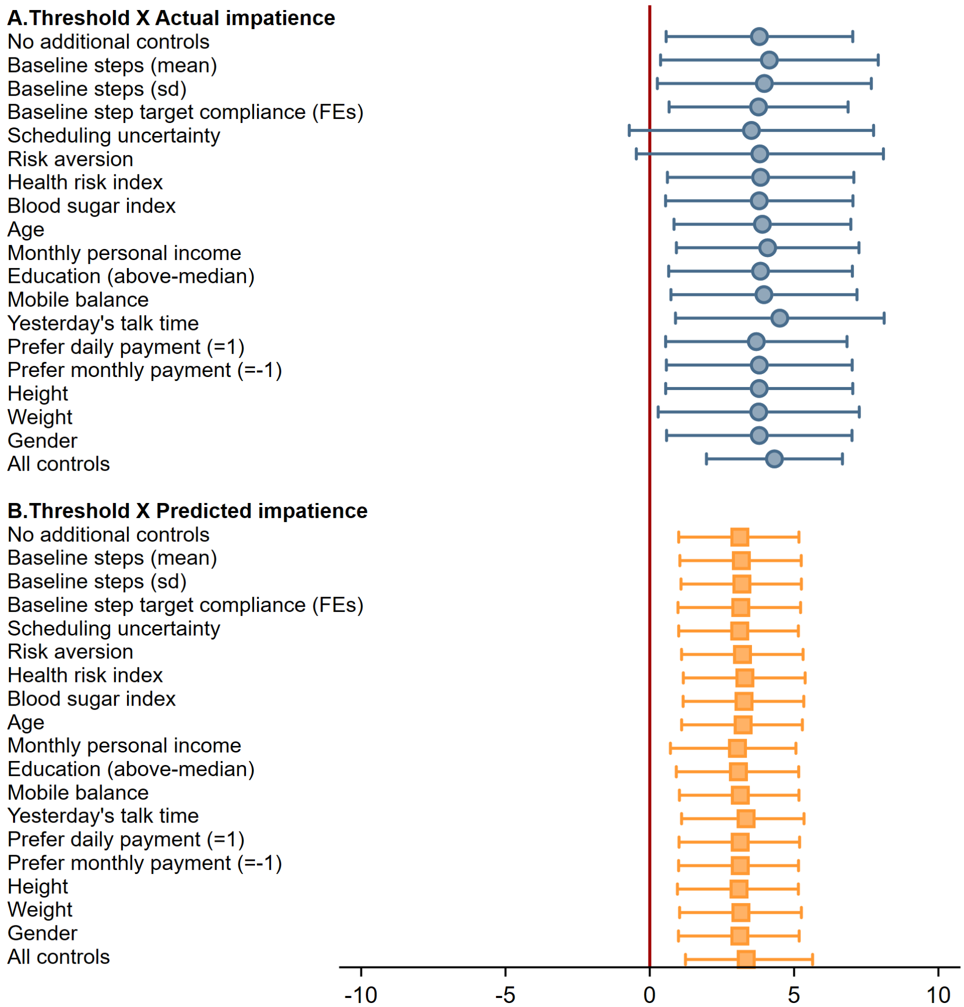


Figure 3: Threshold Heterogeneity by Impatience is Robust to a Variety of Controls

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. Income is winsorized at the 5th and 95th percentiles. 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. All confidence intervals are constructed via bootstrap, with bootstrap draws as in Table 3. See Online Appendix Figure F.1 for a version with our alternate impatience measures.

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 those with chronic disease (Wainwright et al., 2022) or 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.[38] Although participants might misreport their discount rates, evidence suggests that this type of misreporting is limited in the context of incentives for behavior change.[39] 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.

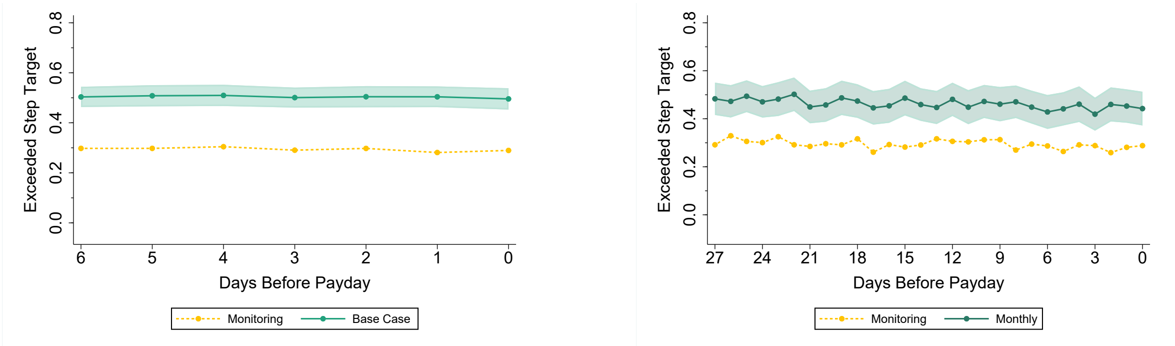
However, given the inherent noise in any impatience measure, the effectiveness of personalization depends on both the quality of the measure and the overall level of impatience in the population. In populations with high overall impatience, like ours, the benefits of personalization may be limited. Online Appendix H explores whether personalizing using any of our impatience measures improves effectiveness relative to applying the threshold universally. We find that the answer is ambiguous: personalization increases compliance but decreases cost-effectiveness. A promising approach to improve the performance of personalization could involve using multiple impatience measures to reduce exclusion errors. For example, assigning the threshold to those who rate highly on either of two measures proves more effective than relying on a single measure alone.

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

[38] 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.

[39] Dizon-Ross and Zucker (2023) explores an incentive program for steps in the same setting as this study. While that paper's goal is to evaluate the use of mechanism design to personalize step targets, and it does not implement any time-bundled contracts, it does show that participants do not manipulate their observable characteristics to avoid assignment to a financially-dominated contract with a higher step target.



(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.

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 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.4). 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.[40]

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).[41] 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

[40]Specifically, Online Appendix Table F.12 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.

[41]Cross-country preference surveys suggest that low impatience over payments is more common in higher-income countries but varies, with some lower-income countries exhibiting low impatience (Falk et al., 2023). Indeed, several studies in developing countries estimate small discount rates over payment (e.g., Mbiti and Weil (2013) in Kenya, and Tanaka et al. (2010) in Vietnam), although some find larger (Kaur et al., 2015).

challenges for time-bundled contracts, which require a delay between effort and payment.

5.5 Effectiveness and Welfare

This paper evaluates ways to increase contract effectiveness, a relevant objective in many situations. In firm and worker applications, maximizing effectiveness is often analogous to profit maximization. In public applications, policymakers are often concerned with maximizing effectiveness, perhaps because it is straightforward to explain and justify. Moving from effectiveness to welfare involves an understanding of objects such as the social cost of public funds which are beyond the scope of this paper. That said, if the marginal social benefit of the incentivized behavior outweighs the marginal social cost in the base case version of a program — as appears to be true in our case[42]— then variations that increase compliance and effectiveness will likely increase social welfare.

One potential concern with our time-bundled threshold contract would be if it improved effectiveness or welfare but was not Pareto improving, instead decreasing some individuals' welfare relative to a no-incentives benchmark. This concern is particularly vivid in light of evidence that commitment contracts can decrease welfare among partially naive individuals who pay upfront for commitment but fail to follow through (e.g., Bai et al., 2021).

Even though individuals do not pay upfront for threshold contracts, there is a potentially analogous issue. Naifs may comply on the early days of a threshold contract (a form of paying upfront) but fail to receive compensation because they do not follow through on the later days. However, there are theoretical reasons to doubt that this would happen much in practice.[43] Empirical evidence also suggests that our threshold contract did not reduce participants' welfare. At endline, most participants expressed interest in continuing the program, with no significant difference between the threshold and other groups, nor between more and less impatient participants in the threshold groups. Additionally, there is no heterogeneity by impatience in the likelihood of complying but not being paid under the threshold.[44]

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.

[42]The estimated social benefits of walking include both large private health benefits (e.g., Warburton et al., 2006; Banach et al., 2023) and positive fiscal externalities such as reduced public healthcare costs (e.g., Sangarapillai et al., 2021), and are estimated to be large relative to the social costs of these interventions.

[43]Specifically, later compliance costs must be much larger than earlier costs for lack of follow through to be an issue: as the compliance approaches the threshold, the incentives for marginal compliance become more and more high powered. See Appendix B.1 and especially footnote 52 for more detail.

[44]Our effectiveness results are specific to time-bundled dynamic contracts and do not hold for all dynamically non-separable contracts. For example, Carrera et al. (2022) empirically analyzes a threshold-style contract that differs from ours in (a) not paying "on the margin" for compliance exceeding the threshold and (b) using a low threshold level (K/T). Both features imply that, in that contract, current effort often *decreases* the payment for future effort and hence make the contract not time-bundled.

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.4 estimates equation (7) 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 (7) using the pedometer data from the 12 weeks after the intervention ended.[45]

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).[46] Figure A.5 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:

$$y_i = \alpha + \beta_1 Incentives_i + \beta_2 Monitoring_i + \boldsymbol{X}_i'\gamma + \varepsilon_i, \tag{10}$$

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

[45] 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.

[46] 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 K suggests that monitoring does not affect steps.

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 our 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.13 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.14 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.[47] As expected, the estimated health improvements are larger 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.15 examines whether the intervention had coincident impacts on mental health or fitness. Incentives improve the mental health index by 0.10 SD. In contrast,

[47]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 15, also outlines this analysis (see the first bullet of Hypothesis 1).

we find no effects on physical fitness, 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.16).

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.[48] In addition, an exploratory analysis shows that the treatment effects on RBS grow over the intervention period (Figure A.6), 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).[49] In contrast, our intervention is scalable, low-cost, and induces lasting behavior change, with the potential to generate health savings that exceed program costs.[50] 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 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

[48]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.

[49]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).

[50]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).

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 of 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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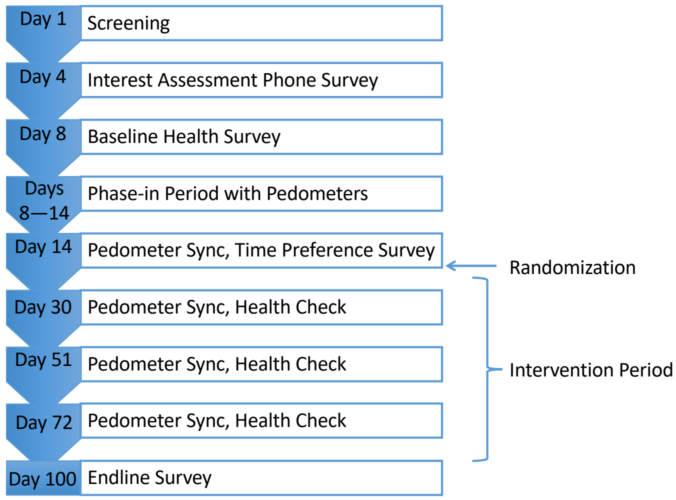
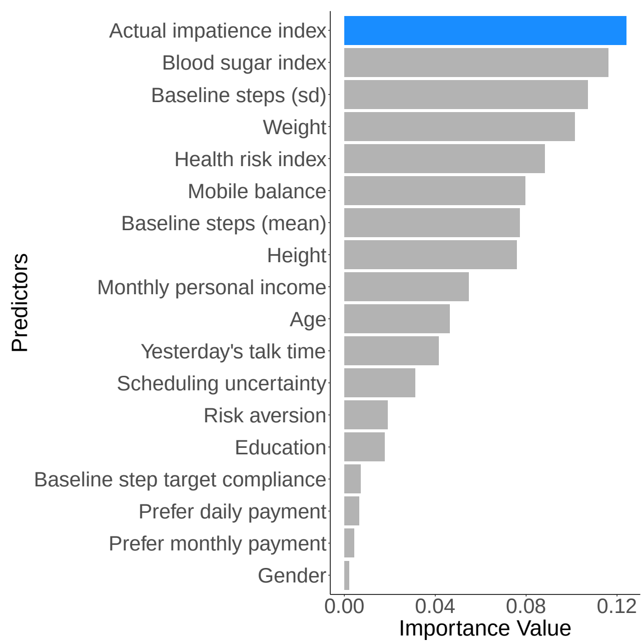
Appendices

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



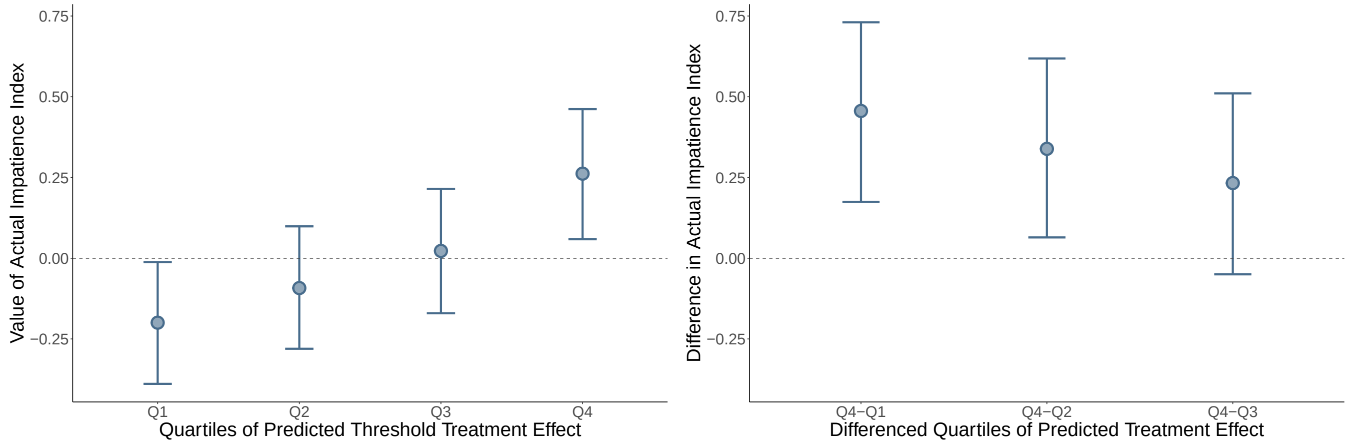
Appendix Figure A.1: Experimental Timeline for Sample Participant

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.



Appendix Figure A.2: Causal Forest Selects Impatience Index as Important Predictor of Threshold Effect

Notes: This figure displays the importance of each predictor included in the causal forest prediction of the Threshold treatment effect on average compliance at the individual level. Variable importance is a weighted sum of the number of splits on the variable of the causal forest at each depth. We limit the sample to those we collected the impatience index from. Predictors include the actual impatience index and controls shown in Panel A of Figure 3, except that this analysis uses continuous versions of the baseline compliance and education variables (because the importance analysis more naturally handles continuous variables). Missing values of predictor variables are imputed with the treatment-group mean; we also include an indicator for whether each variable is missing (each of which the analysis assigned importance values of 0). We implement the Causal Forest using the GRF package in R (Tibshirani et al., 2023). Results for the other impatience measures are shown in Online Appendix Figure F.2.

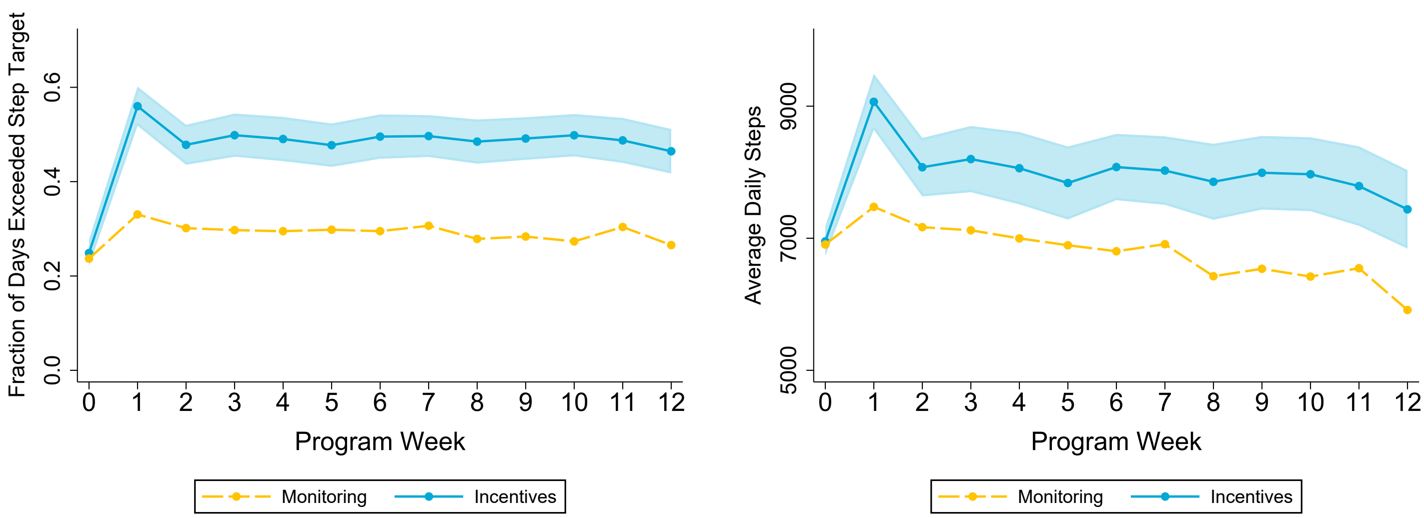


(a) Average Impatience Increases with Predicted Threshold Treatment Effect

(b) Impatience Significantly Higher in Highest vs. Lowest Quartiles of Predicted Threshold Effect

Appendix Figure A.3: Classification Analysis Shows Impatience Varies With Predicted Threshold Effect

Notes: The figure displays heterogeneity in the actual impatience index across quartiles of the predicted treatment effects of Threshold, following the method in Chernozhukov et al. (2018) Panel (a) shows the value of the actual impatience index in each quartile of predicted Threshold treatment effects with 95% confidence intervals, showing that the average level of the impatience index is increasing in the predicted Threshold effect. Panel (b) shows the difference in the actual treatment effect between the most-affected quartile and the other three quartiles with 95% confidence intervals, showing that the difference in the impatience index between the top and bottom quartiles of predicted Threshold effect is statistically significant. We conduct the classification analysis using the GenericML package in R (Welz et al., 2022), which selects the method of best fit among lasso, random forest, and support vector machine; support vector machine is selected in our case. Predictors include the controls shown in Figure 3 and indicators for whether each variable is missing. Qualitatively similar results for the other impatience measures are shown in Online Appendix Figure F.3.

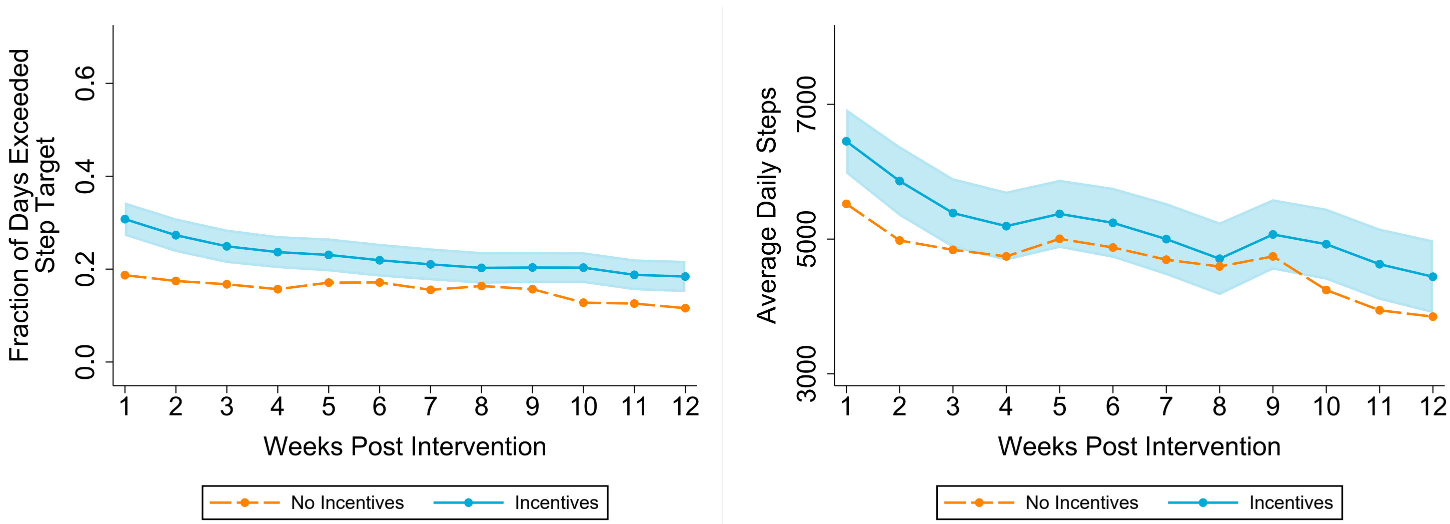


(a) Step-Target Compliance

(b) Daily Steps Walked

Appendix Figure A.4: 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.

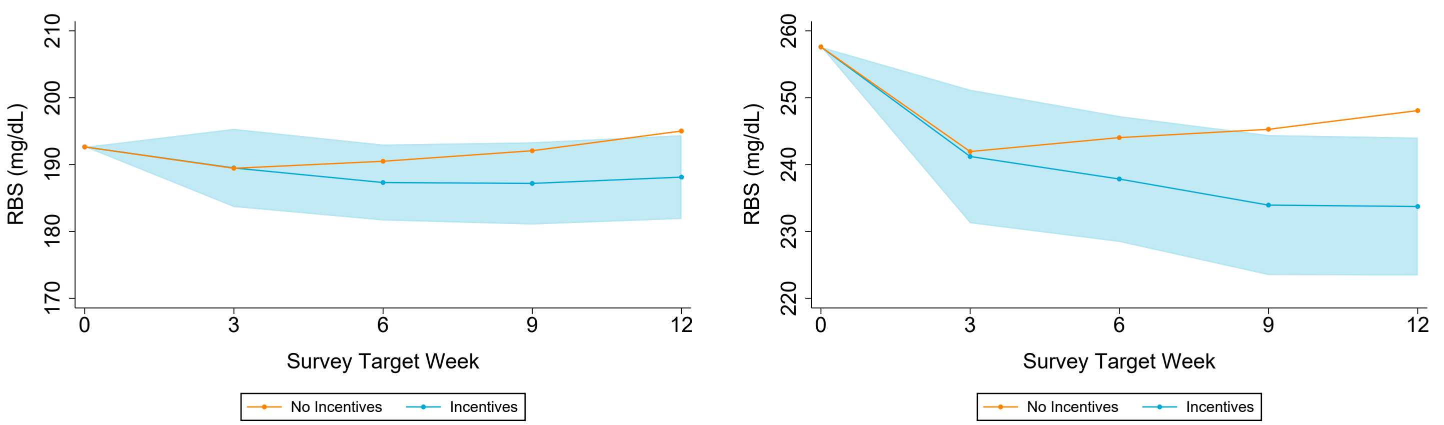


(a) Step-Target Compliance

(b) Daily Steps Walked

Appendix Figure A.5: 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.6: 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. Other impatience measures							
Chose commitment	0.485	0.045	-0.005	-0.027	0.011	0.015	2,871
Simple CTB	0.532	-0.120***	-0.028	-0.003	-0.018	-0.020	3,190

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 index components. 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**
	[-3.49, 9.48]	[-8.20,16.08]	[-0.35, 5.73]	[1.21, 13.24]	[1.35, 14.80]
Threshold	-5.29**	-6.79*	-3.97**	-6.5***	-8.50***
	[-10.06, -0.52]	[-13.86,0.28]	[-7.12, -1.06]	[-11.24, -3.16]	[-13.71, -3.29]
Impatience	-3.11*	-6.70	-1.56	-5.15**	-4.06
	[-6.68, 0.46]	[-15.59,2.20]	[-3.74, 0.67]	[-9.39, -0.30]	[-9.23, 1.12]
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. Bootstrapped 95% confidence are in brackets; 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
	[-0.03, 0.01]	[-0.04,0.02]	[-0.01, 0.01]	[-0.01, 0.02]	[-0.00, 0.04]	[-0.01, 0.01]
Threshold	-0.114***	-0.109***	-0.115***	-0.119***	-0.127***	-0.115***
	[-0.13, -0.10]	[-0.12,-0.10]	[-0.12, -0.11]	[-0.13, -0.11]	[-0.15, -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]	[-0.00, 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. Bootstrapped 95% confidence are in brackets; 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

B.1 Building Intuition: Time-Bundling for Sophisticates and Naifs

The effectiveness of time-bundled contracts decreases in δ for both sophisticates and naifs. However, the exact conditions for compliance on day 1 differ. To build intuition, return to our 2-day example from Section 2.3 and note that, on day 1 of the threshold contract, the individual's motivation to comply is to have the opportunity to be paid $2m'$ for complying on day 2. Letting $w_{2,1}$ be her prediction on day 1 about her compliance on day 2, the value of this opportunity to her is

$$(2m' - \delta e_2) w_{2,1}\big|^{w_1=1}, \tag{11}$$

which is equal to the discounted (by $d = 1$) payment $2m'$ net of the discounted effort costs δe_2 if the individual thinks she will comply on day 2 given compliance on day 1 (i.e., if $w_{2,1}\big|^{w_1=1} = 1$). (This is simply a re-expression of equation (5).)

For sophisticates, who correctly predict their future preferences,

$$w_{2,1}\big|^{w_1=1} = \mathbb{1}\{e_2 < 2m'\}. \tag{12}$$

Thus, for a sophisticate to place a positive value on a day 2 work opportunity (i.e., for expression (11) to be positive), it must be that $e_2 < 2m'$: the payment for day 2 work must be sufficiently large to entail a soft *commitment* for the day 2 self to follow through. The sophisticate complies on day 1 to give her future self strong incentives to comply.

In contrast, naifs believe that their day-2 selves have the same preferences as their day-1 selves. For them,

$$w_{2,1}\big|^{w_1=1} = \mathbb{1}\{\delta e_2 < 2m'\}. \tag{13}$$

Thus, naifs place a positive value on the day 2 opportunity as long as it has positive net present value (NPV) from the day 1 perspective (i.e., as long as discounted payments net of discounted effort costs, $2m' - \delta e_2$ are positive). That is, naifs positively value any lucrative day 2 *option* that they *want* their day 2 selves to execute. Naifs comply on day 1 to give their day 2 selves the *option* to follow-through.[51]

With time-bundled thresholds, these differences in motivation between sophisticates and naifs should not normally affect behavior. The day 2 opportunities that are lucrative enough options to motivate naifs to comply on day 1 are also generally associated with high enough day 2 payments to provide a soft commitment for day 2 compliance. Likewise, any day 2 opportunity that provides a soft commitment that motivates a sophisticate to comply on day 1 will also provide an option that motivates a naif to comply on day 1 (i.e., equation (12) implies equation (13)).[52] By pairing the options that motivate naifs with the commitment that both motivates

[51] In either case, Prediction 1 still holds because the equation (11) value is still weakly decreasing in δ. The equation (11) value is $(dM - \delta e_2)\mathbb{1}\{e_2 < M\}$ for sophisticates and $(dM - \delta e_2)\mathbb{1}\{\delta e_2 < dM\}$ for naifs, both of which are decreasing in δ. To see this in the naive case, note that $(dM - \delta e_2)\mathbb{1}\{\delta e_2 < dM\} = \max\{dM - \delta e_2, 0\}$.

[52] Equations (5), (12), and (13) show that the only difference between sophisticates and naifs is that, if $e_1 + \delta e_2 < 2m$ and $e_2 > 2m$, naifs would comply on day 1 and then fail to follow-through on day 2, while sophisticates would not comply on day 1 because they know they would not follow-through on day 2. However, this behavior should

sophisticates and helps naifs follow through, thresholds work for both types.[53]

B.2 Time-Bundled Threshold Contracts and Impatience Over Effort

In this section, we present a series of propositions that provide the theoretical underpinning for Prediction 1. The propositions demonstrate that, holding all else equal, both compliance and effectiveness in threshold contracts tend to decrease in $\delta^{(t)}$. In contrast, in time-separable contracts, compliance and effectiveness are flat in $\delta^{(t)}$ (Observation 1). Thus, the lower $\delta^{(t)}$ is, the higher compliance and effectiveness are in a threshold relative to time-separable contract.

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 δ. 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, and especially when either (a) K is high relative to T,[54] 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.

We begin with a more formal presentation of the agent's problem from Section 2.1, which is helpful for proving our propositions. We next present the propositions underlying Prediction 1.

B.2.1 Agent Problem

Given the notation and assumptions in Section 2.1, we can express the agent's problem as follows. Let $w_{t,j}$ be the agent's prediction on day j about her compliance on day $t > j$. On day t, the agent chooses compliance, w_t, to maximize expected discounted payments net of effort costs:

be rare, as it requires that $e_2 > e_1/(1-\delta)$, which implies that $e_2 >> e_1$ and/or that δ is very low. The intuition for why, conditional on day 1 compliance, it is rare to not follow through on day 2 is that people sink costs as they approach the threshold. Thus, the marginal incentive to comply is strictly higher on day 2 than on day 1.

[53]In contrast, in some types of time-bundled contracts (other than thresholds), the differences in the decisions of naifs and sophisticates may be greater. For example, we have investigated the full class of 2-day time-bundled contracts. This class also includes contracts where the day 2 wage is not 0 in the absence of day 1 compliance (e.g., a contract paying \$5 for day 2 effort if the agent did not comply on day 1 and \$10 if she did). In such contracts, *options* and *commitment* can be less tightly linked. For example, some time-bundled contracts that are not thresholds function like commitment contracts and are more effective for sophisticates, as day 1 compliance generates a soft commitment for day 2 compliance but not a positive NPV option. In others, day 1 compliance generates an option but not a commitment, and they are more effective for naifs. We have explored all of the non-threshold time-bundled contracts in the 2-day contract space and, importantly, the result that performance is weakly decreasing in δ holds for all of them, not just for time-bundled thresholds.

[54]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.

$$\max_{w_t \in \{0,1\}} \mathbb{E}\left[d^{(T-t)} m_T - \sum_{j=t+1}^{T} \delta^{(j-t)} w_{j,t} e_j \middle| e_1, .., e_t, w_1, .., w_t \right] - w_t e_t, \tag{14}$$

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:

$$V_t(0) < V_t(1) - e_t \tag{15}$$

That is, on day t, the agent complies if the continuation value of complying net of the effort cost is greater than the continuation value of not complying.

B.2.2 Propositions underlying Prediction 1

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 I.1.

Recall that the condition for complying on day 1 is to comply if $e_1 < V_1(1) - V_1(0)$ (equation (15)). 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:

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

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:

$$\begin{aligned} V_1(1) - V_1(0) &= \mathbb{E}\left[(dM - \delta e_2) w_{2,1} | e_1, w_1 = 1\right] - \mathbb{E}\left[-\delta e_2 w_{2,1} | e_1, w_1 = 0\right] \\ &= \mathbb{E}\left[(dM - \delta e_2) \mathbb{1}\{e_2 < M\} + \delta e_2 \mathbb{1}\{e_2 < 0\} | e_1\right] \end{aligned} \tag{17}$$

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 (17) 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 $e_2 < M$, and 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:

$$\begin{aligned} V_1(1) - V_1(0) &= \mathbb{E}\left[(dM - \delta e_2) w_{2,1} | e_1, w_1 = 1\right] - \mathbb{E}\left[-\delta e_2 w_{2,1} | e_1, w_1 = 0\right] \\ &= \mathbb{E}\left[(dM - \delta e_2)\mathbb{1}\{\delta e_2 < dM\} + \delta e_2 \mathbb{1}\{\delta e_2 < 0\} | e_1\right] \\ &= \mathbb{E}\left[\max\{dM - \delta e_2, 0\} + \delta e_2 \mathbb{1}\{e_2 < 0\} | e_1\right] \end{aligned} \tag{18}$$

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 (18) 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.[55] 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,[56] 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.

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 (17) and (18), we know that the value of

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

[56] See equation (22) 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.

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:

$$\begin{aligned} C/P &= \frac{1}{M}\frac{\mathbb{E}[w_1 + w_2]}{\mathbb{E}[w_1 w_2]} = \frac{1}{M}\frac{\mathbb{E}[w_1 + w_1 w_2]}{\mathbb{E}[w_1 w_2]} = \frac{1}{M}\left(\frac{\mathbb{E}[w_1]}{\mathbb{E}[w_1 w_2]} + 1\right) = \frac{1}{M}\left(\frac{\mathbb{E}[w_1]}{\mathbb{E}[w_1]\mathbb{E}[w_2 | w_1 = 1]} + 1\right) \\ &= \frac{1}{M}\left(\frac{1}{\mathbb{E}[w_2 | w_1 = 1]} + 1\right) \end{aligned} \tag{19}$$

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 $\lambda C - P$.)

$$\begin{aligned} 2(\lambda C - P) &= \lambda \mathbb{E}[w_1 + w_2] - M\mathbb{E}[w_1 w_2] \\ &= \lambda (F(e^*) + \mathbb{E}[w_1 \mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}]) - M\mathbb{E}[w_1 \mathbb{1}\{e_2 < M\}] \\ &= \lambda (F(e^*) + \mathbb{E}[\mathbb{1}\{e_1 < e^*\}\mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}]) - M\mathbb{E}[\mathbb{1}\{e_1 < e^*\}\mathbb{1}\{e_2 < M\}] \\ &= \lambda (F(e^*) + Prob(e_1 < e^*, 0 < e_2 < M) + Prob(e_2 < 0)) - MProb(e_1 < e^*, e_2 < M). \end{aligned} \tag{20}$$

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 $g'(e^*) = f(e^*)\,Prob(e_2 \in S|e_1 = e^*)$.[57] 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:

$$\lambda(1 + Prob(0 < e_2 < M|e_1 = e^*)) \geq MProb(e_2 < M|e_1 = e^*) \tag{21}$$

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

$$Prob(e_2 < 0|e_1 = e^*) \leq \frac{\lambda}{M} + \left(\frac{\lambda}{M} - 1\right) Prob(0 < e_2 < M|e_1 = e^*)). \tag{22}$$

If $\lambda > M$, condition (22) 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 (22) 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, let $\delta^{(t)} < 1$ for all $t > 0$ if $\delta^{(t)} < 1$ for any t. 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 I.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.

[57] 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 I.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 I.1. □

B.3 The Effectiveness of Threshold and Linear Contracts

In this section, we compare the effectiveness of time-bundled threshold and time-separable linear contracts under a range of effort cost assumptions, paying particular attention to how the relative effectiveness of thresholds depends on δ. For simplicity, throughout the section, we assume that $T = 2$ and that $K = 2$ and denote the threshold payment as M (i.e., $M = 2m'$).

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 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).[58]

[58] 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.

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,[59] *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 I.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 I.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.*

Proof. See Online Appendix I.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 $1 \leq M < 2$,[60] *then the threshold contract is more effective.*

Proof. See Online Appendix I.2. □

[59] $F_{e_2|e_1}(x)$ is weakly decreasing in e_1 for all x, with $F_{e_t|e_{t'}}(x)$ the conditional CDF of e_t given $e_{t'}$. This assumption flexibly accommodates the range from IID to perfect positive correlation, just ruling out negative correlation.

[60] Note that the principal would never pay $M > 2$ since $M = 2$ achieves 100% compliance regardless of δ.

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,*[61] *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 I.2. □

B.4 Proofs of Predictions Regarding Frequency

In this subsection, we first prove Prediction 3 from Section 2.4. Next, we present and prove a second prediction 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.

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.*[62]

Proof. Equation (3) 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 $\frac{1}{m}$). □

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. □

[61] See proof in Online Appendix I.2 for specific ranges for both naifs and sophisticates.

[62] Although linear utility is necessary for the stark prediction for patient agents, it is not necessary for the prediction that the impact of higher-frequency payments is increasing in the discount rate over payments.

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").[63] The validation sample was randomly selected from a later evaluation of a similar incentive program for exercise (Dizon-Ross and Zucker, 2023) with nearly identical recruitment criteria,[64] 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.18).

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.[65] 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.

[63]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).

[64]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.

[65]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).[66] (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 0.565, 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.[67] 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.

[66] 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).

[67] 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}	0.565	0.556	0.997	0.998	0.178	0.367
	[0.133]	[0.153]	[0.008]	[0.007]	[0.157]	[0.208]
P-value: $\delta_{QH} = 1$	0.001	0.004	0.737	0.803	<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	0.061	<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.

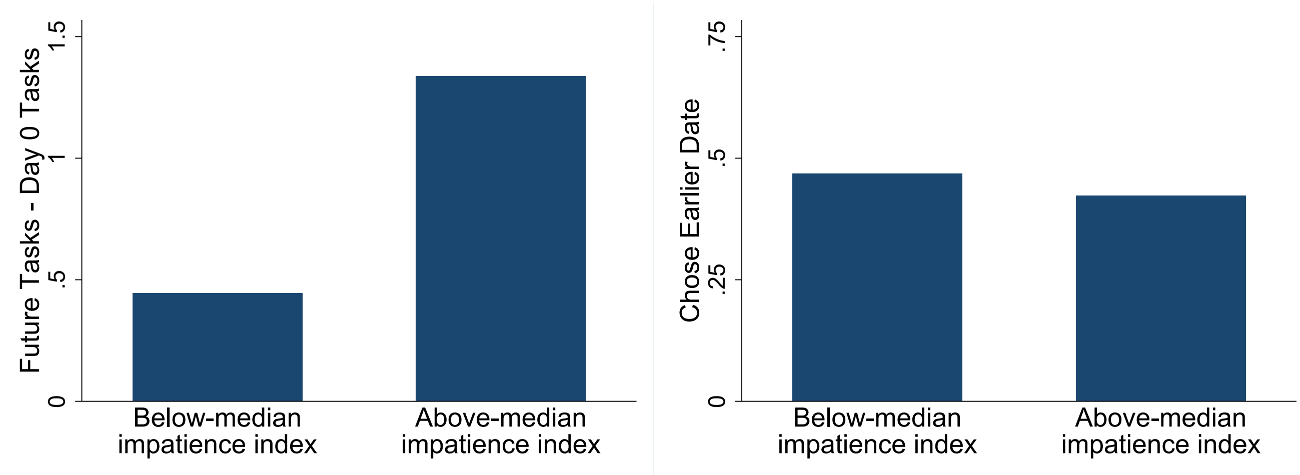
C.1.3 The Impatience Index Correlates with Effort Impatience Measures

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 0.997 and statistically indistinguishable from 1 (column 3). In contrast, we estimate that δ_{QH} is 0.178 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:

$$EffortImpatience_{itw} = \beta_0 + \beta_1 ImpatienceIndex_i + \beta_2 y_{i0w} + \tau_w + \tau_t + \varepsilon_{itw} \tag{23}$$

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.[68] 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

[68] 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 (23) 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 + Mono-tone	All	Changers + Mono-tone	All	No vio-lations	All	No vio-lations
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 × 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., $\delta^{(t)} = \delta^{t}_{Exp}$).[69]

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.12), 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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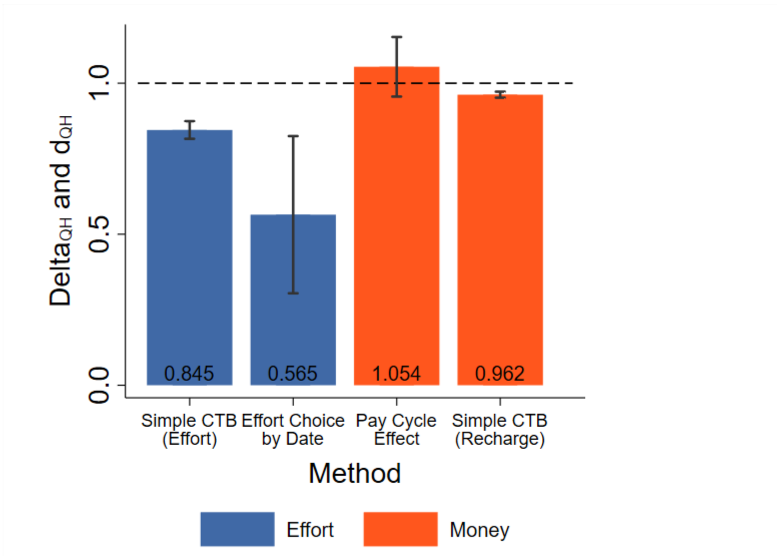
[69]The quasihyperbolic CTB discount factor over recharges is estimated with the equation $\ln\left(\frac{c_t+\omega_1}{c_{t+k}+\omega_2}\right) = \frac{\ln(d_{QH})}{\alpha-1}\mathbb{1}_{t=0} + \frac{1}{\alpha-1}(1+r)$ where c_t is money in the earlier period, c_{t+k} is money in the later period, ω_1 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 0.565 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 0.042 and 0.052, 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 0.953 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%.

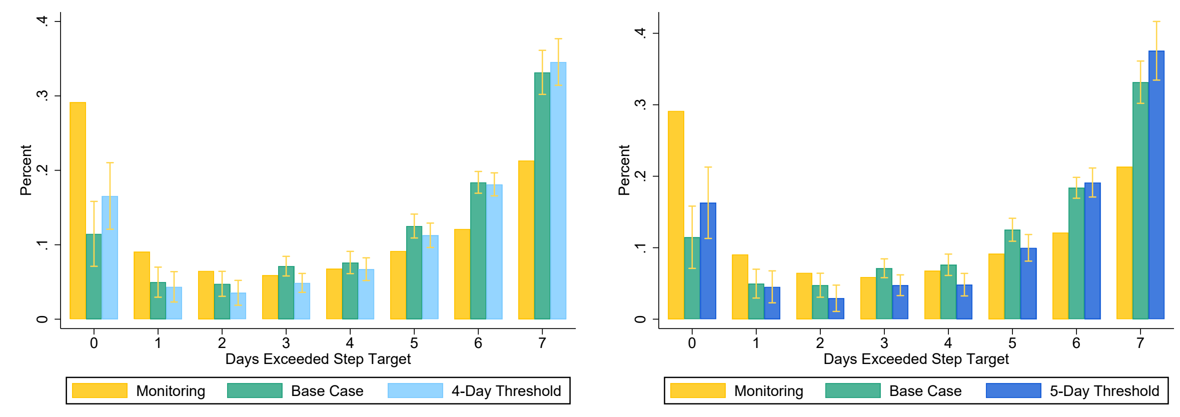
Table C.3 shows that there is no significant or meaningful correlation between any of the measures of impatience over effort and impatience over payment collected in the experimental sample. Similarly, we find that the correlation of the individual-level averages of the recharge impatience and effort 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.4 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.3 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).[70]



(a) 4-Day Threshold (b) 5-Day Threshold

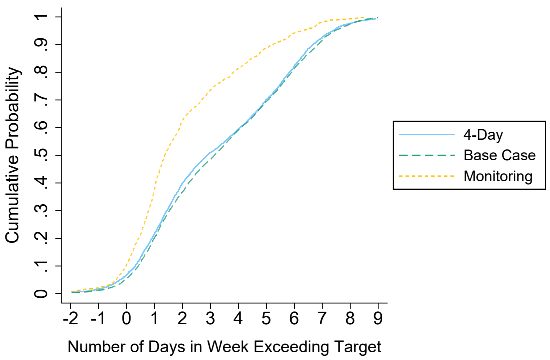
Appendix Figure D.3: Thresholds Modestly Decrease Compliance Right Below the Threshold

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 that, because the data are at the weekly level, we do not control for day-of-week fixed effects. See Online Appendix Table F.17 for the regressions.

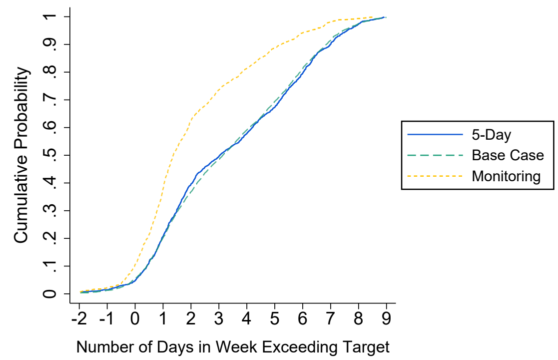
However, the magnitude of these differences are relatively small (especially compared to the differences from Monitoring), leading to only slight differences between Base Case and Threshold in the overall distribution of weekly compliance. Specifically, Panels (a) and (b) of Figure D.4 show the cumulative 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.4 shows similar results for the distribution of individual-level (instead of individual × week-level) compliance. Quantile regressions reveal no significant differences between the

[70]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.

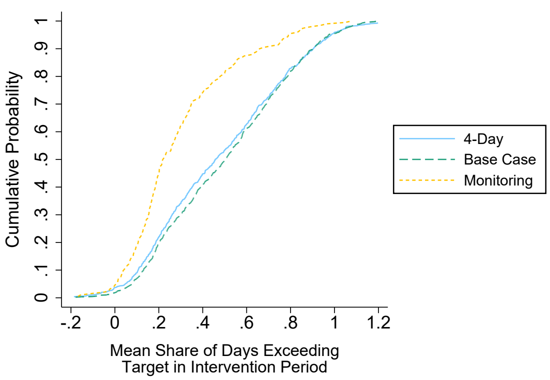
threshold groups and Base Case in the 25th, 50th, or 75th percentiles of the distributions of either individual × week-level or individual-level compliance (see Online Appendix Table F.7). Kolmogorov-Smirnov (KS) tests for the equivalence of the individual-level distributions also fail to reject the null of equal distributions (p-values 0.238 and 0.852 for the 4- and 5-Day Threshold, respectively, relative to Base Case).



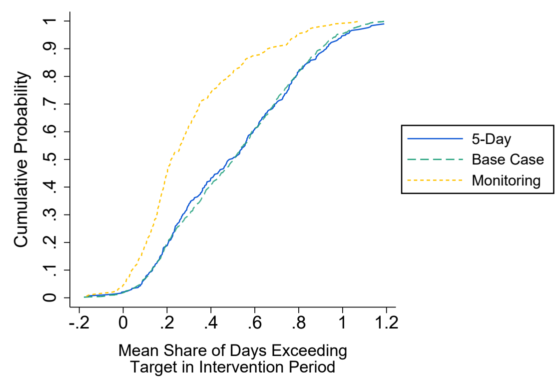
(a) 4-Day Threshold, Weekly Compliance



(b) 5-Day Threshold, Weekly Compliance



(c) 4-Day Threshold, Overall Compliance



(d) 5-Day Threshold, Overall Compliance

Appendix Figure D.4: 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 (9). 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%.

Online Appendix for:

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

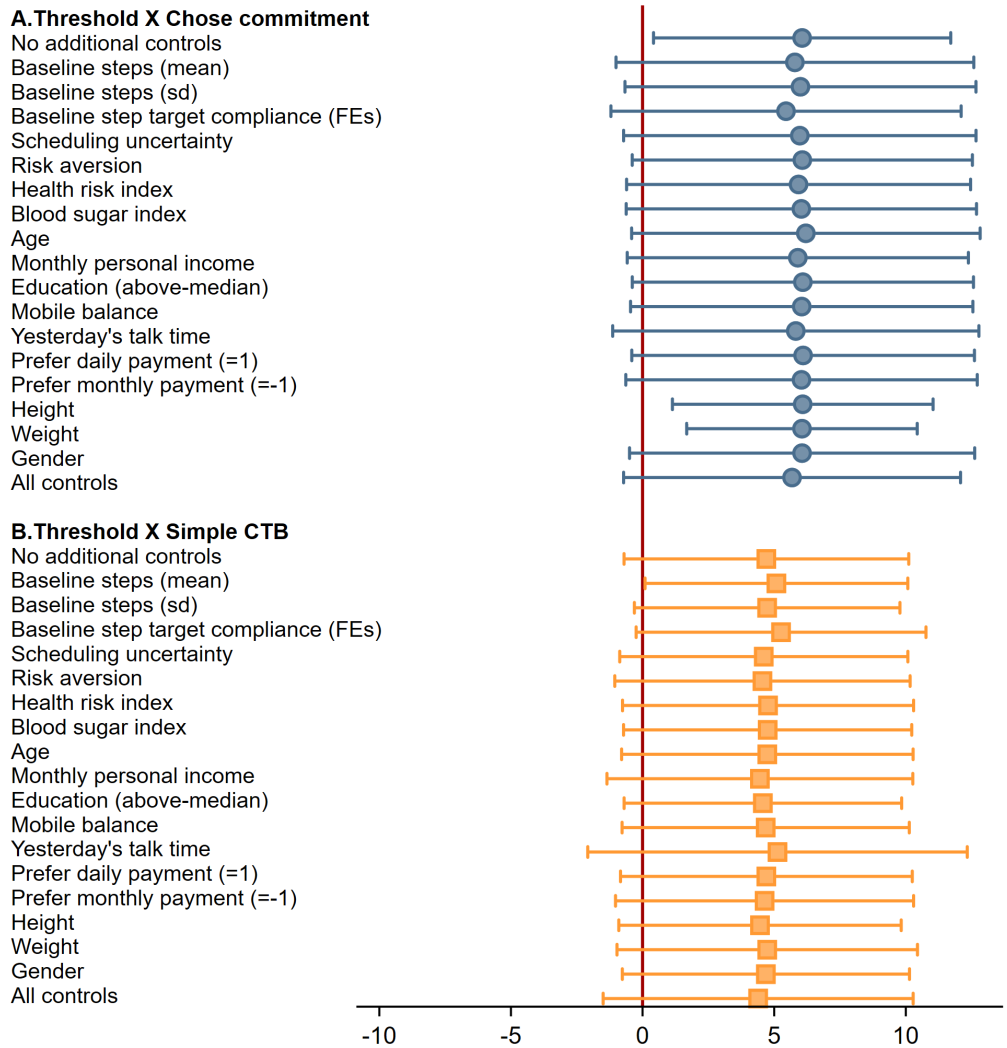
Shilpa Aggarwal
Indian School of Business

Rebecca Dizon-Ross
University of Chicago

Ariel Zucker
UC Santa Cruz

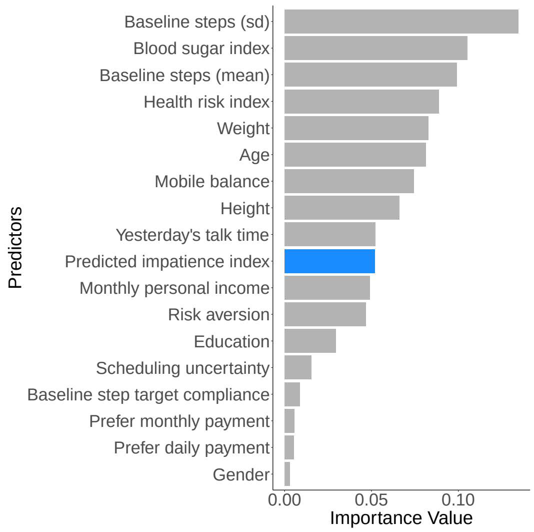
Table of Contents

F Additional Tables and Figures

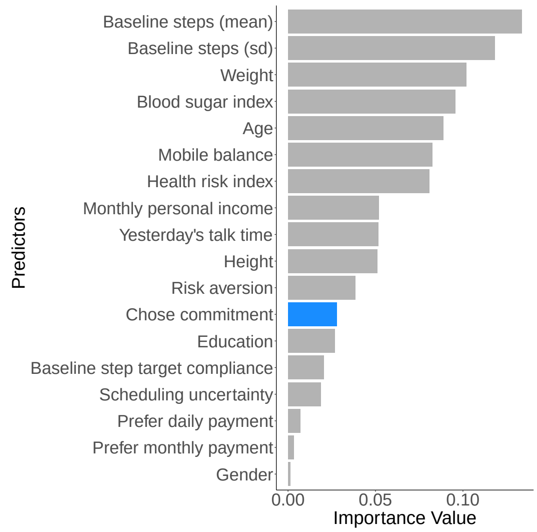


Appendix Figure F.1: Threshold Heterogeneity in Choosing Threshold and Choosing to Walk Later Is Robust to a Variety of Controls

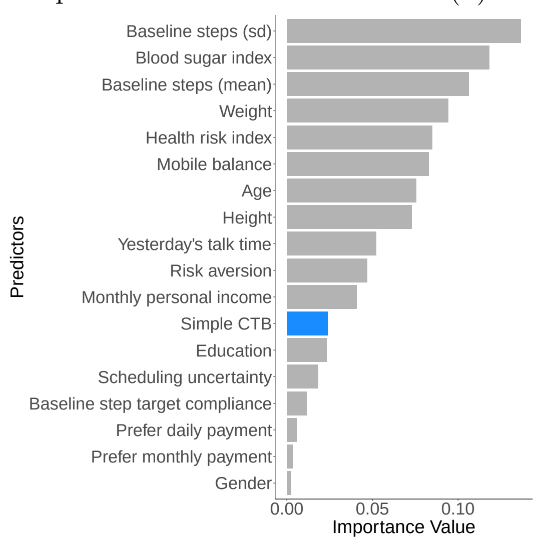
Notes: This figure replicates Figure 3 using different impatience measures. Panel A uses demand for commitment and Panel B uses simple CTB. See the notes to Table 3 for more detail on these impatience measures. All other details are the same as in Figure 3; see Figure 3 notes for more details.



(a) Predicted Impatience



(b) Chose Commitment

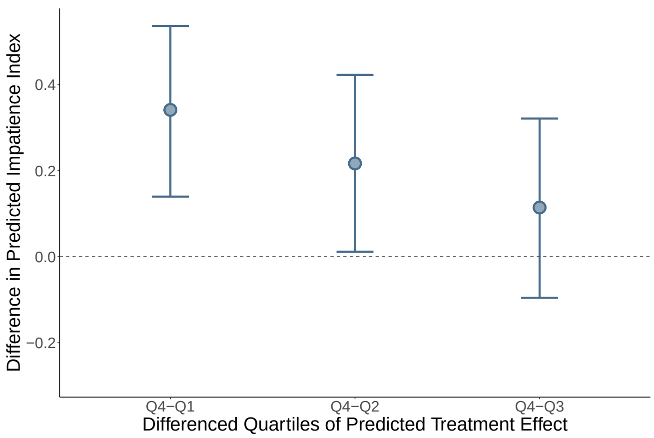


(c) Simple CTB

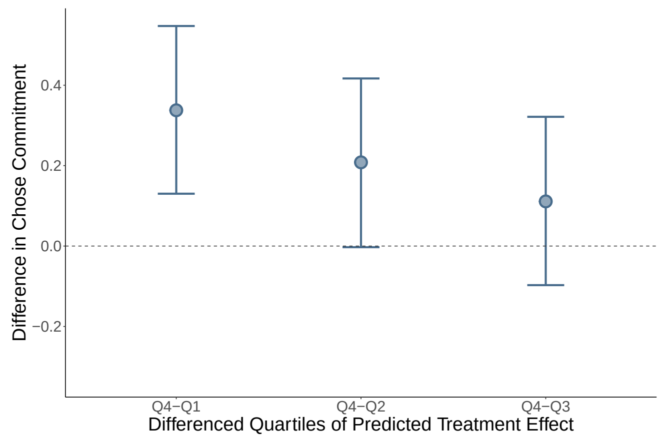
Appendix Figure F.2: Importance Results for Other Impatience Measures

Notes: This figure is analogous to Figure A.2. It displays the importance of each predictor included in a causal forest prediction of the Threshold treatment effect on average compliance at the individual level. Variable importance is a weighted sum of the number of splits on the variable of the causal forest at each depth. Predictors include the controls shown in Panel A of Figure 3, except that this analysis uses continuous versions of the baseline compliance and education variables (because the importance analysis more naturally handles continuous variables). Missing values of predictor variables are imputed with the treatment-group mean; we also include an indicator for whether each variable is missing (each of which the analysis assigned importance values of 0, and hence which we do not depict for brevity). We implement the Causal Forest using the GRF package in R (Tibshirani et al., 2023).

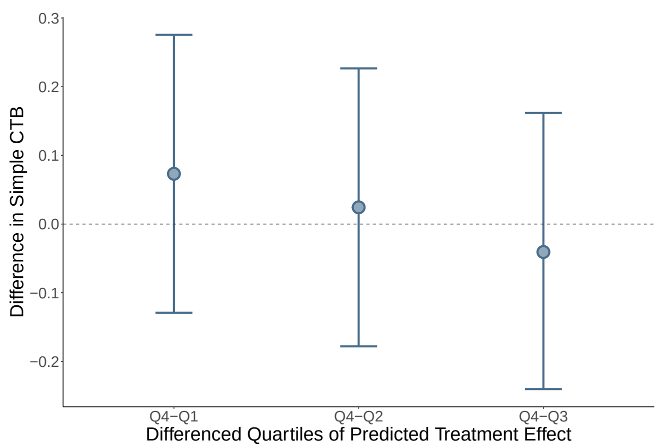
Appendix Figure F.3: Classification Analysis Results for Other Impatience Measures



(a) Difference in Predicted Impatience Across Quartiles of the Predicted Threshold Treatment Effect



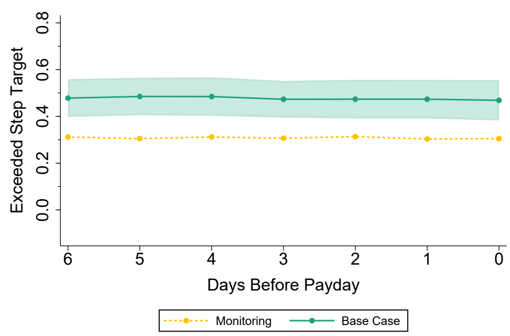
(b) Difference in Chose Commitment Across Quartiles of the Predicted Threshold Treatment Effect



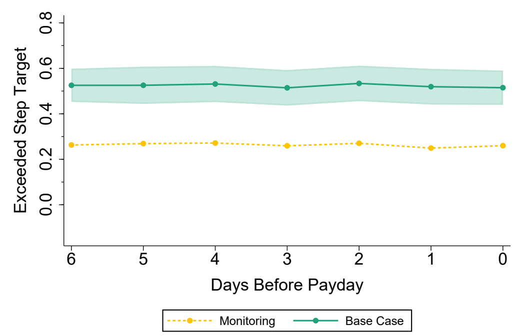
(c) Difference in Simple CTB Across Quartiles of the Predicted Threshold Treatment Effect

Notes: This figure replicates Panel (b) of Figure A.3 using the predicted impatience index, chose commitment and Simple CTB instead of the actual impatience index.

Appendix Figure F.4: No Heterogeneity by Impatience in Compliance Pattern Across the Paycycle



(a) Above Median Impatience Index



(b) Below Median Impatience Index

Notes: The figures show the probability of exceeding the daily 10,000-step target for the base case relative to the monitoring group, according to days remaining until payday. Each Panel is limited to above/below-median values of the impatience index. 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. p-values for the test that the payday spikes are equal across above/below-median samples for each impatience measure are: Impatience index: 0.462; Predicted impatience index: 0.803; Chose commitment: 0.647; Simple CTB: 0.100.

Appendix Table F.1: Participants Understood Their Assigned Contracts

Contract Type	Question	% Correct		
		At Contract Launch	First Call	Any Call
Base Case n=902	How many recharges would you receive on (*payment day of week*) if you walked 10,000 steps on exactly 1 day over the period (*payment day of week*) to (*payment day of week*–1)?	0.99	0.99	1.00
	How many times over the course of this week would you receive recharges if you walked 10,000 steps on exactly 5 days over the period (*payment day of week*) to (*payment day of week*–1)?	1.00	1.00	1.00
4-Day Threshold n=794	What is the minimum number of days that you need to walk to get a recharge?	-	0.90	0.95
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 1 day this week?	0.93	0.98	1.00
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 4 days this week?	0.99	0.99	1.00
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 6 days this week?	0.98	1.00	1.00
5-Day Threshold n=312	What is the minimum number of days that you need to walk to get a recharge?	-	0.88	0.93
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 1 day this week?	0.91	0.96	0.99
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 5 days this week?	0.98	0.99	0.99
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 6 days this week?	0.99	0.99	0.99
Daily n=166	How many times over the course of this week would you receive recharges if you walked 10,000 steps on exactly 1 day ?	1.00	0.98	1.00
	How many times over the course of this week would you receive recharges if you walked 10,000 steps on exactly 5 days ?	-	0.99	0.99
Monthly n=164	How many recharges would you receive on (*payment day of week*) if you walked 10,000 steps on exactly 1 day over this week ?	0.99	0.99	1.00
	How many recharges would you receive on (*payment day of week*) if you walked 10,000 steps on exactly 5 days in this week ?	1.00	1.00	1.00
Monitoring n=203	How do you report your steps to us?	1.00	0.99	1.00
	How large is the Fitbit wearing bonus?	-	0.78	0.99

Notes: This table shows the share of participants who correctly answered questions about their contract. Participants were initially asked these questions when contracts were first explained ("At Contract Launch"). Questions were asked again over the phone at a later date ("First Call"). Those who answered questions incorrectly were asked again in two subsequent follow-up calls. The "Any Call" column represents the proportion of participants who got the questions right at any of these phone calls. Some questions were not asked at the initial contract launch phase. Each participant in the monthly, base case, and threshold groups was always paid on the same day of the week, which is labeled *"payment day of week"*.

Appendix Table F.2: Threshold Heterogeneity Results are Robust to Ways of Constructing the "Chose Commitment" and "Simple CTB" Measures

Dependent variable:	Exceeded step target (×100)							
Impatience measure:	Chose commitment					Simple CTB		
	Average	Either	Both	4-Day	5-Day	Average	Either	Both
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Impatience × Threshold	6.06**	5.31*	5.74*	6.32**	4.75	4.70*	5.16*	3.82
	[0.42, 11.71]	[-0.49,11.11]	[-0.19,11.68]	[0.56, 12.08]	[-3.26, 12.76]	[-0.70, 10.11]	[-0.02, 10.34]	[-1.60, 9.25]
Base Case mean	49.9	49.8	49.9	50	49.8	50.2	50.2	50.2
# Individuals	1,798	1,809	1,798	1,523	1,097	1,967	1,967	1,967
# Observations	144,099	145,005	144,099	122,277	87,990	157,799	157,799	157,799

Notes: This table shows robustness of results in columns 5 and 6 of Table 3 to different ways of constructing the Chose Commitment and Simple CTB variables. For Chose Commitment, "average" is the main specification in Table 3 and is the average of preference for 4-day and 5-day threshold contracts versus the linear contract. "Either" means preferring either 4-day or 5-day threshold, and "Both" means preferring both threshold contracts. "4-day" and "5-day" only look at the preference for 4-day and 5-day threshold respectively. For "Simple CTB", "Average" is the main specification and is the average between choosing to walk more earlier in two CTB-style walking choices, "Either" means choosing to walk earlier in either choice and "Both" means choosing to walk earlier in both choices. Controls are the same as in Table 2. The sample includes the base case and threshold groups. Data are at the individual × day level. Bootstrapped 95% confidence are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.3: Lee Bounds on the Impacts of Incentives on Exercise

Definition of missing:	No steps data	Did not wear Fitbit	No data from Fitbit	Lost data entire period	Withdrew immediately	Mid-period withdrawal	Other reasons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Daily steps							
Regression estimate	1269	1269	1338	1338	1338	1338	1338
(conditional on nonmissing data)	[245]	[245]	[261]	[261]	[261]	[261]	[261]
Lee lower bound	1053	882	1230	1315	1297	1226	1303
	[276]	[209]	[306]	[292]	[270]	[230]	[289]
Lee upper bound	1426	1571	1572	1351	1430	1581	1358
	[328]	[349]	[333]	[277]	[269]	[250]	[279]
B. Met 10k step target							
Regression estimate	0.223	0.223	0.205	0.205	0.205	0.205	0.205
(conditional on nonmissing data)	[0.024]	[0.024]	[0.022]	[0.022]	[0.022]	[0.022]	[0.022]
Lee lower bound	0.215	0.208	0.200	0.204	0.203	0.200	0.204
	[0.030]	[0.029]	[0.022]	[0.024]	[0.023]	[0.020]	[0.022]
Lee upper bound	0.232	0.242	0.216	0.206	0.209	0.217	0.206
	[0.030]	[0.031]	[0.022]	[0.024]	[0.024]	[0.021]	[0.022]
# Individuals	2,607	2,559	2,607	2,568	2,598	2,561	2,566
# Observations	218,988	205,732	218,988	206,488	209,008	211,551	206,320

Notes: This table reports regression estimates and Lee bounds estimates (accounting for different types of missing pedometer data) of the effect of Incentives relative to Monitoring on exercise during the intervention period. Standard errors in parentheses. The regression estimates and Lee bounds condition on data not being missing, using different definitions of missing data in each column. Regression estimates are not comparable to those reported in Table 2 because each column conditions on the "type of missing" indicator in the first row being equal to 0 and does not include controls. Data are at the individual × day level.

Appendix Table F.4: Summaries From Minute-Level Pedometer Data

	Incentives	Monitoring	I - M	p-value: I=M
	(1)	(2)	(3)	(4)
A. Activity (by minute)				
Average daily activity	213	197	17	0.001
Average steps per minute	41	38	3	0.001
B. Time of day				
Average start time	07:11	07:16	5	0.441
Average end time	20:49	20:50	1	0.742
C. High step counts per minute (share)				
Steps > 242	0	0	0	.
Steps > 150	0	0	0	0.322
# Individuals	2,368	201		

Notes: This table presents various statistics at the respondent × minute level in the incentive and monitoring groups for the days on which minute-by-minute data were available (typically 10 days of minute-wise data prior to each sync). "Average daily activity" is the average number of minutes in which a step was recorded each day. "Average steps per minute" is the average steps per minute in which at least one step was recorded. Average start/end time is the average time the first/last step was recorded by the fitbit on that day. The "High step counts per minute (share)" variables are the share of days on which we recorded steps-per-minute over the stated thresholds. High step count thresholds (242 and 150) were determined based on the average number of steps an individual takes when running at 5 mph and 8 mph, respectively. Only one individual's minute-by-minute data coincide with jogging at a pace greater than 5 miles per hour, and only for a total of 15 minutes over one day in the intervention period.

Appendix Table F.5: HbA1c and RBS are Predictive of Each Other

Dependent variable:	Endline HbA1c	Endline RBS
	(1)	(2)
Baseline HbA1c (SDs)	0.60***	0.33***
	[0.045]	[0.057]
Baseline RBS (SDs)	0.25***	0.37***
	[0.044]	[0.055]
# Individuals	560	561

Notes: This table reports estimates from regressing standardized HbA1c (column 1) and standardized RBS (column 2) at endline on standardized HbA1c and standardized RBS at baseline. Standard errors in parentheses. The sample is the control group only. Data are at the individual level. No additional controls are included. Significance levels: * 10%, ** 5%, *** 1%

Appendix Table F.6: Table 2 Results Robust to Different Controls

	No controls				Stratum fixed effects				Lasso-selected controls			
Dependent variable:	Exceeded step target	Daily steps	Daily steps (if > 0)	Earned payment when target met	Exceeded step target	Daily steps	Daily steps (if > 0)	Earned payment when target met	Exceeded step target	Daily steps	Daily steps (if > 0)	Earned payment when target met
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Pooled incentives												
Incentives	0.205***	1337.6***	1271.4***	0.950***	0.200***	1263.7***	1158.0***	0.952***	0.196***	1287.1***	1144.2***	0.952***
	[0.0224]	[261.1]	[246.1]	[0.00231]	[0.0185]	[208.7]	[188.1]	[0.00309]	[0.0180]	[211.4]	[190.3]	[0.00282]
B. Unpooled incentives												
Base Case	0.208***	1356.6***	1208.8***	1.000***	0.210***	1386.2***	1199.1***	1.006***	0.207***	1411.4***	1197.0***	1.005***
	[0.0241]	[277.0]	[258.6]	[1.62e-13]	[0.0201]	[222.0]	[199.4]	[0.00267]	[0.0196]	[225.0]	[201.8]	[0.00223]
Threshold	0.207***	1337.9***	1315.2***	0.890***	0.198***	1214.7***	1139.8***	0.892***	0.194***	1238.0***	1125.3***	0.892***
	[0.0240]	[277.1]	[259.3]	[0.00505]	[0.0199]	[220.8]	[198.0]	[0.00547]	[0.0194]	[223.2]	[200.3]	[0.00533]
Daily	0.207***	1202.7***	1363.9***	1.000***	0.200***	1120.7***	1279.2***	1.003***	0.199***	1126.7***	1245.0***	1.003***
	[0.0345]	[389.5]	[346.0]	[2.02e-13]	[0.0303]	[331.0]	[277.3]	[0.00365]	[0.0302]	[332.2]	[279.2]	[0.00296]
Monthly	0.198***	1568.6***	1482.3***	1.000***	0.177***	1265.7***	1174.2***	1.002***	0.179***	1302.6***	1152.4***	1.000***
	[0.0348]	[393.8]	[365.4]	[3.52e-13]	[0.0288]	[307.4]	[270.2]	[0.00335]	[0.0281]	[311.0]	[272.3]	[0.00271]
Small Payment	0.147***	820.5	658.5	1.000***	0.137***	728.1*	549.8	1.000***	0.128***	740.7*	510.6	0.999***
	[0.0485]	[524.0]	[477.9]	[5.58e-14]	[0.0383]	[386.1]	[334.9]	[0.00499]	[0.0382]	[381.0]	[331.3]	[0.00417]
p-value for Base Case vs												
Daily	0.980	0.630	0.570	.	0.710	0.350	0.730	0.340	0.790	0.310	0.830	0.560
Monthly	0.760	0.520	0.360	.	0.180	0.630	0.910	0.160	0.270	0.670	0.840	0.020
Threshold	0.980	0.910	0.480	<0.001	0.360	0.210	0.620	<0.001	0.350	0.210	0.550	<0.001
Small Payment	0.180	0.260	0.200	.	0.040	0.060	0.030	0.200	0.030	0.050	0.020	0.150
Monitoring mean	0.294	6,774	7,986	0	0.294	6,774	7,986	0	0.294	6,774	7,986	0
# Individuals	2,559	2,559	2,557	2,394	2,559	2,559	2,557	2,394	2,559	2,559	2,557	2,394
# Observations	205,732	205,732	180,018	99,406	205,732	205,732	180,018	99,406	205,732	205,732	180,018	99,406

Notes: This table replicates the Table 2 estimates with different sets of controls. Columns 1–4 do not use controls, columns 5–8 use the same controls as in 2 along with stratum fixed effects, and columns 9–12 use controls selected by double-Lasso. We allow lasso to select from the following list of controls: female, age, age squared, weight, weight squared, indicator for missing weight, height, height squared, indicator for missing height, yearmonth and day of week fixed effects. In addition, column 9 controls for the number of days in phase-in the target was met, its square, and an SMS treatment indicator. Columns 10–12 control for average baseline steps, average baseline steps squared, an indicator for missing baseline steps, and an SMS treatment indicator. See the notes for Table 2 for more information. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.7: Quantile Regression Estimates Show That the Linear and Threshold Contracts Similarly Impact the Distribution of Individual-Level and Weekly Compliance

Dependent variable:	Share of days met step target in intervention period			Share of days met step target in week		
Percentile:	25	50	75	25	50	75
	(1)	(2)	(3)	(4)	(5)	(6)
5-Day Threshold	0.108***	0.238***	0.353***	0.105***	0.228***	0.351***
	[0.024]	[0.031]	[0.047]	[0.020]	[0.035]	[0.053]
4-Day Threshold	0.093***	0.206***	0.323***	0.092***	0.214***	0.327***
	[0.021]	[0.026]	[0.044]	[0.017]	[0.025]	[0.051]
Base Case	0.116***	0.245***	0.336***	0.111***	0.246***	0.328***
	[0.020]	[0.025]	[0.042]	[0.016]	[0.024]	[0.051]
p-value: 5-Day vs 4-Day	.48	.28	.29	.46	.69	.25
p-value: 5-Day vs Base Case	0.705	0.792	0.523	0.712	0.575	0.266
p-value: 4-Day vs Base Case	0.201	0.107	0.560	0.152	0.157	0.948
Monitoring mean	0.292	0.469	0.723	0.294	0.523	0.801
# Individuals	2,133	2,133	2,133	2,168	2,168	2,168
# Observations	2,133	2,133	2,133	24,864	24,864	24,864

Notes: This table shows quantile regressions where the dependent variable is the share of days a participant met their step target in a given week (columns 1–3) or during the intervention period (columns 4–6). Data in columns 1–3 are at the individual × week level; in columns 4–6 they are at the individual level. The sample includes the base case, threshold, and monitoring groups. Controls are the same as in Table 2, except that, because the data are not at the individual × day level, we do not include day-of-week fixed effects. Also, in columns 1–3 we include year-month fixed effects for the first year-month of the intervention period, and in columns 4–6, we include year-month fixed effects for the first year-month of the week. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.8: Threshold Heterogeneity in Chose Commitment Is Robust to Ways of Handling "No Preference" Responses

Dependent variable:	Exceeded step target (×100)			
	Excluding no preference	No preference as Threshold	No preference as Base Case	No preference as separate
	(1)	(2)	(3)	(4)
Impatience × Threshold	6.06**	5.67*	5.52*	6.13*
	[0.42, 11.71]	[-0.49, 11.84]	[-0.23, 11.28]	[-0.27, 12.53]
Base Case mean	49.9	50.2	50.2	50.2
# Individuals	1,798	1,969	1,969	1,969
# Observations	144,099	157,946	157,946	157,946

Notes: This table shows robustness of results in column 5 of Table 3 to different ways of handling participants with no preference between the 4- or 5-day threshold and base case contract. Column 1 uses the same specification as in column 5 of Table 3 by counting no preference as missing. Column 2 counts no preference as choosing Threshold and column 3 counts no preference as choosing Base Case. Column 4 counts no preference as a separate group by adding a dummy and its interaction with the indicator for threshold treatment. Controls are as in Table 2. Bootstrapped 95% confidence are in brackets. Data are at the individual × day level. The sample includes the threshold and base case groups. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.9: Threshold Heterogeneity Results Similar with Steps as Outcome or When Analyze Threshold Groups Separately

Impatience measure:	Impatience index	Above-median impatience index	Predicted impatience index	Above-median predicted index	Chose commitment	Simple CTB
	(1)	(2)	(3)	(4)	(5)	(6)
A. Dependent variable = steps						
Impatience × Threshold	289*	576	238**	521**	580**	157
	[-41, 619]	[-254,1405]	[16, 444]	[8, 925]	[84, 1075]	[-427, 741]
Threshold	-143	-401	-166	-360**	-457***	-253
	[-442, 157]	[-939, 138]	[-367, 41]	[-615, -64]	[-697, -216]	[-671, 164]
Impatience	-209	-444	-229***	-549**	-248	-41
	[-474, 56]	[-1001, 113]	[-379, -72]	[-823, -101]	[-700, 205]	[-461, 380]
Base Case mean	8,098	8,098	8,131	8,131	8,091	8,131
B. Dependent variable = exceeded step target (×100)						
Impatience × 5-Day Threshold	3.52*	6.00	3.66***	7.29**	6.10	3.76
	[-0.05, 7.08]	[-1.73,13.73]	[1.32, 5.94]	[0.88, 11.26]	[-1.26, 13.45]	[-2.22, 9.74]
5-Day Threshold	-1.72	-4.31	-1.71	-4.42***	-4.92***	-3.79
	[-5.16, 1.73]	[-10.03,1.41]	[-4.01, 0.56]	[-7.09, -1.22]	[-8.02, -1.82]	[-8.31, 0.73]
Impatience × 4-Day Threshold	5.00*	7.95	1.76	2.51	6.20	7.10**
	[-0.94, 10.94]	[-4.13,20.02]	[-1.58, 4.58]	[-4.44, 8.64]	[-1.44, 13.83]	[0.20, 14.00]
4-Day Threshold	-0.14	-3.71	0.17	-0.84	-2.81	-3.76
	[-4.53, 4.26]	[-11.18,3.77]	[-2.96, 3.38]	[-4.54, 3.15]	[-7.86, 2.25]	[-9.01, 1.49]
Impatience	-2.97**	-5.03**	-2.39***	-5.32**	-2.37	-2.68
	[-5.36, -0.58]	[-9.96,-0.10]	[-3.90, -0.85]	[-8.15, -0.69]	[-7.23, 2.48]	[-7.31, 1.96]
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: Panel A shows that the Threshold heterogeneity reported in Table 3 is robust to using daily steps as the outcome. Panel B shows heterogeneity in the 4- and 5-day threshold treatments by impatience. The impatience measure changes across columns; its units in columns 1 and 3 are standard deviations. The sample includes the base case and threshold groups only. Specifications in columns 1 and 2 include only participants who were enrolled after we started measuring the impatience index; columns 3–6 include everyone. Threshold pools the 4- and 5-day threshold groups. Bootstrap draws were done at the individual level, and bootstrapped 95% confidence intervals are in brackets. See the notes to Table 3 for a detailed description of the bootstrap procedure. For Panel A: The Gaussian standard errors and p-values for the column 1 $Impatience \times Threshold$ coefficient are 192.76 and 0.134, respectively; for column 2 the corresponding values are 379.15 and 0.129; for column 5 the corresponding values are 300.19 and 0.054; for column 6 the values are 284.4 and 0.580 . For Panel B: The Gaussian standard errors and p-values for the column 1 $Impatience \times 5$ - day $Threshold$ coefficient are 4.16 and 0.088, respectively; for column 2 the corresponding values are 4.16 and 0.223; for column 5 the corresponding values are 4.16 and 0.088; for column 6 the values are 4.16 and 0.088 . The Gaussian standard errors and p-values for the column 1 $Impatience \times 4$ - day $Threshold$ coefficient are 3.08 and 0.223, respectively; for column 2 the corresponding values are 3.08 and 0.223; for column 5 the corresponding values are 3.08 and 0.223; for column 6 the values are 3.08 and 0.223 . Controls are the same as in Table 2. Data are at the individual × day level. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.10: No Significant Heterogeneity in Post-Intervention Persistence by Impatience

Dependent variable:	Post-endline exceeded step target (× 100)			
Impatience measure:	Impatience index		Predicted index	
Sample:	Late		Full	
	(1)	(2)	(3)	(4)
A. Exceeded step target (× 100)				
Impatience × Incentives	0.716	-0.551	-0.023	-0.893
	[-1.162,2.593]	[-1.982,0.881]	[-1.95,2.22]	[-5.88,3.56]
Impatience	-0.901	-0.241	-0.778	-0.112
	[-2.335,0.534]	[-1.533,1.052]	[-2.48,1.40]	[-3.77,5.39]
Incentives	-51.286	8.238	-50.743	8.615
	[-58.814,-43.758]	[5.311,11.166]	[5.75,11.58]	[1.25,11.13]
Baseline steps	21.030	28.474	21.017	28.516
	[14.715,27.345]	[20.958,35.991]	[20.55,34.51]	[5.77,17.34]
Intervention steps	0.392		0.390	
	[0.339,0.444]		[0.00,0.00]	
Base mean: exceeded step target	23	23	23	23
B. Average daily steps				
Impatience × Incentives	179.047	-17.391	55.594	-150.602
	[-172.712,530.806]	[-344.876,310.094]	[-376.99,279.88]	[-799.65,287.12]
Impatience	-219.229	-96.560	-128.002	15.522
	[-494.472,56.014]	[-347.841,154.720]	[-324.84,264.30]	[-366.29,694.08]
Incentives	-4.7e+03	608.318	-4.6e+03	694.355
	[-5.4e+03,-4.0e+03]	[79.120,1137.516]	[262.17,1161.73]	[-706.15,1013.48]
Baseline steps	0.191	0.475	0.189	0.472
	[0.128,0.255]	[0.421,0.528]	[0.43,0.52]	[0.45,0.54]
Intervention steps	0.630		0.628	
	[0.569,0.691]		[0.00,0.00]	
Base mean: steps	5,113	5,113	5,113	5,113
# Individuals	1,122	1,122	1,122	1,122
# Observations	91,756	91,756	91,756	91,756

Notes: This table shows heterogeneity by time preferences in persistence of treatment effects. The sample includes everyone who walked in the post-intervention period. Controls are the same as in Table 2. Base Case is the omitted group, and individual group level dummies are not reported. Because we have no intervention step data for the control group, regressions that include intervention steps only include incentive and monitoring groups. We add a missing intervention period dummy to prevent Control from dropping out of the sample. All units are standard deviations on the indexes. Data are at the individual × day level. 95% confidence intervals bootstrapped at the person level are in brackets; see the notes to 3 for more detail on the bootstrap procedure. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.11: Commitment Device Estimates Robust To Different Ways of Defining Who Chose the Threshold Contract

Dependent variable:	Exceeded step target (×100)				Paid when exceeding step target (×100)			
Definition of preferring threshold:	Both	Either	5-Day	4-Day	Both	Either	5-Day	4-Day
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Heterogeneity method								
Commitment	2.12**	1.85*	1.09	2.46**	6.78***	5.71***	7.99***	5.52***
	[1.08]	[0.99]	[1.65]	[1.10]	[0.45]	[0.41]	[0.94]	[0.47]
Threshold group mean	49.38	49.43	50.34	49.00	88.87	88.90	85.58	90.20
# Individuals	1,798	1,809	1,097	1,523	1,681	1,692	1,034	1,419
# Observations	144,099	145,005	87,990	122,277	71,525	71,944	43,929	60,564
B. Synthetic group method								
Commitment	2.13	1.82	1.03	2.35	6.65***	5.74***	8.12***	5.37***
	[1.41]	[1.40]	[2.28]	[1.59]	[0.64]	[0.65]	[1.41]	[0.67]
Threshold group mean	49.38	49.43	50.34	49.00	88.87	88.90	85.58	90.20
# Individuals	1,931	1,954	879	1,517	1,809	1,833	831	1,415
# Observations	154,336	156,334	70,202	121,438	78,031	78,736	35,331	61,307
Fraction preferring threshold	0.46	0.54	0.45	0.53	0.46	0.54	0.45	0.53

Notes: This table shows the robustness of the estimated effect of a hypothetical commitment device to different definitions of who selected each contract. The outcomes are compliance (columns 1–4) and the fraction of paid compliance (columns 5–8). We use incentivized choices between Base Case and 5- or 4-Day Thresholds to identify who prefers the threshold contract. Preferring the threshold is defined in columns 1 and 5 (2 and 6) as choosing 4-Day and (or) 5-Day Threshold over Base Case and in columns 3 and 7 (4 and 8) as preferring 5-Day (4-Day) Threshold to Base Case. Columns 1 and 5 correspond to the estimates from row 1 of Figure H.1. Panel A estimates the commitment effect relative to Threshold using Method 1 from Figure H.1, which estimates the commitment effect as the fraction of participants preferring the base case contract times the treatment effect of moving from Threshold to Base Case among participants who prefer the Base Case. The sample includes Base Case and the 4- and 5-Day Threshold in columns 1, 2, 5, and 6; Base Case and the 5-Day Threshold in columns 3 and 7; and Base Case and the 4-Day Threshold in columns 4 and 8. Panel B estimates the commitment effect relative to Threshold using Method 2 from Figure H.1, i.e., using a synthetic personalized group. See the notes for Figure H.1 for details for how the synthetic group is constructed. The sample includes the threshold and synthetic commitment groups; for columns 1, 2, and 5, 6 we use the full groups, while in columns 3 and 7 (4 and 8) we exclude the members of both groups that were randomly assigned to 4-Day (5-Day) Threshold. Controls are the same as Table 2. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.12: Walking Does Not Vary Significantly Across the Pay Cycle

Dependent variable:	Exceeded step target (×100)				
Payment frequency:	Weekly		Monthly		
	(1)	(2)	(3)	(4)	(5)
Days before payday	0.11		0.11		
	[0.09]		[0.09]		
Payday		-0.63		-0.63	
		[0.55]		[0.55]	
Payweek					-0.12
					[1.02]
Sample mean	50.19	50.19	50.19	50.19	49.28
# Individuals	890	890	890	890	163
# Observations	71,672	71,672	71,672	71,672	13,333

Notes: The columns show the effect of days until payday on the probability of meeting the step target in the base case and monthly groups; the sample in columns 1 and 2 is restricted to the base case group, and the sample in columns 3–5 is restricted to the monthly group. We control for payday day-of-week fixed effects, day-of-week fixed effects, day-of-week relative to launch survey day-of-week fixed effects, a day-of-contract-period time trend, and the same controls as in Table 2. Data are at the individual × day level. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.13: Effect of Incentives on BMI, Blood Pressure, and Waist Circumference

Sample:	Full sample effects			Above-median baseline blood sugar effects		
	Body mass index	Mean arterial BP	Waist circumference	Body mass index	Mean arterial BP	Waist circumference
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	-0.0525	0.0884	-0.211	0.0195	-0.0811	-0.275
	[0.0409]	[0.426]	[0.284]	[0.0570]	[0.605]	[0.396]
Monitoring	0.0657	1.121	-0.0352	0.00127	-0.478	0.345
	[0.0838]	[0.739]	[0.438]	[0.0830]	[1.083]	[0.590]
Sample	Full	Full	Full	Above-median blood sugar	Above-median blood sugar	Above-median blood sugar
Control mean	26.45	103.02	94.44	26.09	103.96	94.57
# Individuals	3,058	3,056	3,059	1,527	1,529	1,525

Notes: This table shows the effect of incentives on the endline components of the health risk index not included in Table 4. Columns 4–6 restricts to the above-median blood sugar index sample. The blood sugar index is constructed as in Table 4. Controls are as described in Table 4 notes. The sample includes the incentive, monitoring, and control groups. Data are at the individual level. Standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.14: Impacts of Incentives on Health, Robustness to Different Controls

	Full sample effects				Above-median baseline blood sugar sample effects			
	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)
Panel A. No controls								
Incentives	-0.044	-0.068	-5.53	-0.055	-0.092*	-0.15	-11.4*	-0.13**
	[0.043]	[0.11]	[4.37]	[0.047]	[0.053]	[0.14]	[6.12]	[0.060]
Monitoring	0.0073	-0.078	4.62	0.058	-0.070	-0.30	-1.35	-0.11
	[0.074]	[0.19]	[7.94]	[0.078]	[0.088]	[0.22]	[10.8]	[0.10]
p-value: I = M	0.435	0.952	0.153	0.102	0.770	0.463	0.294	0.803
Control mean	0.00	8.44	193.83	0.00	0.64	10.09	248.26	0.45
# Individuals	3,067	3,066	3,067	3,068	1,530	1,529	1,530	1,531
Panel B. Stratum fixed effects								
Incentives	-0.05*	-0.07	-6.70*	-0.05*	-0.10**	-0.13	-14.01**	-0.09**
	[0.03]	[0.07]	[3.44]	[0.03]	[0.05]	[0.12]	[5.85]	[0.04]
Monitoring	-0.02	-0.14	2.10	0.02	-0.06	-0.31	-0.24	-0.05
	[0.05]	[0.12]	[6.36]	[0.04]	[0.08]	[0.19]	[10.37]	[0.07]
p-value: I = M	0.492	0.515	0.124	0.120	0.576	0.278	0.138	0.546
Control mean	0.00	8.44	193.83	0.00	0.64	10.09	248.26	0.45
# Individuals	3,067	3,066	3,067	3,068	1,530	1,529	1,530	1,531
Panel C. Lasso-selected controls								
Incentives	-0.05**	-0.08	-6.04*	-0.05*	-0.10**	-0.15	-11.95**	-0.08**
	[0.03]	[0.07]	[3.52]	[0.02]	[0.05]	[0.12]	[5.90]	[0.04]
Monitoring	-0.03	-0.14	1.29	0.01	-0.07	-0.33*	0.85	-0.05
	[0.05]	[0.12]	[6.61]	[0.04]	[0.08]	[0.20]	[10.48]	[0.07]
p-value: I = M	0.517	0.573	0.220	0.129	0.631	0.306	0.170	0.553
Control mean	0.00	8.44	193.83	0.00	0.64	10.09	248.26	0.45
# Individuals	3,067	3,066	3,067	3,068	1,530	1,529	1,530	1,531

Notes: This table reports the results of the specifications displayed in Table 4 with different controls. Panel A include no controls, Panel B include the same controls as 4 along with stratum fixed effects, Panel C include controls selected by double-Lasso. We allow lasso to select from the following list of controls: female, age, age squared, weight, weight squared, weight missing indicator, height, height squared, height missing indicator, completed endline survey indicator, and date and hour of endline completion fixed effects. Panel C also control for the baseline value of the outcome (or index components for indices), along with an SMS treatment indicator. Standard errors are in brackets. Data are at the individual level. The sample includes the incentive, monitoring, and control groups. *p*-value: I = M is the *p*-value for incentives vs monitoring. See Table 4 for more information on outcome variables and controls. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.15: Impact of Incentives on Fitness and Mental Health

A. Mental Health	Mental health index	Felt happy	Less nervous	Peaceful	Energy	Less blue	Less worn	Less harm to social life
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentives	0.095**	0.088*	0.026	0.054	0.062	0.016	0.090**	0.053
	[0.045]	[0.045]	[0.044]	[0.047]	[0.048]	[0.044]	[0.042]	[0.032]
Monitoring	0.16**	0.074	0.13	0.095	0.032	0.13*	0.17***	0.049
	[0.073]	[0.075]	[0.077]	[0.083]	[0.082]	[0.075]	[0.066]	[0.053]
p-value: M = I	0.34	0.82	0.14	0.59	0.68	0.09	0.14	0.93
Control mean	0.00	3.06	3.48	3.35	3.30	3.86	4.40	4.71
# Individuals	3,068	3,068	3,068	3,068	3,068	3,068	3,068	3,068

B. Fitness	Fitness time trial index	Seconds to walk 4m	Seconds for 5 sit-stands
	(1)	(2)	(3)
Incentives	0.024	0.042	-0.10
	[0.045]	[0.043]	[0.12]
Monitoring	0.069	0.080	-0.088
	[0.077]	[0.076]	[0.19]
p-value: M = I	0.50	0.57	0.94
Control mean	0.00	3.88	13.18
# Individuals	2,890	2,825	2,793

Notes: The Mental health index averages the values of seven questions adapted from RAND's 36-Item Short Form Survey. A large value of the Fitness time trial index indicates low fitness. The sample includes the incentive, monitoring, and control groups. Controls are the same as described in the Table 4 notes, along with the same set of additional controls described in the Table F.16 notes. Robust standard errors are in brackets. Data are at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.16: Impacts of Incentives on Diet and Addictive Consumption

A. Healthy diet

	Healthy diet index	Wheat meals	Meals with vegeta-bles	Servings of fruit	Negative of rice meals	Negative of junkfood pieces	Negative of spoons sugar in coffee	Negative of sweets yester-day)	Avoid un-healthy food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Incentives	0.052	0.028	0.060**	0.038	0.029	-0.020	-0.019	-0.028	0.0037
	[0.044]	[0.029]	[0.030]	[0.035]	[0.033]	[0.066]	[0.047]	[0.038]	[0.018]
Monitoring	0.023	0.019	0.082	0.062	-0.0068	0.13	-0.026	-0.048	-0.040
	[0.085]	[0.053]	[0.054]	[0.062]	[0.060]	[0.10]	[0.081]	[0.082]	[0.033]
p-value: M = I	0.71	0.85	0.66	0.68	0.51	0.08	0.92	0.80	0.14
Control mean	0.00	0.49	0.58	0.53	-2.34	-0.91	-1.12	-0.35	0.83
# Individuals	3,068	3,068	3,068	3,068	3,068	3,068	3,068	3,068	3,068

B. Addictive consumption

	Addictive good consumption index	Average daily areca	Average daily alcohol	Average daily cigarettes
	(1)	(2)	(3)	(4)
Incentives	-0.014	0.034	-0.036	-0.056
	[0.037]	[0.037]	[0.028]	[0.095]
Monitoring	-0.0036	0.015	-0.016	-0.018
	[0.060]	[0.068]	[0.038]	[0.14]
p-value: M = I	0.85	0.76	0.46	0.77
Control mean	0.00	0.13	0.11	1.02
# Individuals	3,068	3,068	3,068	3,068

Notes: The Healthy Diet Index is composed of the average values of eight diet questions, standardized by their average and standard deviation in the control group; a larger value indicates a healthier diet. The Addictive Good Consumption Index is an index created by the average self-reported daily consumption of areca, alcoholic drinks, and cigarettes, standardized by their average and standard deviation in the control group; a larger value indicates higher consumption. The omitted category is Control. 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), an SMS treatment indicator, 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. Standard errors, in brackets, are clustered at the individual level. The sample includes the incentive, monitoring, and control groups. Data are at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.17: Impact of the Base Case and Threshold Contracts on the Histogram of Weekly Compliance

Dependent variable:	Met step target exactly X times in the week							
Days	0	1	2	3	4	5	6	7
Base Case	-18.85***	-4.78***	-2.08**	1.42**	0.99	3.62***	6.86***	12.82***
	[2.37]	[1.08]	[0.88]	[0.70]	[0.79]	[0.88]	[0.81]	[1.65]
5-Day Threshold	-14.11***	-5.04***	-3.66***	-1.24	-2.09**	0.93	7.45***	17.75***
	[2.72]	[1.20]	[0.97]	[0.78]	[0.83]	[1.02]	[1.14]	[2.29]
4-Day Threshold	-13.28***	-5.27***	-3.35***	-1.25*	-0.11	2.31**	6.57***	14.38***
	[2.43]	[1.09]	[0.88]	[0.67]	[0.81]	[0.90]	[0.86]	[1.74]
p-value: Base Case = 5-Day Threshold	0.015	0.734	0.012	0.000	0.000	0.001	0.580	0.017
p-value: Base Case = 4-Day Threshold	0.000	0.385	0.007	0.000	0.031	0.038	0.692	0.276
Monitoring Mean	43.70	13.56	8.92	5.76	6.58	6.71	5.72	9.05
# Individuals	2,167	2,167	2,167	2,167	2,167	2,167	2,167	2,167
# Observations	24,721	24,721	24,721	24,721	24,721	24,721	24,721	24,721

Notes: This table shows the results from Figure D.3. The table shows regressions of an indicator for meeting the step target exactly X times in the week in Base Case, Threshold, and Monitoring. Data are at the individual × week level. Controls are the same as in Table 2, except that, because the data are at the individual × week (not individual × day) level, we exclude day-of-week fixed effects. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table F.18: Main Sample and Validation Sample Have Similar Characteristics

	Main sample	Validation sample	p-value	Norm. Diff.
	(1)	(2)	(3)	(4)
A. Demographics				
Age	49.56	50.55	0.331	-0.120
	(8.51)	(7.98)		
Female (=1)	0.42	0.41	0.815	0.028
	(0.49)	(0.50)		
Labor force participation (=1)	0.74	0.75	0.963	-0.006
	(0.44)	(0.44)		
Household size	3.91	3.73	0.370	0.126
	(1.62)	(1.08)		
B. Health				
Overweight (=1)	0.61	0.65	0.513	-0.079
	(0.49)	(0.48)		
BMI	26.42	27.06	0.226	-0.131
	(4.34)	(5.31)		
Systolic BP (mmHg)	133.38	135.83	0.290	-0.134
	(19.16)	(17.21)		
Diastolic BP (mmHg)	88.48	91.17	0.045	-0.246
	(11.10)	(10.76)		
C. Walking - phase-in				
Exceeded step target (=1)	0.25	0.21	0.321	0.116
	(0.32)	(0.34)		
Average daily steps	7004.04	6539.98	0.331	0.119
	(3981.43)	(3837.98)		
F-test for joint orthogonality				
p-value			0.48	
Sample size				
Number of individuals	3232	71		

Notes: Means are reported for each variable and standard deviations are in parentheses. Main sample is our primary experimental sample. Validation sample is the sample used to validate our impatience index as described in Appendix C. Norm. Diff. is normalized differences. All variables are as in Table 1. The number of individuals with pedometer data differs from the total number of individuals because a few participants withdrew immediately. The F-statistic is obtained by running regressions with all characteristics. Data are at the individual level.

G Misreporting Steps, Confusion, and Suspensions

Procedures to Curb Misreporting Because incentive payments were determined by self-reported data and not pedometer data, we implemented a number of checks to ensure integrity of step reporting. Within each 28-day sync period, respondents who incorrectly over-reported meeting a 10k step target on more than 25% of days were flagged for cheating and suspended from receiving recharges for 7 days, and those who over-reported on 10–25% of days were flagged for cheating but only given a warning. Those who were flagged for cheating more than once were terminated from the program. Fewer than 5% of Incentive participants were suspended for cheating and only 1 was terminated (Table G.1)

During the intervention, we also attempted to flag participants who appeared to be confused about how to read their pedometers or report properly. We flagged those whose reported steps were either more than 10% higher than their pedometer steps or more than 15% lower than their pedometer steps on 40% of days as "confused" (unless their misreporting was indicative of cheating). Those who were flagged received a refresher from the surveyors on how to use the step-reporting system. We did not require pedometer and reported steps to match exactly because our pedometers record daily steps until midnight, but respondents typically reported their daily steps before midnight. As a result, we expected pedometer and reported steps to diverge slightly, either because respondents continued to walk after reporting their steps or because respondents (incorrectly) estimated the number of additional steps they would take post-reporting, and reported that amount instead.

We also took measures to encourage regular reporting for all groups. We offered a 50 INR "pedometer wearing and reporting bonus" to participants during the pre-intervention period if they wore the pedometer and reported steps on 80% of days to ensure that all participants were familiar with the step reporting system. At contract launch, we also briefly encouraged all but Control participants to report steps regularly during the intervention period, and offered a larger 200 INR pedometer wearing and reporting bonus for wearing and reporting during the intervention period. Finally, if participants did not report for a number of consecutive days, we would send them a text message reminder to report.

Rates of Misreporting and Confusion Our analysis only uses pedometer data (not reported data), so misreporting would not bias our conclusions. However, it is still interesting to examine the prevalence of misreporting. The prevalence of misreporting, defined as reporting steps above 10,000 when the pedometer itself records fewer than 10,000 steps, is less than 5% and, interestingly, balanced across incentive and monitoring groups (column 1 of Table G.2). The balance with the monitoring group, who had no incentives to over-report, suggests that over-reporting was mainly unintentional participant mistakes. The incentive group also appeared to put more effort into making correct step reports, with fewer divergences in either the positive or the negative directions (columns 2-4 of Table G.2).

Appendix Table G.1: Summary Statistics on Audits and Suspensions

	Count		Share	
	Incentives	Monitoring	Incentives	Monitoring
	(1)	(2)	(3)	(4)
Shared Fitbit ever	3	0	0.004	0.000
Suspended for cheating	100	N/A	0.042	N/A
Terminated for cheating	1	N/A	0.000	N/A
Total:	2,404	203	0.92	0.08

Notes: We randomly audited around 1,000 individuals from both the incentive and monitoring groups to look for evidence of pedometer sharing. The first row in columns 3 and 4 is conditional on being audited.

Appendix Table G.2: Misreporting, Confusion and Cheating by Treatment Group

Variable type:	Reporting	Confusion		
Dependent variable:	Incorrectly reported over 10k steps	Over-reported or under-reported	Over-reported by at least 10%	Under-reported by at least 15%
	(1)	(2)	(3)	(4)
Incentives	0.0079	-0.081***	-0.059***	-0.022**
	[0.01]	[0.02]	[0.02]	[0.01]
Monitoring mean	0.049	0.272	0.167	0.104
# Individuals	2,542	2,542	2,542	2,542
# Observations	173,131	173,131	173,131	173,131

Notes: Each observation is a respondent × day. Column 2 shows whether a respondent over-reported by at least 10% or under-reported by at least 15%. The omitted group is the monitoring group. Controls are the same as Table 2. Standard errors, in brackets, are clustered at the individual level. The sample includes the incentive and monitoring groups. Significance levels: * 10%, ** 5%, *** 1%.

H Personalizing Time-Bundled Thresholds

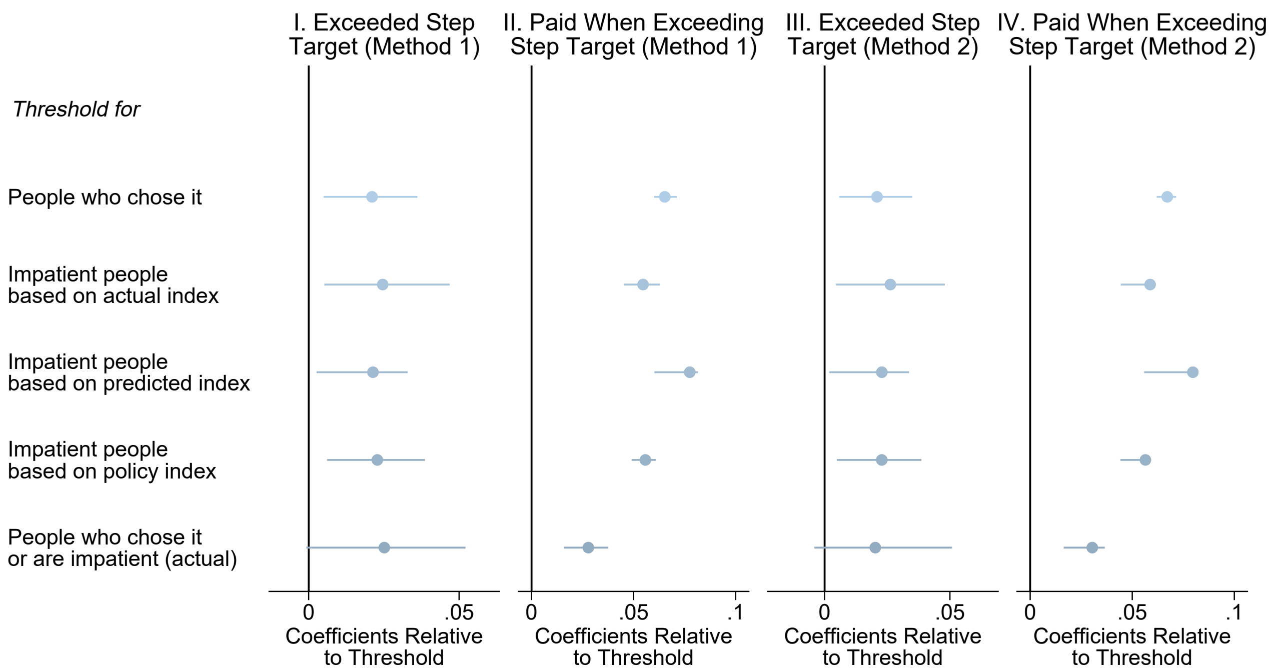
We now compare the performance of personalizing the assignment of time-bundled thresholds to assigning the threshold to everyone. While our experiment did not include personalized assignment mechanisms, we gathered impatience measures prior to randomization which, paired with random assignment, allow us to estimate how personalization would have performed. For example, relative to the threshold treatment, a commitment device that allowed participants to choose their treatments would differ in assigning the linear contract to those who preferred it. The estimated treatment effect of the commitment device relative to the threshold would thus be $p^L \times \tau^L_{BC-TH}$ where p^L is the proportion of participants who preferred the linear contract offered in the Base Case, and τ^L_{BC-TH} is the estimated treatment effect of Base Case relative to Threshold among participants who preferred the linear contract.[71]

Column I of Figure H.1 displays the estimated treatment effects of personalization (relative to assigning everyone to Threshold) on compliance.[72] The first 4 rows show the effects of personalizing with choice, the actual impatience index, the predicted impatience index, and the policy prediction (described in Section Appendix E), respectively. Because all of the impatience measures predict higher compliance in Threshold (Table 3), personalizing based on each of them significantly increase compliance by roughly 2 pp.[73] However, as shown in column II of Figure H.1, each method also significantly decreases cost-effectiveness, with effects ranging from 5-7 pp. Thus, no personalized approach unambiguously outperforms assigning everyone to the threshold. This may in part reflect the imperfection of each impatience measure. Indeed, using multiple measures to decrease exclusion errors is a promising approach – Row 5 shows that assigning the threshold to those who have above-median impatience index *or* who chose it improves cost-effectiveness without decreasing compliance relative to either measure alone.

[71]For expositional simplicity, we describe this estimate as though we implemented a single threshold contract, while in fact we implemented two. We measured preferences for each threshold contract (4- and 5-day) relative to the linear contract. Over 90% of participants either always preferred linear or always preferred threshold payment, so our main specification uses an indicator that the participant preferred *both* threshold contracts as the measure of preferring the threshold (and 1 minus that indicator as the measure of preferring linear) and uses the pooled threshold groups to calculate τ^L_{BC-TH}. The estimates are robust to two other methods: (1) using an indicator that the participant preferred *either* threshold contract, and (2) only using the 4- (or 5-) day contract preference and threshold treatment effects.

[72]Column III of Figure H.1 shows that the results are robust to a different estimation method: for each measure of impatience, we construct a "synthetic personalized group" consisting of participants who were randomly assigned the contract that they "should" have been according to that impatience measure (e.g., people with below-median actual impatience randomly assigned to the base case group). We then compare this synthetic personalized group to the threshold groups, as described in the Figure H.1 notes.

[73]The success of choice-based personalization in our setting compared to, for example, Bai et al. 2021 may reflect the relatively high demand for commitment (around 50%). This may in turn reflect that people are relatively sophisticated in the domain of walking (Dizon-Ross and Zucker, 2023).



Appendix Figure H.1: Personalizing Thresholds Increases Compliance but Decreases Cost-Effectiveness

Notes: This figure compares the effect of personalizing the assignment of linear and threshold contracts relative to assigning all participants to the threshold contract. The first row assigns the threshold to the people who prefer both threshold contracts to the linear contract, and the linear contract to those who do not. Rows 2-4 assign the threshold contract to those with above-median values of the respective index and the linear contract to those with below-median. Row 5 assigns the threshold contract to those who are assigned it in rows 1 *or* 2. Columns I and II show estimates from the method described in the main text of Section H ("Method 1"), while Columns III and IV show estimates from the "synthetic personalized group" method ("Method 2"). In Method 1, the effect of each assignment mechanism is calculated by multiplying the fraction of people assigned to the linear contract by the treatment effect of Base Case relative to Threshold in that subgroup. For Method 2, the synthetic personalized groups are constructed by duplicating the Threshold and Base Case groups and keeping only participants who preferred and were randomly assigned to the Base Case or likewise for the Threshold (Row 1), have above-median actual impatience and assigned to Threshold or below-median actual impatience and assigned to Base Case (Row 2), etc. To compare the outcomes of each synthetic personalized group with the threshold groups, each observation is weighted by the inverse of the probability of assignment to their treatment group (Base Case or Threshold) to account for over-representation of people who prefer the contract that is more frequently assigned. Confidence intervals are bootstrapped to account for the randomness in the fraction of people assigned threshold in each subgroup. All comparisons come from regressions with the same controls as Table 2. The sample sizes for rows 1-5 are 1798, 1075, 1969, 1746 and 953respectively for Method 1, and 1836, 1010, 1811, 1685 and 1021for Method 2.

I Theoretical Predictions: Additional Proofs

I.1 Proofs of Section B.2 Propositions

We begin by proving Proposition 1 for $T \geq 2$. We then prove Propositions 3, 4, and 5.

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 will be weakly decreasing in $\delta^{(t)}$ for all $t \leq T - 1$.*

Proof. Let $V_{t,j}^{(1)}$ be the value of being on day t having complied on all previous days 1 through $t-1$, where the value is evaluated from the perspective of the agent on day $j \leq t$. Let $V_{t,j}^{(0)}$ be the value of being on day t having *not* complied on at least one of the previous days 1 through $t-1$, again evaluated from the day j perspective. And let $V_{t,j}^{(1-0)} = V_{t,j}^{(1)} - V_{t,j}^{(0)}$. Correspondingly, let $w_t(e_t, 1)$ be the compliance decision on day t if the person has effort cost e_t and has complied on all prior days, and let $w_t(e_t, 0)$ be the compliance decision on day t if the person has effort cost e_t and has not complied on all prior days. If the person has complied on all previous days, we thus have that day t compliance is as follows:

$$w_t(e_t, 1) = \begin{cases} 1 & \text{if } e_t < V_{t+1,t}^{(1-0)} \\ 0 & \text{otherwise} \end{cases} \tag{24}$$

and as follows if the person has not complied on all previous days

$$w_t(e_t, 0) = \begin{cases} 1 & \text{if } e_t < 0 \\ 0 & \text{otherwise} \end{cases} \tag{25}$$

We look at naifs first and then sophisticates. For both types, we begin by examining day T and then use the day T result to show results for days $t < T$. On day j, naifs think that, on day T, conditional on complying on days 1 through $T-1$, their day-T self will comply if $\delta^{(T-j)} e_T < d^{(T-j)} M$, or equivalently if $d^{(T-j)} M - \delta^{(T-j)} e_T > 0$. Their value if they comply is the discounted payment net of discounted effort costs, $d^{(T-j)} M - \delta^{(T-j)} e_T$. Hence, we have

$$\begin{aligned} V_{T,j}^{(1)} &= \mathbb{E}\left[\left(d^{(T-j)} M - \delta^{(T-j)} e_T\right) \mathbb{1}\{d^{(T-j)} M - \delta^{(T-j)} e_T > 0\} \middle| e_1, \ldots, e_j\right] \ , \ j = 1, \ldots, T \\ &= \mathbb{E}\left[\max\{d^{(T-j)} M - \delta^{(T-j)} e_T, 0\} | e_1, \ldots, e_j\right] \ , \ j = 1, \ldots, T \end{aligned} \tag{26}$$

They also think that, on any day t including T, if they haven't complied on all days through $t-1$, they will comply if $\delta^{(t-j)} e_t < 0$, which is equivalent to $e_t < 0$, which yields

$$V_{t,j}^{(0)} = \mathbb{E}\left[-\delta^{(t-j)} e_t \mathbb{1}\{e_t < 0\} | e_1, \ldots, e_j\right] \ , \ j = 1, \ldots, t \tag{27}$$

As a result, we have that:

$$V_{T,j}^{(1-0)} = \mathbb{E}\left[\max\{d^{(T-j)} M - \delta^{(T-j)} e_T, 0\} + \delta^{(T-j)} e_T \mathbb{1}\{e_T < 0\} | e_1, \ldots, e_j\right] \tag{28}$$

To show that this expectation is decreasing in $\delta^{(T-j)}$, we show that the argument, $\max\{d^{(T-j)} M - \delta^{(T-j)} e_T, 0\} + \delta^{(T-j)} e_T \mathbb{1}\{e_T < 0\}$, is decreasing in δ for all values of e_T. Consider two cases:

1. Case 1: $e_T > 0$. In this case,

$$\max\{d^{(T-j)}M - \delta^{(T-j)}e_T, 0\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\} = \max\{d^{(T-j)}M - \delta^{(T-j)}e_T, 0\},$$

which is decreasing in $\delta^{(T-j)}$.

2. Case 2: $e_T \leq 0$ In this case, letting $u = -e_T \geq 0$, we have

$$\begin{aligned}
&\max\{d^{(T-j)}M - \delta^{(T-j)}e_T, 0\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\} \\
&= \begin{cases} \max\{d^{(T-j)}M + \delta^{(T-j)}u, 0\} - \delta^{(T-j)}u & \text{if } e_T \neq 0 \\ d^{(T-j)}M & \text{if } e_T = 0 \end{cases} \\
&= d^{(T-j)}M,
\end{aligned}$$

which is invariant to $\delta^{(T-j)}$.

Thus, $\max\{d^{(T-j)}M - \delta^{(T-j)}e_T, 0\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\}$ is weakly decreasing in $\delta^{(T-j)}$ for all e_t, and so, by taking expectations, equation (28) must also be decreasing in $\delta^{(T-j)}$.

In addition, on day j, naifs think that, conditional on having complied on days 1 through $t-1$, they will comply on day $t \geq j$, if $\delta^{(t-j)}e_t < V^{(1-0)}_{t+1,j}$. So, for $t \leq T-1$ we have:

$$\begin{aligned}
V^{(1)}_{t,j} &= \mathbb{E}\left[\left(V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_t\right)\mathbb{1}\left\{\delta^{(t-j)}e_t < V^{(1-0)}_{t+1,j}\right\}|e_1,\ldots,e_j\right], \ j = 1,\ldots,t \\
&= \mathbb{E}\left[\max\{V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_t, 0\}|e_1,\ldots,e_j\right], \ j = 1,\ldots,t
\end{aligned}$$

Combined with equation (27) this yields:

$$V^{(1-0)}_{t,j} = \mathbb{E}\left[\max\{V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_t, 0\} + \delta^{(t-j)}e_t\mathbb{1}\{\delta^{(t-j)}e_t < 0\}\middle|e_1,\ldots,e_j\right], \ j = 1,\ldots,t \quad (29)$$

Equations (28) and (29) thus recursively define all of the $V^{(1-0)}_{t,j}$ for any $t \leq T$ and $j \leq t$. Since we already showed that $V^{(1-0)}_{T,j}$ is weakly decreasing in $\delta^{(T-j)}$ for all $j \leq T$ (equation (28)), we can then use reverse induction from $t = T,\ldots,j$ using equations (28) and (29) to see that $V^{(1-0)}_{t,j}$ is decreasing in all $\delta^{(T-j)},\ldots,\delta^{(t-j)}$ for any $t \leq T$ and $j \leq t$.[74]

[74] We make the induction hypothesis that $V^{(1-0)}_{t+1,j}$ is weakly decreasing in $\delta^{(1)},\ldots,\delta^{(t)}$ and show that, under this hypothesis, $V^{(1-0)}_{t,j}$ is also weakly decreasing in $\delta^{(1)},\ldots,\delta^{(t)}$. Since we have already shown that $V^{(1-0)}_{T,j}$ is decreasing in all $\delta^{(1)},\ldots,\delta^{(T-1)}$, the result then follows. To show that $V^{(1-0)}_{t,j}$ is weakly decreasing in all $\delta^{(1)},\ldots,\delta^{(t+1)}$, we show that the argument of equation (29), $\max\{V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_t, 0\} + \delta^{(t-j)}e_t\mathbb{1}\{\delta^{(t-j)}e_t < 0\}$, is decreasing in $\delta^{(t-j)}$ for all e_t. Again there are two cases:

1. Case 1: $e_t > 0$. In this case,

$$\max\{V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_T, 0\} + \delta^{(t-j)}e_t\mathbb{1}\{e_t < 0\} = \max\{V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_T, 0\},$$

which is weakly decreasing in $\delta^{(t-j)}$ under the induction hypothesis.

2. Case 2: $e_t \leq 0$ In this case, letting $u = -e_t \geq 0$, we have

$$\max\{V^{(1-0)}_{t+1,j} - \delta^{(t-j)}e_T, 0\} + \delta^{(t-j)}e_t\mathbb{1}\{e_t < 0\} = \max\{V^{(1-0)}_{t+1,j} + \delta^{(t-j)}u, 0\} - \delta^{(t-j)}u = V^{(1-0)}_{t+1,j},$$

which is again weakly decreasing in $\delta^{(t-j)}$ under the induction hypothesis.

The fact that $V_{t,j}^{(1-0)}$ is decreasing in all $\delta^{(T-j)},\dots,\delta^{(t-j)}$ for any $t \leq T$ and $j \leq t$ shows that day t compliance is also weakly decreasing in all $\delta^{(T-t)},\dots,\delta^{(t-t)}$, since one complies on day t if $e_t < V_{t+1,t}^{(1-0)}$ (equation (24)). Hence, overall compliance C from days $1,\dots,T$, is weakly decreasing in $\delta^{(1)},\dots,\delta^{(T-1)}$ for naifs.

Sophisticates know that, conditional on complying on all prior days, on day T they will comply if $e_T < M$. Thus, equation (28) becomes:

$$V_{T,j}^{(1-0)} = \mathbb{E}\left[\left(d^{(T-j)}M - \delta^{(T-j)}e_T\right)\mathbb{1}\left\{e_T < M\right\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\}\middle|e_1,\dots,e_j\right] \quad j = 1,\dots,T \tag{30}$$

This is weakly decreasing in $\delta^{(T-j)}$ since the argument is weakly decreasing in $\delta^{(T-j)}$ for all e_T:

1. $e_T > 0$: In this case, $\left(d^{(T-j)}M - \delta^{(T-j)}e_T\right)\mathbb{1}\left\{e_T < M\right\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\} = \left(d^{(T-j)}M - \delta^{(T-j)}e_T\right)\mathbb{1}\left\{e_T < M\right\}$, which is weakly decreasing in $\delta^{(T-j)}$.
2. $e_T \leq 0$: In this case, $\left(d^{(T-j)}M - \delta^{(T-j)}e_T\right)\mathbb{1}\left\{e_T < M\right\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\} = \left(d^{(T-j)}M - \delta^{(T-j)}e_T\right) + \delta^{(T-j)}e_T = d^{(T-j)}M$, which is invariant to $\delta^{(T-j)}$.

Thus, $V_{T,j}^{(1-0)}$ is weakly decreasing in $\delta^{(T-j)}$.

Sophisticates also know that, on day $t \leq T-1$, if they have complied on all previous days, they will comply if $e_t < V_{t+1,t}^{(1-0)}$ and so equation (29) becomes:

$$V_{t,j}^{(1-0)} = \mathbb{E}\left[\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t\right)\mathbb{1}\left\{e_t < V_{t+1,t}^{(1-0)}\right\} + \delta^{(t-j)}e_t\mathbb{1}\{e_t < 0\}\middle|e_1,\dots,e_j\right] \quad j = 1,\dots,t \tag{31}$$

Since we showed above that $V_{T,j}^{(1-0)}$ is weakly decreasing in $\delta^{(T-j)}$ for all $j \leq T$, one can thus use equation (31) and the same reverse induction argument as for naifs to show this implies that $V_{t,j}^{(1-0)}$ is decreasing in all $\delta^{(T-j)},\dots,\delta^{(t-j)}$ for all $j \leq t \leq T$.[75] By the same argument used for naifs, this then implies overall compliance C is weakly decreasing in $\delta^{(1)},\dots,\delta^{(T-1)}$ for sophisticates. □

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, let $\delta^{(t)} < 1$ for all $t > 0$ if $\delta^{(t)} < 1$ for any t. Fix all parameters other than $\delta^{(t)}$ for some $t \leq T-1$.*

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[75]Again the induction hypothesis is that $V_{t+1,j}^{(1-0)}$ is weakly decreasing in $\delta^{(1)},\dots,\delta^{(t)}$. One can then use equation (31) to show that this implies that $V_{t,j}^{(1-0)}$ is weakly decreasing in $\delta^{(1)},\dots,\delta^{(t)}$ because the argument, $\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t\right)\mathbb{1}\left\{e_t < V_{t+1,j}^{(1-0)}\right\} + \delta^{(t-j)}e_t\mathbb{1}\{e_t < 0\}$, is weakly decreasing in $\delta^{(1)},\dots,\delta^{(t)}$ for all e_t. There are two cases::

1. $e_t > 0$: In this case, $\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t\right)\mathbb{1}\left\{e_t < V_{t+1,j}^{(1-0)}\right\} + \delta^{(t-j)}e_t\mathbb{1}\{e_t < 0\} = \left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t\right)\mathbb{1}\left\{e_t < V_{t+1,j}^{(1-0)}\right\}$, which is weakly decreasing in $\delta^{(t-j)}$ under the induction hypothesis.
2. $e_t \leq 0$: In this case, $\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t\right)\mathbb{1}\left\{e_t < V_{t+1,j}^{(1-0)}\right\} + \delta^{(t-j)}e_t\mathbb{1}\{e_T < 0\} = V_{t+1,j}^{(1-0)}$, which is weakly decreasing in $\delta^{(t-j)}$ under the induction hypothesis.

Since we have already shown that $V_{T,j}^{(1-0)}$ is weakly decreasing in $\delta^{(1)},\dots,\delta^{(T-1)}$ the result is thus shown.

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. We first examine compliance and then examine effectiveness.

To gain intuition for the compliance result, first think about a person who is fully patient over both effort and payment: $\delta^{(t)} = 1$ and $d^{(t)} = 1$ for all t. That person will comply on all days if $e < m'$ (with m' the per-day reward in the threshold contract) and on no days if $e \geq m'$. In contrast, we now show that when people are impatient over effort, they often will comply even when $e > m'$.

When people are impatient, there are two cases. The first (less interesting) case is where it would be worthwhile for the agent to comply on at least K days in a separable contract paying m': $e < d^{(T-K+1)}m'$. In that case, the threshold does not "bind" and the person just complies on all days t for which $e < d^{(T-t)}m'$. Compliance is just like in the separable contract paying m' and is invariant to $\delta^{(t)}$.

The second (interesting) case is where the agent would not comply on at least K days in a separable contract paying m' ($e \geq d^{(T-K+1)}m'$) and so the threshold "binds." In this case, note that agents will never comply more than K days total.[76]

A naif who is impatient over effort (i.e., for whom $\delta^{(t)} < 1$ for all $t > 0$) will never comply before day $T - K + 1$ (i.e., before the last K days). In period $T - K + 1$, the naif will comply if on day $T - K + 1$:

$$\sum_{t=T-K+1}^{T} \delta^{(t-(T-K+1))} e \leq d^{(K-1)} K m' \tag{32}$$

Compliance on day $T-K+1$ is thus decreasing in $\delta^{(t)}$ for all t from 1 to K. If the naif complies on day $T-K+1$, the naif will then comply on all future days. Hence, compliance is decreasing in $\delta^{(t)}$ for all t from 1 to K.

A sophisticate who is impatient over effort will always comply when a naif with the same discount rates would. In addition, the sophisticate may comply before the last K days as well.[77]

To formalize the sophisticate's conditions for compliance, consider all combinations of size K taken from the days 1 through T. There will be $\binom{T}{K}$ such combinations.[78] Order each combination chronologically and index the ordered days as days $j = 1, ..., K$ with values t_1 through t_k (e.g., if the combination is day 1 and day 3, then $t_1 = 1$ and $t_2 = 3$). A sophisticate will comply exactly K times if, for *any* of the $\binom{T}{K}$ combinations, *all* of the following K constraints

[76] Once people have reached the threshold, they will only comply on the other days if they would have complied on those days for a piece rate of m' and, since the agent would not have complied K days in a separable contract pay m', there will be no additional days that satisfy that criterion after they have reached the threshold.

[77] For example, take the case where $T = 3$ and $K = 2$. There may be cases where the individual would not find it worthwhile to comply on day 2, since $(1 + \delta^{(1)})e > 2dm'$, but would find it worthwhile to comply on day 1, since $(1 + \delta^{(2)})e < 2dm'$. In that case, the sophisticate would comply on days 1 and 3.

[78] In our example with $T = 3$ and $K = 2$, the combinations would be $1, 3$ and $2, 3$.

hold:

$$\sum_{j=1}^{K} \delta^{(t_j - t_1)} e \leq d^{(T-t_1)} K m'$$
$$\sum_{j=2}^{K} \delta^{(t_j - t_2)} e \leq d^{(T-t_2)} K m'$$
$$.....$$
$$\sum_{j=K}^{K} \delta^{(t_j - t_K)} e \leq d^{(T-t_K)} K m' \tag{33}$$

Since any of these constraints is weakly more likely to hold the lower any $\delta^{(t_j - t_1)}$, the result is thus shown for sophisticates as well.

Having shown that compliance in the threshold contract is weakly decreasing in $\delta^{(t)}$, we now just need to show that cost-effectiveness is not increasing in $\delta^{(t)}$ and the effectiveness result follows. To show this, we note that, in the perfect correlation case, regardless of $\delta^{(t)}$, any agent who complies on at least one day will always follow through to reach the threshold and achieve payment. Payments will thus be $m'C$ and cost-effectiveness will thus be $\frac{1}{m'}$ regardless of the discount factors. This is invariant to $\delta^{(t)}$. □

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. We first consider different values of e_H and e_L. First, if $e_H < m'$, then $\sum_{t=1}^{3} w_t = 3$ for all δ and so the prediction trivially goes through. Second, if $e_L \geq m'$, then $\sum_{t=1}^{3} w_t = 0$ for $\delta = 1$. However, some people with $\delta < 1$ may walk in at least one period due to the standard cost-bundling effect (e.g., if they have costs of e_L every period and if $e_L + \delta e_L < 2m'$, then they would walk twice). Thus, the prediction goes through in that case as well. We thus have proved the prediction in the cases where $e_H < m'$ and $e_L \geq m'$ and so we next consider the cases where $e_H \geq m'$ and $e_L < m'$.

To prove the prediction, we examine all 8 potential sequences of costs and prove it separately for each case. Note that we only consider the cases where $e_H \geq m'$ and $e_L < m'$.

1. Cases 1 and 2: e_L, e_L, e_L and e_H, e_H, e_H. Since in these cases, costs are constant across periods, the prediction goes through by using the same arguments as in the proof for the case when costs are perfectly correlated across periods (Proposition 7b).
2. Case 3: e_H, e_H, e_L: Again, neither sophisticates nor naifs walk in period 1 but both walk in period 2 and period 3 if $e_H + \delta e_L < 2m'$ (note that by the assumptions above, since

$e_L < m'$, they will always follow-through so there is no follow-through constraint). Thus total compliance is decreasing in δ.

3. Case 4: e_H, e_L, e_H. Again, nobody walks in period 1. Sophisticates walk in periods 2 and 3 if $e_L + \delta e_H < 2m'$ and $e_H < 2m'$. Naifs walk in period 2 if $e_L + \delta e_H < 2m'$ and in period 3 if they've walked in period 2 and $e_H < 2m'$. Again, total compliance is decreasing in δ.
4. Case 5: e_L, e_H, e_H. Sophisticates walk in period 1 if $e_L + \delta^2 e_H < 2m'$ and they know they will follow through ($e_H < 2m'$). Naifs walk in period 1 if $e_L + \delta^2 e_H < 2m'$. Neither type walks in period 2 since $e_H \geq m'$. Both types walk in period 3 if they walked in period 1 and $e_H < 2m'$. Again total compliance is thus decreasing in δ.
5. Cases 6, 7, and 8: e_L, e_H, e_L; e_L, e_L, e_H; and e_H, e_L, e_L. All people, regardless of δ, walk in the two periods where the cost is e_L, since $e_L + e_L < 2m'$. Nobody walks in the period where the cost is e_H since they know they will walk in the other periods and $e_H \geq m'$. Thus, the prediction (trivially) holds.

To prove the effectiveness part of the result, we examine sophisticates first and then naifs and show that cost-effectiveness is non-increasing in δ for both types. Sophisticates will always get paid for every day they comply. Thus, regardless of δ, if compliance is non-0, cost-effectiveness will be $\frac{1}{m'}$, and hence non-increasing in δ. In contrast with sophisticates, naifs can sometimes not receive payment for a day on which they comply. In case 4, naifs will walk on day 2 if $e_L + \delta e_H < 2m'$ but not walk on day 3—and hence not be paid—if $e_H > 2m'$. Those two conditions are more likely to hold in conjunction the lower is δ. Similarly in case 5, naifs will walk on day 1 if $e_L + \delta^2 < 2m'$ but not receive payment if $e_H > 2m'$, which is again more likely to occur the lower is δ. Since having days of compliance that the principal does not have to pay for increases cost-effectiveness, this means that the lower is δ, the weakly higher cost-effectiveness is for naifs.

Hence, since we have shown that compliance is decreasing in δ whereas cost-effectiveness is non-increasing (and in particular, flat for sophisticates and weakly decreasing for naifs), then we have shown that *effectiveness* is also weakly decreasing in δ.

□

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. We begin by examining compliance and then turn to effectiveness. For the compliance result, we first define some useful notation. Let X_t be the "walking stock" coming into period

t (i.e., sum from period 1 to period $t-1$ of whether the person complied $X_t = \sum_{i=1}^{t-1} w_i$). Let $w_t(X_t)$ be a dummy for whether the person complies in period t as a function of the walking stock coming into period t.

To examine compliance, we work backward. In period 3, behavior will depend on the walking stock X_3:

$$\begin{aligned} w_3(2) &= \mathbb{1}\{e_3 < m'\} \\ w_3(1) &= \mathbb{1}\{e_3 < 2m'\} \\ w_3(0) &= \mathbb{1}\{e_3 < 0\}. \end{aligned}$$

We show that the prediction holds by showing that it holds under all potential cases for e_3.

Case 1: $m' \leq e_3 < 2m'$ In this case, walking in period 3 is

$$\begin{aligned} w_3(2) &= 0 \\ w_3(1) &= 1 \\ w_3(0) &= 0. \end{aligned}$$

Note that this implies the person will never walk three times. Walking in period 2 is

$$\begin{aligned} w_2(1) &= \mathbb{1}\{e_2 \leq \delta e_3\} \\ w_2(0) &= \mathbb{1}\{e_2 + \delta e_3 < 2m'\}. \end{aligned}$$

In period 1, consider two cases:

1. $e_2 + \delta e_3 < 2m'$: she knows she will walk at least twice, and the only question is whether to walk now or later. If $e_1 < min\{\delta e_2, \delta^2 e_3\}$, then she will walk in period 1; if not, then she will wait and walk in periods 2 and 3. Either way, she walks twice.
2. $e_2 + \delta e_3 \geq 2m'$: she knows she will not walk later, so she will walk if $e_1 + min\{\delta e_2, \delta^2 e_3\} < 2m'$.

Thus we can see that when $m' \leq e_3 < 2m'$, overall compliance is as follows:

$$\text{Days walked} = \begin{cases} 2 & \text{if } e_2 + \delta e_3 \leq 2m' \text{ OR } e_1 + \delta min\{e_2, \delta e_3\} \leq 2m' \\ 0 & \text{otherwise.} \end{cases}$$

Thus, compliance is obviously decreasing in δ.

Case 2: $e_3 \geq 2m'$ In this case, the person will never walk in period 3 regardless of the walking stock. Thus, overall compliance is as follows:

$$\text{Days walked} = \begin{cases} 2 & \text{if } e_1 + \delta e_2 < 2m' \text{ AND } e_2 < 2m' \\ 0 & \text{otherwise.} \end{cases}$$

This is again decreasing in δ.

Case 3: $e_3 < m'$ In this case, walking in period 3 is

$$w_3(2) = 1$$
$$w_3(1) = 1$$
$$w_3(0) = 0.$$

There are two cases to consider for e_2:

1. $e_2 < m'$: in this case (for $\delta \leq 1$), discount rates do not matter since the person will walk regardless in periods 2 and 3. Then they walk in period 1 if $e_1 < m'$.
2. $e_2 \geq m'$: in this case, the person will not walk in period 2 with walking stock 1. Thus, the maximum the person will ever walk is two periods (the first or the second and then the third).

$$\text{Days walked} = \begin{cases} 2 & \text{if } (e_1 + \delta^2 e_3 < 2m' \text{ \& } e_3 < 2m') \text{ or } (e_2 + \delta e_3 < 2m' \text{ \& } e_3 < 2m') \\ 0 & \text{otherwise.} \end{cases}$$

Thus days walked is again weakly decreasing in δ.

Thus, we have shown the compliance portion of the result, as we have shown that compliance is weakly decreasing in δ for all potential values of e_3.

To prove the effectiveness part of the result, note that sophisticates will always get paid for every day they comply. Thus, regardless of δ, if compliance is non-0, cost-effectiveness will be $\frac{1}{m'}$. Hence, since compliance is decreasing in δ whereas cost-effectiveness is non-increasing, then effectiveness is also decreasing in δ.

□

I.2 Proofs of Section B.3 Propositions

We now provide the proofs for Propositions 6 - 8b.

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,[79] *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. Take a linear contract with payment m that induces compliance $C > 0$. Equation (3) implies that compliance in a linear contract is $C = \frac{1}{T}\sum_{t=1}^{T} F(d^{(T-t)}m)$, which simplifies to $C = F(m)$when $d^{(T-t)} = 1$. Recall that the cost-effectiveness of a linear contract is $\frac{1}{m}$ (see Section 2.2).

[79]Note that this assumption flexibly accommodates the range from IID to perfect positive correlation, just ruling out negative correlation.

(a) Naifs: Consider a threshold contract that pays $M = m + \varepsilon$. On day 1, the naive agent thinks that, conditional on complying on day 1, she will comply on day 2 if $\delta e_2 < M$. The perceived probability of day 2 compliance conditional on day 1 compliance is $F_{e_2|e_1}(\frac{m+\varepsilon}{\delta})$. For $\delta \simeq 0$, $F_{e_2|e_1}(\frac{m+\varepsilon}{\delta}) \simeq 1$. Hence, for $\delta \simeq 0$, on day 1, the naive agent will comply if $e_1 + \delta E[e_2|e_1] < m + \varepsilon$; the probability of effort on day 1 thus approaches $F(m)$ as $\delta \to 0, \varepsilon \to 0$. Conditional on complying on day 1, the probability of compliance on day 2 then approaches $F_{e_2|e_1<m}(m)$. This is equal to $F(m)$ if costs are IID and is weakly greater than $F(m)$ under our more general assumption that e_2 is weakly increasing in e_1. Overall compliance is thus equal to $0.5(F(m) + F(m)F_{e_2|e_1<m}(m)) = 0.5(C + CF_{e_2|e_1<m}(m)) \geq 0.5C(1 + C)$. Expected payment per period then approaches $0.5mF(m)F_{e_2|e_1<m}(m) = 0.5mCF_{e_2|e_1<m}(m)$. Cost-effectiveness thus approaches $\frac{1}{m}\left(1 + \frac{1}{F_{e_2|e_1<m}(m)}\right) \geq 2/m$. This means the contract generates compliance of at least $(1 + C)/2$ times that of the linear contract and has at least 2 times higher cost-effectiveness. If costs are IID, $F_{e_2|e_1<m}(m) = F(m) = C$, and so cost-effectiveness approaches $\frac{1}{m}\left(1 + \frac{1}{C}\right)$, which is $1 + 1/C$ times larger than the cost-effectiveness of the linear contract.

(b) Sophisticates with IID costs: Now consider a threshold contract that pays $M = m/p' + \varepsilon$ for p' defined as a fixed point to $F(m/p') = p'$. The intermediate value theorem tells us that such a solution exists for $p' \in [C, 1]$ because F is continuous, $F(m/1) \leq 1$, and $F(m/C) \geq F(m) = C$.

Under this threshold contract, conditional on working in the first period, the probability of working in the second period is $F(M) = F(m/p' + \varepsilon) \geq F(m/p') = p'$, with $F(M) \simeq p'$ for $\varepsilon \simeq 0$. Hence, the expected payment conditional on working in the first period is $MF(M) \geq \frac{m}{p'}p' = m$, with this payment approximately m for $\varepsilon \simeq 0$. Therefore, for $\delta \simeq 0$, the probability of effort in the first period is at least $C = F(m)$, and approaches $F(m)$ for $\varepsilon \to 0, \delta \to 0$.

Taking $\varepsilon \to 0$ and then $\delta \to 0$: Total compliance in this contract is approximately $\frac{1}{2}(F(m) + F(m)F(M)) = \frac{1}{2}C(1 + p')$, with $\frac{1}{2}C(1 + p') \geq \frac{1}{2}C(1 + C)$ since $p' \geq C$. Payment per period is approximately $\frac{1}{2}MCp'$, with C the probability of working in the first period and p' the probability of working in the second period conditional on working in the first period; we have $\frac{1}{2}MCp' \simeq \frac{1}{2}\frac{m}{p'}Cp' = \frac{1}{2}mC$. Hence, cost-effectiveness is approximately $(\frac{1}{2}C(1+p'))/(\frac{1}{2}mC) = (1+p')/m \geq (1+C)/m$. □

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. As before, with perfect correlation, the agent takes effort either in both periods or in neither of a threshold contract. Thus the cost-effectiveness of the threshold contract will be $1/m$ and is thus the same as the cost-effectiveness of the linear contract. Therefore, whichever contract has higher compliance will be more effective. On day 1 of the linear, the agent complies if $e_1 < dm$, and on day 2 if $e_2 < m$, and so compliance in the linear contract is $\frac{1}{2}\left(F(dm) + F(m)\right) \leq F(m)$. In the threshold contract, on day 1 (and consequently day 2) the agent complies if $e_1(1 + \delta)d2m$, and so compliance is $F\left(\frac{2d}{1+\delta}m\right)$. Thus, if $\frac{2d}{1+\delta}m > m$ (i.e., if $\delta < 2d - 1$), the threshold contract has higher compliance (and hence effectiveness) than the linear. If that is not true, then the linear could have higher effectiveness. □

Proposition 7b (Perfect Correlation)**.** *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.

Proof. With perfect correlation, the agent takes effort either in both periods or in neither of a threshold contract. Therefore, as long as the agent ever exerts effort, the cost-effectiveness is equal to 2 divided by the threshold payment.

Suppose a linear contract paying m induces $C > 0$ and has cost-effectiveness $\frac{1}{m}$. Note that, because $C = \frac{1}{2}(F(dm) + F(m))$, this implies that $F(m) \geq C$.

Consider a threshold contract with payment $M = m\frac{1+\delta}{d}$. Note that this contract will have cost effectiveness of $2\frac{d}{(1+\delta)m}$, which is $2\frac{d}{(1+\delta)}$ times the cost-effectiveness of the linear contract. On day 1 (and consequently day 2), the agent complies under the threshold contract if $e_1(1+\delta) < dM$ (where the left side comes from the fact that $e_1 = e_2$). With payment $M = m\frac{1+\delta}{d}$, the agent thus complies if $e_1 < m$. Thus, the threshold contract achieves compliance of $F(m) \geq C$. □

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. There is a type-specific "cutoff value" such that 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 $1 \leq M < 2$,[80] then the threshold contract is more effective.

Proof. Note that we take the general solution for compliance and payments for threshold contracts from the proof for Proposition 8b.

For a linear contract with payment level $\frac{M}{2}$, we have:

$$C = \frac{M}{2}$$
$$P = \frac{M^2}{4}$$
$$\frac{C}{P} = \frac{2}{M}$$
$$E = \lambda\frac{M}{2} - \frac{M^2}{4}$$

Now we consider multiple cases for what the threshold contract compliance and payments would be depending on the parameters.

(a) $0 < M < 1$ We begin with naifs and then move to sophisticates. For naifs, there are two cases:

Case 1: $M < \delta$ for Naifs In this case, $E[e_2|e_2 < M/\delta] = \frac{M}{2\delta}$, giving that

$$e_1^* = (M - \delta\frac{M}{2\delta})\frac{M}{\delta} = \frac{M^2}{2\delta}$$

[80] Note that the principal would never pay $M > 2$ since $M = 2$ achieves 100% compliance regardless of δ.

Thus,

$$\begin{aligned} C &= .5\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right] \\ P &= .5\frac{M^4}{2\delta} \end{aligned}$$

Thus, cost-effectiveness is:

$$\frac{C}{P} = \frac{1+M}{M^2}$$

and effectiveness is:

$$E = .5\lambda\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right] - .5\frac{M^4}{2\delta}$$

The threshold has higher cost-effectiveness if:

$$\frac{2}{M} < \frac{1+M}{M^2}.$$

This holds if $2M < 1 + M$ which is always true for $M < 1$. Thus, the threshold is always more cost-effective in this case.

The threshold has higher compliance if:

$$\frac{M}{2} < .5\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right]$$

which simplifies to

$$\delta < \left[\frac{M}{2} + \frac{M^2}{2}\right].$$

This expression is not satisfied because $M < \delta$. Therefore, in this case, the threshold has lower compliance, and may have lower effectiveness. In fact, for $M < \delta$, whether the threshold has higher effectiveness depends on λ, the principal's marginal return to compliance: the higher λ, the more likely the threshold is to have higher effectiveness. Thus, in this range of relatively large δ we are above the cutoff value for naif types, and it is possible that the threshold will have either higher or lower effectiveness.

Case 2: $\delta < M$ for Naifs Because $M > \delta e_2$,

$$e_1^* = E[(M - \delta e_2)\mathbb{1}\{M - \delta e_2 > 0\}] = E[M - \delta e_2] = M - \delta/2$$

Thus,

$$\begin{aligned} C &= .5(M - \delta/2)(1 + M) \\ P &= .5(M - \delta/2)M^2 \end{aligned}$$

giving cost-effectiveness of

$$\frac{C}{P} = \frac{1+M}{M^2}$$

and effectiveness of

$$E = .5\lambda(M - \delta/2)(1 + M) - .5(M - \delta/2)M^2.$$

The cost-effectiveness of the threshold contract is the same as in case 1, and so the threshold contract is again always more cost-effective.

Compliance of the threshold contract is higher than in the linear if:

$$.5M < .5(M - \frac{\delta}{2})(1 + M)$$

which simplifies to:

$$\begin{aligned}
M &< (M - \frac{\delta}{2})(1 + M) \\
M &< M(1 + M) - \frac{\delta}{2}(1 + M) \\
M &< M + M^2 - \frac{\delta}{2}(1 + M) \\
0 &< M^2 - \frac{\delta}{2}(1 + M) \\
\frac{\delta}{2}(1 + M) &< M^2 \\
\delta &< \frac{2M^2}{1 + M}
\end{aligned}$$

Note that, for $M < 1$, it is always true that $\frac{2M^2}{1+M} < M$.

Hence we can see that $\frac{2M^2}{1+M}$ is the cutoff value for naifs. For naifs, if $\delta < \frac{2M^2}{1+M}$ and $M < 1$, the threshold will always be more effective than the linear contract.

For sophisticates, there is just one case:

Case 3: $M < 1$ for Sophisticates In this case,

$$e_1^* = \left(M - \delta\frac{M}{2}\right)M = M^2(1 - \delta/2)$$

Thus,

$$\begin{aligned}
C &= .5(M^2 + M^3)(1 - \delta/2) \\
P &= .5(M^4)(1 - \delta/2)
\end{aligned}$$

Thus, cost-effectiveness is:

$$\frac{C}{P} = \frac{1 + M}{M^2}$$

The cost-effectiveness of the threshold contract is the same as in cases 1 and 2, and so, again, the threshold is always more cost-effective.

The compliance of the threshold contract is higher if:

$$.5M < .5(M^2 + M^3)(1 - \delta/2)$$

which holds if all of the following hold:

$$1 < (M + M^2)(1 - \delta/2)$$
$$\frac{1}{M + M^2} < 1 - \delta/2$$
$$\delta < 2 - \frac{2}{M + M^2}$$

Thus, the cutoff value for sophisticates is $2 - \frac{2}{M+M^2}$. If $\delta < 2 - \frac{2}{M+M^2}$, the threshold contract is more effective. For larger δ, the linear contract may be more effective.

(b) $1 \leq M < 2$ Here naifs and sophisticates behave the same and there are two cases.

Case 4: $1 < M < 1 + \delta/2$ In this case, because $M > \delta e_2$ and $M > e_2$

$$e_1^* = M - \delta/2$$

Because $M - \delta/2 < 1$,

$$\begin{aligned} C &= (M - \delta/2) \\ P &= .5M(M - \delta/2) \end{aligned}$$

giving

$$\frac{C}{P} = \frac{2}{M}.$$

This is the same cost-effectiveness as the linear contract. Hence, whichever contract has higher compliance will have higher effectiveness. Threshold compliance will be higher if:

$$\begin{aligned} M/2 &< (M - \delta/2) \\ \delta/2 &< M/2 \\ \delta &< M \end{aligned}$$

which is always true assuming that $\delta \leq 1$, since $M > 1$. Hence the threshold is always more effective.

Case 5: $1 + \delta/2 < M < 2$ Again, because $M > \delta e_2$ and $M > e_2$

$$e_1^* = M - \delta/2$$

Because $M - \delta/2 > 1$,

$$\begin{aligned} C &= 1 \\ P &= .5M \end{aligned}$$

giving

$$\frac{C}{P} = \frac{2}{M},$$

which is again the same as the cost-effectiveness of the linear contract. Hence, the threshold will have higher effectiveness if it has higher compliance, which is true if

$$M/2 < 1,$$

which will always be the case for $M < 2$. Hence, the threshold is always more effective. $\square$

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,[81] 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. We begin with a more precise statement of the result, before proceeding to prove the result. Specifically, the following describes how the effectiveness of optimal threshold contract relative to the optimal linear one depends on the value of δ in different ranges of λ values:

(a) Naifs for $0 < \lambda < 0.225$, and naifs and sophisticates for $0.225 \leq \lambda < 1$ and $3 \leq \lambda \leq 2 + \sqrt{2}$. In these cases, there is 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.

(b) Naifs and Sophisticates for $1 \leq \lambda < 3$. In this case, the threshold contract is more effective than the linear contract for all δ, with the gap decreasing in δ.

(c) Sophisticates for $\lambda < 0.225$ and naifs and sophisticates for $\lambda > 2 + \sqrt{2}$. In this case, the linear contract is always more effective, with the gap increasing in δ.

To prove the result, we begin by calculating the optimal linear and threshold contracts. For both, we proceed in two steps: we first solve for the compliance, effectiveness, and cost-effectiveness of any given linear or threshold contract, and then we solve for the optimal contract. Finally, we compare the optimal linear and threshold contracts within different ranges of λ.

Linear Contract Compliance and Effectiveness: Consider a linear contract with payment level $\frac{M}{2}$. Substituting this into the formulas from Section 2, we have the following values for compliance, daily payment, cost-effectiveness, and effectiveness, respectively:

$$C = \frac{M}{2}$$
$$P = \frac{M^2}{4}$$
$$\frac{C}{P} = \frac{2}{M}$$
$$E = \lambda\frac{M}{2} - \frac{M^2}{4}$$

[81] See the beginning of the proof for specific ranges for both naifs and sophisticates.

Optimal Linear Contract: We want to choose the payment level to maximize contract effectiveness. The first-order condition for maximizing effectiveness is:

$$\frac{\partial E}{\partial M} = \frac{\lambda}{2} - \frac{M}{2} = 0$$

Denoting the arg max as M^{L*}, the payment level in the optimal linear contract is thus:

$$M^{L*} = \lambda$$

and the effectiveness of the optimal linear contract (which we will denote as E^{L*}) is:

$$\begin{aligned} E^{L*} &= \lambda\frac{M^{L*}}{2} - \frac{M^{L*2}}{4} \\ &= \lambda\frac{\lambda}{2} - \frac{\lambda^2}{4} \\ &= \frac{\lambda^2}{4} \end{aligned}$$

Threshold Contract Compliance and Effectiveness: We begin by solving for compliance, payments, and effectiveness in a two period threshold contract with payment level M. In the two-period IID threshold case, the agent complies in period 2 if they complied in period 1 and $e_2 < M$. Moreover, equation (15) implies that the agent will comply in period 1 if:

$$e_1 < E[(M - \delta e_2)w_{2,1}|w_1 = 1]. \tag{34}$$

Let $e_1^* = E[(M - \delta e_2)w_{2,1}|w_1 = 1]$ be the maximum effort cost that results in compliance. For naifs, for whom $w_{2,1}|^{(w_1=1)} = \mathbb{1}\{M - \delta e_2 > 0\}$,

$$\begin{aligned} e_1^* &= E[(M - \delta e_2)\mathbb{1}\{M - \delta e_2 > 0\}] \\ &= E[M - \delta e_2|\delta e_2 < M] \times Prob(\delta e_2 < M) \\ &= (M - \delta E[e_2|e_2 < M/\delta])F(M/\delta) \end{aligned}$$

For sophisticates, for whom $w_{2,1}|^{(w_1=1)} = \mathbb{1}\{M - e_2 > 0\}$,

$$\begin{aligned} e_1^* &= E[(M - \delta e_2)\mathbb{1}\{M - e_2 > 0\}] \\ &= E[M - \delta e_2|e_2 < M] \times Prob(e_2 < M) \\ &= (M - \delta E[e_2|e_2 < M])F(M) \end{aligned}$$

Compliance and payments are functions of e_1^*:

$$\begin{aligned} C &= .5[F(e_1^*) + F(e_1^*)F(M)] \\ P &= .5MF(e_1^*)F(M) \end{aligned}$$

Effectiveness depends on the size of M and δ. When $\mathbf{0 < M < 1}$, we explore two cases for naifs and a single case for sophisticates based on the relative size of δ.:

Case 1: $0 < M < \delta < 1$ for Naifs In this case, $E[e_2|e_2 < M/\delta] = \frac{M}{2\delta}$, giving the following values for e_1^*, C, and P :

$$\begin{aligned} e_1^* &= (M - \delta\frac{M}{2\delta})\frac{M}{\delta} \\ &= \frac{M^2}{2\delta} \\ C &= .5\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right] \\ P &= .5\frac{M^4}{2\delta} \end{aligned}$$

Thus, cost-effectiveness and effectiveness, respectively, are:

$$\frac{C}{P} = \frac{1+M}{M^2}$$

and

$$E = .5\lambda\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right] - .5\frac{M^4}{2\delta}$$

Case 2: $0 < \delta < M < 1$ for Naifs In this case, because $M > \delta e_2$, the value e_1^* is:

$$e_1^* = E[(M - \delta e_2)\mathbb{1}\{M - \delta e_2 > 0\}] = E[M - \delta e_2] = M - \delta/2$$

This yields compliance and payments of:

$$\begin{aligned} C &= .5(M - \delta/2)(1 + M) \\ P &= .5(M - \delta/2)M^2 \end{aligned}$$

This gives cost-effectiveness and effectiveness, respectively, of:

$$\frac{C}{P} = \frac{1+M}{M^2}$$

and

$$E = .5\lambda(M - \delta/2)(1 + M) - .5(M - \delta/2)M^2.$$

Case 3: $0 < M < 1$ for Sophisticates In this case, the value e_1^* is:

$$e_1^* = \left(M - \delta\frac{M}{2}\right)M = M^2(1 - \delta/2)$$

So compliance and payments are:

$$\begin{aligned} C &= .5(M^2 + M^3)(1 - \delta/2) \\ P &= .5(M^4)(1 - \delta/2) \end{aligned}$$

and cost-effectiveness and effectiveness, respectively, are:

$$\frac{C}{P} = \frac{1+M}{M^2}$$

and

$$E = .5\lambda(M^2 + M^3)(1 - \delta/2) - .5(M^4)(1 - \delta/2).$$

For larger values of M, such that $\mathbf{1 \leq M < 2}$, naifs and sophisticates behave the same way. We consider two more cases.

Case 4: $1 < M < 1 + \delta/2$ for Naifs and Sophisticates In this case, because $M > \delta e_2$ and $M > e_2$, the value e_1^* is:

$$e_1^* = M - \delta/2$$

Furthermore, because $M - \delta/2 < 1$, compliance and payments are:

$$\begin{aligned} C &= (M - \delta/2) \\ P &= .5M(M - \delta/2) \end{aligned}$$

giving cost-effectiveness and effectiveness, respectively, of

$$\frac{C}{P} = \frac{2}{M}$$

and

$$E = \lambda(M - \delta/2) - .5M(M - \delta/2).$$

Case 5: $1 + \delta/2 < M < 2$ for Naifs and Sophisticates Again, because $M > \delta e_2$ and $M > e_2$, the value e_1^* is:

$$e_1^* = M - \delta/2$$

Because in this case $M - \delta/2 > 1$, compliance and payments are:

$$\begin{aligned} C &= 1 \\ P &= .5M \end{aligned}$$

giving cost-effectiveness and effectiveness, respectively, of

$$\frac{C}{P} = \frac{2}{M}$$

and

$$E = \lambda - .5M.$$

Having solved for compliance, payments, and effectiveness for naifs and sophisticates and for all M between 0 and 2, we now derive the payment level of the optimal threshold contract, which we denote as M^{T*}, and its effectiveness, which we denote as E^{T*}. We first consider sophisticates and then naifs.

Optimal threshold contract for sophisticates:
Aggregating cases 3-5 above, we have that effectiveness for sophisticates is as follows:

$$E = \begin{cases} .5(1-\delta/2)\left(\lambda(M^2+M^3)-M^4\right) & \text{if } M<1 \\ \lambda(M-\delta/2)-M^2/2+\delta M/4 & \text{if } 1\le M<1+\delta/2 \\ \lambda - M/2 & \text{if } 1+\delta/2\le M \end{cases}$$

The derivative of effectiveness with respect to the payment level M is:

$$\frac{\partial E}{\partial M} = \begin{cases} .5(1-\delta/2)\left(\lambda(2M+3M^2)-4M^3\right) & \text{if } M<1 \\ \lambda - M+\delta/4 & \text{if } 1\le M<1+\delta/2 \\ -1/2 & \text{if } 1+\delta/2\le M \end{cases}$$

The payment level of the optimal threshold contract, M^{T*}, will set this derivative equal to zero. Note that if $1+\delta/2 \le M$, it follows that $\frac{\partial E}{\partial M}<0$ (since $M=1+\delta/2$ achieves full compliance). Hence, M^{T*} is always smaller than $1+\delta/2$. However, the exact value of M^{T*} depends on the value of λ. We consider three cases, (A) - (C).

Case A: $\lambda \ge 1+\delta/4$
In this case, we have that $\frac{\partial E}{\partial M}|^{1\le M<1+\delta/2} = \lambda - M+\delta/4 > 0$ for $1\le M<1+\delta/2$. In addition, $\frac{\partial E}{\partial M}|^{M<1} = .5(1-\delta/2)\left(\lambda(2M+3M^2)-4M^3\right)$ is always positive.[82] Combined with the fact that $\frac{\partial E}{\partial M}|^{M>1+\delta/2}<0$, the optimal payment is:

$$M^{T*}|^{\lambda>1+\delta/4} = 1+\delta/2.$$

and the effectiveness of the optimal threshold contract is

$$\begin{aligned} E^{T*}|^{\lambda>1+\delta/4} &= \lambda - M^*/2 \\ &= \lambda - .5 - \delta/4 \end{aligned}$$

Case B: $\lambda < 1-\delta/4$
In this case, $\frac{\partial E}{\partial M}|^{1\le M<1+\delta/2} = \lambda - M+\delta/4<0$ for all $1\le M<1+\delta/2$. Recall that $\frac{\partial E}{\partial M}|^{M>1+\delta/2}<0$ in all cases. Hence $\frac{\partial E}{\partial M}|^{M>1}<0$, which implies that the optimum must have $M\le 1$.

We hence set the $\frac{\partial E}{\partial M}|^{M<1}=0$, which yields:

$$\frac{\partial E}{\partial M}|^{M<1} = .5(1-\delta/2)\left(\lambda(2M+3M^2)-4M^3\right)=0$$

which implies

$$\lambda(2M+3M^2)-4M^3=0$$

[82] This is because, given $\lambda\ge 1$, the function $\lambda(2M+3M^2)-4M^3$ increases at $M=0$ and is never 0 in (0,1].

or that

$$\lambda(2+3M) - 4M^2 = 0$$

The solution to this quadratic is:

$$M = \lambda\left(\frac{3}{8} + \sqrt{\frac{9}{64} + \frac{1}{2\lambda}}\right)$$

This M falls in the region $M < 1$ whenever $\lambda < \frac{4}{5}$. When $\lambda \geq \frac{4}{5}$, $\frac{\partial E}{\partial M}|^{M<1} > 0$ for all $M < 1$, which (combined with the fact that $\frac{\partial E}{\partial M}|^{M>1} < 0$) implies that the optimal M must be at the "kink point" where M=1:

$$M^{T*}|^{\lambda<1-\delta/4 \text{ \& } \lambda>4/5} = 1$$

Note that having $\lambda \geq \frac{4}{5}$ while $\lambda < 1 - \delta/4$ implies a relatively low δ.

Thus we have:

$$M^{T*}|^{\lambda<1-\delta/4} = \begin{cases} \lambda\left(\frac{3}{8} + \sqrt{\frac{9}{64} + \frac{1}{2\lambda}}\right) & \text{if } \lambda < 4/5 \& \lambda < 1-\delta/4 \\ 1 & \text{if } \lambda \geq 4/5 \& \lambda < 1-\delta/4 \end{cases}$$

This implies that maximized effectiveness when $\lambda < 4/5$ is:

$$\begin{aligned} E^{T*}|^{\lambda<1-\delta/4 \text{ \& } \lambda<4/5} &= .5(1-\delta/2)\left(\lambda(M^2+M^3) - M^4\right)|^{M=\lambda\left(\frac{3}{8}+\sqrt{\frac{9}{64}+\frac{1}{2\lambda}}\right)} \\ &= \frac{1}{16}(2-\delta)\left(\frac{3}{8} + \frac{1}{8}\sqrt{9+\frac{32}{\lambda}}\right)^2 \lambda^3 \left(4 + \lambda\left(\frac{15}{16} + \frac{1}{16}\left(-9 - \frac{32}{\lambda}\right) + \frac{1}{8}\sqrt{9+\frac{32}{\lambda}}\right)\right) \end{aligned}$$

When $\lambda \geq 4/5$, maximized effectiveness is:

$$\begin{aligned} E^{T*}|^{\lambda<1-\delta/4 \text{ \& } \lambda\geq 4/5} &= \lambda(M-\delta/2) - M^2/2 + \delta M/4|^{M=1} \\ &= \lambda(1-\delta/2) - 1/2 + \delta/4 \\ &= \lambda - 1/2 - \delta(\lambda/2 - 1/4) \end{aligned}$$

Note that both of these are decreasing in δ (where the latter holds because $\lambda/2 - 1/4 > 0$ when $\lambda > 4/5$.

Case C: $1 - \delta/4 \leq \lambda < 1 + \delta/4$ In this case, we have that $\frac{\partial E}{\partial M}|^{1\leq M<1+\delta/2} = \lambda - M + \delta/4 = 0$ somewhere in the region of $1 \leq M < 1 + \delta/2$ — that is, there is a local max in this region.

There are two subcases.

Subcase C(i): $1 - \delta/4 \leq \lambda < 1 + \delta/4$ **and** $\lambda \geq 4/5$

If $\lambda \geq 4/5$, then $\frac{\partial E}{\partial M}|^{M<1} > 0$, which means that the optimum must be the local max in the region of $1 \leq M < 1 + \delta/2$.

We thus solve for this local maximum by finding the M at which $\frac{\partial E}{\partial M}|^{1\leq M<1+\delta/2}$ is 0:

$$\frac{\partial E}{\partial M}|^{1\leq M<1+\delta/2} = \lambda - M^* + \delta/4 = 0$$

which implies that

$$M^* = \lambda + \delta/4$$

which means that

$$\begin{aligned} E^{T*}\Big|^{1-\delta/4<\lambda<1+\delta/4\ \&\ \lambda>4/5} &= \lambda(M^* - \delta/2) - M^{*2}/2 + \delta M^*/4 \\ &= \lambda(\lambda + \delta/4 - \delta/2) - (\lambda + \delta/4)^2/2 + \delta(\lambda + \delta/4)/4 \\ &= \lambda^2 - \lambda\delta/4 - \lambda^2/2 - \lambda\delta/4 - \delta^2/32 + \lambda\delta/4 + \delta^2/16 \\ &= \lambda^2/2 - \lambda\delta/4 + \delta^2/32 \end{aligned}$$

Note again that this is decreasing in δ for all $\lambda > 4/5$ and $\delta \leq 1$.[83]

Subcase C(ii): $1 - \delta/4 \leq \lambda < 1 + \delta/4$ **and** $\lambda < 4/5$

In this case, there are two local maxima: one when $M < 1$ and one when $1 \leq M < 1+\delta/2$. The global maximum thus is the larger of those two values:

$$E^{T*} = \max\Bigg\{ \lambda^2/2 - \lambda\delta/4 + \delta^2/32, \\ \frac{1}{16}(2-\delta)\left(\frac{3}{8} + \frac{1}{8}\sqrt{9 + \frac{32}{\lambda}}\right)^2 \lambda^3 \left(4 + \lambda\left(\frac{15}{16} + \frac{1}{16}\left(-9 - \frac{32}{\lambda}\right) + \frac{1}{8}\sqrt{9 + \frac{32}{\lambda}}\right)\right)\Bigg\}$$

We next aggregate the cases into a single solution for the effectiveness of the most effective threshold for sophisticates as a function of δ. We then compare the most effective threshold and linear contracts as δ changes. However, the solution function depends on λ.

Threshold vs. Linear Effectiveness with $\lambda \geq 4/5$.

When $\lambda \geq 4/5$, we aggregate the effectiveness function of the optimal threshold contract from cases A-C as:

$$E^{T*}|^{\lambda \geq 4/5} = \begin{cases} \lambda - 1/2 - \delta(\lambda/2 - 1/4) & \text{if } \lambda < 1 - \delta/4 \\ \lambda^2/2 - \lambda\delta/4 + \delta^2/32 & \text{if } 1 - \delta/4 \leq \lambda < 1 + \delta/4 \\ \lambda - .5 - \delta/4 & \text{if } \lambda > 1 + \delta/4. \end{cases}$$

We can rewrite effectiveness more transparently as a function of δ. If $4/5 \leq \lambda < 1$, we have:

$$E^{T*} = \begin{cases} \lambda - 1/2 - \delta(\lambda/2 - 1/4) & \text{if } \delta < 4(1 - \lambda) \\ \lambda^2/2 - \lambda\delta/4 + \delta^2/32 & \text{if } \delta \geq 4(1 - \lambda) \end{cases}$$

and if $1 \leq \lambda$, we have

$$E^{T*} = \begin{cases} \lambda^2/2 - \lambda\delta/4 + \delta^2/32 & \text{if } \delta > 4(\lambda - 1) \\ \lambda - .5 - \delta/4 & \text{if } \delta \leq 4(\lambda - 1) \end{cases}$$

[83] This is because the function $-\lambda/4 + \delta/16$ is negative for all $\lambda \geq 4/5$ as long as $\delta < 16/5$.

Note that each of these functions is continuous in δ. Moreover, because each segment is decreasing in δ, we achieve the important result: $\frac{\partial E^{T*}}{\partial \delta} < 0$. That is, the effectiveness of the most effective threshold contract is decreasing in δ.

Now we compare the effectiveness of the optimal threshold and linear contracts in the region $\lambda \geq 4/5$. First consider the case where $4/5 \leq \lambda < 1$. For $\delta < 4(1-\lambda)$, $E^{T*} > E^{L*}$ would require $\delta > \frac{\lambda^2/4-\lambda+1/2}{1/4-\lambda/2}$, but this value is greater than $4(1-\lambda)$ for $4/5 \leq \lambda < 1$. So the linear contract is always more effective if $\delta < 4(1-\lambda)$. For $\delta \geq 4(1-\lambda)$, in order for $E^{T*} > E^{L*} = \lambda^2/4$, it would require that $\lambda^2/2 - \lambda\delta/4 + \delta^2/32 > \lambda^2/4$ or $\delta < (4-2\sqrt{2})\lambda$. Since $(4-2\sqrt{2})\lambda > 1$ if $\lambda > \frac{1}{4-2\sqrt{2}} \approx 0.85$, the threshold contract will always be more effective for $\lambda > 0.85$ and $\delta \geq 4(1-\lambda)$. And then for $\lambda \leq 0.85$, which contract is more effective depends on the exact value of δ.

In case where $\lambda \geq 1$, if $\delta > 4(\lambda-1)$, $E^{T*} > E^{L*}$ would require $\delta < (4-2\sqrt{2})\lambda$, which is always true for $\lambda \geq 1$. If $\delta \leq 4(\lambda-1)$, $E^{T*} > E^{L*}$ would require $\delta < -\lambda^2 + 4\lambda - 2$. This holds for all $\delta \in [0,1]$ if $\lambda < 3$, for some δ if $3 \leq \lambda < 2+\sqrt{2}$, and no δ if $\lambda \geq 2+\sqrt{2}$.

Threshold vs. Linear Effectiveness with $\lambda < 4/5$ Now, we write the effectiveness of the optimal threshold contract as a function of λ and δ when $\lambda < 4/5$.

Let $\xi(\lambda) = \frac{1}{16}\left(\frac{3}{8} + \frac{1}{8}\sqrt{9+\frac{32}{\lambda}}\right)^2 \lambda^3 \left(4 + \lambda\left(\frac{15}{16} + \frac{1}{16}\left(-9 - \frac{32}{\lambda}\right) + \frac{1}{8}\sqrt{9+\frac{32}{\lambda}}\right)\right)$. Then we have

$$E^{T*}|^{\lambda<4/5.} = \begin{cases} (2-\delta)\,\xi(\lambda) & \text{if } \lambda < 1-\delta/4 \\ \max\left\{(2-\delta)\,\xi(\lambda), \lambda^2/2 - \lambda\delta/4 + \delta^2/32\right\} & \text{if } 1-\delta/4 \leq \lambda \end{cases}$$

or equivalently:

$$E^{T*}|^{\lambda<4/5.} = \begin{cases} (2-\delta)\,\xi(\lambda) & \text{if } \delta < 4(1-\lambda) \\ \max\left\{(2-\delta)\,\xi(\lambda), \lambda^2/2 - \lambda\delta/4 + \delta^2/32\right\} & \text{if } 4(1-\lambda) \leq \delta \end{cases}$$

If $\lambda < 0.75$, then $\delta < 4(1-\lambda)$ and so we have the $E^{T*} = (2-\delta)\,\xi(\lambda)$. This function is continuous and decreasing in δ.

Threshold effectiveness will in this case be higher than linear if

$$(2-\delta)\,\xi(\lambda) > \lambda^2/4.$$

This implies that threshold effectiveness is higher if

$$\delta < 2 - \frac{\lambda^2}{4\xi(\lambda)}.$$

Since the function $2 - \frac{\lambda^2}{4\xi(\lambda)}$ is negative for $\lambda \leq 0.225$, the linear contract is always more effective for this range of λ. For $\lambda > 0.225$, there is a cutoff value for δ where the optimal threshold contract is more effective for δ below the threshold.

If $0.75 \leq \lambda < 0.8$, we need some additional analysis on the function E^{T*}. Both $(2-\delta)\,\xi(\lambda)$ and $\lambda^2/2 - \lambda\delta/4 + \delta^2/32$ are continuous for $\delta \in [0,1]$, and E^{T*} is continuous at $\delta = 4(1-\lambda)$ since $(2-\delta)\,\xi(\lambda) > \lambda^2/2 - \lambda\delta/4 + \delta^2/32$ at $\delta = 4(1-\lambda)$. Also the maximum of two continuous functions is continuous, so E^{T*} is continuous in δ. Then if $E^{T*} > E^{L*}$ when $\delta = 0$ and $E^{T*} < E^{L*}$ when $\delta = 1$, there is some threshold value of δ for which the linear and threshold contracts will have the same effectiveness, and above that the threshold will have higher effectiveness and below that the linear will. This is true as long as $\lambda > 0.225$, which holds for all λ in this interval. So again there is a cutoff value for δ below which the threshold contract is more effective.

Optimal threshold contract for naifs: Again using the formulas from the proof of Proposition 8a, we have that effectiveness is as follows:

$$E = \begin{cases} .5\lambda\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right] - .5\frac{M^4}{2\delta} & \text{if } M \leq \delta \\ .5\lambda(M-\delta/2)(1+M) - .5(M-\delta/2)M^2 & \text{if } \delta < M < 1 \\ \lambda(M-\delta/2) - M^2/2 + \delta M/4 & \text{if } 1 \leq M < 1+\delta/2 \\ \lambda - M/2 & \text{if } 1+\delta/2 \leq M \end{cases}$$

The derivative of effectiveness w.r.t. M is hence:

$$\frac{\partial E}{\partial M} = \begin{cases} 0.5\lambda\left[\frac{M}{\delta} + \frac{3M^2}{2\delta}\right] - \frac{M^3}{\delta} & \text{if } M \leq \delta \\ -0.5M^2 + 0.5\lambda(1+M) + 0.5\lambda(-(\delta/2)+M) - M(-(\delta/2)+M) & \text{if } \delta < M < 1 \\ \lambda - M + \delta/4 & \text{if } 1 \leq M < 1+\delta/2 \\ -1/2 & \text{if } 1+\delta/2 \leq M \end{cases}$$

Note that this is the same as sophisticates when $M \geq 1$.

Again we derive the payment and effectiveness of the optimal contract based on the value of λ. We consider two cases, (D) and (E).

Case D: $\lambda \geq 4/5$
When $\lambda > 4/5$, $\frac{\partial E}{\partial M} > 0$ for all $M \leq \delta$, and we have the following cases:

- If $\lambda \geq \frac{1.5-\delta/2}{1.5-\delta/4}$, $\frac{\partial E}{\partial M} > 0$ for all $M < 1$ and the sophisticate results go through. Note that $\lambda \geq \frac{1.5-\delta/2}{1.5-\delta/4}$ implies $\lambda \geq 1-\delta/4$.
- If $\lambda < 1-\frac{\delta}{4}$, $\lambda < \frac{1.5-\delta/2}{1.5-\delta/4}$. $\frac{\partial E}{\partial M} < 0$ for $M \geq 1$ and also for some $M \in (\delta, 1)$, so there is an optimum in $(\delta, 1)$ and it is global, so the sophisticate results go through as well.
- If $1-\frac{\delta}{4} \leq \lambda < \frac{1.5-\delta/2}{1.5-\delta/4}$, there are two local optima, one in $(\delta, 1)$ and another in $[1, 1+\frac{\delta}{2})$, so the global optimum is the maximum between the two. Also threshold efficiency is decreasing in δ for a given λ, and increasing in λ for a given δ. Also, at $\lambda = 4/5$, there is a cutoff value of δ when linear contract becomes more effective. So we can let $\delta = \frac{1.5-1.5\lambda}{1/2-\lambda/4}$, and solve for the λ value such that $E^{T*} = E^{L*}$, and the solution is $\lambda = 0.81$. So there is a cutoff value of δ for when linear contract becomes more effective if $\lambda < 0.81$; otherwise the threshold contract is always more effective.

Case E: $\lambda < 4/5$
From the discussion of sophisticates, we know in this case that if $\lambda < 1-\delta/4$, the optimum will have $M < 1$; if $1-\delta/4 \leq \lambda < 4/5$, there will be another local optimum in $[1, 1+\delta/2)$, and the global

optimum will be the maximum between the two. Explicitly, in case $\lambda < 1 - \delta/4$, we have

$$M^* = \begin{cases} \frac{\frac{3}{4}\lambda + \sqrt{\frac{9}{16}\lambda^2 + 2\lambda}}{2} & \text{if } M^* \le \delta \\ \frac{\lambda + \delta/2 + \sqrt{\lambda^2 - \frac{1}{2}\delta\lambda + 3\lambda + \frac{\delta^2}{4}}}{3} & \text{if } M^* > \delta \end{cases}$$

Let $\delta^* = \frac{\frac{3}{4}\lambda + \sqrt{\frac{9}{16}\lambda^2 + 2\lambda}}{2}$. This turns out to be the solution for $\delta = \frac{\lambda + \delta/2 + \sqrt{\lambda^2 - \frac{1}{2}\delta\lambda + 3\lambda + \frac{\delta^2}{4}}}{3}$, so we have

$$M^* = \begin{cases} \frac{\frac{3}{4}\lambda + \sqrt{\frac{9}{16}\lambda^2 + 2\lambda}}{2} & \text{if } \delta \ge \delta^* \\ \frac{\lambda + \delta/2 + \sqrt{\lambda^2 - \frac{1}{2}\delta\lambda + 3\lambda + \frac{\delta^2}{4}}}{3} & \text{if } \delta < \delta^* \end{cases}$$

So

$$E^{T*} = \begin{cases} .5\lambda \left[\frac{M^{*2}}{2\delta} + \frac{M^{*3}}{2\delta}\right] - .5\frac{M^{*4}}{2\delta} & \text{if } \delta \ge \delta^* \\ .5\lambda(M^* - \delta/2)(1 + M^*) - .5(M^* - \delta/2)M^{*2} & \text{if } \delta < \delta^* \end{cases}$$

When $1-\delta/4 \le \lambda < 4/5$, there is another optimum at $M = \lambda+\delta/4 \in [1, 1+\delta/2)$. For simplicity, let $E_1 = .5\lambda \left[\frac{M^{*2}}{2\delta} + \frac{M^{*3}}{2\delta}\right] - .5\frac{M^{*4}}{2\delta}$, $E_2 = .5\lambda(M^* - \delta/2)(1+M^*) - .5(M^* - \delta/2)M^{*2}$, and $E_3 = \frac{1}{2}\lambda - \frac{\delta\lambda}{4} - \frac{\delta^2}{16}$ which is $\lambda(M - \delta/2) - M^2/2 + \delta M/4$ evaluated at $\lambda + \delta/4$. We have

$$E^{T*} = \begin{cases} \max\{E_1, E_3\} & \text{if } \delta \ge \delta^* \\ \max\{E_2, E_3\} & \text{if } \delta < \delta^* \end{cases}$$

We know that E^{T*} is continuous in δ since the maximum of a function is continuous in the parameter if it is maximized on a compact domain, and in this case we are considering $M \in [0, 1 + \delta/2]$. So we can compare E^{T*} and E^{L*} by analyzing their values at $\delta = 0$ and $\delta = 1$. If $E^{T*} < E^{L*}$ at one endpoint and $E^{T*} > E^{L*}$ at another, we can conclude that there is a cutoff δ where threshold contract becomes more effective beyond.

If $\delta = 0$, then $E = 0.5\lambda(M - \delta/2)(1 + M) - 0.5(M - \delta/2)M^2$. This function is maximized on the region from $0 < M < 1$ (i.e., $\frac{\partial E}{\partial M} = 0$) when $M = \frac{1}{6}\left(\delta + 2\lambda + \sqrt{\delta^2 + 12\lambda - 2\delta\lambda + 4\lambda^2}\right)$. The corresponding maximized value of effectiveness is greater than the effectiveness of the optimal linear contract, $\lambda^2/4$, when $\delta = 0$, for all $\lambda > 0$.

If $\delta = 1$, then $E = \max\{E_1, E_3\}$, which is less than the effectiveness of the optimal linear contract, $\lambda^2/4$ for all λ.

Hence, we have that maximized effectiveness from the threshold is greater than maximized effectiveness from the linear, $E^{T*} > E^{L*}$, when $\delta = 0$, while the opposite is true, $E^{T*} < E^{L*}$, when $\delta = 1$. Since maximized effectiveness is continuous in δ,[84] this implies that there is a cutoff δ for which the effectiveness of the optimal threshold is the same as the effectiveness of the optimal linear, and that the effectiveness of the optimal threshold is above the linear for δ below the threshold (and vice verse for δ above the threshold).

□

[84]This follows because $.5\lambda \left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right] - .5\frac{M^4}{2\delta} = .5\lambda(M - \delta/2)(1 + M) - .5(M - \delta/2)M^2$ when $M = \delta$.

J CTB Time Preference Measurement

We adapted the convex time budget (CTB) methodology of Andreoni and Sprenger (2012a) to try to measure time preferences in two domains, walking and mobile recharges. Unfortunately, it did not work for either domain. As a result, we do not use the full CTB measures for analysis and instead use the simple versions of CTB described in Section 4.2. In Section J.1 we summarize why we believe our full CTB measurement was not a reliable measure of time preferences in this setting. In Section J.2 we briefly summarize evidence that the Simple CTB measures worked better. Section J.3 further expands upon section J.1 and provides additional evidence.

J.1 Performance of the Full CTB

We believe our implementation of the full CTB methodology of Andreoni and Sprenger (2012a) was unsuccessful because respondents did not understand it. The complex methodology was difficult to explain to our participants, who had limited familiarity with screens, sliders, or complicated exercises. Due to survey length constraints, we also included fewer questions (and gave less practice) than previous laboratory studies.

A number of patterns in the data suggest that participant understanding was limited. First, law of demand violations are far more common than in previous studies.[85] As shown in Table J.1, 57% of the sample violated the law of demand at least once. For reference, participants in Augenblick et al. (2015) had 16 opportunities to violate monotonicity, while ours had just 2. If understanding were similar in both contexts one would expect a higher share of the Augenblick et al. (2015) sample to ever violate the law of demand, but the share in their sample was only 16%.

Second, the CTB estimates do not correlate with any of the behaviors one would expect them to. The CTB estimates in the steps/effort domain do not correlate with exercise and health, and the estimates in the recharge domain do not correlate consistently with our proxies for impatience over recharges (e.g., balances).

Finally, there are a number of other problems with the full CTB data, such as low follow-through on the incentivized activity and low convergence of the parameters. We describe these issues in more depth in Section J.3.

For all of these reasons, we do not think our CTB estimates are a reliable measure of discount rates in this setting and do not use them for analysis.

J.2 Performance of the Simple CTB

The Simple CTB measures seem to have performed better than the full CTB exercise. For example, only 18% of the participants had any law-of-demand violations in these simpler questions, much lower than the 57% in the full CTB, even though participants had the same number of opportunities for violations in both question sets. The 18% estimate is much closer to the 16% found in Augenblick et al. (2015). The percent of future-biased choices (19%) is

[85] We can only examine law of demand violations in the effort domain because we did not include exchange rate variation in the recharge domain, so cannot estimate the demand curve.

Appendix Table J.1: Law of Demand Violations in Effort Allocations

	# of violators	% of sample
	(1)	(2)
Violates 0/7	1,318	41.3
Violates 7/14	1,493	46.8
Violates at least once	1,805	56.6
Violates both	1,006	31.5
Total:	3,232	100

Notes: This table summarizes law of demand violations in the full CTB in the recharge domain. Violators allocate more steps to the future date when we increase the interest rate from 1 to 1.25. We varied the exchange rate for two questions: today versus 7 days from now, and 7 versus 14 days; rows 1 and 2 show violations for these two questions separately and row 3 and 4 show percentages of people who violated at least once or both.

also closer to what is found in Augenblick et al. (2015) (which finds 17%) than to the higher estimates from the full CTB (26%).

Note that these estimates come from the performance of the simple CTB over recharges but not over effort; given the specific questions we asked in the effort domain, we cannot calculate law of demand violations nor future bias, so we cannot compare the measures on that front. However, as shown in Table A.1, the simple CTB over effort correlates in the expected direction with exercise (i.e., people who look more impatient under the simple CTB have lower steps). In contrast, the full CTB estimates do not correlate in the expected direction with any behaviors. Hence, the simple CTB still appears to be the better measure for our context.

J.3 Implementation of the Full CTB

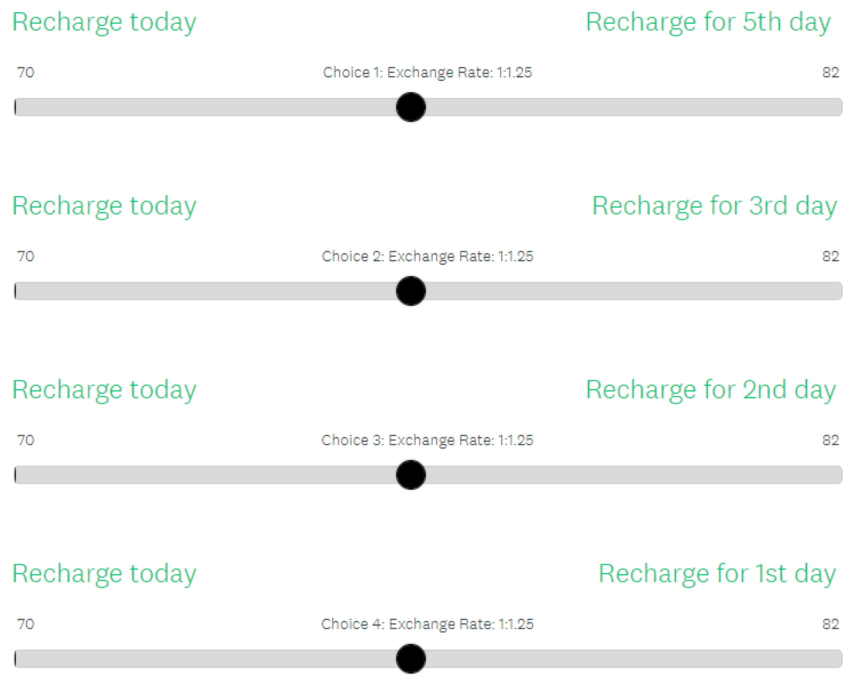
We first discuss the methodology used for the full CTB. We then show that the full CTB measures do not correlate with the behaviors that we would expect. Finally, we describe additional problems with the full CTB implementation.

J.3.1 Estimation Methodology

Our full CTB uses the full CTB methodology of Augenblick et al. (2015). In each CTB choice in our full CTB module, the participant is asked to allocate a fixed budget of either steps or mobile recharges between a "sooner" and a "later" date using a slider bar. In particular, each choice allows the respondent to choose an allocation of consumption on the sooner and later dates, c_t, c_{t+k} that satisfies the budget constraint

$$c_t + \frac{1}{r} c_{t+k} = m \tag{35}$$

where the sooner date t, the later date $t + k$, the interest rate r, and the budget m change between each choice. A sample slider screen allowing for such choices is shown in Figure J.1.



Appendix Figure J.1: Sample Decision Screen for Mobile Recharges

Notes: In this example, the interest rate, r, is 1.25; the total budget, m, is 140; the "sooner" date is Today; and the "later" date decreases from 5 days from today in the first choice to 1 day from today in the final choice. The sliders are shown positioned at the choice $(c_t = 70, c_{t+k} = 82)$.

We asked participants to make six allocations in the recharge domain, and eight allocations in the step domain, as summarized in Table J.2. We assume a time-separable and good-separable CRRA utility function with quasihyperbolic discounting[86]. In the domain of recharges, individuals will then seek to maximize utility,

$$U(c_t, c_{t+k}) = \frac{1}{\alpha}(c_t - \omega)^\alpha + \beta\delta^k \frac{1}{\alpha}(c_{t+k} - \omega)^\alpha \tag{36}$$

and in the step domain, individuals will seek to minimize costs of effort

$$C(c_t, c_{t+k}) = \frac{1}{\alpha}(c_t + \omega)^\alpha + \beta\delta^k \frac{1}{\alpha}(c_{t+k} + \omega)^\alpha \tag{37}$$

The variation in consumption choices as the budget constraint varies identify the time preference parameters—in particular, the daily discount factor δ and the present-bias parameter β—as well as the concavity or convexity of preferences α. Due to budget and time constraints, we had to keep the module short and so did not implement interest rate variation for the recharge tradeoffs, only for the step tradeoffs. Thus α is identified for the effort estimation only, not the recharge one; for the recharge estimation, we calibrate α using the estimate of α from Augenblick et al. (2015) in the financial payment domain.

[86]Unlike in Appendix C.2 where the quasihyperbolic discounting model we used only has one parameter δ_{QH} or d_{QH}, here we use both β and δ since we estimated them simultaneously.

We recover individual-level structural estimates of time preference and concavity parameters from the allocations (c_t, c_{t+k}) using a two-limit Tobit specification of the intertemporal Euler condition following Augenblick et al. (2015).

$$\log\left(\frac{c_t+\omega}{c_{t+k}+\omega}\right) = \frac{\log(\beta)}{\alpha-1}1_{t=0} + \frac{\log(\delta)}{\alpha-1}k - \frac{1}{\alpha-1}\log(r) \tag{38}$$

Details on the estimation strategy can be found in the Online Appendix of Augenblick et al. (2015). Because our predictions concern overall impatience, not whether an individual is time-consistent, on the time preference side, we want one single summary measure capturing impatience. To do so, we estimate two different variants. In one, we set $\beta = 1$ for everyone at the estimation stage and simply estimate δ at the individual level. In the second, we estimate the equation as above, allowing both β and δ to vary at the individual level, and use $\beta \times \delta$ as our measure of individual-level impatience. In both estimation procedures, we allow α to vary at the individual-level in the steps domain, since we considered individual-level convexity of the step function to be an important potential confound.[87] However, the results we describe next are similar if we do not allow α to vary at the individual-level for steps.

Appendix Table J.2: CTB Allocation Parameters

Summary of convex time budget allocations					
Question no.	t	k	r	Recharge domain	Step domain
1	7	7	1	X	X
2	0	7	1	X	X
3	0	5	1	X	X
4	0	3	1	X	X
5	0	2	1	X	X
6	0	1	1	X	X
7	7	7	1.25		X
8	0	7	1.25		X

Notes: This table summarizes the parameters of the six CTB allocations made over recharges, and the eight CTB allocations made over steps.

Our CTB environment builds on a number of features from previous studies. First, the choices are made after the one-week phase-in period in which all participants have pedometers and report their daily steps, ensuring that participants are familiar with the costs of walking. This allows for meaningful allocations of steps between sooner and later dates. Second, the responses are designed to be incentive compatible; all respondents were informed that we would

[87] Indeed, when we estimate impatience (e.g., δ) but do not allow α to vary, that estimated δ correlates as strongly with α as it does with the δ estimated allowing α to vary, suggesting that convexity is an important confound indeed.

implement their choice from a randomly selected survey question. We set the probabilities such that for most respondents the randomly selected survey question was a multiple price list of lotteries over money (which measures risk preferences), but for a few a CTB allocation was selected. Because the allocations might have interfered with any walking program offered, we excluded the 40 respondents who were randomly selected to receive one of their allocations from the experimental sample.[88] To try to ensure that participants complete the allocated steps, we offer a large cash completion bonus of 500 INR in the step domain if the allocation is selected to be implemented, and the steps are completed as allocated, with the bonus to be delivered 15 days from the date of the survey (which is 1 day after the latest "later" day used).

We also take a number of precautions to avoid various potential confounds, including confounds reflecting fixed costs or benefits of taking an action, or confounds due to the time of day of measurement.[89] However, we were not able to address one potential confound to our estimates of time-preferences across individuals fully: variation across people in the cost of walking over time, or in the benefit of receiving a recharge over time. For example, an individual with a particularly busy week after the time-preference survey, and therefore relatively high costs to steps in the near-term relative to the distant future, will appear to be particularly impatient over steps in our data (he will wish to put off walking). An individual with a relatively free week just after the time-preference survey will instead appear particularly forward-looking (he will not wish to put off walking). The same concerns can also arise with recharges.

J.3.2 CTB Estimates: Problems with Convergence and Lack of Correlation

Table J.3 displays the summary statistics as well as the convergence statistics. The CTB parameter estimates themselves are not robust and are inconsistent with typical priors. First,

[88] This means we have CTB data from a total of 3,232 people: the 3,192 in the experimental sample plus the 40 selected to receive "real-stakes" allocations. For completeness, we summarize in this section the CTB data for all 3,232 but the results are the same if we restrict to the experimental sample.

[89] To avoid confounds related to fixed costs or benefits, such as the effort of wearing a pedometer or the psychological benefit of receiving a free recharge, we include minimum allocations on both sooner and later days in each domain. The minimum allocations were chosen to be high enough that any fixed costs would be included (e.g. one could not easily achieve the minimums by simply shaking the pedometer) but low enough to avoid corner solutions. In the step domain, this required a modification of the CTB methodology: individual-specific minimum allocations. Our step allocations also featured individual-specific total step budgets m, which were chosen to be large enough that achieving them would require some effort beyond simply wearing the pedometer but small enough that participants would certainly achieve them in exchange for the completion bonus. Specifically, minimum steps on each day are calculated as $\frac{X}{10}$, and the total step budget m is $X + 2\frac{X}{10}$, respectively, where $X \in \{3000, 4000, 5000\}$ is the element closest to the participant's average daily walking during the phase-in period. That is, minimum steps are one of 300, 400, or 500 on each day, and the total step budget is one of 3,600, 4,800, or 6,000. To avoid confounding impatience with the time of day that the baseline time-preference survey was administered (which could influence the desirability of walking and/or recharges delivered in the next 24 hours), as well as to capture heterogeneity in time preferences including any present-bias for very short beta-windows, we required that all walking on any date be conducted within a 2 hour period, which was chosen to start at the time immediately after the time-preference survey would end (e.g., if the survey ended at 4pm, the time period for any day's walking would be 5-7pm). The short window could potentially bias our overall measures of impatience downwards, as uncertainty about future schedules in a short time window could lead participants to want to get their walking done early when they had more certainty over their schedule. However, our primary purpose was to capture heterogeneity in time-preferences, and we considered the potential loss in validity of aggregate time preference estimates to be worth the ability to capture heterogeneity in time preferences in the time frames near to the present.

we do not have estimates for a large, endogenous share of the sample. The estimates do not converge (i.e., we are unable to estimate discount rate parameters) for 38 to 44% of the sample in the recharge domain, and 23 to 44% of the sample in the steps domain. Moreover, many of the participants with estimates that converge in the effort domain have an estimated $\alpha < 1$, which violates the first order conditions for estimation and is often associated with non-sensible δ and β estimates. When we exclude these estimates, we are left with estimates for only 34 to 38% of the sample in the effort domain. Second, we have a high rate of negative estimated discount rates: 43% for steps and 61% for recharges. This is more than the usual rate of negative individual-level estimates.

Appendix Table J.3: Summary Statistics For CTB Parameters

	Full sample		$\alpha > 1$	
Parameters estimated:	$\beta\delta$	δ	$\beta\delta$	δ
	(1)	(2)	(3)	(4)
A. Effort				
Beta	2.066	–	1.573	–
Delta	0.883	0.997	1.015	0.999
Alpha	0.244	0.723	1.673	1.576
% of sample:	77.2	56.3	34	38
# Individuals:	2,494	1,821	1,092	1,225
B. Recharges				
Beta	0.972	–	–	–
Delta	0.989	0.996	–	–
% of sample:	55.9	62.2	–	–
# Individuals:	1,808	2,011	–	–

Notes: This table displays means and convergence rates of individual-level CTB parameters in both the effort and recharge domains. Columns 1 - 2 display average values for the parameters from the full sample of individuals with parameters that converged. In the effort domain, in columns 3 - 4, we ignore all individuals whose estimated *alpha* was below 1, as handled similarly in Andreoni and Sprenger (2012a), as that is inconsistent with the first order conditions. We winsorize all parameters at the top and bottom 1 percentiles. We allow α to vary at the individual level in the effort domain, and in the recharge domain, we calibrate α to be 0.975, which is the estimated value in Augenblick et al. (2015). Delta is estimated by allowing δ to vary at the individual level and setting β to 1. Beta-delta is estimating by allowing both δ and β to vary. We derive these two parameters from an estimation that allows δ and β to vary at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Tables J.4 and J.5 show that the estimated CTB parameters do not correlate in the expected direction with measured behaviors. In particular, Table J.4 shows that the CTB estimates in the steps/effort domain do not correlate with exercise and health,[90] and Table J.5 shows that the estimates in the recharge domain do not correlate with recharge balances, usage, or credit constraint proxies. The CTB measures do correlate at the 1% level with our measure of marginal propensity to consume recharges, but the correlations go in opposite directions for the two CTB measures (δ from an estimation setting $\beta = 1$ vs. $\beta\delta$ estimated allowing both parameters to vary) so is likely noise.

Appendix Table J.4: CTB Estimates of Discount Factors Over Steps Do Not Correlate With Measured Behaviors

Covariate type:	Exercise		Baseline indices			
Dependent variable:	Daily steps	Daily exercise (min)	Health index	Negative vices index	Healthy diet index	# Individuals
Delta	-0.018	0.009	-0.038	0.010	0.025	1,342
Beta-delta	0.016	0.018	0.014	0.010	0.027	1,086

Notes: This table displays the correlations between CTB parameters in the effort domain and a few baseline health covariates. We normalize impatience variables so that a higher value corresponds to greater impatience, and we normalize health outcomes so that higher values correspond to healthier outcomes. All CTB parameters have been winsorized at the top and bottom 1 percentile to remove outliers. Delta is measured from an estimation that allows δ and α to vary at the individual level, while excluding β. Beta-delta is a measure of beta times the average delta over one week. We estimate the two parameters by allowing β, δ, and α to vary at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table J.5: CTB Estimates of Discount Factors Over Recharges Do Not Correlate With Other Proxies for Impatience Over Recharges

Covariate type:	Recharge variables			Credit constraint proxies			
Dependent variable:	Negative mobile balance	Negative yesterday's talk time	Prefers daily (=1)	Prefers monthly (=-1)	Negative wealth index	Negative monthly household income	# Individuals
Delta	0.027	0.012	-0.141***	0.045	-0.010	-0.008	1,837
Beta-delta	-0.002	-0.022	0.145***	-0.019	-0.015	0.033	1,652

Notes: This table displays the correlations between CTB parameters in the recharge domain and baseline measures that should be related to credit constraints and discount rates over recharges. We normalize impatience variables so that a higher value corresponds to greater impatience, and we normalize the proxies so that higher values correspond to higher expected discount rates; hence, the prediction is that coefficients should be positive. All CTB parameters have been winsorized at the top and bottom 1 percentile to remove outliers. We use two main estimation specifications, and to identify parameters, we calibrate α to be 0.975, the value of α estimated in Augenblick et al. (2015). Delta is estimated by allowing δ to vary at the individual level and excluding β. Beta-delta is a measure of the average delta over one week multiplied by beta. We derive these two parameters from an estimation that allows δ and β to vary at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

[90] Table J.4 shows the correlations when we exclude the effort estimates from participants with estimated $\alpha < 1$, but the results are similar when we include all estimates together.

J.3.3 Additional Problems With the Full CTB Data

Finally, we provide more detail on other problems with the Full CTB, in addition to the law of demand violations, the lack of convergence, and the lack of correlation described earlier.

First, in the effort task, there was low follow-through on the incentivized activity: fewer than 50% of participants selected to complete the step task did so despite large rewards (500 INR) for completion. While this partly reflects a logistical glitch (we failed to give respondents intended reminder calls the day before their activity), the lack of follow-through may also indicate a lack of respondent understanding. Regardless, the poor follow-through is problematic methodologically: identification requires that, when participants make their allocation decisions, they think they will follow-through with certainty, which seems unrealistic given how few followed through in practice.

Second, respondents on average allocated more steps to today than the future even when the interest rate was 1:1. Although they could be future-biased, the following other potential explanations are concerning for interpretation: respondents were confused; they saw steps as consumption instead of a cost (violating the first order conditions underlying estimation); or uncertainty over future walking costs and schedules led participants to want to finish steps sooner, which would confound discount rate estimates with risk aversion and uncertainty.

Third, day-specific shocks appear to be important in practice. 19% of respondents' allocations of steps to the sooner date are neither monotonically weakly increasing nor monotonically weakly decreasing across questions which feature the same sooner date (today) but a monotonically decreasing later date (questions 2-6). These allocations cannot be rationalized with a discount rate that is either weakly decreasing *or* increasing with lag length without day-specific utility shocks. The same holds for 24% of respondents in the recharge domain. These types of shocks would also confound estimation.

K Monitoring Treatment Impacts on Walking

The health results suggest that the monitoring treatment had limited impact, although the results are somewhat imprecise. Did the monitoring treatment not affect exercise, or were the exercise impacts too small to translate into measurable health impacts? We now present an analysis of the effects of monitoring on exercise. Because we do not have pedometer walking data from the control group, we use a before-after design. We find that monitoring alone has limited impact on overall steps. Monitoring does however change the distribution of steps, increasing the share of days on which participants met the 10,000-step target but decreasing the steps taken on other days for a null effect on total exercise.

Our before-after design compares pedometer-measured walking in the monitoring group during the phase-in period (during which we had not given participants a walking goal and just told them to walk the same as they normally do) to their behavior during the intervention period. This strategy will be biased either in the presence of within-person time trends in walking, or if the phase-in period directly affects walking behavior. We control for year-month fixed effects to help address time trends, but the latter concern is more difficult, as the phase-in period likely did increase walking above normal, either because of Hawthorne effects or because participants received a pedometer and a step-reporting system, which are two of the elements of the monitoring treatment itself (the other three remaining that we can still evaluate are (a) a daily 10,000 step goal, (b) positive feedback for meeting the step goal through SMS messages and the step-reporting system, and (c) periodic walking summaries). Thus, we consider a pre-post comparison of walking in the monitoring group to be a lower bound of the monitoring program treatment effect.

One can visualize the variation used for our pre-post estimate in Figure A.4, Panels (a) and (b). Walking increases immediately during the intervention period for the monitoring group, although the effects decay over time.

We next estimate the pre-post monitoring effect controlling for date effects. In order to increase the precision of our estimated year-month fixed effects, we include the incentive group in the regression as well since that group is much larger. We thus estimate the following difference-in-differences regression using data from both the intervention and phase-in periods for the incentive and monitoring groups:

$$\begin{aligned} y_{it} = {} & \alpha + \beta_1 Intervention\,Period_{it} + \beta_2 incentives_i + \beta_3 (Intervention\,Period_{it} \times incentives_i) \\ & + \boldsymbol{X}_i' \gamma + \boldsymbol{\mu}_m + \varepsilon_{it}, \end{aligned} \tag{39}$$

where y_{it} are daily pedometer outcomes measured during both the phase-in and the intervention period, $Intervention\,Period_{it}$ is an indicator for whether individual i has been randomized into their contract at time t, $incentives_i$ is an indicator for whether i is in an incentive treatment group, $\boldsymbol{X}_i$ is a vector of individual-specific controls, and $\boldsymbol{\mu}_m$ is a vector of month fixed effects. The coefficient β_1 — the coefficient of interest — is the pre-post difference in pedometer outcomes within the monitoring group (controlling for aggregate time effects).

Table K.1 presents the results. Column 2 shows that the monitoring group achieves the 10,000-step target on approximately 7% more days in the intervention period than in the phase-in period, an effect significant at the 1% level and equal to roughly 36% of the estimated impact of incentives. In contrast, the estimated effect on steps is very small in magnitude, varies across specifications, and is in fact sometimes negative (columns 4-6). Thus, the monitoring treatment, if anything, appears to do more to make walking consistent across days than it does to increase total steps.

Appendix Table K.1: Impacts of Monitoring (Pre-Post) and Incentives (Difference-In-Differences) on Exercise Outcomes

	Achieved 10K steps			Daily steps		
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	0.012	0.013	0.012	66.7	66.4	48.9
	[0.024]	[0.024]	[0.014]	[268.1]	[266.9]	[112.3]
Intervention period	0.057***	0.073***	0.064***	-130.4	108.0	-18.5
	[0.020]	[0.020]	[0.020]	[237.8]	[240.8]	[234.1]
Intervention period X Incentives	0.19***	0.19***	0.19***	1270.9***	1258.9***	1212.7***
	[0.021]	[0.021]	[0.021]	[248.6]	[249.2]	[243.4]
Year-month FEs	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes
Monitoring phase-in mean	.24	.24	.24	6,904.8	6,904.8	6,904.8
# Individuals	2,604	2,604	2,604	2,604	2,604	2,604
Observations	221,214	221,214	221,214	221,214	221,214	221,214

Notes: This table shows coefficient estimates from regressions of the form specified in equation (39). The outcomes are from daily panel data from the pedometers. Standard errors, in brackets, are clustered at the individual level. Individual controls are the same as Table 2. The omitted category is Monitoring in the phase-in period. The coefficient in the second row, on $Intervention\,Period_{it}$, corresponds to the pre-post estimate of the Monitoring effect. Significance levels: * 10%, ** 5%, *** 1%.