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

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July 10, 2022

Abstract

Promoting lifestyle changes such as regular exercise is critical for the global fight against diabetes. One barrier to lifestyle change is impatience (i.e., heavy discounting of the future), which makes short-run financial incentives for lifestyle change a promising approach and also makes it important to ensure the incentives work well in the face of impatience. We evaluate whether providing incentives for exercise to diabetics can help address the problem of diabetes in India. We also test a novel prediction, namely that "time-bundled" contracts, where the payment for future effort is increasing in current effort, are more effective when agents are impatient. We find positive results on both fronts. First, incentives increase daily steps by roughly 20 percent (13 minutes of brisk walking) and improve blood sugar. Second, consistent with our prediction, time-bundled contracts work better for more impatient people.

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1 Introduction

Chronic lifestyle diseases such as type 2 diabetes and hypertension represent a severe threat to health and development in low and middle income countries (LMICs). Chronic diseases cause 70% of the deaths in LMICs and are associated with substantial morbidity, premature mortality, and lost productivity (World Health Organization, 2020). 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).

There is widespread agreement that the key to addressing the growing health and economic burden of chronic disease is to promote three lifestyle changes: more physical activity, healthier diet, and less tobacco and alcohol use (World Health Organization, 2009). Each of these changes can prevent disease onset, decrease the rate of costly complications, and avert premature mortality. However, a large portion of patients diagnosed with chronic lifestyle diseases do not adopt the recommended lifestyle changes despite the high personal stakes (Carpenter et al., 2019), and existing evidence-based interventions promoting lifestyle change are prohibitively expensive (Howells et al., 2016). Policymakers are thus particularly interested in scalable interventions to promote lifestyle change.

One potential barrier to lifestyle change is that people heavily discount the future — that is, they are impatient (e.g., Mahajan, Michel, and Tarozzi, 2020; Augenblick and Rabin, 2019). People with chronic lifestyle diseases may be especially impatient (Reach et al., 2011; Wainwright et al., 2022). Lifestyle changes, from exercising to forgoing dessert, involve short-run costs but only longer-run benefits. As a result, impatient people will tend not to make these changes even if the eventual benefits are large relative to the costs. A common approach to address impatience in other contexts is to provide short-run positive reinforcement, such as small financial incentives (e.g., Banerjee, Duflo, Glennerster, and Kothari, 2010; Royer, Stehr, and Sydnor, 2015). However, whether incentives can in fact promote lifestyle change among those with chronic lifestyle diseases is not well understood. Moreover, we have little knowledge of how to best structure incentives so that they work well when people are impatient — in any context, lifestyle change or otherwise.

This paper makes two distinct contributions. First, we test whether a scalable program that provides incentives for exercise to diabetics can help decrease the burden of chronic disease in LMICs. Second, we shed light on how to improve the performance of incentives when agents are impatient over effort. To make the latter contribution, we test a novel prediction that "time-bundled contracts," which make the payment for future effort increase in current effort, are more effective when agents discount effort costs more. Notably, this prediction is about the "primitive" discount rate from an individual's utility function, which applies to all utility costs and benefits, including effort costs. This primitive discount rate is distinct from the discount rate over payment, which instead reflects the available borrowing and saving opportunities. As shown in Augenblick, Niederle, and Sprenger (2015), even if individuals have a high primitive discount rate over utility costs like effort, their discount rate over payment can be low since the payment discount rate should equal the market interest rate for people with access to borrowing and saving.

To illustrate the intuition for why time-bundled contracts work well when people are impatient over effort costs, imagine you need a worker to perform two days of work. Consider first a time-bundled "threshold" contract that pays a lump sum on day two if and only if she works 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 separately for each day of work, the minimum payment to induce two days of work must be higher, at \$20: \$10 per day of effort. Time-bundled contracts thus 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 contracts should be effective for all types of people with high discount rates over effort: time-consistent or time-inconsistent and, among time-inconsistent, both those who are sophisticated and even those who are "naïve" (or unaware) about their own present bias. We consider the effectiveness for naïfs to be an important feature. Naïve time inconsistency is common (for example, Mahajan et al. (2020) estimate that 50% of a sample of Indian adults are naïfs). Naïfs are also difficult to motivate (Bai, Handel, Miguel, and Rao, 2020). The effectiveness for naïfs differentiates time-bundled contracts from commitment contracts, another approach used to motivate time-inconsistent people.² For commitment contracts to be effective, people must be sophisticated about the differences between their preferences and discount rates in the future relative to the present-day. In contrast, time-bundled contracts directly leverage *present-day* discount rates, which even naïfs understand. That is, even naïfs will sell their future effort at a discount today.

To make our two contributions, we partnered with the Government of Tamil Nadu, one of the most populous states in India, to conduct a randomized controlled trial (RCT) evaluating an incentive program for exercise among a sample of diabetics and prediabetics in urban areas. With a diabetes prevalence rate of 24% among adults in urban Tamil Nadu (Ranasinghe et al., 2021), the government funded this study to try to identify a lifestyle intervention that they could scale up across the state to address their exploding diabetes epidemic.

¹This example assumes a zero short-run interest rate on payments for simplicity.

²Commitment contracts provide people with the option to undertake dominated actions in order to compel their future selves into a specific action. For example, a commitment contract for day 2 work might, on day 1, collect money from workers, and then only return the money to the workers if they worked on day 2.

The program monitors participants' walking for 3 months using pedometers and provides them with small financial incentives in the form of mobile phone credits if they achieve a daily step target of 10,000 steps. To evaluate the program, we randomly assign participants to a treatment group that receives the program or a control group that does not. To test the prediction that time-bundled contracts work better for impatient people, within the treatment group, we randomly vary whether payment is a linear function of the number of days the participant complies with the step target or whether payment is instead a time-bundled threshold function. The time-bundled threshold contract only rewards compliance with the step target if the step target is met a minimum number of days that week. We compare the different contracts to assess their average efficacy and to test our prediction that time-bundling will have heterogeneous impacts by impatience over effort.

Our evaluation demonstrates that incentives for exercise could be a cost-effective intervention to help decrease the burden of chronic disease in India and beyond. The incentives program substantially increases exercise. Providing an incentive of just 20 INR (0.33 USD) per day of compliance with the step target increases compliance by 20 percentage points (pp) off of a base of 30%. Average daily steps increase by 1,300, equivalent to 13 additional minutes of brisk walking, roughly a 20 percent increase. Moreover, roughly 40% of the treatment effect on walking continues for several months after the intervention ends. This is more persistence than many other exercise interventions (e.g., Royer et al., 2015; Acland and Levy, 2015) and suggests that people formed healthy habits that could last (Hussam, Rabbani, Reggiani, and Rigol, 2022).

The treatment effects on exercise are of standalone interest: our sample has high rates of diabetes and hypertension, and, in the long run, regular exercise has been shown to prevent complications from both (Lee et al., 2012). Moreover, we find that the improvements in exercise induced by incentives translate to modest improvements in blood sugar, the primary clinical marker for diabetes, even in the short run. The program also improves an index of cardiovascular health that includes blood pressure and body mass index (BMI) in addition to blood sugar, and increases an index of mental health. Taken together, these impacts on exercise and health are promising for policy, especially since – unlike previous evidence-based interventions promoting lifestyle change among diabetics – our program is scalable and low cost.

Since chronic disease is associated with impatience, we next examine strategies to improve the performance of incentives for impatient people. Our headline finding is that time-bundled contracts meaningfully increase relative effectiveness for those who are impatient over effort, consistent with our theoretical prediction. Specifically, heterogeneity analysis using a baseline measure of impatience shows that, relative to linear contracts, time-bundled threshold contracts increase compliance with the step target by 6 pp more for people with above-median impatience than for those with below-median impatience, a large difference relative to the sample-average effect of either contract (20 pp). The 6 pp difference represents the gap between a 3 pp positive effect among those with above-median impatience and a 3 pp negative effect among those with below-median impatience. The level of impatience is thus pivotal to whether linear or time-bundled contracts generate more effort.

We also find that, on average, across the full sample, the time-bundled threshold contract performs better than the linear contract—it achieves the same sample-average level of compliance as the linear contract, but does so at a lower cost.³ We show that this improves the performance of the contract from the perspective of a policymaker who wants to maximize the benefits of compliance net of the incentive costs.⁴ While it is theoretically ambiguous whether time-bundled contracts will outperform linear contracts in general, we show that this result is more likely to hold when discount rates over effort are high, as they may be in our sample given the high levels of chronic disease.

These results imply that policymakers can improve the performance of incentives by customizing whether people receive linear or time-bundled contracts based on their impatience. This type of personalization is likely feasible. Andreoni, Callen, Khan, Jaffar, and Sprenger (2018) use discount rates estimated through a simple effort allocation experiment to successfully personalize incentive contracts with the goal of equalizing worker effort across days. One concern with personalizing time-bundled contracts is that some are dominated by linear contracts from the participant perspective, paying out weakly less payment for any level of effort. This creates concerns that participants will game the system to avoid assignment to time-bundled contracts. However, Dizon-Ross and Zucker (2022) demonstrate that, in the context of walking incentive contracts, people are unlikely to manipulate observable characteristics in order to avoid assignment to dominated contracts.⁵ Our results also indicate a role for customizing the use of time-bundled contracts at the population level by using time-bundled contracts among particularly impatient populations such as people with chronic disease.

To place our findings on time-bundling in context, we also assess a more standard strategy for adjusting incentives for impatience: increasing the frequency of payment. 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) describe more frequent payment as the main way to adjust incentives for present bias. However, they also acknowledge that increasing

³This finding contributes to a small literature comparing linear to nonlinear contracts. While most existing experiments focus on differential selection into nonlinear contracts (e.g., Larkin and Leider, 2012; Kaur, Kremer, and Mullainathan, 2015), our experiment is one of the few to randomize whether incentives are linear or nonlinear. DellaVigna and Pope (2018) also randomize contract linearity but do not examine cost-effectiveness.

⁴This statement depends on the assumption that the benefits of compliance are linear in compliance. We also discuss other potential objectives later in the paper.

⁵Specifically, Dizon-Ross and Zucker (2022) find that few people manipulate observed baseline walking levels downward in order to avoid assignment to dominated exercise incentive contracts, even when manipulation is nearly effortless, as they recognize that such manipulation might dampen their own long-run exercise.

payment frequency should only be effective if people are impatient over *payments*, which even those who are impatient over effort may not be.

We find that increasing the frequency of payment has no impact in our setting, indicating that participants have low discount rates over the contract payments (mobile phone credits). While it is possible that people would have been more impatient over payments delivered with a different modality, limited impatience over payments is not rare (Augenblick et al., 2015; Andreoni and Sprenger, 2012; DellaVigna and Pope, 2018).⁶ Thus, increasing payment frequency may not always be an effective way to adjust incentives for impatience. This makes it important to identify other ways to adjust incentives for impatience and highlights the significance of our finding that time-bundled contracts are one such way.

Contributions to the Literature Our first contribution is to show that incentives for exercise are a scalable, effective lifestyle intervention that can help decrease the burden of chronic disease in resource-poor settings. Prior evaluations of incentives for diabetics have targeted non-exercise outcomes with limited success; for example, Long (2012) provides diabetics in the US with incentives to lower their blood sugar and finds no impact.⁷ In contrast, building on previous work showing that incentives increase walking among healthy populations in the developed world (e.g., Bachireddy et al., 2019; Finkelstein et al., 2008, 2016; Patel et al., 2016), we find that exercise incentives deliver positive behavioral and health impacts for diabetics in the developing world. Moreover, relative to other exercise incentive programs, our program stands out for its relatively large and persistent effect on behavior, for its measurable impacts on downstream health outcomes, and for its low cost. The success of our targeted exercise incentive program contracts with the lack of impact of more broad-based, comprehensive wellness interventions in the US (Jones, Molitor, and Reif, 2019).

Our second contribution is to the time preferences literature: we show theoretically and empirically that time-bundled threshold contracts are effective for a wide range of people with impatient preferences over effort. Researchers have primarily motivated impatient agents with commitment devices (e.g., Royer et al., 2015; Ashraf, Karlan, and Yin, 2006). Commitment is a useful tool, but it is not a panacea. Take-up of commitment devices is modest (Laibson, 2015), undermining their use as a broad policy solution. Moreover, commitment devices are only effective for sophisticated time-inconsistents; they are less effective—and can even be harmful for naïfs (Bai et al., 2020). In contrast, time-bundled contracts do not require sophistication; if anything, we show that naïvete opens up another channel for time-bundled contracts to be

⁶Augenblick et al. (2015) and Andreoni and Sprenger (2012) find limited impatience over payment via cash and via check, respectively, and DellaVigna and Pope (2018) find that decreasing the lag until a payment is made into an mTurk account has limited quantitative impact on effort.

⁷Sen et al. (2014) show that incentives for glucose monitoring improve monitoring but not blood sugar. VanEpps et al. (2019) and Desai et al. (2020) evaluate a program in multiple US states that provides prediabetics with incentives for attendance in a health program and for weight loss, and do not find clear impacts on weight.

effective. Our theoretical insight that time-bundling can motivate impatient people relates to work by Jain (2012), who shows that firms can theoretically increase productivity by offering multi-period quotas to salespeople who are present-biased over both payments and effort.

We add to the literature that examines alternative approaches to commitment contracts to motivate impatient agents. O'Donoghue and Rabin (1999b) and Carrera, Royer, Stehr, and Sydnor (2020) both examine ways to help time-inconsistent procrastinators avoid delay in completing a single task.⁸ Andreoni, Callen, Khan, Jaffar, and Sprenger (2018) customize contracts to agent time preferences with the goal of making agents exert the same effort on two different days. DellaVigna and Pope (2018) examine whether decreasing the lag between effort and payment increases effort. Our distinctiveness from these related studies lies in the novel approach (time-bundled contracts) used to increase effort.

A secondary contribution to the time preferences literature is to study the implications of domain-specific discounting for contract design. Although it is well known that there is a distinction between discount rates over payment and effort (Augenblick et al., 2015), the vast majority of papers examining dynamic contracts assume the same discount rate for both (e.g., Lazear, 1981; Chassang, 2013). We show that allowing these discount rates to differ has interesting implications: while more frequent payment is effective for those who discount payment highly, time-bundling is effective for those who discount effort highly.

The paper proceeds as follows. Section 2 presents our theoretical predictions. Sections 3 and 4 discuss the study setting and design. Section 5 presents the impacts of incentives on exercise and health. Section 6 explores time-bundled contracts and impatience. 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 agents have high discount rates over effort. We first specify the agent'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 not time-bundled. Next, we examine the effect of making the contract time-bundled (i.e., making the payment for future effort increase in current effort). We show that, for multiple types of impatience, including time-inconsistent sophistication and time-inconsistent naivete, the effectiveness of the time-bundled contract is often increasing in the discount rate over effort. While one can come up with parameter assumptions under which the result does not hold, we show that it holds in many typical and empirically relevant cases.

⁸O'Donoghue and Rabin (1999b) examine how to adjust "temporal incentive schemes" that reward agents based on when they complete a single task. They find that, to avoid delay among time-inconsistent procrastinators, the optimal incentive typically involves an increasing punishment for delay over time. Carrera et al. (2020) examine whether they can help time-inconsistent procrastinators overcome startup costs by offering higher incentives upfront in a separable contract but find the approach to be ineffective empirically.

Finally, we briefly analyze a potential strategy for adjusting incentives for impatience over payment rather than effort: increasing the frequency of payments. We show that this strategy is effective if agents have high discount rates over payment.

2.1 Set-Up

Each day, an individual chooses whether to complete a binary action. The principal then gives the individual a payment whose amount depends on the individual's past and present actions. Define w_t as an indicator for whether the individual "complies" (i.e., completes the action) on day t. Let m_t be the payment made to the individual on day t.

To solve for compliance, we assume that individual choices maximize the following reducedform utility function: $\Gamma \propto \Gamma$

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

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.⁹

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

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 the beginning of day t. Note that e_t can be negative, reflecting that agents

⁹The model's qualitative predictions are robust to relaxing this assumption.

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 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). 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 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:

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

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

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

Holding all else constant, compliance is thus independent of $\delta^{(t)}$.¹²

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

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

¹² In particular, 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 $\delta^{(t)}$.

¹⁰This 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 6.4.

Observation 1. Holding all else constant, neither compliance, cost-effectiveness, nor effectiveness in the linear contract depend on $\delta^{(t)}$.

We will see that this result contrasts with 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. Appendix B presents our formal mathematical results, which we label as propositions. In the main text, we present the testable implications, which we label as predictions.

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 below which no incentive is received, and above which payment is a linear function of the number of days of compliance. Total payment in the threshold contract is thus:

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

An important question is when threshold contracts are more effective than linear contracts. Appendix B.2 compares the overall effectiveness of threshold and linear contracts formally, yielding two main takeaways. First, threshold contracts can substantially increase effectiveness when the discount rate over effort is high. Second, the discount rate over effort can be pivotal to whether threshold or linear contracts are more effective.

While the specific Appendix B.2 results require assumptions that may not hold in practice (e.g., some results require that δ be sufficiently small), they yield a testable policy implication that holds under much more general assumptions:

Prediction 1 (Threshold Effectiveness and Impatience Over Effort). Holding all else equal, time-bundled threshold contracts tend to perform better relative to linear contracts, with respect to compliance and effectiveness, when the discount factor over effort, $\delta^{(t)}$, is smaller.

Prediction 1 is based on a series of propositions, presented in Appendix B.3. The propositions show that, holding all else equal, compliance and effectiveness in time-bundled threshold contracts tend to decrease in $\delta^{(t)}$ under a broad range of assumptions. In contrast, in linear contracts, both compliance and effectiveness are flat in $\delta^{(t)}$ (Section 2.2). Thus, the lower $\delta^{(t)}$ is, the higher compliance and effectiveness tend to be in a time-bundled threshold relative to linear contract. Specifically, Proposition 4 examines threshold contracts with K = T (i.e., where one must comply on all days in order to receive payment). We show that, for all T, *compliance* is weakly decreasing in δ . To examine effectiveness, Proposition 5 considers the case where T = 2and makes the reasonable assumption that e_2 is weakly increasing in e_1 , an assumption which flexibly accommodates the range from IID costs to perfect positive correlation and just rules out negative correlation. We show that, in that case, under relatively general conditions,¹³ *effectiveness* in the threshold contract is also weakly decreasing in δ .

To gain tractability to examine threshold contracts with K < T and threshold effectiveness when T > 2, we then make additional assumptions about the effort cost distribution. Proposition 6 shows that, if costs are perfectly positively correlated over time, both compliance and effectiveness are weakly decreasing in the threshold contract for any T and any $K \leq T$. Finally, to relax the perfect correlation assumption, Proposition 7 examines a simplified version of the model where costs can either be high or low, all costs are known from day 1, K = 2 and T = 3. Again, we show that both compliance and effectiveness are higher when $\delta^{(t)}$ is lower.

Thus, overall, the propositions suggest that, when either (a) K is high relative to T,¹⁴ or (b) costs are positively correlated across periods, our prediction tends to hold. 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.¹⁵

Intuition We illustrate the intuition for Prediction 1 by considering a simplified case, with d = 1, T = 2, K = 2, and with effort costs that are weakly positive and known from day 1.

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. The value she places on that day 2 opportunity is

$$(2m' - \delta e_2)w_{2,1}\Big|^{w_1=1},\tag{5}$$

which is equal to the discounted (with d = 1) payment 2m', net of the discounted effort costs δe_2 , in the states of the world where the individual thinks she will comply on day 2 if she complies on day 1 (i.e., where $w_{2,1}|^{w_1=1} = 1$). Importantly, because the future effort cost is discounted, the value is weakly decreasing in δ for both sophisticates and naïfs: impatient people value the opportunity more.

¹³See Proposition 5 for the exact condition; it entails there not being "too much" inframarginal behavior. When there is too much inframarginal behavior, not only will the effectiveness prediction not hold but incentives also become less likely to be a cost-effective approach.

¹⁴Thresholds where K/T is very low may not always be better for impatient naïfs than patient people because they include more days where current and future effort are substitutes, which can cause naïfs to procrastinate.

¹⁵Individually-demeaned steps in a group that did not receive incentives have a correlation of 0.4 across days. Raw (i.e., not demeaned) steps have a correlation of 0.7 across days.

The fact that impatient people value the day 2 opportunity more is what underlies the threshold's greater effectiveness for them. Individuals comply on day 1 if the value of the day 2 opportunity outweighs their day 1 effort cost:

$$w_{1} = \begin{cases} 1 & \text{if } e_{1} < (2m' - \delta e_{2})w_{2,1} \end{vmatrix}^{w_{1}=1} \\ 0 & \text{otherwise.} \end{cases}$$
(6)

Since $(2m' - \delta e_2)w_{2,1}|^{w_1=1}$ is weakly decreasing in δ , impatient people comply more on day 1. On day 2, individuals comply if $w_1 = 1$ and the payment exceeds their effort costs:

$$w_2 = \begin{cases} 1 & \text{if } e_2 < 2m' \text{ and } w_1 = 1\\ 0 & \text{otherwise.} \end{cases}$$
(7)

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.¹⁶

Sophisticates and Naïfs Although Prediction 1 holds for both sophisticates and naïfs, the exact compliance conditions differ (specifically, the terms projecting future behavior, $w_{j,t}$). In the two-day example, for sophisticates, who correctly predict their future preferences,

$$w_{2,1}\Big|^{w_1=1} = \mathbb{1}\{e_2 < 2m'\}.$$
 (8)

Thus, for a sophisticate to place a positive value on a day 2 work opportunity (i.e., for expression (5) 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, naïfs believe that their day-2 selves have the same preferences as their day-1 selves. For them,

$$w_{2,1}\Big|^{w_1=1} = \mathbb{1}\{\delta e_2 < 2m'\}.$$
(9)

Thus, naïfs 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 effort costs, $2m' - \delta e_2$ are positive). That is, naïfs positively value any lucrative day 2 "option" that they want their day 2 selves to execute. Naïfs comply on day 1 to give their day 2 selves the option to follow-through.¹⁷

¹⁶Effectiveness follows from compliance since an increase in compliance without a decrease in cost-effectiveness implies higher effectiveness, and the Appendix B.3 propositions show that, depending on the cost distribution, threshold cost-effectiveness tends to be flat or decreasing with $\delta^{(t)}$.

¹⁷In either case, Prediction 1 still holds because the equation (5) value is still weakly decreasing in δ . The equation (5) 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 naïfs, both of which are decreasing in δ . To see this in the naïve case, note that $(dM - \delta e_2)\mathbb{1}\{\delta e_2 < dM\} = \max\{dM - \delta e_2, 0\}$.

With time-bundled thresholds, these differences between sophisticates and naïfs should not normally affect behavior. The day 2 opportunities that are lucrative enough *options* to motivate naïfs 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 (8) implies equation (9)).¹⁸ By pairing the *options* that motivate naïfs with the *commitment* that both motivates sophisticates and helps naïfs follow through, thresholds work for both types.

In contrast, as discussed in Online Appendix E, there are other types of time-bundled contracts (other than thresholds) in which naïfs and sophisticates can make very different decisions.¹⁹ In those contracts, *options* and *commitment* are less tightly linked. For example, there are time-bundled contracts where compliance on day 1 generates a soft *commitment* for day 2 compliance but does not generate a positive NPV *option*. These contracts function exactly like commitment contracts and are effective for sophisticates only.²⁰ Importantly, thresholds never fall in this category.

2.4 Payment Frequency and Impatience over Payment

We now briefly explore a potential strategy for improving the performance of incentives in the case that people are impatient over payment rather than effort: increasing payment frequency. Specifically, we return to the base case separable linear contract from equation (2) and analyze compliance under different payment frequencies by changing the length of the payment period T. See Appendix B.4 for proofs.

Prediction 2 (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

¹⁸ To be more specific, comparison of equations (8) and (9) shows that the only potential difference between sophisticates and naifs is that, if $\delta e_2 < 2m' < e_2$, naifs could comply on day 1 while sophisticates would not. However, this behavior should be rare in practice. To see the reason, compare equations (6) and (7): because people sink costs as they move toward the threshold, the marginal incentive to comply is strictly higher on day 2 (where it is 2m') than on day 1 (where it is $2m' - \delta e_2$). Thus, if a naïf complies on day 1, they generally follow-through on day 2. Failure to follow-through for the naif requires that $e_2 > e_1/(1-\delta)$ which implies that e_2 is substantially higher than e_1 and/or that δ is very low.

¹⁹Online Appendix E investigates 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).

²⁰Specifically, generating a commitment means that the payment for day 2 compliance is greater than e_2 if and only if $w_1 = 1$. Generating an option means that the payment for day 2 compliance is greater than δe_2 (rather than e_2) if and only if $w_1 = 1$. Thus, if a contract pays between δe_2 and e_2 for day 2 work if $w_1 = 0$ but more than e_2 for day 2 work if $w_1 = 1$, day 1 compliance generates a commitment without generating an option. In such contracts, sophisticates might comply on day 1 even when their effort cost exceeds the maximum potential financial benefit of day 1 compliance in order to induce their day 2 self to comply, while naïfs will not.

patient over the receipt of financial payments ($d^{(t)} = 1$), then payment frequency does not affect compliance or effectiveness.²¹

In addition to containing a policy-relevant prediction for how to improve contract effectiveness in the face of impatience over payment, Prediction 2 also provides a way to use the empirical variation in compliance across payment frequencies to make inferences about the discount factor over payments. Our final prediction follows Kaur et al. (2015) to show an additional way to use empirical data to make inferences about the discount factor over payments, which will be useful for interpreting our results.

Prediction 3 (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.

2.5 Empirical Tests

Predictions 1 and 2 informed the design of our experiment. To assess Prediction 1, among participants who receive incentives in our experiment, we randomly vary whether the contract is linear or has a threshold. We then test for heterogeneity in the effect of the threshold relative to the linear contract based on a baseline measure of impatience over effort (we address potential confounds to the impatience measure in Section 6.2). To shed light on the potential role of payment frequency, we randomize the payment frequency, allowing us to understand both the effect of changing payment frequency and (per Prediction 2) whether agents are meaningfully impatient over payments. Both of these hypothesis tests were specified *ex ante*.²²

3 Experimental Design

3.1 Sample Selection and Pre-Intervention Period

We conducted our experiment in an urban area of South India. India is facing a diabetes epidemic, and prevalence is higher both in southern states and in urban areas. We selected our sample through a series of public screening camps in the city of Coimbatore, Tamil Nadu. To recruit diverse socioeconomic groups, we held the camps in various locations including the government hospital, markets, religious institutions, and parks. 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

²¹Although 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.

²²Before launching our experiment, we prepared a pre-analysis plan that guided our design and power calculations. While we did not polish it to post publicly in the AEA registry, one can find it at https://www.chicagobooth.edu/-/media/faculty/rebecca-dizon-ross/research/PAP_NCD_2015.pdf.

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."²³ After screening, we contacted eligible individuals by phone and invited them to participate in a program encouraging walking.

Surveyors visited the participants at their homes or workplaces to conduct a baseline health survey, deliver lifestyle modification advice, and enroll them in a one-week phase-in period designed to collect baseline walking data and to familiarize participants with program procedures. Surveyors demonstrated how to wear a pedometer properly, report steps, and check text messages from our reporting system (described in Section 3.3). Surveyors asked respondents 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 told participants what treatment group they had been randomly assigned to by guiding them through a contract describing the intervention period. We exclude from the sample all participants who withdrew or were found ineligible prior to randomization, leaving a final experimental sample of 3,192 individuals. The sample represents 41% of the screened, eligible population (see Table A.1 for the share of people dropped in each stage of the enrollment process). We screened and enrolled the sample on a rolling basis from Oct. 2016 to Oct. 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 into one of six incentive contracts for walking, as shown in Figure 1 and described next.

 $^{^{23}}$ 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.

3.2.1 Incentive Groups

All incentive groups received payments for accurately reporting steps above the daily 10,000step target through the automated step-reporting system. We delivered all incentive payments as mobile recharges (credits to the participant's mobile phone account).²⁴ After reporting steps, participants immediately received text-message confirmations of their step report, payment earned, and the payment date. We also sent participants weekly text messages summarizing their walking behavior and total payments earned.

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

The Base Case This group received a linear contract paying 20 INR 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 all other contracts differ from it in exactly one dimension: linearity, payment frequency, or payment amount. We can compare any other group to the base case group to assess the effect of changing a single contract dimension.

Time-Bundled Threshold Contracts The *threshold* treatment groups differ from the base case incentive group only in linearity: while the base case is a linear contract, the threshold contracts use time-bundled threshold payment functions. The 4-day threshold group 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, the 5-day threshold group 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 groups to walk at least four or five days per week when initially explaining the contracts. For those in the threshold groups, the target days-per-week was the same as their assigned threshold level; for those in the other groups, it was randomly assigned in the same proportion as the threshold groups were divided between the 4- and 5-day groups. We follow our *ex ante* analysis plan and pool the 4-day threshold and 5-day threshold treatment groups for our main analyses in order to maximize statistical power.²⁵

²⁴The 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).

 $^{^{25}}$ We sometimes also show the results for the two thresholds separately as exploratory analyses. 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.



Figure 1: Experimental Design

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

Receiving payments more frequently could increase the salience of step target compliance and trust in the payment system. To hold salience and trust in the payment system constant, all incentive groups received daily feedback on step target compliance and received a test payment of 10 INR the night before their incentive contract launched.

Payment Amount Our final incentive group, the *small payment group*, differs from the base case group only by the amount of incentive paid. This group received 10 INR, instead of the base case 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 2 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 testing whether the full program impacts health.

Monitoring Monitoring group 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 The pure control group 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.²⁶

²⁶To accommodate a request from our government partners, we also tested one additional intervention. Ten percent of the sample, cross-randomized across all other treatments, received the "SMS treatment," which consisted of weekly text message reminders to engage in healthy behaviors. We control for the SMS treatment in our regressions.

3.2.3 Contract Understanding

To ensure participants understood their contracts, a few days after each participant was told about their contract, a surveyor would call them to ask several questions testing their understanding of their contracts. 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 G.1).

3.3 The Intervention Period and After

To measure steps, we gave monitoring and incentive group participants Fitbit Zip pedometers for the duration of the intervention. Since most participants did not have regular internet access to sync their pedometer data, these data were not available in real time. Instead, we asked participants to report their daily step count to an automated calling system, which called participants every evening and prompted them to enter their daily steps from the pedometer. Incentive payments were based on these reports. 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.²⁷

We visited all participants three times during the 12-week intervention period. The primary purpose was to sync pedometers, but we also conducted short surveys to collect biometric and mobile phone usage 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 group 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, but surveyors still returned every four weeks to sync their pedometers.

4 Data and Outcomes

This section first describes our baseline data sources and presents summary statistics. Next, it describes our two sources of outcomes data: pedometer data and a health survey.

²⁷Online Appendix D contains detailed statistics on misreporting. Misreporting rates are similar across monitoring and incentive groups, suggesting misreports were primarily accidental.

4.1 Baseline Data: Health, Walking, and Time Preference

We have three sources of baseline data: a baseline health survey, a week of pedometer data, and a time-preference survey. 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 the previous 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, 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.

Finally, following the phase-in period, we conducted a baseline time-preference survey to measure impatience over effort. Since we will present average treatment effects before we present heterogeneity based on measures of effort impatience, we defer our description of the effort impatience measures until Section 6.2.1, after we present the average treatment effects.

4.2 Summary Statistics

The first column of Table 1 displays the baseline characteristics of our sample. The sample is, on average, 49.4 years old and has slightly more males than females. The average monthly household income is approximately 16,000 INR (about 200 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 2–4 of Table 1 show means for the pure control, monitoring, and incentive groups, while columns 5–9 show means separately for each incentive subgroup. To explore balance, we jointly test the equality of all characteristics in each of our three "comparison" groups (control, monitoring, and the base case incentive groups—the reference group for all incentive subgroups) with each of the treatment groups. All tests fail to reject the null that all differences are zero. Online Appendix Table G.2 shows that covariates are also balanced within the subsample for whom we have data during the post-intervention period.

	Full sample	Control	Monitoring	Incentives pooled	Daily	Base case	Monthly	Threshold	Small payment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Demographics									
Age (from BL)	49.54 (8.52)	49.78 (8.19)	50.28 (8.95)	49.44 (8.55)	49.57 (8.60)	49.60 (8.33)	48.80 (8.94)	49.41 (8.71)	49.11 (7.84)
Female $(=1)$	0.42 (0.49)	0.46 (0.50)	0.43 (0.50)	0.41 (0.49)	0.44 (0.50)	0.41 (0.49)	0.38 (0.49)	0.41 (0.49)	0.48 (0.50)
Labor force participation $(=1)$	0.75 (0.44)	$\begin{array}{c} 0.73 \\ (0.45) \end{array}$	$ \begin{array}{c} 0.72 \\ (0.45) \end{array} $	$\begin{array}{c} 0.75 \\ (0.43) \end{array}$	$0.75 \\ (0.43)$	0.74 (0.44)	$ \begin{array}{c} 0.81 \\ (0.39) \end{array} $	$\begin{array}{c} 0.75 \\ (0.43) \end{array}$	0.70 (0.46)
Per capita income (INR/month)	4463 (3638)	4488 (4483)	4620 (3160)	4447 (3447)	4068 (2765)	4477 (3496)	4599 (3235)	4461 (3570)	4341 (2615)
Household size	3.91 (1.62)	$3.94 \\ (1.54)$	3.82 (1.51)	$3.91 \\ (1.64)$	$3.92 \\ (1.45)$	3.89 (1.70)	3.74 (1.59)	$3.96 \\ (1.65)$	3.58 (1.29)
B. Health									
Diagnosed diabetic $(=1)$	0.67 (0.47)	0.67 (0.47)	0.68 (0.47)	0.66 (0.47)	0.62 (0.49)	0.68 (0.47)	0.62 (0.49)	0.67 (0.47)	0.59 (0.50)
Blood sugar index	0.01 (0.92)	0.01 (0.93)	0.05 (0.91)	0.01 (0.92)	0.00 (0.95)	0.02 (0.90)	0.01 (0.99)	0.01 (0.93)	-0.15 (0.83)
Hba1c (mmol/mol)	8.69 (2.33)	8.69 (2.36)	8.74 (2.40)	8.69 (2.32)	8.59 (2.37)	8.73 (2.28)	8.68 (2.45)	8.70 (2.33)	8.35 (2.14)
Random blood sugar (mmol/L)	192.42 (89.39)	191.32 (88.73)	196.07 (86.67)	192.51 (89.87)	195.58 (91.54)	193.26 (88.25)	193.30 (98.14)	192.23 (90.42)	177.38 (77.00)
Systolic BP (mmHg)	133.34 (19.16)	$133.20 \\ (20.28)$	134.08 (17.72)	$133.35 \\ (19.01)$	$135.12 \\ (21.35)$	133.29 (19.10)	134.05 (19.19)	132.88 (18.38)	135.62 (21.42)
Diastolic BP (mmHg)	88.48 (11.12)	88.47 (11.51)	88.54 (10.12)	88.49 (11.09)	89.47 (12.68)	88.20 (10.77)	88.51 (10.13)	88.48 (11.11)	90.00 (13.19)
HbA1c: Diabetic $(=1)$	$\begin{array}{c} 0.82 \\ (0.38) \end{array}$	$ \begin{array}{c} 0.82 \\ (0.38) \end{array} $	$\begin{array}{c} 0.81 \\ (0.39) \end{array}$	$ \begin{array}{c} 0.82 \\ (0.38) \end{array} $	0.77 (0.42)	0.84 (0.36)	$\begin{array}{c} 0.79 \\ (0.41) \end{array}$	$\begin{array}{c} 0.81 \\ (0.39) \end{array}$	0.77 (0.42)
BP: Hypertensive $(=1)$	0.49 (0.50)	0.46 (0.50)	$\begin{array}{c} 0.51 \\ (0.50) \end{array}$	0.49 (0.50)	$\begin{array}{c} 0.53 \\ (0.50) \end{array}$	0.49 (0.50)	$\begin{array}{c} 0.51 \\ (0.50) \end{array}$	0.49 (0.50)	0.45 (0.50)
Overweight $(=1)$	$\begin{array}{c} 0.61 \\ (0.49) \end{array}$	$0.62 \\ (0.49)$	$0.66 \\ (0.47)$	$0.60 \\ (0.49)$	$\begin{array}{c} 0.57 \\ (0.50) \end{array}$	0.60 (0.49)	$\begin{array}{c} 0.57 \\ (0.50) \end{array}$	0.60 (0.49)	0.67 (0.48)
BMI	26.37 (4.30)	26.43 (4.24)	$26.46 \\ (3.63)$	26.36 (4.36)	26.46 (5.33)	26.45 (4.51)	26.38 (4.82)	26.24 (4.01)	26.99 (4.10)
C. Walking - Phase-in									
Exceeded step target $(=1)$	0.25 (0.32)	$\begin{array}{c} 0.25 \\ (0.31) \end{array}$	0.24 (0.32)	0.25 (0.32)	$0.25 \\ (0.32)$	0.23 (0.30)	0.27 (0.33)	$0.26 \\ (0.33)$	0.27 (0.34)
Average daily steps	7009 (3982)	7081 (3953)	6906 (3701)	7007 (4015)	7068 (4198)	$6823 \\ (3966)$	7446 (3869)	7084 (4037)	7018 (4195)
D. Impatience over effort									
Impatience index (SD's)	0.09 (0.99)	0.00 (1.00)	0.05 (0.89)	0.12 (0.99)	0.04 (0.95)	0.14 (1.05)	0.18 (0.91)	0.09 (0.97)	0.26 (0.91)
Predicted index (SD's)	-0.05 (1.00)	0.00 (1.00)	-0.15 (0.94)	-0.06 (1.01)	-0.09 (1.02)	-0.02 (1.00)	-0.02 (1.09)	-0.08 (1.00)	-0.12 (0.97)
E. Mobile Recharges									
Current mobile balance (INR)	29.26 (49.42)	30.80 (48.79)	29.48 (48.68)	28.98 (49.88)	28.61 (38.54)	29.69 (52.08)	28.55 (63.65)	28.45 (47.96)	30.05 (36.59)
Yesterday's talk time (INR)	6.61 (8.79)	7.22 (10.14)	6.47 (8.95)	6.44 (8.36)	5.86 (6.25)	6.58 (8.77)	7.67 (9.19)	6.31 (8.28)	4.94 (5.77)
Prefers daily payment $(=1)$	0.17 (0.38)	0.18 (0.38)	0.16 (0.37)	0.17 (0.38)	0.20 (0.40)	0.17 (0.37)	0.20 (0.40)	0.17 (0.38)	0.18 (0.39)
Prefers monthly payment $(=1)$	0.24 (0.43)	0.25 (0.43)	0.28 (0.45)	0.24 (0.43)	0.27 (0.45)	0.24 (0.43)	0.23 (0.42)	0.24 (0.43)	0.26 (0.44)
F-tests for Joint Orthogonality	NI/A	NT / A	0.01	0.41	0.77	0 50	0.41	0.50	0.54
r-value (relative to control) P-value (relative to monitoring) P-value (relative to base case)	N/A N/A N/A	N/A 0.81 0.58	0.81 N/A 0.85	0.41 0.95 N/A	$0.77 \\ 0.86 \\ 0.55$	0.58 0.85 N/A	$0.41 \\ 0.59 \\ 0.81$	$0.50 \\ 0.98 \\ 0.96$	$0.54 \\ 0.71 \\ 0.51$
Sample size									
Number of individuals	3.192	585	203	2.404	166	902	164	1.106	66
Percent of sample	100.0	18.3	6.4	75.3	5.2	28.3	5.1	34.6	2.1
Number of ind. with ped. data	2,582	_	200	2,359	163	890	163	1,079	64

Table 1: Baseline Summary Statistics in Full Sample and by Treatment Group

Notes: Standard deviations are in parentheses. BMI is body mass index, and BP is blood pressure. Overweight means BMI above 25. Hypertensive means systolic BP above 140 or diastolic BP above 90. The Threshold count pools both the 4-day and 5-day threshold groups. In the incentive and monitoring groups, the number of individuals with pedometer data ("Number of ind. with ped. data") differs from the total number of individuals because a few participants withdrew immediately. The likelihood of immediate withdrawal is not significantly different between incentive and monitoring (*p*-value > 0.7, Table A.2 column 5).

4.3 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.

4.3.1 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 (≈ 3 USD) if participants wore their pedometer (i.e., had positive steps) on at least 70% of days. As a result, pedometer wearing rates are high, and the difference between treatment groups is small: 85% in monitoring versus 88% in incentives. However, the difference is significant at the 10% level (Table A.2, column 2). To address the imbalance, we report Lee (2009) bounds accounting for missing step data due to not wearing pedometers.²⁸ Our primary specifications do not condition on wearing the pedometer (instead setting 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 daily by each pedometer on the time that the participant put it on and the time that they took it off.²⁹ Reassuringly, Panel B of Table A.4 shows that these times are balanced across groups.

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% (relative to 87% in the intervention period), post-intervention wearing rates are balanced across arms, and our results are robust to a Lee bounds exercise (Tables A.5 and A.6).

 $^{^{28}}$ 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 is balanced across incentive and monitoring groups (column 2, Table A.2). While our main specifications drop days with missing pedometer data, Table A.3 shows robustness to alternate specifications and Lee bounds. While missing data is balanced overall, one specific source of missing data (mid-intervention withdrawals) is imbalanced (column 5 of Table A.2), but results are robust to Lee bounds accounting specifically for that source (column 5 of Table A.3).

²⁹Specifically, for a subset of days, the pedometers record data on minute-wise (instead of day-wise) step counts, allowing us to back out the first and last minute the pedometer was worn.

Another concern is that participants might give their pedometers to someone else. Our data suggest that this concern is limited. First, we performed 836 unannounced audit visits to participants' homes. In 99.6% of visits, participants were not sharing their pedometers. Second, we check whether participants' minute-wise step counts exceed expectations given their age. This happened very rarely and is balanced across incentive and monitoring groups (Table A.4).

4.4 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 of the treatment groups (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 we pre-specified HbA1c as our only blood sugar measure, we had some problems measuring it in the field.³⁰ As such, while piloting, we also decided to measure RBS, which is also strongly associated with diabetes severity (Bowen et al., 2015).³¹ RBS is much easier to reliably measure in the field. Our measures of RBS and HbA1c both have predictive power for the other. (Table A.7 shows that baseline RBS has strong predictive power for endline HbA1c in the control group even conditional on baseline HbA1c, and that the reverse is true as well.) As a result, our preferred measure incorporates both the HbA1c and RBS measurements, but we also present the measures separately as pre-specified.

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 use these three measures to construct a standardized "health risk index" that also includes the two blood sugar measures.

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). For all our indices, for individuals who have nonmissing responses to at least one index component, we impute missing components as the sample mean following Kling et al. (2007).

³⁰The only available measurement tool (the SD A1cCare analyzer from SD Biosensor) was temperaturesensitive and error prone, and its measurements did not line up with lab measurements (the gold standard).

³¹The main downside of RBS as a clinical measure is that it is more sensitive to recent activity such as eating; however, proper glycemic control involves minimizing RBS spikes and so, on average, across the sample, RBS can give us a good measure of the glycemic control of our sample (Dandona, 2017).

5 Incentives and Chronic Disease

In this section, we explore whether incentives for exercise deliver results in the global fight against chronic disease. First, we test whether incentives increase exercise, both during and after the intervention. Exercise is a critical intermediate health outcome for this population since, in the long term, it reduces complications from diabetes and hypertension and averts premature mortality (World Health Organization, 2009). Second, we directly test whether incentives for exercise improve blood sugar and cardiovascular health.

5.1 Incentives and 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:

$$y_{it} = \alpha + \beta Incentives_i + \mathbf{X}'_i \gamma + \mathbf{X}'_{it} \lambda + \varepsilon_{it}, \qquad (10)$$

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 X_i and X_{it} are vectors of individual- and day-level controls, respectively, described in the notes to Table 2. 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. Table 2 shows the results. Figure 2 also displays the results graphically.

Incentives have large impacts on walking, increasing the share of days that participants reach their 10,000-step target by 20 pp (column 1 of Table 2 and Figure 2(a)). This effect does not simply reflect participants shifting steps from one day to another: column 2 of Table 2 and Figure 2(b) show 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, on average. This treatment effect is at the high end of effect sizes 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).

This analysis excludes the control group, for whom we have no pedometer data during the intervention period. Because monitoring itself may have an independent positive impact, these estimates are likely conservative for the overall impact of incentives. That said, a comparison of steps within the monitoring group between the baseline and intervention periods (controlling for time effects) suggests that, while monitoring may modestly increase the likelihood of exceeding the step target, it does not appear to increase steps (Online Appendix K).

Dependent variable:	Exceeded step target	Daily steps	Daily steps $(if > 0)$
	(1)	(2)	(3)
Incentives	0.196^{***} [0.0180]	1286.2^{***} [211.4]	$\frac{1144.3^{***}}{[190.3]}$
Monitoring mean # Individuals # Observations	0.294 2,559 205,732	6,774 2,559 205,732	7,986 2,557 180.018

Table 2: Incentives Increase Average Walking

Notes: This table shows the treatment effect of incentives (relative to monitoring) on walking. The columns show coefficient estimates from regressions based on equation (10) using intervention-period pedometer data. In column 1, "Exceeded step target" is an indicator variable equal to 1 if the individual exceeded their step target. All specifications control for the average of the dependent variable during the phase-in period (before randomization) and its second order polynomial, a dummy for the SMS treatment, and a set of controls selected separately for each specification using the post double selection lasso method of Belloni et al. (2017). The set of controls lasso selected over included the following individual-level controls: age, weight, height, gender, and their second-order polynomials, as well as the following day-level-controls: month-year and day-of-week fixed effects. In columns 1 and 2, lasso selected age and a subset of the day-of-week fixed effects. The sample includes the incentive and monitoring groups. The omitted category in all columns is the monitoring group. The Threshold group pools the 4- and 5-day Threshold groups. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.



Figure 2: Incentives Increase Average Walking

Notes: The figure displays the impact of the pooled incentive treatments on walking during the intervention period. The confidence interval represents the test of equality between the incentive and monitoring groups with the same controls selected by lasso in Table 2. Panel A shows the average probability of exceeding the daily step target; Panel B shows average daily steps walked.

The treatment 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 Table A.3 reports Lee bounds which account for the nonrandom patterns of missing data. Both strategies find similar effects. The estimates are also robust to excluding the control variables from the regression (Table A.8).

Figure 3 shows that incentives have a striking impact on the distribution of daily steps. Although there is bunching at 10,000 steps in both groups, the bunching in the incentive group is substantially more pronounced. This suggests that the financial incentives are motivating individuals to comply with their daily step targets.



Figure 3: Incentives Shift the Distribution of Steps Walked per Day

Notes: The figure displays the impact of the pooled incentive groups relative to the monitoring group during the intervention period. The confidence intervals represent tests of equality between the incentive and monitoring groups with the same controls selected by lasso in Table 2.

5.1.1 Evolution over Time and Persistence of Exercise Effects

We now analyze how the exercise impacts evolve over time, both during and after the intervention. We begin with their evolution during the intervention. Panels A and B of Figure 4 estimate equation (10) separately by week of the intervention. After an initial spike at week 1, the effect of incentives on walking remains stable during the full intervention period. This suggests that policymakers could extend the program further with similar effects, an interesting finding since insurers and governments are increasingly rolling out longer-term (and even permanent) incentive programs.





(d) Daily Steps Walked Post Intervention

Figure 4: Incentive Effects are Steady through the 12-Week Program and Persist Afterward

Notes: Panels A and C show the average probability of exceeding the step target and Panels B and D show the average daily steps walked. Panels A and B depict the intervention period and Panels C and D depict the 12 weeks subsequent to the intervention. Week 0 in Panels A and B is the phase-in period (before randomization). "No incentives" in Panels C and D represents the pooled monitoring and control groups; the panels look very similar when we compare with the control group only (Online Appendix Figure G.1). The shaded areas represent a collection of confidence intervals from tests of equality within each weekly period between the incentive and comparison groups from regressions with the same controls selected by lasso in Tables 2. All graphs are unconditional on wearing the pedometer. See Figure A.2 for versions of the figures that condition on wearing the pedometer; they suggest that the reason that steps trend downwards in all groups over time in panels B and D is that pedometer wearing rates declined over time.

Do the effects of incentives also 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 (10) using the pedometer data from the 12 weeks after the intervention ended. Since we have data from the control group as well during this period, the *Incentives* coefficient now represents the effect of incentives relative to the control and

monitoring groups pooled. We pool the comparison groups for power given that sample size is limited because we only have data for the post-intervention period from a third of our sample.³²

Table 3 shows that the incentive group walks significantly more then the pooled comparison groups even after incentives end. The treatment effect on steps is statistically significant and large: around 8% of the comparison group mean in both columns 2 and 3. For comparison, the treatment effect of incentives relative to monitoring during the intervention period was 20% of the monitoring group mean. Hence, a meaningful portion of the treatment effect appears to have persisted.³³ Panels C and D of Figure 4 suggest that some of the effect persisted until the end of the measurement period. Our short-run incentive program may thus induce habit formation, enabling long-term impacts.

	Post-intervention					
Dependent variable:	Exceeded step target	Daily Steps	Daily Steps (if > 0)			
	(1)	(2)	(3)			
Incentives	0.063^{***} $[0.01]$	462.1^{**} [221.58]	611.5^{***} [196.90]			
No incentives mean # Individuals # Observations	$0.216 \\ 1,122 \\ 91,756$	5,687 1,122 91,756	7,347 1,122 62,858			

Table 3: The Effects of Incentives Persist After the Intervention Ends

Note: This table shows the average treatment effect of incentives relative to the control and monitoring groups (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 columns 1 and 2 only include days where the participant wore the pedometer (i.e., had step count > 0). Controls are selected separately for each column using the post-double selection lasso method of Belloni et al. (2017), where the set of controls that lasso selected from is the same as in Table 2. In each column, Lasso only selected a subset of the year-month and day-of-week fixed effects in all specifications (with the specific subset chosen varying by column). Table A.9 shows that the results are robust to excluding controls. The number of individuals differs from the total number of individuals recruited for the post-intervention period because roughly 10% of participants withdrew immediately. The likelihood of immediate withdrawal is not significantly different between the incentive and comparison groups (Table A.5 column 5), and Table A.6 shows that the results are robust to a Lee bounds exercise. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

 $^{^{32}}$ The results are similar when we compare incentives with control alone (Online Appendix Figure G.1); with only 72 people, the post-intervention monitoring group is too small to analyze alone.

 $^{^{33}}$ Note that we are comparing the effect of incentives relative to control in the post-intervention period with the effect of incentives relative to *monitoring* in the intervention period. While this comparison overstates the degree of persistence if there is a positive effect of monitoring on steps, Online Appendix K suggests that monitoring does not affect steps.

5.2 Health Effects

We now examine whether the incentives program measurably improves health in the short run. Our experiment was powered to detect the difference between the incentive and pure control groups. We lack statistical power to compare health outcomes (which are relatively noisy) in the monitoring group with the other groups, although we show it for completeness. Table 4 reports results from regressions of the following form:

$$y_i = \alpha + \beta_1 Incentives_i + \beta_2 Monitoring_i + \mathbf{X}'_i \gamma + \varepsilon_i, \tag{11}$$

where y_i is a health outcome at endline for individual *i*, and X_i is a vector of controls, shown in the table notes. The coefficient of interest is β_1 , the effect of incentives relative to the control group.

Table 4 shows that the incentive program moderately improves blood sugar and overall cardiovascular health. Column 1 presents the treatment effect on our preferred blood sugar measure, the standardized index incorporating both the longer-term HbA1c and shorter-term RBS measures of blood sugar control. Incentives improve the index by 0.05 standard deviations. Columns 2 and 3 display HbA1c and RBS separately. Column 4 shows that incentives improve the overall health risk index by 0.05 SDs, significant at the 10% level.

Since health outcomes among those with more severe diabetes might be more responsive to exercise, our *ex ante* analysis plan included an analysis of the health impacts separately among those with higher blood sugar. To do so, we estimate the following regression:

$$y_{i} = \alpha + \beta_{1} Incentives_{i} + \beta_{2} Incentives_{i} \times LowBloodSugar_{i} + \beta_{3} Monitoring_{i} + \beta_{4} Monitoring_{i} \times LowBloodSugar_{i} + \beta_{5} LowBloodSugar_{i} + \mathbf{X}_{i}'\gamma + \varepsilon_{i},$$
(12)

with $LowBloodSugar_i$ an indicator for having below-median baseline values of the blood sugar index (i.e., less severe diabetes). β_1 is the coefficient of interest, telling us the treatment effect of incentives among those with above-median values of the blood sugar index at baseline (i.e., with LowBloodSugar = 1). β_2 then allows us to test if the effect among those with above-median baseline blood sugar is significantly different from those without.

The results, shown in columns (5)-(8) of Table 4, indicate that the treatment effects are larger among those with more severe diabetes, although we can only reject equality for the blood sugar index (at the 10% level). Among the sample with above-median blood sugar, incentives decrease the blood sugar index by 0.10 SDs and decrease RBS by 12 mg/DL.

Both the full sample and subsample treatment effects on blood sugar are moderately-sized but meaningful from a clinical perspective.³⁴ In addition, an exploratory analysis of the treat-

³⁴For example, to interpret the RBS result, note that, for RBS measured in the morning, a value of less than

	Full sample effects				Heterogeneity by baseline blood sugar			
Dependent variable:	Blood sugar index (1)	HbA1c (2)	Random blood sugar (3)	Health risk index (4)	Blood sugar index (5)	HbA1c (6)	Random blood sugar (7)	Health risk index (8)
Incentives	-0.05** [0.03]	-0.08 [0.07]	-6.0^{*} [3.5]	-0.05* [0.03]	-0.10** [0.05]	-0.1 [0.1]	-11.6^{**} [5.9]	-0.08** [0.04]
Incentives \times below- median blood sugar					0.09^{*} [0.05]	$\begin{array}{c} 0.1 \\ [0.1] \end{array}$	11.0 [7.0]	$0.08 \\ [0.05]$
Monitoring	-0.03 $[0.05]$	-0.1 $[0.1]$	1.3 [6.6]	$0.01 \\ [0.04]$	-0.07 $[0.08]$	-0.3 [0.2]	$1.2 \\ [10.5]$	-0.05 $[0.07]$
Monitoring \times below- median blood sugar					$0.08 \\ [0.09]$	$0.3 \\ [0.2]$	-1.5 [12.6]	$0.1 \\ [0.09]$
$p-value: I = M^{\dagger}$ Control mean [‡] # Individuals	$0.517 \\ 0.0 \\ 3,067$	$0.609 \\ 8.4 \\ 3,066$	0.220 193.8 3,067	$0.146 \\ 0.0 \\ 3,068$	$0.627 \\ 0.6 \\ 3,067$	$0.309 \\ 10.1 \\ 3,066$	$0.171 \\ 248.3 \\ 3,067$	$0.531 \\ 0.5 \\ 3,068$

Table 4:	Incentives	Moderately	Improve	Blood Sug	ar and	Cardiovascula	r Health
		•/	1	C	,		

Notes: † Incentives = Monitoring. ‡ In columns 1-4 we report means of the full control group and in columns 5-8 we report means of control individuals with above-median values of the baseline blood sugar index.

Observations are at the individual-level. Columns 1-4 display OLS estimates of equation (11). Columns 5-8 display display OLS estimates of equation (12); note that $LowBloodSugar_i$ in equation (12), which is the indicator for having below-median baseline values of the blood sugar index (i.e., less severe diabetes), is labeled as "below-median blood sugar" in the table. (Online App. Table G.3 shows that the estimates are nearly quantitatively identical if we analyze heterogeneity based on baseline HbA1c instead of the baseline blood sugar index, and Online App. Table G.4 shows that we reach similar conclusions, particularly for the high blood sugar sample, when, instead of using OLS to analyze the treatment effects, we use an instrumental variables analysis, using the dummies for each of the different incentive subtreatments as instruments for intervention-period steps.) HbA1c is the average plasma glucose concentration (%). Random blood sugar is the blood glucose level (mg/dL). The blood sugar index is constructed by taking the mean of endline HbA1c and random blood sugar 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, random blood sugar, mean arterial blood pressure, body mass index, and waist circumference standardized by their average and standard deviation in the control group. See Online Appendix Table G.5 for treatment effects on the other components of that index not shown here. Each physical health outcome that either appears as an outcome variable or is an index component is trimmed using World Health Organization guidelines to trim biologically implausible health outcome measurements (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 a set of controls selected separately for each specification using the post double selection lasso method of Belloni et al. (2017). The set of controls lasso selected over included the following controls: age, weight, height, gender, and their second-order polynomials, as well as month-year and day-of-week fixed effects for endline completion dates. The only control selected was for HbA1c, where lasso selected a subset of the month-year fixed effects for the endline completion date; no additional controls were selected for any other dependent variables. Columns 5-8 additionally control for the indicator for below-median blood sugar. Table A.10 shows that the estimates are similar, just less precise, when we omit the control variables from the regressions. Robust standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

ment effects on RBS suggests that the effects may amplify over time. Specifically, since we measured RBS (but not HbA1c) every 3 weeks during the intervention period, we can track how the RBS treatment effects evolve. Figure 5 shows that the treatment effect of incentives increases at each subsequent measurement. This suggests that the effects might continue to grow if either the program were extended or (as we show) the exercise effects persist.



Figure 5: Blood Sugar Treatment Effects Grow Over Time

(a) Full sample

(b) Above-median blood sugar sample

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 if we compare incentives with control alone, precision is just slightly lower; see Table G.6 in the Online Appendix. For each survey, we regress random blood sugar on the incentives dummy and control for the same controls selected by lasso for 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 .06 and .02 for the Panels A and B, respectively (see Table G.6 in the Online Appendix).

Table 5 examines whether the intervention had coincident impacts on mental health or fitness. Incentives improve the mental health index by 0.09 SD. In contrast, we find no effects on physical fitness, perhaps because we could only 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 G.7).

¹⁰⁰ mg/dl would be normal, values of 100-125 mg/dl would indicate prediabetes, while values above 126 mg/dl indicate diabetes. Thus an improvement of 6 or 12 mg/dl would bring someone near the diabetes threshold either a quarter or half of the way towards normal (healthy) blood sugar.

Dependent variable:	$\begin{array}{c} \text{Mental health index} \\ (1) \end{array}$	Fitness time trial index (2)
Incentives	0.092^{**} [0.045]	$0.015 \\ [0.045]$
Monitoring	0.16^{**} [0.072]	$0.071 \\ [0.074]$
p-value: Incentives = Monitoring	0.255	0.386
Control mean	0.0	0.0
# Individuals	3,068	2,890

 Table 5: Incentives Also Improve Mental Health

Notes: Observations are at the individual-level. Both specifications control for the baseline value of the index components, the index components squared and a set of controls selected separately for each specification using the post double selection lasso method of Belloni et al. (2017). The set of controls lasso selected over included the following controls: age, weight, height, gender, and their second-order polynomials, as well as month-year and day-of-week fixed effects for endline completion dates. The Mental health index averages the values of seven questions adapted from RAND's 36-Item Short Form Survey (SF-36). A large value of Fitness Time Trial Index indicates low fitness: it is an index created by the average two trials of endline seconds to walk four meters, and the seconds to complete five sit-stands standardized by their average and standard deviation in the control group. See Online Appendix Table G.8 for treatment effects on the individual components of the mental health and fitness indices. Each component of the fitness time trial index is trimmed using World Health Organization guidelines to trim biologically implausible health outcome measurements (i.e., z-scores < -4 or > 4). Robust standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

5.3 Incentives and Chronic Disease: Results Summary and Discussion

Overall, these results are promising from a policy perspective. The exercise results show that incentives substantially increase exercise throughout the entire intervention period. Some of the effect even persists after the intervention period ends. Exercise has important long-run health benefits for diabetics (Praet and van Loon, 2009; Qiu et al., 2014; Lee et al., 2012), and even in the short run we find that incentives translate to meaningful improvements in blood sugar, cardiovascular health, and mental health.

Our work thus provides a rare example of a scalable and effective lifestyle intervention that can be delivered in resource-poor settings with limited health infrastructure. Interventions previously shown to improve exercise among diabetics and prediabetics have required highly trained staff to engage in frequent and personally-tailored interactions with participants (Aziz et al., 2015; Qiu et al., 2014), and hence have had limited scalability. Developing scalable approaches to generate exercise among those with diabetes and other chronic diseases is a crucial policy priority.

Our intervention is scalable and relatively low-cost. The per-person program cost of the incentive program is 1,700 INR or 26 USD. That is equal to just 7% of the estimated annual direct cost of care for a diabetic in Tamil Nadu, or 21% of the direct cost of care during the 3-month intervention period (Tharkar et al., 2010). Interventions generating similar levels of exercise among diabetics in other contexts have produced cost savings of at least the same order

of magnitude, even without effects that persist like we find (Nguyen et al., 2007, 2008).

Thus, our results suggest that incentive programs could be an important tool to help decrease the burden of chronic disease in India. Given these promising results, we now examine how to further improve the program in the face of impatience.

6 Incentives, Impatience, and Time-bundled Contracts

This section investigates the implications of impatience for the design of incentives, primarily exploring time-bundled thresholds, our contract variation designed to improve effectiveness in the face of impatience over effort. First, we compare the effectiveness of the time-bundled threshold and linear contracts in the full sample. Second, we test our main theoretical prediction: that time-bundled thresholds increase compliance and effectiveness more among those who are impatient over effort than among those who are not (Prediction 1). Third, we evaluate a more standard strategy for improving compliance and effectiveness in the face of impatience (over payment): increasing the frequency of payment. Finally, we discuss the potential policy implications of our findings and the welfare implications of improving contract effectiveness.

6.1 Average Effectiveness of Time-bundled Threshold Contracts

We first compare the sample-average performance of the threshold and linear contracts. Our theoretical analysis (e.g., Appendix Proposition 3) suggests that, under plausible conditions—such as the effort discount rates being sufficiently high—time-bundled thresholds can be more effective overall than linear contracts, making this an interesting comparison.

In order to establish that time-bundled threshold contracts are effective on average, we can show that they result in weakly more compliance and weakly higher cost-effectiveness than linear contracts in the full sample, with one inequality strict (Section 2). We thus examine compliance and cost-effectiveness in turn.

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

$$y_{it} = \alpha + \sum_{j} \beta_{j} \times \left(incentives^{j}\right)_{i} + \mathbf{X}_{i}^{\prime}\gamma + \mathbf{X}_{it}^{\prime}\theta + \varepsilon_{it},$$
(13)

where y_{it} are daily walking outcomes and $(incentives^j)_i$ is an indicator for whether individual i is enrolled in incentive treatment group $j \in (\text{daily, base case, monthly, threshold, small payment})$. The β_j coefficients capture the average effect of each incentive treatment group relative to the monitoring group. Table 6 displays the results.

The effect of the threshold treatment on compliance is very similar to the effect of the base

Dependent variable:	Exceeded step target	Daily steps	Daily steps $(if > 0)$	
	(1)	(2)	(3)	
Base case	0.207^{***} [0.0196]	1411.4^{***} [225.0]	$\frac{1197.1^{***}}{[201.8]}$	
Daily	0.199^{***} [0.0302]	1126.7^{***} $[332.2]$	1245.1^{***} [279.2]	
Monthly	0.179^{***} [0.0281]	1302.6^{***} [311.0]	1152.5^{***} [272.3]	
Threshold	0.194^{***} $[0.0194]$	1238.1^{***} [223.2]	1125.5^{***} [200.3]	
Small payment	0.128^{***} [0.0382]	740.8^{*} [381.0]	510.5 [331.3]	
P-value for base case vs				
Daily	0.79	0.31	0.83	
Monthly	0.27	0.67	0.84	
Threshold	0.35	0.21	0.55	
Small payment	0.03	0.05	0.02	
Monitoring mean	0.294	6,774	7,986	
# Individuals	2,559	2,559	2,557	
# Observations	205,732	205,732	180,018	

Table 6: All Incentive Contracts Increase	Walking
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Notes: We report incentive effects (relative to monitoring) separately by each incentive treatment group. The columns show coefficient estimates from regressions based on equation (13) using daily intervention-period pedometer data. Each column uses the same controls selected by lasso in Table 2; the results are robust to excluding controls (Online Appendix Table G.9). The sample includes the incentive and monitoring groups. The omitted category in all columns is the monitoring group. The Threshold group pools the 4- and 5-day Threshold groups. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

case (linear) treatment on compliance, with the estimates within 1.3 pp of each other and the difference not statistically significant (p-value=0.36). Figure 6 displays the result graphically. It also shows the 4-day threshold group and 5-day threshold groups separately—neither has meaningfully different compliance than the base case.



Figure 6: Adding a Time-Bundled Threshold Does Not Significantly Affect Average Walking

Notes: The figure compares the time-bundled threshold treatments with the base case (linear) incentive treatment. Panel A shows the average probability of exceeding the daily step target during the intervention period; Panel B shows average daily steps walked during the intervention period. The confidence intervals represent tests of equality between the base case incentive group and each other treatment group, with the same controls as selected by lasso in Table 2. The Threshold group pools the 4- and 5-day threshold groups.

Cost-effectiveness and Overall Effectiveness However, the threshold contracts are more cost-effective than the base case contract. Individuals in the threshold group 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. 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. As a result, the cost-effectiveness of the threshold contracts are 11% and 17% higher than that of the base case contract (Table A.11).

Because the threshold contracts have the same compliance and are more cost-effective than the base case, they are more effective overall. For comparison, the small payment treatment is also more cost-effective than the base case (it pays half as much per day complied), but this comes at the cost of reduced compliance (Table 6).

Variance and Effectiveness in Other Settings Equal compliance and higher cost-effectiveness only necessarily imply higher effectiveness when the benefits of compliance are linear (Section

2). While the health benefits of compliance appear to be linear in our setting (e.g., Warburton et al., 2006), there are many other settings of interest with nonlinear benefits. In those settings, effectiveness will depend not just on average compliance but on the variance of compliance across payment periods.

Theory suggests that thresholds can increase the variance of compliance by decreasing the likelihood of intermediate effort (i.e., effort just below the threshold) (Grant and Green, 2013). To assess this prediction empirically, Figure 7 displays histograms of the number of days the step target was met per week in the threshold and base case groups. The threshold contracts significantly increase variance, causing more individuals to achieve their step target zero or seven days in the week.³⁵ This implies that, relative to linear contracts, thresholds are likely to be less effective in settings where the benefits of compliance are concave, and more effective in settings where the benefits of compliance are convex.



Figure 7: Time-Bundled Threshold Contracts Increase Variance Across Weeks

Notes: This figure shows the distribution of the number of days walked each week during the intervention period. Data are at the respondent-week level. Confidence intervals represent a test of equality between the base case and 4- or 5-day treatment from a regression with the same controls selected by lasso as in Table 2.

6.2 Heterogeneity in Threshold Effects by Impatience over Effort

We now assess whether time-bundled threshold contracts perform better relative to linear contracts when individuals are more impatient over effort (Prediction 1). To do so, we perform two analyses. First, we examine the heterogeneity by impatience in the effect of the threshold relative to the base case. Since Prediction 1 regards heterogeneity in the threshold effect *holding*

³⁵Online Appendix Table G.10 substantiates this conclusion, presenting a formal test of the effect of the threshold on the variance of week-level compliance. While the increase in dispersion and in zeroes in the threshold treatment is consistent with theory, the increase in density at seven days in particular (instead of at the specific threshold level of four or five) is perhaps surprising. Potential explanations include habit formation or that it is easier to schedule walking every day in a given week than on a subset of days.
all else constant, this heterogeneity analysis will only be a direct test of the theory if impatience is not correlated with other variables that influence the effectiveness of the threshold. To shed light on whether this condition holds here, we control for many covariates interacted with the threshold and show that the estimated relationship is robust. Moreover, even if there were omitted variables affecting the estimate, our heterogeneity estimate may be the one that is more 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.

Second, to tie our data to our theory more precisely, in Appendix C, we calibrate a model to determine whether the gap in predicted compliance between the threshold and linear contracts varies with the discount rate over effort. We find that it does: projected compliance in the most effective time-bundled contract increases by 3 pp relative to the linear contract for each 10 pp decrease in the discount factor.

We next describe how we measure impatience over effort before presenting our results on heterogeneity in the effect of thresholds by impatience over effort.

6.2.1 Measurement of 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." Since our sample was somewhat elderly and had difficulty with the more complicated screen-based measures often used in the literature (e.g., Andreoni and Sprenger 2012), we included simple measures that the full sample could comprehend.

Our primary measure of impatience over effort is an index of easy-to-comprehend, surveybased measures of impatience and procrastination taken from the psychology literature. Specifically, the questions are a subset of the Tuckman (1991) and Lay (1986) scales, with the subset chosen *ex ante* by our field team as being most appropriate for our setting. The questions, listed in Panel A of Table A.12, ask respondents to respond on a Likert scale of agreement with statements such as "I'm continually saying 'I'll do it tomorrow'."

These scales have been validated as being predictive of real behaviors such as poor academic performance (Kim and Seo, 2015). Indeed, this index (hereafter: the impatience index) also predicts behavior in our sample: those with higher values of the index walk less and have worse diets at baseline (Table A.12). We construct the index by standardizing all question responses and taking the average, as we specified when we included the questions in the survey.

In Online Appendix I, we perform additional validation of our impatience index by showing that it predicts an incentivized measure of impatience over effort and that it does not predict an incentivized measure of impatience over payments. To do so, we gathered data from a separate sample of similar participants and elicited their incentivized choices regarding the number of effort tasks they wanted to complete on different days (e.g., the same day, a week later) for different piece rates, following the methodology of Augenblick (2018). (We did this after the original experiment was complete; we were unaware of the Augenblick (2018) methodology when we conducted our experiment in 2016.) Reassuringly, we show that those with higher values of our impatience index also make more effort-impatient choices in the Augenblick (2018) exercise, signing up for relatively more tasks in the future than the present.³⁶ In contrast, we show that those with higher values of our impatience index do not make more impatient choices in the payment (mobile recharge) domain, as shown using incentivized choices on a multiple price list (Andreoni and Sprenger, 2012). Interestingly, there is also no correlation between the incentivized measures of impatience over effort and over recharges. The discount rates over the two domains may thus be relatively independent here. Indeed, in the data collected for our main study, we also find that our impatience index does not correlate with any proxies for impatience over recharges, such as recharge balances and recharge usage (Table A.13).

We began collecting our impatience index partway through the data collection,³⁷ so it is only available for the latter half of the sample. Luckily, that sample size is sufficient to achieve statistically significant results. That said, to check the robustness of our results in the full sample, we create a "predicted index" using a LASSO prediction based on three survey questions on self-control in specific domains (e.g., exercise, diet) that were similar in nature to the impatience index questions and were collected from all participants. Panel B of Table A.12 lists the questions used for prediction and shows that the predicted index correlates in the expected direction with behavior measures such as the health risk index.

³⁶Specifically, Appendix Figure I.2(a) shows that those with above-median values of our impatience index have over twice as large a gap between the tasks chosen for the future relative to the present than those with below-median values of our impatience index. We also perform a structural estimation, following Augenblick (2018), DellaVigna and Pope (2018), and John and Orkin (2021), which also suggests that our impatience index meaningfully predicts the discount factor over effort. We estimate the average discount factor over effort costs incurred 1, 7, and 8 days in the future. Across the full sample, we estimate a discount factor of 0.9, which is both economically and statistically different from 1. However, only those with above-median values of the impatience index appear to have a discount factor less than 1. The estimated discount factor among those with below-median values of the impatience index is economically and statistically indistinguishable from 1.

³⁷We initially planned to use the convex time budget (CTB) methodology of Andreoni and Sprenger (2012) to measure impatience over effort costs, as well as over mobile recharges, as described in Online Appendix J. However, these measures are difficult to implement in the field. Challenges surfaced early in our data collection which made the measures unusable for analysis, at which point we added our impatience index measure.

The key challenge for us in implementing CTB was that it was hard to get respondents to understand the paradigm, and likely as a result, we have an order of magnitude more law-of-demand violations than lab-based studies with college students. Other evidence suggesting a lack of understanding include our estimates not converging for 44% of the sample and respondents failing to follow through on their chosen allocations, as described in Online Appendix J. Further, the impatience measures estimated using this methodology do not correlate in the expected direction with any behaviors.

6.2.2 Heterogeneity by Baseline Impatience

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

$$y_{it} = \alpha + \beta_1 Impatience_i \times Thresh_i + \beta_2 Thresh_i + \beta_3 Impatience_i + \mathbf{X}'_i \pi + \mathbf{X}'_{it} \theta + \varepsilon_{it}, \quad (14)$$

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 the 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 the threshold (relative to the base case) varies with impatience. Our prediction is that $\beta_1 > 0$.

Table 7 shows that, consistent with our prediction, 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 the threshold group relative to the linear group by 4 pp (statistically significant at the 5% level). Column 2 uses a dummy for having an above-median value of the impatience measure. We include this estimate because it is easier to interpret, although it has lower statistical power since it does not leverage all the underlying variation in the data. Relative to the base case, the threshold generates 6 pp higher compliance for those with above-median impatience than those below the median, a large increase relative to the sample-average effect of either contract (20 pp). The difference is significant at the 10% level. 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 slightly more precise) results.

Figure 8 presents a visualization of column 4; it shows that, relative to the linear contract, the threshold contract increases compliance among the more impatient while decreasing it among the less impatient. The difference between the effects is the 6 pp effect. We previously showed that, theoretically, the discount rate over effort could be pivotal to whether the linear or threshold contract has higher compliance (e.g., Proposition 3 in Appendix B.2). The fact that that is the case here has important implications for policy: efforts by policymakers to personalize threshold assignment based on agent impatience could substantially increase compliance.

Impatience over effort is correlated with other factors, such as baseline exercise levels, that

Dependent variable:	Exceeded step target $(\times 100)$						
Impatience measure:	Impatience index	Above median impatience index	Predicted impatience index	Above median predicted index			
Sample:	Late	Late	Full	Full			
	(1)	(2)	(3)	(4)			
Impatience \times Threshold	3.67^{**} [0.15, 7.19]	5.8^* [-0.62,12.23]	3.1^{***} [0.87, 5.18]	6.39^{*} [-0.40, 10.41]			
Threshold	-1.51 [-4.85, 1.82]	-3.94 [-8.97,1.09]	-1.1 [-3.33, 0.88]	-3.47** [-5.93, -0.15]			
Impatience	-3.62*** [-6.06, -1.19]	-6.14** [-11.09,-1.19]	-2.05** [-3.54, -0.46]	-4.74^{*} [-7.77, 0.58]			
# Individuals	1,075	1,075	1,969	1,969			
# Observations	86,215	86,215	$157,\!946$	$157,\!946$			
Base case mean	50.4	50.4	50.2	50.2			

Table 7: Time-Bundled Thresholds Increase Compliance More for the Impatient

Notes: This table shows heterogeneity by impatience in the effect of threshold contracts relative to linear contracts. The impatience measure changes across columns; its units in columns 1 and 3 are standard deviations. The sample includes the base case and threshold incentive groups only. The "Late" sample includes only participants who were enrolled after we started measuring the impatience index; the Full sample includes everyone. The Threshold group pools the 4- and 5-day threshold groups. See Online Appendix Table G.11, Panel B for results with the Threshold group disaggregated (unpooled). (Panel A of that table shows results using daily steps as the outcome.) Bootstrap draws were clustered at the individual level, and bootstrapped 95% confidence intervals are in brackets. For the regressions that use the predicted impatience index, to construct the 95% confidence interval, we conduct three steps in each bootstrap sample: 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 (14). The Gaussian standard errors and *p*-values for the column 1 *Impatience × Threshold* coefficient are 1.9 and 0.053, respectively; for column 2, the corresponding values are 3.81 and 0.128. Controls are the same as selected by lasso in Table 2. Significance levels: * 10%, ** 5%, *** 1%.



Figure 8: Impatience is Pivotal to Compliance Under the Time-Bundled Threshold

may also independently influence the performance of thresholds. For example, if impatient people are more likely to also have counterfactual walking that is right below the threshold level (as opposed to above or far below), that could independently cause them to respond more to the threshold. To shed light on whether this type of factor plays a role in the heterogeneity we see, Figure 9 examines the robustness of the Table 7 estimates to controlling for other baseline covariates and their interactions with the 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), which could both influence the performance of threshold contracts, among other controls.

Reassuringly, Figure 9 shows that the coefficients on the interaction of impatience and the threshold remain stable as we add these additional controls. Panel A shows stability of the coefficient from column (1) using the actual impatience index as the measure of impatience, and Panel B shows stability of the coefficient from column (3) using the predicted impatience index. The stability of both coefficients suggests that it is likely impatience itself (and not its correlates) driving the estimated relationships.

Another potential confound that was difficult to measure at baseline (and hence which we

Notes: The chart plots the effect of the threshold contract relative to the base case, estimated separately for those with below-median predicted impatience (left bar) versus above-median predicted impatience (right bar). The height of the vertical arrow shows the difference between the treatment effects, with the 95% confidence interval in brackets. All estimates come from Table 7 column 4.



Figure 9: 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 7 to including various additional controls, interacted with *Threshold*, in the regression. As a reference, the first "No additional controls" row just displays the $Threshold \times Impatience$ coefficient, and 95% confidence interval, from column (1) of Table 7. The next 11 rows show estimates of the *Threshold* \times *Impatience* coefficient from the same regression model, each estimated with two additional controls: a control for the main effect of the covariate listed in the row title, and a control for that same 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 11 rows (both main effects and interactions with Threshold). Panel B is analogous but based on column (3) of Table 7. Thus, Panel A shows robustness of the Threshold \times Impatience coefficient when the actual impatience index is the measure of Impatience whereas Panel B shows robustness when the predicted impatience index is the measure of *Impatience*. Baseline steps (mean) and baseline steps (sd) represent the mean and standard deviation, resepctively, 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. Risk aversion is an incentivized measure from a multiple price list. Scheduling uncertainty represents the individual's stated frequency of facing unexpected events (such as business duties) that would prevent them from walking for 30 minutes in a given day. Income is winsorized at the 5th and 95th percentiles. The unit of observation is a respondent \times day. All confidence intervals constructed via bootstrap, with bootstrap draws done at the individual level, as in Table 7.

do not control for) is the individual-level propensity for habit formation. However, we can measure the propensity for forming habits at endline by assessing how much of the treatment effect of incentives persists after payments stop. Table G.12 in the Online Appendix reassuringly suggests that the propensity to form habits is not correlated with impatience in our setting, as impatience does not predict the persistence of incentive effects after payments stop.

Prediction 1 suggested that, in addition to increasing compliance more among the impatient, threshold contracts should also increase *effectiveness* more among the impatient. Since we have already demonstrated the compliance result, demonstrating the effectiveness result requires us to show that, relative to the base case, thresholds do not have lower cost-effectiveness among the impatient than the patient. Table A.11 shows that this is true.

6.2.3 Time-bundled Thresholds Result Summary

Consistent with our theoretical predictions, time-bundled thresholds generate meaningfully greater compliance and effectiveness among the impatient than the patient. In the full sample, they increase effectiveness by increasing cost-effectiveness without decreasing compliance. Taken together, these findings suggest that impatience in our sample could contribute to the good performance of the time-bundled threshold contract here.

These findings have important policy implications, suggesting that time-bundled thresholds are a useful tool to adjust incentives for impatience over effort. Policymakers could use time-bundled thresholds when they are incentivizing more impatient populations. They could also personalize the assignment of time-bundled thresholds within a population, for example by assigning them based on observable predictors of impatience.³⁸

6.3 Payment Frequency

We conduct two primary analyses to understand the roles of payment frequency and the discount rate over financial payments in incentive design:

- 1. Between-treatment: We compare average compliance in the daily, weekly (base case), and monthly groups. We assess how payment frequency affects compliance and use Prediction 2 to shed light on the discount rate over payment.
- 2. Within-treatment: Within the base case and monthly groups, we examine how compliance changes as the payday approaches to shed light on the discount rate over payment using Prediction 3.

The approaches are complementary. The between-treatment approach answers the policy

³⁸While such an assignment mechanism might give participants incentive to manipulate their observables, particularly if the time-bundled threshold contract is dominated by the linear contract as it is here, Dizon-Ross and Zucker (2022) shows that, in the domain of incentives for behavior change, participants do not manipulate observables to avoid assignment to dominated contracts.

question of whether payment frequency matters, while the within-treatment approach has more statistical power. The within-treatment approach also rules out potential confounds for making inferences about discount rates over payment using between-treatment effects.³⁹

Between Treatment Figure 10 and Table 6 both 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.



(a) Probability Exceeded Step Target

(b) Average Daily Steps



Notes: Panel A shows the average probability of exceeding the daily step target during the intervention for the three different frequency treatments (the base case treatment pays weekly). Panel B shows average daily steps during the intervention. Confidence interval bars represent tests for equality between each group and the base case incentive group and are from regressions with the same controls selected by lasso in Table 2.

We thus do not find evidence that increasing payment frequency in the range from daily to monthly affects compliance—a perhaps surprising finding given the conventional wisdom. The lack of between-treatment frequency effects implies that the discount rate over our financial payments is small and has a relatively flat shape over the range from one day to one month. One important caveat to these results is that the between-treatment effects are somewhat imprecise, and we have limited power to reject large discount rates. We address this issue with the within-treatment analysis.

³⁹Our design mitigates some of these potential confounds, such as feedback frequency and salience (Section 3.2.1), but a couple of confounds remain. If utility were concave in payments, then the fact that higher-frequency payments break payments into smaller chunks would improve compliance and cause us to overestimate the discount rate. If instead people preferred lumpier payments since they serve as commitment devices for savings (Casaburi and Macchiavello, 2019), we would underestimate the discount rate.



(a) Weekly Payment Cycle

(b) Monthly Payment Cycle

Figure 11: 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 (i.e., weekly) 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 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.

Within treatment The within-treatment analysis confirms the suggestive evidence of flat and low discount rates from the between-treatment analysis. Figure 11 shows how compliance within the base case weekly (Panel A) and monthly (Panel B) treatments changes as the payment date approaches. If agents are impatient over payments, compliance should increase as the payday approaches (Prediction 3), yet we find that walking behavior is remarkably steady across the payment cycle. We also estimate 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, shown in Online Appendix Table G.13, are not significantly different from zero and suggest that, if anything, compliance *decreases* as the payment date approaches.

Our estimates are also more precise here than in the between-treatment analysis, allowing us to rule out even small effects. For example, if we assume linearity of compliance in lag to payment, then the confidence interval around the slope in the weekly treatment rules out the possibility that, because of monetary discounting, daily payments would generate a mere 0.5 pp more compliance than weekly.

Although these results are consistent with recent evidence from other settings in showing limited discounting over payments (e.g., Augenblick et al. 2015; DellaVigna and Pope 2018), the absence of payday spikes conflicts with Kaur et al. (2015). The reasons why discount rates over payment vary across contexts are an open question for future work.

6.4 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 concepts 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, then variations that increase compliance and effectiveness have high potential to increase social welfare. This is likely the case here since the estimated social benefits of walking are large relative to the private costs and incentive amounts (Reiner et al., 2013).

One potential concern with our time-bundled threshold contract would be if it improved effectiveness and/or social welfare but did not cause a Pareto improvement, instead decreasing the welfare of some individuals relative to a no-incentives benchmark. This concern is particularly vivid in light of evidence that pre-commitment contracts can decrease welfare among partially naïve individuals who pay upfront for commitment but fail to follow through (e.g., Bai et al., 2020).

Even though individuals do not pay upfront for threshold contracts, there is a potentially analogous issue. Naïfs 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, as described in Section 2.3, there are theoretical reasons to doubt that this would happen much in practice.⁴⁰ Two pieces of empirical evidence also suggest that our threshold contract did not reduce participants' welfare. First, at endline, we asked participants whether they were interested in continuing the program. The vast majority said that they were, with no significant difference between the threshold group and other groups and, within the threshold group, no significant difference between the more and less impatient (Online App. Table G.14). Second, impatient people are no more likely (and in fact are less likely) than patient people to comply and *not* be paid for it under threshold contracts (Online App. Table G.15).

7 Conclusion

This paper makes two important contributions. First, we show that an incentive program for walking improves health and leads to a large and persistent increase in walking among a population suffering from chronic disease. Existing evidence-based interventions promoting

⁴⁰ 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 Section 2.3 and especially footnote 18 for more detail.

lifestyle change in similar populations are intensive and prohibitively expensive (Howells et al., 2016). Our study provides 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.

Second, we provide new insights into how to adjust incentives for impatience. We show both theoretically and empirically that, relative to linear contracts, the performance of time-bundled contracts is higher among agents 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 naïve, thus broadening the arsenal for motivating impatient or time-inconsistent agents. The intuition behind the prediction is that agents who discount their future effort more place a higher value on future work opportunities. Time-bundled contracts link better future work opportunities with effort today, thus providing particular motivation for the impatient to exert more effort today. The success of the time-bundled contract in improving performance in the face of impatience is particularly notable when contrasted with the failure in our sample of the conventional strategy for adjusting incentives for impatience: higher-frequency payment. To be effective, more frequent payment requires people to be impatient over *payment*, which even people with high primitive discount rates may not be. In contrast, the success of time-bundled contracts relies on high primitive discount rates.

Our insight that impatience increases the value of time-bundling for the principal in principalagent 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 she does not exert enough effort today, so effort today increases her future payoff to effort. While standard models show one reason such contracts enhance effort is simply the high stakes of job loss, our work suggests that these contracts have extra bite if the agent discounts her 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 linear contracts in other populations with high discount rates of effort. A final interesting topic to study is how to personalize time-bundled contracts at scale at the individual level. One option is to use targeting based on observables as done in Dizon-Ross and Zucker (2022) for a different dominated contract characteristic. 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 and C. The Online Appendix contains Appendices D - L and is available at: faculty.chicagobooth.edu/-/media/faculty/rebecca-dizon-ross/research/incentivedesignapp.pdf





Notes: This figure shows an experimental timeline for a participant. Visits were scheduled according to the participants' availability. We introduced variation into 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: Incentive Effects During and After Intervention, Conditional on Wearing the Pedometer





(d) Daily Steps Walked Post Intervention

Notes: Figure is the same as Figure 4, except that the outcomes are all conditional on wearing the pedometer.

Total scr Total e	Total screened: 57,599 Total eligible: 7,781						
Stage:	# Individuals	% of total eligible					
	(1)	(2)					
Successfully contacted	6,965	90%					
Interested in enrolling	enrolling 5,552						
Completed baseline survey	3,438	44%					
Successfully enrolled	$3,\!192$	41%					

Appendix Table A.1: Enrollment statistics

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 No data from L Fitbit Fitbit ent		Lost data entire period	Immediate withdrawal	Mid- intervention withdrawal	Other reasons			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)		
Incentives	-0.0128 [0.0175]	-0.0279^{*} [0.0143]	0.0158 [0.0124]	-0.00145 [0.00509]	0.00588 [0.00738]	$\begin{array}{c} 0.0164^{**} \\ [0.00704] \end{array}$	-0.00505 [0.00592]		
# 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		
Monitoring mean	0.19	0.15	0.05	0.00	0.01	0.01	0.02		

Notes: Each observation is an individual×day. There are two reasons why data can be missing: people did not wear their pedometers (column 2) or we do not have data from the person's pedometer (column 3). Columns 2 + 3 = Column 1 except that column 2 conditions on there not being missing data for consistency with our main step analyses whereas columns 1 and 3 do not (column 2 results similar without this restriction). Columns 4-7 summarize reasons for why steps data might have been missing, and sum up to column 3. Some people have no data during the entire intervention period (columns 4 and 5) because their pedometers broke and all intervention data was lost (4), or because they withdrew immediately after being assigned a treatment group (5). Others only have missing data for part of the intervention period, either because they withdrew midway through the period (6) or had a broken Fitbit or a failed sync (7). "Did not wear Fitbit" takes value 1 when steps = 0 for that day. Controls are the same selected by lasso in Column 1 of Table 2. Significance levels: * 10%, ** 5%, *** 1%.

Definition of missing:	No steps data	Did not wear Fitbit	No data from Fitbit	Lost data entire period	Withdrew immedi- ately	Mid-period withdrawal	Other reasons
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
	[255]	[286]	[273]	[291]	[279]	[275]	[253]
Lee upper bound	1426	1571	1572	1351	1430	1581	1358
	[333]	[307]	[280]	[300]	[304]	[279]	[227]
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
	[0.024]	[0.031]	[0.023]	[0.022]	[0.022]	[0.022]	[0.019]
Lee upper bound	0.232	0.242	0.216	0.206	0.209	0.217	0.206
	[0.025]	[0.030]	[0.023]	[0.022]	[0.022]	[0.023]	[0.019]
# 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	$211,\!551$	$206,\!320$

Appendix Table A.3: Lee bounds on the impacts of incentives on exercise

Notes: This table reports regression estimates and Lee bounds estimates (accounting for different types of missing pedometer data) of the effect of incentives 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. All estimates are of the effect of incentives pooled relative to the monitoring group. 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.

	Incentives	Monitoring	I - M	P-value: I=M
_	(1)	(2)	(3)	(4)
A. Activity (by minute)				
Average daily activity	213	197	16	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 n	ninute (share)			
Steps > 242	0	0	0	-
Steps > 150	1.3e-06	0	1.3e-06	-
# Individuals:	2,368	201		

Appendix Table A.4:	Summaries fro	m the minute-level	pedometer	data
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Notes: This table presents various statistics at the respondent \times minute level. 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 coincides 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.

Dep. Variable:	No Steps data	${\bf Reason}$ no steps data		Reason no data from Fitbit				
	Did not wear No Fitbit		No data from Fitbit	Lost data entire period	Immediate withdrawal	Mid- intervention withdrawal	Other reasons	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Incentives	0.00334 [0.0231]	0.00908 [0.0208]	-0.00478 [0.0204]	0.00193 [0.00389]	0.00596 [0.0192]	-0.00856^{*} [0.00499]	-0.00411 [0.00605]	
# Individuals	1,254	1,122	1,254	1,254	1,254	1,254	1,254	
# Observations	105,336	91,756	105,336	105,336	105,336	105,336	105,336	
No incentives mean	0.40	0.31	0.13	0.00	0.10	0.01	0.02	

Appendix Table A.5: Missing pedometer data during the post-intervention period

Notes: This table reports the reasons that we do not have step data by treatment status in the post-intervention period. Each observation is an individual \times day. Controls are the same as in Column 1 of Table 3. Column 1 reports Fitbit data missing for any reason, which can include mid-intervention withdrawal, Fitbit sync issues, and not wearing the Fitbit, amongst others. Columns 2 + 3 = Column 1, except that column 2 conditions on there being no missing data for consistency with our main step analyses whereas columns 1 and 3 do not. Columns 4-7 summarize reasons for why steps data might have been missing, and the variables in columns 4-7 sum up to the variable in column 3. Some people have no data during the entire measurement period, as summarized in columns 4 and 5. The omitted category is the pooled control and monitoring groups. "Did not wear Fitbit" takes value 1 when Fitbit data is non-missing and Fitbit steps = 0. As shown in Table G.2, there were 1,254 individuals who we enrolled in the post-intervention period and 1,122 whom we have pedometer data from. Significance levels: * 10%, ** 5%, *** 1%.

Definition of missing:	No steps data	Did not wear Fitbit	No data from Fitbit	Lost data entire period	Withdrew immedi- ately	Mid-period withdrawal	Other reasons
A. Daily steps							
Regression estimate	765	765	471	471	471	471	471
(conditional on nonmissing data)	[238]	[238]	[246]	[246]	[240]	[240]	[240]
Lee lower bound	731 [293]	$689 \\ [325]$	366 $[291]$	459 [235]	448 [299]	$304 \\ [273]$	459 [239]
Lee upper bound	840 [320]	934 [282]	503 [269]	515 [254]	554 [280]	522 [266]	515 [246]
B. Met 10k step target							
Regression estimate	0.102	0.102	0.068	0.068	0.068	0.068	0.068
(conditional on nonmissing data)	[0.020]	[0.020]	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]
Lee lower bound	$0.101 \\ [0.025]$	0.099 [0.021]	0.063 [0.017]	0.067 [0.017]	0.067 [0.022]	0.060 [0.019]	0.067 [0.017]
Lee upper bound	$0.105 \\ [0.024]$	0.110 [0.024]	0.069 [0.018]	0.070 [0.017]	0.072 [0.026]	0.070 [0.017]	0.070 [0.017]
# Individuals	$1,\!254$	1,122	1,254	$1,\!128$	$1,\!248$	$1,\!122$	$1,\!128$
# Observations	$105,\!336$	91,756	105,336	92,260	102,340	92,430	92,260

Appendix Table A.6: Lee Bounds Estimates of Exercise Effects in the Post-Intervention Period

Notes: Table reports regression estimates and Lee bounds accounting for different types of missing pedometer data in the post-intervention period. The regression estimates condition on data not being missing, using different definitions of missing data in each column, and then the Lee bounds are estimated again allowing the definition of missing data to vary by column. Panel A reports results using average daily steps as the dependent variable, and Panel B reports results using proportion of days met 10k step target as the dependent variable. The omitted category is the monitoring and control groups pooled. The number of observations is reported for the Lee bounds regressions. The regression estimates are not directly comparable to those reported in Table 3 because each column conditions on the "type of missing" indicator in the first row being equal to 0 and does not include controls. The reason the column 1 and 2 estimates are so much larger than the column 3-7 estimates is that the column 1 and 2 estimates on steps conditional on wearing the pedometer, e.g., column 3 of Table A.9. In contrast, the column 3-7 estimates treat data as missing only if it was missing by the definition of missing at the top of the column, and so instead treat days where the individual does not wear the pedometer as 0's. These estimates are thus more analogous to the treatment effect estimates on 2 of Table A.9.

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

Appendix Table A.7: HbA1c and RBS are predictive of each other

Notes: This table reports estimates from regressing standardized HbA1c at endline (in column (1)) and standardized RBS (in column (2)) at endline on standardized HbA1c and standardized RBS at baseline. Standard errors in parentheses. The sample is the control group only. Significance levels: * 10%, ** 5%, *** 1%

Appendix Table A.8: Impacts of Incentives on Walking, Without Baseline Controls

Dependent variable:	Exceeded step target	Daily steps	Daily steps $(if > 0)$
-	(1)	(2)	(3)
Incentives	0.196***	1286.2^{***}	1144.3***
	[0.0180]	[211.4]	[190.3]
Monitoring mean	0.294	6,774	7,986
# Individuals	2,559	2,559	2,557
# Observations	205,732	205,732	180,018

Notes: This table replicates the Table 2 estimates without including the baseline controls. The Threshold group pools the 4- and 5-day threshold groups. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.9: Impacts of Incentives on Post-Intervention Walking, Without Baseline Controls

		Post-intervention	
Dependent variable:	Exceeded step target	Daily Steps	Daily Steps (if > 0)
	(1)	(2)	(3)
Incentives	0.068^{***} [0.02]	470.8^{*} [246.30]	765.6^{***} $[239.11]$
No incentives mean # Individuals # Observations	$0.22 \\ 1,122 \\ 91,756$	5687.35 1,122 91,756	$7347.39 \\ 1,122 \\ 62,858$

Notes: Notes: This table replicates the Table 3 estimates without including the baseline controls. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

	Blood sugar index (1)	HbA1c (2)	Random blood sugar (3)	Health risk index (4)	Blood sugar index (5)	HbA1c (6)	Random blood sugar (7)	Health risk index (8)
Incentives	-0.044 [0.043]	-0.068 [0.11]	-5.53 [4.37]	-0.055 $[0.047]$	-0.092* [0.053]	-0.15 $[0.14]$	-11.4^{*} [6.12]	-0.13** [0.060]
Incentives \times below- median blood sugar					0.088 [0.062]	$0.14 \\ [0.16]$	11.1 [7.23]	0.15^{*} [0.084]
p-value: Incentives = Monitoring # Individuals	$0.435 \\ 3,067$	$0.952 \\ 3,066$	$0.153 \\ 3,067$	$0.102 \\ 3,068$	$0.770 \\ 3,067$	0.463 3,066	$0.294 \\ 3,067$	0.803 3,068

Appendix Table A.10: Impacts of incentives on health, without baseline controls

Notes: This table reports the results of the specifications displayed in Table 4 without controls. Standard errors are in brackets and clustered at the individual-level. We also control for the monitoring dummy and it's interaction with the below-median blood sugar dummy but suppress those coefficients for brevity. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.11: Threshold treatments increase cost-effectiveness relative to the 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	0.056	0.056		
4-day Threshold	0.055	0.055	0.056	0.055	0.055		
5-day Threshold	0.059	0.058	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 and so the units are days complied per INR. The Threshold group pools the 4-day and 5-day threshold groups.

Covariate type:	Exe	Exercise		Baseline Indices		
Dependent variable:	Daily steps	Daily exercise (min)	Negative health risk index	Negative vices index	Healthy diet index	# Individ- uals
A. Impatience Index Measures						
Impatience index	-0.080***	-0.070***	-0.017	-0.052	-0.185***	1,760
1. I'm always saying: I'll do it tomorrow	-0.059	-0.100***	-0.012	-0.031	-0.150***	1,760
2. I usually accomplish all the things I plan to do in a day	-0.054	-0.053	-0.012	-0.043*	-0.151^{***}	1,760
3. I postpone starting on things I dislike to do	-0.041*	0.006	0.004	-0.053	0.047	1,760
4. I'm on time for appointments	-0.053	0.002	-0.021	0.010	-0.097***	1,760
5. I often start things at the last minute and find it difficult to complete them on time	-0.041*	-0.066***	-0.009	-0.043*	-0.209***	1,760
B. Predicted index measures						
Predicted index	0.001	-0.038	-0.061***	0.020	0.005	$3,\!232$
1. In the past week, how many times have you found yourself exercising less than you had planned?	0.016	-0.009	-0.060***	0.010	0.027	3,232
2. In the past 24 hours, how many times have you found yourself eating foods you had planned to avoid?	-0.001	0.053***	-0.059***	0.015	0.033^{*}	3,232
3. Do you worry that if you kept a higher balance on your phone, you would spend more on talk time?	-0.027	-0.063***	-0.018	0.025	-0.038	3,232

Appendix Table A.12: Measures of impatience over effort correlate in the expected direction with baseline measures of exercise, health, and behavior

Notes: This table displays the correlations between our impatience measures and a number of baseline health and behavior measures. We normalize impatience variables so that a higher value corresponds to greater impatience, and we normalize health and behavior measures so that higher values correspond to healthier behavior; hence we expect all correlations to be negative. Panel A displays the impatience index along with the five questions from which it is generated. Panel B shows the predicted index along with the three questions from which it is generated. See Online App. Table G.16 for summary statistics on the components of each index. The health index includes an individual's measures of HbA1c, random blood sugar, blood pressure, body mass index, and waist measurement. The vices index includes an individual's daily number of wheat meals, vegetable meals, rice meals, spoonfuls of sugar, and fruit, junk food, and sweets intake, as well as whether a respondent goes out of his or her way to avoid unhealthy foods. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table A.13: No correlation between measures of impatience over effort and recharges

Covariate type:	Recharge variables				Credit constraint proxies		
Dependent variable:	Negative mobile balance	Negative yesterday's talk time	Prefers daily payment (=1)	Prefers monthly payment (=-1)	Negative wealth index	Negative monthly household income	# Individuals
Impatience Index Predicted Impatience Index	0.033 0.021	-0.075 -0.012	-0.035 -0.003	0.033 -0.005	0.044* -0.034*	0.037^{*} 0.025	1760 3232

Notes: This table displays the correlations between the predicted and actual impatience indices meant to capture impatience over effort (in the rows) and baseline measures meant to proxy for the discount rates over recharges (in columns). 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; hence the prediction is that coefficients should be positive if there is indeed a correlation. Significance levels: * 10%, ** 5%, *** 1%.

B Theoretical Predictions Appendix

B.1 Agent Problem

Given the notation and assumptions in Section 2.1, we can express the agent's problem as follows. On day t, the agent chooses compliance, w_t , to maximize expected discounted payments net of effort costs:

$$\max_{w_t \in \{0,1\}} \mathbb{E}\left[d^{(T-t)} m_T - \sum_{j=t+1}^T \delta^{(j-t)} w_{j,t} e_j \middle| e_1, .., e_t, w_1, .., w_t \right] - w_t e_t,$$
(15)

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:

$$V_t(0) < V_t(1) - e_t$$

or
 $e_t < V_t(1) - V_t(0).$ (16)

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 The Effectiveness of Threshold and Linear Contracts

In this section, we compare the effectiveness of threshold and 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 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.

Proposition 1. 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 naïve and e_2 is weakly increasing in e_1 , in a first order stochastic dominance sense,⁴¹ 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

⁴¹ This assumption flexibly accommodates the range from IID to perfect positive correlation, just ruling out negative correlation.

threshold contract with K = 2 that has at least 1 + C times higher cost-effectiveness and that generates compliance at least $\frac{1+C}{2}$ of the linear contract.

Proof. See Online Appendix H.1.

The potential improvements from threshold contracts demonstrated by Proposition 1 are quantitatively large. For example, when costs are IID and agents are naïve with sufficiently low δ , for a linear contract that generates C = .5, there exists a threshold contract that generates at least 75% as much compliance with three times higher cost-effectiveness.

Next, we show results across the full range of δ (not just for δ sufficiently low). While we make additional assumptions on the effort distribution for tractability, our next two 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.

Proposition 2. [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.

Proof. See Online Appendix H.1.

Proposition 3. [IID Uniform] Let d = 1 and T = 2. Fix all parameters other than δ . Let costs be independently drawn each day from a uniform[0,1] distribution.

(a) Take any threshold contract paying 0 < M < 1. Whether there exists a more effective linear contract depends on δ . Define $\frac{(M+1)^2}{2}$ as the "cutoff value" for naïfs and $2 - \frac{4}{(1+M)^2}$ as the "cutoff value" for sophisticates. If δ is less than the cutoff value for a given type, there does not exist a linear contract that is more effective for that type; any linear contract with at least as high cost-effectiveness will generate strictly lower compliance.⁴² In contrast, if δ is greater than the cutoff value, there always exists a linear contract that is more effective that the same cost-effective than the threshold for that type; in particular, a contract with the same cost-effectiveness and strictly higher compliance.

(b) Take any threshold contract paying $1 \leq M < 2.^{43}$ Regardless of δ , there does not exist a linear contract that is more effective.

Proof. See Online Appendix H.1.

B.3 Threshold Contracts and Impatience Over Effort

In this section, we present a series of propositions that provide the theoretical underpinning for Prediction 1 from Section 2.3. In particular, the propositions demonstrate that, holding all else equal, both compliance and effectiveness in threshold contracts tend to decrease in $\delta^{(t)}$. We begin by examining compliance in threshold contracts with T = K.

⁴²For sophisticates, when $M < \sqrt{2} - 1$, the cutoff value is negative and so δ is never below the cutoff.

⁴³Note that the principal would never pay M > 2 since M = 2 achieves 100% compliance regardless of δ .

Proposition 4 (T = K, Threshold Compliance and Impatience Over Effort). Let T > 1. Fix all parameters other than $\delta^{(t)}$. Take any threshold contract with threshold level K = T; denote the threshold payment M. Compliance in the threshold contract is weakly decreasing in $\delta^{(t)}$ for all $t \leq T - 1$.

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

Recall that the condition for complying on day 1 is to comply if $e_1 < V_1(1) - V_1(0)$ (equation (16)). With the threshold contract, we have that:

$$V_1(1) - V_1(0) = \mathbb{E}\left[(dM - \delta e_2)w_{2,1}|e_1, w_1 = 1\right] - \mathbb{E}\left[-\delta e_2 w_{2,1}|e_1, w_1 = 0\right]$$
(17)

We examine this expression separately for sophisticates and naïfs.

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:

$$V_{1}(1) - V_{1}(0) = \mathbb{E}\left[(dM - \delta e_{2})w_{2,1}|e_{1}, w_{1} = 1\right] - \mathbb{E}\left[-\delta e_{2}w_{2,1}|e_{1}, w_{1} = 0\right]$$

= $\mathbb{E}\left[(dM - \delta e_{2})\mathbb{1}\left\{e_{2} < M\right\} + \delta e_{2}\mathbb{1}\left\{e_{2} < 0\right\}|e_{1}\right]$ (18)

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 .

Thus, equation (18) is weakly decreasing in δ . That means that day 1 compliance is decreasing in δ . Hence, day 2 compliance is as well 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 naïfs. For naïfs, 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:

$$V_{1}(1) - V_{1}(0) = \mathbb{E}\left[(dM - \delta e_{2})w_{2,1}|e_{1}, w_{1} = 1\right] - \mathbb{E}\left[-\delta e_{2}w_{2,1}|e_{1}, w_{1} = 0\right]$$

$$= \mathbb{E}\left[(dM - \delta e_{2})\mathbb{1}\{\delta e_{2} < dM\} + \delta e_{2}\mathbb{1}\{\delta e_{2} < 0\}|e_{1}\right]$$

$$= \mathbb{E}\left[\max\{dM - \delta e_{2}, 0\} + \delta e_{2}\mathbb{1}\{e_{2} < 0\}|e_{1}\right]$$
(19)

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 .

Thus, equation (19) is weakly decreasing in δ . Hence day 1 compliance (and hence day 2 and total compliance) are also decreasing in δ for naïfs.

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 sence. 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 generally weakly decreasing in δ .

Proposition 5 (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.⁴⁴ 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,⁴⁵ 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 4 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 (18) and (19), we know that the value of e^* will depend on the agent's sophistication and, importantly, decrease in δ .

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

$$w_1 = \mathbb{1}\{e_1 < e^*\}$$

$$w_2 = w_1 \mathbb{1}\{e_2 < M\} + (1 - w_1)\mathbb{1}\{e_2 < 0\}$$

$$= w_1 \mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}$$

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, $w_2 = w_1w_2$. Therefore we have:

$$C/P = \frac{1}{M} \frac{\mathbb{E}[w_1 + w_2]}{\mathbb{E}[w_1 w_2]} = \frac{1}{M} \frac{\mathbb{E}[w_1 + w_1 w_2]}{\mathbb{E}[w_1 w_2]} = \frac{1}{M} \left(\frac{\mathbb{E}[w_1]}{\mathbb{E}[w_1 w_2]} + 1 \right) = \frac{1}{M} \left(\frac{\mathbb{E}[w_1]}{\mathbb{E}[w_1]\mathbb{E}[w_2 | w_1 = 1]} + 1 \right)$$
$$= \frac{1}{M} \left(\frac{1}{\mathbb{E}[w_2 | w_1 = 1]} + 1 \right)$$
(20)

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

 $^{^{44}}$ This assumption flexibly accommodates the range from IID to perfect positive correlation, just ruling out negative correlation.

⁴⁵See equation (23) 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.

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 $\lambda C - P$.)

$$2 (\lambda C - P) = \lambda \mathbb{E}[w_1 + w_2] - M \mathbb{E}[w_1 w_2]$$

= $\lambda (F(e^*) + \mathbb{E}[w_1 \mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}]) - M \mathbb{E}[w_1 \mathbb{1}\{e_2 < M\}]$
= $\lambda (F(e^*) + \mathbb{E}[\mathbb{1}\{e_1 < e^*\}\mathbb{1}\{0 < e_2 < M\} + \mathbb{1}\{e_2 < 0\}]) - M \mathbb{E}[\mathbb{1}\{e_1 < e^*\}\mathbb{1}\{e_2 < M\}]$
= $\lambda (F(e^*) + Prob(e_1 < e^*, 0 < e_2 < M) + Prob(e_2 < 0)) - MProb(e_1 < e^*, e_2 < M).$
(21)

We now take a derivative with respect to e^* . Let $g(e^*) = Prob(e_1 \le e^*, e_2 \in S)$, where S is some set. It is straightforward to show that $g'(e^*) = f(e^*) Prob(e_2 \in S | e_1 = e^*)$.⁴⁶ 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:

$$\lambda(1 + Prob(0 < e_2 < M | e_1 = e^*)) \ge MProb(e_2 < M | e_1 = e^*)$$
(22)

or

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

or

$$Prob(e_2 < 0|e_1 = e^*) \le \frac{\lambda}{M} + \left(\frac{\lambda}{M} - 1\right) Prob(0 < e_2 < M|e_1 = e^*)).$$
 (23)

If $\lambda > M$, condition (23) 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

⁴⁶To show this, note that

$$g(e^* + \epsilon) - g(e^*) = Prob(e^* < e_1 \le e^* + \epsilon, e_2 \in S) = Prob(e^* < e_1 < e^* + \epsilon)Prob(e_2 \in S|e^* < e_1 \le e^* + \epsilon) \\ = (F(e^* + \epsilon) - F(e^*))Prob(e_2 \in S|e^* < e_1 \le e^* + \epsilon).$$

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

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 (23) holds), the effectiveness of a threshold contract is decreasing in δ .

We now turn to examine threshold contracts with T < K. To gain tractability, we begin with the case where costs are perfectly correlated across periods.

Proposition 6 (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 separable contract will be constant with $\delta^{(t)}$. In contrast, compliance and effectiveness in the threshold contract will be weakly decreasing in $\delta^{(t)}$. Hence, compliance and effectiveness in the threshold relative to separable contract will be decreasing in $\delta^{(t)}$.

Proof. See Online Appendix H.2.

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.

Proposition 7. 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 $e_H \ge e_L$. Let agents observe the full sequence of costs e_1, e_2, e_3 on day 1. Let $\delta^{(t)} = \delta^t$ (i.e., let the discount factor over effort be exponential) and let $d^{(t)} = 1$. Fix all parameters other than δ . Consider a threshold contract with K = 2, where the agent must thus comply on at least 2 days in order to receive payment. Compliance and effectiveness in the threshold contract are weakly higher for someone with a discount factor $\delta < 1$ than for someone with discount factor $\delta = 1$.

Proof. See Online Appendix H.2.

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

Proposition 8. Let T = 3. Let costs be weakly positive and let agents observe the full sequence of costs e_1, e_2, e_3 on day 1. Let $\delta^{(t)} = \delta^t$ (i.e., let the discount factor over effort be exponential) and let $d^{(t)} = 1$. Fix all parameters other than δ . Consider a threshold contract with K = 2, where the agent must thus comply on at least 2 days in order to receive payment. For sophisticates, compliance and effectiveness in the threshold contract are weakly decreasing in the discount factor δ .

Proof. See Online Appendix H.2.

B.4 Proofs of Predictions 2 and 3

Prediction 2 (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.⁴⁷

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 3 (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.

⁴⁷Although 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 Model Calibration for Threshold vs. Base Case

We calibrate a model using the empirical distribution of walking costs to show that, in this setting, the predicted performance of the threshold treatment increases meaningfully with impatience over effort. We begin with the Section 2 framework. To tractably examine contracts with 7-day payment periods and with 4- and 5-day thresholds, we simplify the model by assuming that the effort discount rate is exponential with discount factor δ (i.e., that $\delta^{(t)} = \delta^t$), that $d^{(t)} = 1$, and that all future effort costs are known on day 1.

We first estimate the CDF of effort costs, as described in Online Appendix F. We then use the estimated CDF to calibrate the model and predict how relative compliance in the base case and threshold contracts would vary with δ . Figure C.1 displays the results, with δ on the x-axis and the gap between compliance in the threshold and base case linear contract on the y-axis (shown separately for the 4- and 5-day thresholds).



Appendix Figure C.1: Threshold Relatively More Effective for More Impatient

Notes: The figure shows the difference between compliance in each Threshold contract relative to the Base Case as predicted by our calibrated model. δ represents the exponential discount factor over effort.

The downward-sloping curves in the figure confirm the theoretical intuition from our model: for people who are more impatient over effort (smaller δ), there are larger compliance gains from thresholds. This is true for both naïfs and sophisticates with moderate levels of impatience.⁴⁸ In addition, the increase in performance of the threshold contract as impatience increases is quantitatively important, especially for the 5-day threshold contract, where the threshold has more bite, and where we see stronger results empirically as well (Online Appendix Table G.11, Panel B). For example, decreasing the effort discount rate from 1 to 0.9 increases relative compliance in the 5-day threshold contract by 3 pp among both sophisticates and naïfs.⁴⁹

⁴⁸As naïfs become more impatient ($\delta < 0.85$), the linear contract starts to gain relative to the 4-day threshold, as naïfs begin to procrastinate in early periods under the threshold contract. However, even very impatient naïfs still do better with the threshold than completely patient people ($\delta = 1$), which is our theoretical prediction when the threshold level is less than the number of days (Proposition 7 in App. B.3).

⁴⁹The calibration overestimates the average effect of the threshold, which in practice we found to be zero. This is likely because our model does not incorporate uncertainty regarding future effort costs. However, our interest is heterogeneity by impatience, which we do not believe will change by incorporating uncertainty.

Online Appendix for:

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

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D 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 over 40% of days were flagged for cheating and suspended from receiving recharges for 7 days. Those who were flagged for cheating more than once were terminated from the program. Fewer than 5% of the incentive group was suspended for cheating and only 1 participant was terminated (Table D.1)

During the intervention, we also attempted to flag participants who appeared to be confused about how to read their pedometers or report properly. Our pedometers record daily steps until midnight, and because respondents typically reported their daily steps via our IT system before midnight, we expected that even if people report correctly, reported steps might be slightly under pedometer steps. We tagged those whose reported steps were either more than 10% higher than their pedometer steps or more than 15% lower than their pedometer steps as "confused." Those who were flagged as simply "confused" received tutorials from the surveyors on how to use the step-reporting system.

Rates of Misreporting and Confusion Although our analysis only uses pedometer data (not reported data), so misreporting would not bias our conclusions, 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 D.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 D.2).

	Co	ount	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	

Appendix Table D.1: Summary statistics on audits and suspensions

*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 D.2: Misreporting, confusion and cheating by contract 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	0.0068 [0.01]	-0.080*** [0.02]	-0.059*** [0.02]	-0.021** [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 \times day. Column 2 shows whether a respondent over-reported by at least 10% or underreported by at least 15%. The omitted group is the monitoring group. Dates limited to each individual's contract period. Controls include baseline steps as well as all other variables included in Table 2 to maintain consistency with other step analyses. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

E Two-Day Time-Bundled Contracts

This section examines the full space of two-day time-bundled contracts. We begin by describing additional notation and modeling assumptions. Next, we formally define the option and commitment values described in Section 2.3, and characterize which time-bundled contracts can have option and commitment value. Finally, we present three formal results. First, we show that contracts must have option value to generate more effort from naïfs than from patient people. Second, we show that contracts must have commitment value to generate more effort from sophisticates than from patient people. Finally, we show that contracts can generate more effort from a sophisticated than naïve person only if day 1 compliance is analogous to taking up a commitment contract for future compliance.

Assumptions and Notation We begin from the setup presented in Section 2.1 with T = 2. For consistency with previous literature on contract design and impatience (e.g., Bai et al., 2020; O'Donoghue and Rabin, 1999a) we make the additional assumption that all effort costs are revealed on day 1. Finally, for notational simplicity we assume $d = 1.5^{50}$ Thus, an impatient person is one with $\delta < 1$, and a patient person is one with $\delta = 1$.

Three parameters define a two-day contract:

1. m_1 : the payment for day 1 compliance.

2. m_{2L} : the payment for day 2 compliance if the agent did *not* comply on day 1.

3. m_{2H} : the payment for day 2 compliance if the agent *did* comply on day 1.

For example, two-day thresholds have $m_1 = 0, m_{2L} = 0$, and $m_{2H} = M$.

As we will show, m_{2L} and m_{2H} are the two key contract parameters that interact with time preferences. Time-bundled contracts are defined by a dynamic complementarity, i.e., $m_{2H} > m_{2L}$.

Definitions of Option and Commitment Value We say a contract holds *option value* for an agent if day 1 compliance under the contract is pivotal to the agent *wanting* to comply on day 2. This occurs when the Option Value Condition is met:

Option Value Condition: $m_{2L} \le \delta e_2 < m_{2H}$. (24)

In this case, complying on day 1 creates an option for future paid compliance that the day 1 self wants the day 2 self to follow through on. That is, without day 1 compliance $(w_1 = 0)$, the agent does not want her day 2 self to comply since $\delta e_2 \ge m_{2L}$. If instead the agent were to comply on day 1 $(w_1 = 1)$, she does want her day 2 self to comply since $\delta e_2 \ge m_{2L}$.

In contrast, a contract holds *commitment value* for an agent if day 1 compliance is pivotal to the agent *actually* following through on day 2. This occurs when the Commitment Value Condition holds:

Commitment Value Condition: $m_{2L} \le e_2 < m_{2H}$. (25)

In this case, complying on day 1 commits the day 2 self to follow through: she will comply on day 2 if $w_1 = 1$ (since $e_2 < m_{2H}$) but will not comply on day 2 if $w_1 = 0$ (since $e_2 < m_{2L}$).

For patient agents, the option and commitment value conditions are equivalent. For impa-

⁵⁰The necessary assumption is that $\delta \leq d$, which implies that agents are weakly more eager for their future self to undertake effort than from their present self. We would call an agent patient if $\delta = d$ and impatient if $\delta < d$.

tient agents, however, a contract can hold option value only, commitment value only, both, or neither.

Contract Categories Columns (1) and (2) of Table E.1 divide the full space of timebundled contracts into four categories. The categories are defined by whether the contracts meet the Option and Commitment Value conditions (equations (24) and (25)) for some $\delta < 1$, taking e_2 as given. Specifically, Option + Commitment contracts satisfy equations (24) and (25) for some $\delta < 1$, Commitment-only contracts satisfy equation (25) for some $\delta < 1$ but do not satisfy equation (24) for any $\delta < 1$, Option-only contracts satisfy equation (24) for some $\delta < 1$ but do not satisfy equation (25) for any $\delta < 1$, and Inframarginal contracts never satisfy either condition.

	Contract definitions		Compliance weakly decreasing in δ among: ^{<i>a</i>}		
	m_{2L}	m_{2H}	Naïfs	Sophisticates	
Contract	(1)	(2)	(3)	(4)	
Option + commitment	$< e_2$	$> e_2$	Y	Y	
Commitment-only	e_2	$> e_2$	Ν	Y	
Option-only	$< e_2$	$\leq e_2$	Y	Ν	
Inframarginal	$> e_2$	$> e_2$	Ν	Ν	

Appendix Table E.1: Two-day time-bundled contracts

^a If column says "N" for "No, compliance is not weakly decreasing in δ ," it means that compliance is invariant to δ (as opposed to increasing in δ).

Results We now show three results. The first result shows that contracts with option value can induce extra effort from naïfs, and the second result shows that contracts with commitment value can induce extra effort from sophisticates (as summarized in Columns (3) and (4) of Table E.1.) Many *threshold* contracts are Option+commitment contracts, which explains why thresholds can work for both naïfs and sophisticates.⁵¹ Our third result clarifies the relationship between our definitions of commitment value, option value and standard commitment contracts: when a contract has commitment value but not option value, then day 1 compliance is analogous to taking up a commitment contract for future compliance. As such, these contracts only work for sophisticates, not naïfs.

Proposition 9. Fix all parameters other than δ . For naïfs, compliance is weakly decreasing in δ for Option-only and Option + commitment contracts, but invariant to δ under Commitment-only and Inframarginal contracts.

Proof. See Online Appendix H.3.

⁵¹Since thresholds have $m_1 = m_{2H} = 0$ and $m_{2H} = M$, they are Option+commitment contracts when $M > e_2$.

Proposition 10. Fix all parameters other than δ . For sophisticates, compliance is weakly decreasing in δ for Commitment-only and Option + commitment contracts, but invariant to δ under Option-only and Inframarginal contracts.

Proof. See Online Appendix H.3.

Corollary 1. Fix all parameters other than δ . If a sophisticate with a given $\delta < 1$ complies on day 1 of a time-bundled contract when neither a naïf with the same δ nor a patient person with $\delta = 1$ would comply, then two things must be true:

(1) The Commitment Value condition must hold (equation (25)) but the Option Value condition must not (equation (24)), which implies the following holds:

Commitment Value Without Option Value Condition: $\delta e_2 < m_{2L} \leq e_2 < m_{2H}$. (26) (2) The sophisticate must be complying on day 1 even though $e_1 \geq m_1 + m_{2H} - m_{2L}$. That is, the sophisticate complies on day 1 even though the cost of day 1 effort exceeds the maximum potential financial benefit from day 1 compliance.

Proof. See Online Appendix H.3.

Intuition and Discussion We can see the intuition behind the results by considering the condition for complying on day 1. The agent complies on day 1 if:⁵²

$$e_{1} < m_{1} + \begin{cases} m_{2H} - m_{2L} & \text{if the individual believes on day 1 that she will comply on} \\ & \text{day 2 regardless of her day 1 behavior} \\ m_{2H} - \delta e_{2} & \text{if the individual believes on day 1 that she will comply on} \\ & \text{day 2 if and only if she complies on day 1 (the "pivotal"} (27) \\ & \text{case}) \\ 0 & \text{if the individual believes on day 1 that she will not comply} \\ & \text{on day 2 regardless of her day 1 behavior} \end{cases}$$

Equation (27) shows that, in order for compliance to depend on δ —and, in particular, in order for compliance to be weakly decreasing in δ —we must be in the pivotal case where the impatient individual believes on day 1 that she will comply on day 2 if and only if she complies on day 1.⁵³

Propositions 9 and 10 rest on the fact that, because sophisticates and naïfs have different beliefs about their future compliance, different circumstances put them in the pivotal case. Naïfs believe that their future self will follow through on their current self's preferences, and are thus in the pivotal case when their day 1 compliance is pivotal to *wanting* their day-2 self to comply (when the Option Value Condition holds). In contrast, because sophisticates understand their future preferences, they are in the pivotal case when their day 1 compliance is pivotal to their day-2 self *actually* complying (when the Commitment Value Condition holds).

⁵²This comes from equation (16) in Section 2.1 which says that the agent complies on day 1 if $e_1 < V_1(1) - V_1(0)$.

⁵³This follows not just for day 1 compliance but for overall compliance because, on day 2, the agent will comply if $e_2 < m_2$, which is affected by δ only through δ 's effect on m_2 via day 1 compliance.
Two-day threshold contracts cannot satisfy the Commitment Value Condition without also satisfying the Option Value Condition (i.e., equation (26) cannot hold for thresholds). Equation (26) requires $m_{2L} > 0$, whereas threshold contracts have $m_{2L} = 0$ by definition. Thus, by Corollary 1, no two-day threshold contract will yield higher compliance from a sophisticate than from a naïf with the same δ .

When equation (26) does hold, the naïf is mistaken about her day 2 compliance: she thinks she will comply on day 2 regardless of her day 1 action, but in reality will only comply on day 2 if she complies on day 1. Because of her overoptimistic beliefs, unlike the sophisticate, she is unwilling to pay a cost for commitment (Corollary 1). One way to see that sophisticates are effectively paying a cost for commitment when equation (26) holds is to note that sophisticates are willing to pay more than dollar for dollar to increase the day 2 payment from m_{2L} to m_{2H} .⁵⁴

An interesting question is how Option + commitment contracts are able to induce more effort from impatient naïfs than from patient people while Commitment-only contracts are not. Relative to Commitment-only contracts, Option + commitment contracts "increase the stakes" by decreasing m_{2L} , which helps the naïf realize that she would only comply on day 2 if she complies on day 1. By worsening the consequences of noncompliance on day 1, the Option + commitment contracts help guide naïfs to take the action in their own best interest.

F Calibrating the CDF of Walking Costs

Here, we describe how we estimate the cumulative distribution function (CDF) of walking costs using the step data for our calibration exercise in Section 6.2.

To estimate values of the CDF, we take advantage of the fact that payments are not discounted on payday, and so the probability of walking on paydays is equal to the probability that walking costs are less than the payment amount—or the CDF of the payment amount. Therefore, average payday walking in the small payment treatment uncovers F(10), and average payday walking in the base case and daily groups uncover F(20). Average walking in the monitoring treatment uncovers F(0). We also use two additional moments: the probability of walking for the 4-day (5-day) threshold group when one had already walked three days (four days), and it is the last day of the contract period uncovers F(80) (F(100)). With these five data points in hand, we fit a linear (i.e., uniform) walking cost CDF using a linear regression of F(x) on (x).

Because the final two CDF values are estimated on a selected sample, they are only valid for estimating the population-level cost distribution if costs are IID across people. Therefore, while the calibration exercise is useful for exposition, our model may not perfectly fit observed behavior.

⁵⁴That is, $V_{m_{2L}}^{m_{2H}}|^{Soph\&(\delta e_2 < m_{2L} \le e_2 < m_{2H})} = m_{2H} - \delta e_2 > m_{2H} - m_{2L}$.

G Additional Tables and Figures



(a) Incentive Effects After Intervention: Incentive Relative to Control Only

(b) Daily Steps Walked Post Intervention

Appendix Figure G.1: Incentive Effects After Intervention: Incentive Relative to Control Only

Notes: Figure is the same as Panels C and D of Figure 4, except that instead of comparing the incentive group with the monitoring and control groups pooled, we compare the incentive group with the control group alone and exclude the monitoring group.

Contract Type	Questions	% 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 $(payment \ day \ of \ week - 1)$?	0.89	0.99	1	
	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)?	0.89	1	1	
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	
n = 101	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 1 day this week?	0.92	0.98	1	
	How many recharges would you receive at the end of this week if you walked 10,000 steps on exactly 4 days this week?	0.97	0.99	1	
	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	1	
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?	0.79	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?	0.87	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?	0.86	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 ?	0.97	0.98	1	
	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	
$\begin{array}{l}\text{Monthly}\\n = 164\end{array}$	How many recharges would you receive on (<i>payment day of week</i>) if you walked 10,000 steps on exactly 1 day over this week ?	0.95	0.99	1	
101	How many recharges would you receive on (<i>payment day of week</i>) if you walked 10,000 steps on exactly 5 days in this week ?	0.94	1	1	
Monitoring $n = 205$	How do you report your steps to us?	0.89	0.99	1	
n = 200	How large is the Fitbit wearing bonus?	-	0.78	0.99	

Appendix Table G.1: Participants understood their assigned contracts

Notes: 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 and have been left blank in this table. For each question, column 1 shows the percent of the sample who got the answers right on their first call and column 2 shows the percent who got the answer right on any call. 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". This was the day they launched in the program (e.g., if a base case participant started in the program on a Wednesday, they would be paid every Wednesday for their exercise between the previous Wednesday to Tuesday). Although monthly group participants were paid at the end of the month, their payment day of the week would still correspond with this day.

Appendix Table G.2: Baseline Summary Statistics in Post-intervention Sample, by Treatment Group

	Full sample	No incentives	Incentives
	(1)	(2)	(3)
A. Demographics			
Age (from BL)	49.31	49.90	49.11
0. ()	(8.62)	(8.34)	(8.71)
Female $(=1)$	0.48	0.52	0.47
	(0.50)	(0.50)	(0.50)
Labor force participation $(=1)$	0.69	0.68	0.70
	(0.46)	(0.47)	(0.46)
Per capita income (INR/month)	4379	4434	4360
	(3008)	(2673)	(3116)
Household size	3.88	3.85	3.88
	(1.59)	(1.56)	(1.60)
B. Health			
Diagnosed diabetic $(=1)$	0.62	0.62	0.62
	(0.49)	(0.49)	(0.49)
Blood sugar index	1.00	1.00	1.00
	(0.02)	(0.04)	(0.02)
Hba1c (mmol/mol)	8.63	8.55	8.66
	(2.55)	(2.59)	(2.54)
Random blood sugar $(mmol/L)$	186.74	185.11	187.31
	(88.27)	(89.18)	(88.00)
Systolic BP (mmHg)	133.40	133.94	133.21
	(19.44)	(20.05)	(19.23)
Diastolic BP (mmHg)	88.55	88.80	88.46
	(11.22)	(11.29)	(11.20)
HbA1c: Diabetic $(=1)$	0.77	0.75	0.77
	(0.42)	(0.44)	(0.42)
BP: Hypertensive $(=1)$	0.49	0.48	0.49
	(0.50)	(0.50)	(0.50)
Overweight $(=1)$	0.63	0.67	0.62
DIG	(0.48)	(0.47)	(0.49)
BMI	(4.20)	26.88	26.50
	(4.29)	(4.28)	(4.29)
C. Walking - Phase-in			
Exceeded step target $(=1)$	(0.23)	0.23	0.24
A 1.11	(0.31)	(0.30)	(0.31)
Average daily steps	6720 (2025)	6678	6734
	(3925)	(3048)	(4017)
D. Impatience over effort			
Impatience index (SD's)	0.02	-0.02	0.03
	(0.97)	(0.98)	(0.97)
Predicted index (SD's)	-0.07	0.01	-0.09
	(1.01)	(1.02)	(1.00)
E. Mobile Recharges			
Current mobile balance (INR)	27.38	26.86	27.57
	(43.47)	(37.20)	(45.44)
Yesterday's talk time (INR)	5.87	5.97	5.84
	(8.25)	(8.72)	(8.09)
Prefers daily payment $(=1)$	(0.17)	0.16	(0.18)
	(0.38)	(0.36)	(0.38)
Prefers monthly payment $(=1)$	(0.23)	(0.25)	(0.23)
	(0.42)	(0.43)	(0.42)
F-tests for Joint Orthogonality			
P-value (relative to no incentives)	N/A	N/A	0.60
Sample size			
Number of individuals	1,254	321	933
Number of ind, with ped, data	1.122	289	833

Notes: This table shows baseline summary statistics and tests for balance within the post-intervention sample only. "No incentives" represents the Control and Monitoring groups pooled; we pool them in the analysis for power. We do not show the incentive subgroups separately as we pool them in all analyses of the post-intervention data. The number of individuals with pedometer data ("Number of ind. with ped. data") differs from the total number of individuals because a few participants withdrew immediately. The likelihood of immediate withdrawal is not significantly different between the incentive and comparison groups (Table A.5 column 5).

Dependent variable:	Blood sugar index (1)	HbA1c (2)	Random blood sugar (3)	Health risk index (4)
Incentives	-0.10**	-0.2	-12.1**	-0.08^{*}
	[0.05]	[0.1]	[5.9]	[0.04]
Incentives \times below-median HbA1c	$0.08 \\ [0.05]$	0.1 [0.1]	9.1 $[7.0]$	$0.05 \\ [0.05]$
Monitoring	-0.02	-0.2	6.2	0.01
	[0.08]	[0.2]	[10.7]	[0.07]
Monitoring \times below-median HbA1c	-0.02	0.2	-11.4	-0.004
	[0.09]	[0.2]	[12.6]	[0.09]
p-value: Incentives = Monitoring	0.260 0.7	0.679	0.056	0.149
Control mean		10.1	249.2	0.5
# Individuals	3,062	3,061	3,062	3,063

Appendix Table G.3: Table 4 Column 5-8 Results Robust to Analyzing Heterogeneity By Baseline HbA1c

Notes: All columns in this table display results from specifications including interactions with the below-median HbA1c indicator. The outcome variables are the same included in Table 4 (see table notes for variable definitions). Controls are the same as selected by lasso for each outcome variable in Table 4. Standard errors are in brackets and clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Sample:	Full sample effects				Heterogeneity by baseline blood sugar			
-	Blood sugar index (1)	HbA1c (2)	Random blood sugar (3)	Health risk index (4)	Blood sugar index (5)	HbA1c (6)	Random blood sugar (7)	Health risk index (8)
Average steps (000's)	-0.03 [0.02]	-0.02 [0.04]	-4.2* [2.3]	-0.04** [0.02]	-0.06** [0.03]	-0.03 [0.07]	-8.5** [3.8]	-0.05** [0.03]
Average steps (000's) \times below-med. blood sugar index					0.06^{*} [0.03]	0.04 $[0.09]$	8.6^{**} [4.4]	0.04 [0.03]
Control mean # Individuals	$0.0 \\ 3.038$	8.4 3.037	193.8 3.038	$0.0 \\ 3.039$	0.6	10.1 3.037	248.3 3.038	0.5 3.039

Appendix Table G.4: An IV Specification Also Shows Treatment Effects on Health

Notes: This table reports the effect of the average number of steps per day at baseline on the same outcome variables as Table 4 (see table notes for outcome variable definitions). Results are from 2SLS specifications. In columns 1-4 the endogenous regressor is average steps per day during the intervention (divided by 1000) and instruments are treatment dummies for the Base Case, Daily, Monthly, Pooled Threshold, and Small Payment groups. The regression includes the incentive, control, and monitoring groups; since we do not have steps from the control group, we replace their steps with the monitoring mean. In columns 5-8 the endogenous regressors are average steps per day during the intervention (divided by 1000) and its interaction with the below-median blood sugar index indicator. The instruments are the aforementioned treatment dummies and their interactions with the below-median blood sugar index indicator. Additionally, in columns 5-8 we include below-median blood sugar index indicator as a control. The blood sugar index was constructed by taking the mean of endline HbA1c and random blood sugar standardized by their average and standard deviation in the control group. Controls are the same as selected by lasso for each outcome variable in Table 4. Standard errors are in brackets and clustered at the individual-level. Significance levels: * 10%, ** 5%, *** 1%.

Sample:	Ful	l sample effe	ects	Heterogeneity by baseline blood sugar			
	Body mass index (1)	Mean arterial BP (2)	Waist circumfer- ence (3)	Body mass index (4)	Mean arterial BP (5)	Waist circumfer- ence (6)	
Incentives	-0.0490 [0.0398]	0.00358 [0.426]	-0.183 [0.288]	0.0212 [0.0579]	-0.166 $[0.608]$	-0.286 [0.409]	
Incentives \times below-med. blood sugar index				-0.139* [0.0799]	$0.336 \\ [0.855]$	0.201 [0.576]	
Monitoring	0.0611 [0.0833]	1.051 [0.738]	-0.00181 [0.450]	-0.00634 $[0.0831]$	-0.385 $[1.062]$	$0.285 \\ [0.605]$	
Monitoring \times below-med. blood sugar index				0.156 [0.173]	3.026^{**} [1.470]	-0.683 $[0.905]$	
Control mean # Individuals	$26.5 \\ 3,058$	$103.0 \\ 3,056$	$94.4 \\ 3,059$	26.1 3,058	104.0 3,056	94.6 3,059	

Notes: This table shows the effect of incentives on endline outcomes comprising the health risk index not included in Table 4. Columns 4-6 display results from specifications including interactions with the below-median blood sugar index indicator. The blood sugar index is constructed by taking the mean of endline HbA1c and random blood sugar standardized by their average and standard deviation in the control group. The health risk index created by taking the average of endline HbA1c, random blood sugar, mean arterial blood pressure, body mass index, and waist circumference standardized by their average and standard deviation in the control group. Controls are selected by lasso using the same method and set of variables described in the Table 4 notes. Standard errors are in brackets and clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

	Random blood sugar					
	(1)	(2)	(3)	(4)		
Incentives \times Week	-0.59*	-0.48	-1.25**	-1.04*		
	[0.31]	[0.35]	[0.53]	[0.62]		
Sample	Full	Full	Above-median blood sugar	Above-median blood sugar		
Monitoring included? [†]	Yes	No	Yes	No		
<i>p</i> -value: Incentives $\times Week = 0$	0.06	0.18	0.02	0.10		
# Individuals	$3,\!135$	2,900	1,565	$1,\!442$		
# Observations	14.157	13.120	7.051	6,506		

Appendix Table G.6: Blood Sugar Treatment Effects Grow Over Time

Notes: † Columns where the monitoring group is not included report results from specifications where the sample is restricted to the control and pooled incentives groups only. Observations are at the individual \times survey level. We regress random blood sugar on the interaction between survey week (the actual week in which the survey was conducted) and the incentives dummy. We control for individual fixed effects, survey week fixed effects and cluster standard errors at the individual level. Column 1 includes the full sample, column 2 includes individuals in either the incentives or control groups. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table G.7: Impacts of incentive program and monitoring on diet and addictive consumption.

	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	0.040 [0.044]	0.022 [0.029]	0.053^{*} [0.030]	0.032 [0.035]	0.025 [0.033]	-0.024 $[0.066]$	-0.025 $[0.047]$	-0.029 $[0.038]$	0.00084 $[0.018]$	
Monitoring	0.012 [0.085]	0.013 [0.053]	$0.063 \\ [0.053]$	$0.064 \\ [0.061]$	-0.012 [0.060]	$0.12 \\ [0.10]$	-0.033 $[0.081]$	-0.053 $[0.085]$	-0.044 $[0.033]$	
Control mean	0.00	0.49	0.58	0.53	-2.34	-0.91	-1.12	-0.35	0.83	
P-value: $M = I$	0.72	0.84	0.85	0.57	0.50	0.09	0.91	0.77	0.13	
# Individuals	3,068	3,068	3,068	3,068	3,068	3,068	3,068	3,068	3,068	
B. Addictive con	nsumption	Addicti consumpt	ve good ion index	Average c	laily areca	Averag	e daily bhol	Averag cigar	e daily ettes	
		(1)		('	(2)		(3)		(4)	

	(1)	(2)	(3)	(4)
Incentives	-0.021	0.031	-0.036	-0.059
Monitoring	[0.037] -0.012 [0.060]	[0.036] 0.0087 [0.068]	[0.028] -0.017 [0.027]	[0.094] -0.026
Control mean	0.00	0.13	0.11	[0.14]
P-value: $M = I$	0.87	0.72	0.47	0.79
# Individuals	3,068	3,068	3,068	3,068

Notes: The Healthy Diet Index is an index created by the average values of eight diet questions, standardized by their average and standard deviation in the control group. The Addictive Good Consumption Index is an index created by the average self-reported average daily consumption of areca, alcoholic drinks, and cigarettes, standardized by their average and standard deviation in the control group. A larger value indicates more consumption. A larger value indicates a healthier diet. The omitted category in all columns is the pure control group. For the two indices, controls were selected (using the same set of potential controls used in Table 2), separately for each specification using the post double selection lasso method of Belloni et al. (2017), along with second order polynomials of all questions underlying the indices at baseline. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

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	0.093^{**} [0.045]	0.090^{**} [0.045]	0.028 [0.045]	0.052 [0.048]	0.066 $[0.047]$	0.013 [0.043]	0.088^{**} [0.042]	0.056^{*} [0.032]	
Monitoring	0.17^{**} [0.072]	0.074 [0.075]	0.15^{*} [0.077]	0.11 [0.082]	0.027 [0.081]	0.13^{*} [0.075]	0.17^{***} [0.066]	0.051 [0.053]	
Control mean	0.00	3.06	3.48	3.35	3.30	3.86	4.40	4.71	
P-value: $M = I$	0.25	0.81	0.09	0.44	0.59	0.07	0.12	0.91	
# Individuals	3,068	3,068	3,068	3,068	3,068	3,068	3,068	3,068	
B. Fitness	Fitness tim	e trial index		Seconds to v	valk 4m	Secor	ids for 5 si	t-stands	
	(1)		(2)		(3)			
Incentives	0.011 $[0.044]$			0.032 $[0.041]$	2 L]	-0.099 $[0.12]$			
Monitoring	0.068 $[0.073]$			0.079 [0.071]			-0.077 $[0.19]$		
Control mean	0.	00		3.88			13.18		
P-value: $M = I$	0.	37		0.45	0.89				
# Individuals	2,8	890		2,825			2,793		

Appendix Table G.8: Impact of incentives on fitness and mental health

Notes: The Mental health index averages the values of seven questions adapted from RAND's 36-Item Short Form Survey (SF-36). A large value of the Fitness time trial index indicates low fitness. The omitted category is the pure control group. Controls are selected by lasso using the same method and with the same set of variables described in the Table 4 notes. Robust standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Dependent variable:	Exceeded step target	Daily steps	Daily steps $(if > 0)$
	(1)	(2)	(3)
Base case	0.207^{***} [0.0196]	$1411.4^{***} \\ [225.0]$	1197.1*** [201.8]
Daily	0.199^{***} [0.0302]	$\frac{1126.7^{***}}{[332.2]}$	1245.1^{***} [279.2]
Monthly	0.179^{***} [0.0281]	1302.6^{***} [311.0]	1152.5^{***} [272.3]
Threshold	0.194^{***} [0.0194]	1238.1^{***} [223.2]	$\frac{1125.5^{***}}{[200.3]}$
Small payment	0.128^{***} [0.0382]	740.8^{*} [381.0]	510.5 [331.3]
P-value for base case vs			
Daily	0.79	0.31	0.83
Monthly	0.27	0.67	0.84
Threshold	0.35	0.21	0.55
Small payment	0.03	0.05	0.02
Monitoring mean	0.294	6,774	7,986
# Individuals	2,559	2,559	2,557
# Observations	205,732	205,732	180,018

Appendix Table G.9: Table 6 results robust to excluding controls

Notes: This table replicates the Table 6 estimates without including the baseline controls. The Threshold group pools the 4- and 5-day threshold groups. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table G.10: Threshold Contracts Increase Variance Across Weel	eks
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	Variance Measure
Threshold	0.15***
	[0.03]
Base Case mean	2.52
# Individuals	2,557
# Observations	29,189

Notes: Observations are individual × week. The sample includes the Threshold and Base Case groups. We follow Iachine et al. (2010) and define the variance measure as: $z_{ijk} = |x_{ijk} - \bar{x}_{ik}|$, where z_{ijk} is the number of days walked by individual *i* in treatment group *j* in week *k*. We then regress this measure on a Threshold group dummy; the null hypothesis (which we reject) is that the Threshold coefficient is 0 and that variance is equal across treatment groups. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Impatience measure:	Impatience index	Above median impatience index	Predicted impatience index	Above median predicted index
A. Dependent Variable = Steps				
Impatience \times Threshold	300	484	222**	509**
	[-91, 691]	[-311,1279]	[5, 422]	[27, 914]
Threshold	-160	-366	-168	-357^{**}
	[-499, 180]	[-895, 163]	[-373, 32]	[-615, -66]
Impatience	-190	-333	-225***	-547**
	[-491, 110]	[-904, 238]	[-371, -70]	[-816, -117]
Base case mean	8,098	8,098	8,131	8,131
B. Dependent Variable = Exceeded	Step Target (×100)			
Impatience \times 5-day Threshold	3.76^*	6.01	3.61^{***}	7.41^{**}
	[-0.21, 7.72]	[-1.34,13.36]	[1.33, 5.81]	[0.70, 11.77]
5-day Threshold	-2.04	-4.56	-1.81	-4.56^{***}
	[-5.60, 1.53]	[-10.17,1.05]	[-4.08, 0.40]	[-7.18, -1.27]
Impatience \times 4-day Threshold	4.29	6.50	1.40	2.14
	[-1.88, 10.46]	[-4.38,17.38]	[-1.92, 4.24]	[-5.18, 8.24]
4-day Threshold	0.36	-2.41	0.50	-0.36
	[-4.19, 4.91]	[-9.13,4.32]	[-2.73, 3.60]	[-3.84, 3.67]
Impatience	-3.15**	-5.09**	-2.20***	-5.07**
	[-5.74, -0.56]	[-10.09,-0.08]	[-3.66, -0.68]	[-7.86, -0.42]
Base case mean	50.4	50.4	50.2	50.2
# Individuals	1,075	1,075	1,969	1,969
# Observations	86,215	86,215	$157,\!946$	$157,\!946$

Appendix Table G.11: Robustness of Threshold Heterogeneity Results

Notes: Panel A shows that the threshold heterogeneity reported in Table 7 is robust to using daily steps as the outcome. Panel B shows heterogeneity in the 4-day and 5-day threshold treatments by impatience with threshold groups disaggregated (unpooled). The impatience measure changes across columns; its units in columns 1 and 3 are standard deviations. The sample includes the base case and time-bundled threshold incentive groups only. Specifications in columns 1 and 2 include only participants who were enrolled after we started measuring the impatience index; columns 3 and 4 include everyone. The Threshold group pools the 4- and 5-day threshold groups. Bootstrap draws were done at the individual level, and bootstrapped 95% confidence intervals are in brackets. For Panel A: The Gaussian standard errors and *p*-values for the column 1 *Impatience* × *Threshold* coefficient are 1.9 and 0.053, respectively; for column 2 the corresponding values are 3.81 and 0.128. For Panel B: The Gaussian standard errors and *p*-values for the column 1 *Impatience* × 4 - day *Threshold* coefficient are 3.09 and 0.106, respectively; for column 2 the corresponding values are 5.66 and 0.186. Controls are the same selected by lasso in Table 2. Significance levels: * 10%, ** 5%, *** 1%.

Dependent variable:	Post-endline exceeded step target (\times 100)				
Impatience measure:	Impatier	nce index	Predicted	l index	
Sample:	La	ate	Full		
	(1)	(2)	(3)	(4)	
A. Exceeded step target (\times 100))				
Impatience \times Incentives	0.729 [-1.658,3.115]	-0.461 [-2.478, 1.557]	0.086 [-1.05, 3.01]	-0.812 [-1.80,2.46]	
Impatience	-0.832 [-3.136, 1.473]	-0.422 [-2.611,1.766]	-1.097 [-3.10, 0.65]	-0.485 [-2.68, 1.21]	
Incentives	-53.278 [-59.309,-47.246]	7.882 [3.887,11.878]	-52.689 [-56.95, -46.30]	8.107 [5.57,11.55]	
Baseline steps	-2.676 [-249.358,244.006]	-0.934 [-90.239,88.371]	0.016 [18.75,26.77]	0.576 $[35.58,43.06]$	
Intervention Steps	$\begin{array}{c} 0.402 \\ [0.362, 0.442] \end{array}$		0.400 [0.36,0.43]		
B. Average daily steps					
Impatience \times Incentives	182.048 [-248.897,612.992]	-27.847 [-426.004,370.310]	80.107 [-198.74,472.87]	-132.889 [-388.18,282.76]	
Impatience	-209.174 [-590.284,171.937]	-73.547 [-437.087,289.994]	-174.115 [-477.20,139.71]	-39.182 [-321.48,266.15]	
Incentives	-4.8e+03 [-5.6e+03, -4.0e+03]	564.046 [-118.657,1246.750]	-4.8e+03 [-5145.10,-4055.76]	630.499 [261.60,1164.48]	
Baseline steps	0.121 [-0.020,0.261]	0.416 [0.225,0.608]	0.156 [0.14,0.24]	0.377 [0.43,0.52]	
Intervention steps	0.639 [$0.580, 0.698$]		0.637 [0.59,0.66]		
Base Mean: Exceeded Step Target	23	23	23	23	
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	

Appendix Table G.12: No Significant Heterogeneity in Post-Intervention Persistence by Impatience

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 selected by lasso in Table 2. The base contract 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 treatment groups and the monitoring group. We add a missing intervention period dummy to prevent the control group from dropping out of the sample. The first 3 columns are all based on the predicted index of impatience, and units are standard deviations on the index. 95 percent confidence intervals bootstrapped at the person level are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Dependent variable:	Exceeded step target $(\times 100)$					
Payment frequency:	We	ekly		Monthly		
	(1)	(2)	(3)	(4)	(5)	
Days before payday	0.07 [0.10]		0.07 [0.05]			
Payday		-0.45 [0.58]		0.14 [1.07]		
Payweek					-0.14 $[1.07]$	
# Individuals	890	890	163	163	163	
# Observations	$71,\!672$	71,672	13,333	13,333	$13,\!333$	
Sample mean	50.2	50.2	49.3	49.3	49.3	

Appendix Table G.13: Walking does not vary significantly across the pay cycle

Notes: The columns show the effect of days until payday on the probability of meeting the step target in the weekly and monthly frequency groups. The sample in columns 1 and 2 is restricted to the base case (weekly) treatment group, and the sample in columns 3 and 4 is restricted to the monthly treatment group. Note that intervention launch visits were made seven days per week, allowing us to control for day-of-week and payday day-of-week when estimating payment cycle effects. To address the concern that launch survey dates were endogenous to participants' schedules, we randomly varied the delay between the survey date and the contract launch (and hence the payday). We then control for fixed effects of day-of-week relative to the launch survey date, thereby isolating variation in the payment cycle within a given number of days from the survey day-of-week relative to launch survey day-of-week fixed effects, a day-of-contract-period time trend, and the same controls selected by lasso in Table 2. Standard errors, in brackets, are clustered at the individual level. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table G.14: Threshold contracts do not significantly decrease satisfaction at endline

Dependent variable:	Interest in continuing program			
Threshold	-0.0220 [0.0153]	-0.00934 $[0.0198]$		
Above median predicted impatience \times Threshold		-0.0328 $[0.0306]$		
Above median predicted impatience		0.0560^{***} $[0.0215]$		
Base case (omitted) mean	0.880	0.880		
# Individuals	2556	2556		

Notes: This table shows predictors of satisfaction with the walking program. We ask respondents at endline if they are interested in continuing the program for an extra 3 months. The impatience measure is a dummy for being above-median on the predicted impatience index. Controls are the same as Table 2, as well as the main effect for impatience and treatment indicators (both main effects and interactions with impatience) for being in the daily, monthly, small payment, or monitoring treatments. The omitted group is the base case (weekly) group. The Threshold group pools the 4- and 5-day threshold groups. Standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

Dependent variable:	# Weeks complied yet unpaid		# Days compli	ed yet unpaid
	(1)	(2)	(3)	(4)
Impatience \times Threshold	-0.29 [-0.75,0.18]	-0.44 [-1.13, 0.24]	-2.02 [-5.28, 1.25]	-3.10 [-7.90,1.70]
Impatience	0.49^{***} [0.21,0.77]	0.56^{***} [0.15,0.97]	3.41^{***} [1.46,5.37]	3.92^{***} [1.05,6.79]
Threshold	-0.44** [-0.84,-0.045]	-0.77*** [-1.11,-0.42]	-3.08** [-5.85,-0.31]	-5.38*** [-7.80,-2.96]
Sample	4-day Threshold	5-day Threshold	4-day Threshold	5-day Threshold
TH mean	2.08	2.59	14.54	18.16
Base mean	2.55	3.42	17.86	23.94
# Individuals	909	646	909	646

Appendix Table G.15: Thresholds don't "fool" people into walking more

Notes: This table shows whether Thresholds increase the likelihood that respondents meet the step target at least once per week, but not enough times to be paid. Our impatience measure is the predicted impatience index. Since impatience is normalized so that higher values represent higher levels of impatience, positive coefficients on Impatience × Threshold might imply that the threshold contract is fooling partial sophisticates and naïfs into walking. Regressions include the base case group and either the 4-day threshold group (columns 1 and 3) or the 5-day threshold group (columns 2 and 4). Each observation is a person-week. The dependent variables are: a dummy for having weekly compliance greater than 0 but strictly less than 4 days (column 1) or 5 days (column 2), and the number of days complied in the week conditional on complying less than 4 days (column 3) or 5 days (column 4). For those in the threshold groups this would be the equivalent of a dummy for complying in the week but not being paid, and so we define the dependent variable analogously for the base case group even though they would in practice have always been paid in those cases. Controls are the same selected by lasso in Table 2. 95 percent confidence intervals bootstrapped at the participant level in brackets. Significance levels: * 10%, ** 5%, *** 1%.

	Full Sample	Control	Monitor- ing	Incentives pooled	s Daily	Base case	Monthly	Threshold	l Small payment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Impatience index									
1. I am continually saying: I'll do it tomorrow	2.22 (1.46)	2.07 (1.40)	2.14 (1.30)	2.26 (1.48)	2.13 (1.50)	2.28 (1.47)	2.40 (1.38)	2.23 (1.49)	2.51 (1.68)
2. I usually accomplish all the things I plan to do in a day	0.64 (0.97)	0.57 (0.89)	0.67 (0.98)	$0.66 \\ (0.98)$	0.59 (0.97)	$0.66 \\ (0.99)$	0.60 (0.87)	0.67 (0.99)	0.68 (1.03)
3. I postpone starting on things I dislike to do	3.97 (1.31)	3.76 (1.37)	4.17 (1.11)	4.00 (1.30)	4.07 (1.27)	4.04 (1.27)	3.99 (1.28)	3.98 (1.31)	3.68 (1.56)
4. I'm on time for appointments	0.47 (0.86)	0.49 (0.88)	0.37 (0.73)	0.47 (0.86)	0.48 (0.82)	0.50 (0.93)	0.52 (0.90)	0.44 (0.81)	0.51 (0.84)
5. I often start things at the last minute and find it difficult to complete them on time	2.51 (1.54)	2.53 (1.53)	2.30 (1.47)	2.52 (1.55)	2.29 (1.48)	2.51 (1.54)	2.72 (1.58)	2.49 (1.55)	3.19 (1.58)
# Individuals	1,740	316	111	1,313	86	487	93	610	37
B. Predicted index									
1. In the past week, how many times have you found yourself exercising less than you had planned?	0.53 (1.01)	0.56 (1.02)	0.45 (0.93)	0.52 (1.02)	0.51 (1.04)	0.58 (1.05)	0.55 (1.08)	0.48 (0.98)	0.47 (1.01)
2. In the past 24 hours, how many times have you found yourself eating foods you had planned to avoid?	0.21 (0.54)	0.17 (0.49)	0.25 (0.56)	0.21 (0.55)	0.21 (0.61)	0.23 (0.58)	0.25 (0.70)	0.20 (0.49)	0.15 (0.44)
3. Do you worry that if you kept a higher balance on your phone, you would spend more on talk time?	0.13 (0.34)	0.15 (0.35)	0.10 (0.31)	0.13 (0.34)	0.11 (0.32)	0.12 (0.33)	0.13 (0.34)	0.14 (0.34)	0.12 (0.33)
# Individuals	3,192	585	203	2,404	166	902	164	1,106	66

Appendix Table G.16: Summary statistics of measures of impatience over effort

Notes: Standard deviations in parentheses. Components of the impatience index range from 1 to 5, 1 being very false and 5 being very true. Responses to question 2 from the predicted index range from 0 to 3. The Threshold column pools 4- and 5-day threshold groups.

H Theoretical Predictions: Additional Proofs

H.1 Proofs of Section B.2 Propositions

We now provide the proofs for Propositions 1, 2 and 3.

Proposition 1. 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 naïve and e_2 is weakly increasing in e_1 , in a first order stochastic dominance sense,⁵⁵ 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) and footnote 12 show that compliance in a linear contract is $C = \frac{1}{T} \sum_{t=1}^{T} F(d^{(T-t)}m)$, which with $d^{(T-t)} =$ 1 simplifies to C = F(m). Recall that the cost-effectiveness of a linear contract is $\frac{1}{m}$ (see Section 2.2). (a) Naïfs: Consider a threshold contract that pays $M = m + \varepsilon$. On day 1, the naïve 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 naïve 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 $0.5(F(m) + F(m)F_{e_2|e_1 < m}(m)) = 0.5(C + CF_{e_2|e_1 < m}(m)) \ge 0.5C(1 + C)$. 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 $\frac{1}{m} \left(1 + \frac{1}{F_{e_2|e_1 < m}(m)} \right) \ge 2/m$. 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/Ctimes 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 $F(M) = F(m/p' + \varepsilon) \ge F(m/p') = p'$, with $F(M) \simeq p'$ for $\varepsilon \simeq 0$. Hence, the expected payment conditional on working in the first period is $MF(M) \ge \frac{m}{p'}p' = m$, 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 $\frac{1}{2}C(1+p') \ge \frac{1}{2}C(1+C)$ since $p' \ge C$. Payment per period is approximately $\frac{1}{2}MCp'$, with C the probability of working in the first period and p' the probability of

⁵⁵Note that this assumption flexibly accommodates the range from IID to perfect positive correlation, just ruling out negative correlation.

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 $(\frac{1}{2}C(1+p'))/(\frac{1}{2}mC) = (1+p')/m \ge (1+C)/m$.

Proposition 2 (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.

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 $F(m) \ge C$.

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 $F(m) \ge C$.

Proposition 3 (IID Uniform). Let d = 1. Fix all parameters other than δ . Let costs be independently drawn each day from a uniform [0,1] distribution.

(a) Take any threshold contract paying 0 < M < 1. Whether there exists a more effective linear contract depends on δ . Define $\frac{(M+1)^2}{2}$ as the "cutoff value" for naïfs and $2 - \frac{4}{(1+M)^2}$ as the "cutoff value" for sophisticates. If δ is less than the cutoff value for a given type, there does not exist a linear contract that is more effective for that type; any linear contract with at least as high cost-effectiveness will generate strictly lower compliance.⁵⁶ In contrast, if δ is greater than the cutoff value for a given type, there always exists a linear contract that is more effective than the threshold for that type, in particular, a contract with the same cost-effectiveness and strictly higher compliance.

(b) Take any threshold contract paying $1 \leq M < 2.57$ Regardless of δ , there does not exist a linear contract that is more effective.

Proof. To prove the proposition, we take a given threshold contract with payment M, solve for compliance C, payments P, and cost-effectiveness C/P, and then construct a linear contract with the same cost-effectiveness. If that linear contract generates higher compliance then it is more effective; if it does not then there is no linear contract that is more effective, as the linear contract that generates as much compliance would be strictly less cost-effective. Note that, in the two-period, IID, threshold case, the solution to equation (16) is that the agent will comply if:

$$e_1 < E[(M - \delta e_2)w_{2,1}|w_1 = 1].$$
(28)

Let $e_1^* = E[(M - \delta e_2)w_{2,1}|w_1 = 1]$. For na \ddot{i} fs, for whom $w_{2,1}|^{(w_1=1)} = \mathbb{1}\{M - \delta e_2 > 0\},\$

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

= $E[M - \delta e_2|\delta e_2 < M] \times Prob(\delta e_2 < M)$
= $(M - \delta E[e_2|e_2 < M/\delta])F(M/\delta)$

⁵⁶For sophisticates, for $M < \sqrt{2} - 1$, the cutoff value is negative and so δ will never be below the cutoff.

⁵⁷Note that the principal would never pay M > 2 since M = 2 achieves 100% compliance regardless of δ .

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

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

= $E[M - \delta e_2|e_2 < M] \times Prob(e_2 < M)$
= $(M - \delta E[e_2|e_2 < M])F(M)$

Given our definition of e_1^* , compliance and payments are :

$$C = .5[F(e_1^*) + F(e_1^*)F(M)]$$

$$P = .5MF(e_1^*)F(M)$$

(a) 0 < M < 1 We begin with naïfs and then move to sophisticates. For naïfs, there are two cases: Case 1: $M < \delta$ for naïfs In this case, $E[e_2|e_2 < M/\delta] = \frac{M}{2\delta}$, giving that

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

Thus,

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

Thus, cost-effectiveness is:

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

Compare this with the linear contract with

$$m = \frac{M^2}{M+1}$$

Since the cost-effectiveness of a linear contract is $\frac{1}{m}$, this linear contract has the same cost-effectiveness as a threshold with payment M. Compliance under the linear contract is $F(m) = \frac{M^2}{M+1}$. Thus, the threshold is more effective if

$$\frac{M^2}{M+1} < .5\left[\frac{M^2}{2\delta} + \frac{M^3}{2\delta}\right]$$

and the linear is more effective if the inequality is reversed. Note that the inequality above simplifies to

$$\delta < \frac{(M+1)^2}{4}$$

This will not hold when $\delta = 1$, but will begin to hold at some point as $\delta \to 0$.

Case 2: $\delta < M$ for naïfs In this case the threshold is always more effective. 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,

$$C = .5(M - \delta/2)(1 + M)$$

 $P = .5(M - \delta/2)M^2$

giving cost-effectiveness of

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

Compare this with the linear model with

$$m = \frac{M^2}{M+1}$$

They have the same cost effectiveness. Compliance in the linear is $F(m) = \frac{M^2}{M+1}$. To show that the threshold is always preferred in case 2 we need show that

$$.5(M - \delta/2)(1 + M) > \frac{M^2}{M+1}$$

or

$$(M - \delta/2)(1 + M)^2 > 2M^2$$

Now, $M - \delta/2 > M/2$ and so

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

So it suffices to show that

$$\frac{M}{2}(M^2 + 2M + 1) > 2M^2$$

or

 $M^2 + 2M + 1 > 4M$

or

$$M^2 - 2M + 1 > 0$$

which follows since $M^2 - 2M + 1 = (M - 1)^2$.

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,

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

$$P = .5(M^4)(1 - \delta/2)$$

Thus, cost-effectiveness is:

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

Compare this with the linear model with the same cost-effectiveness

$$m = \frac{M^2}{M+1}$$

Compliance in the linear is $F(m) = \frac{M^2}{M+1}$. Thus, the threshold is more effective if

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

This is equivalent to:

$$\frac{4}{2-\delta} < (1+M)^2$$

or, equivalently

$$\delta < 2 - \frac{4}{(1+M)^2}$$

Note that when $M < \sqrt{2} - 1$, δ would have to be negative for the inequality to hold; hence, in that case, linear contracts are always better. Note also that $2 - \frac{4}{(1+M)^2} < M$ for all M < 1 so the "cutoff" value is always below M.

(b) $1 \leq M < 2$ Here naïfs 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 $M - \delta/2 < 1$,

$$C = (M - \delta/2)$$

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

giving

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

Compare with linear with m = M/2. This has compliance C = M/2. Because $M > \delta$, it follows that $M - \delta/2 > M/2$ and so the threshold is 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 $M - \delta/2 > 1$,

$$C = 1$$
$$P = .5M$$

giving

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

This is strictly better than the linear with m = M/2 which will only yield C = M/2 < 1.

H.2 Proofs of Section B.3 Propositions

We begin by proving Proposition 4 for $T \ge 2$. We then prove Propositions 6, 7, 8 and ??.

Proposition 4 (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:

$$w_t(e_t, 1) = \begin{cases} 1 & \text{if } e_t < V_{t+1,t}^{(1-0)} \\ 0 & \text{otherwise} \end{cases}$$
(29)

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

$$w_t(e_t, 0) = \begin{cases} 1 & \text{if } e_t < 0 \\ 0 & \text{otherwise} \end{cases}$$
(30)

We look at naïfs 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, naïfs 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

$$V_{T,j}^{(1)} = \mathbb{E}\left[\left(d^{(T-j)}M - \delta^{(T-j)}e_T\right)\mathbb{1}\left\{d^{(T-j)}M - \delta^{(T-j)}e_T > 0\right\} \middle| e_1, \dots, e_j\right], \ j = 1, \dots, T$$
$$= \mathbb{E}\left[\max\{d^{(T-j)}M - \delta^{(T-j)}e_T, 0\} \middle| e_1, \dots, e_j\right], \ j = 1, \dots, T$$
(31)

They also think that, on day 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

$$V_{t,j}^{(0)} = \mathbb{E}\left[-\delta^{(t-j)}e_t \mathbb{1}\{e_t < 0\} | e_1, \dots, e_j\right], \ j = 1, \dots, t$$
(32)

As a result, we have that:

$$V_{T,j}^{(1-0)} = \mathbb{E}\left[\max\{d^{(T-j)}M - \delta^{(T-j)}e_T, 0\} + \delta^{(T-j)}e_T\mathbb{1}\{e_T < 0\}|e_1, \dots, e_j\right]$$
(33)

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

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

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 (33) must also be decreasing in $\delta^{(T-j)}$.

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

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

Combined with equation (32) this yields:

$$V_{t,j}^{(1-0)} = \mathbb{E}\left[\max\{V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t, 0\} + \delta^{(t-j)}e_t \mathbb{1}\{\delta^{(t-j)}e_t < 0\} | e_1, \dots, e_j\right], \ j = 1, \dots, t$$
(34)

Equations (33) and (34) thus recursively define all of the $V_{t,j}^{(1-0)}$ for any $t \leq T$ and $j \leq t$. Since we already showed that $V_{T,j}^{(1-0)}$ is weakly decreasing in $\delta^{(T-j)}$ for all $j \leq T$ (equation (33)), we can then use reverse induction from $t = T, \ldots, j$ using equations (33) and (34) to see that $V_{t,j}^{(1-0)}$ is decreasing in all $\delta^{(T-j)}, \ldots, \delta^{(t-j)}$ for any $t \leq T$ and $j \leq t$.⁵⁸

The fact that $V_{t,j}^{(1-0)}$ is decreasing in all $\delta^{(T-j)}, \ldots, \delta^{(t-j)}$ for any $t \leq T$ and $j \leq t$ shows that day t compliance is also weakly decreasing in all $\delta^{(T-t)}, \ldots, \delta^{(t-t)}$, since one complies on day t if $e_t < V_{t+1,t}^{(1-0)}$ (equation (29)). Hence, overall compliance C from days $1, \ldots, T$, is weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(T-1)}$ for naïfs.

Sophisticates know that, conditional on complying on all prior days, on day T they will comply if

⁵⁸We make the induction hypothesis that $V_{t+1,j}^{(1-0)}$ is weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(t)}$ and show that, under this hypothesis, $V_{t,j}^{(1-0)}$ is also weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(t)}$. Since we have already shown that $V_{T,j}^{(1-0)}$ is decreasing in all $\delta^{(1)}, \ldots, \delta^{(T-1)}$, the result then follows. To show that $V_{t,j}^{(1-0)}$ is weakly decreasing in all $\delta^{(1)}, \ldots, \delta^{(t+1)}$, we show that the argument of equation (34), $\max\{V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t, 0\} + \delta^{(t-j)}e_t \mathbb{1}\{\delta^{(t-j)}e_t < 0\}$, is decreasing in $\delta^{(t-j)}$ for all e_t . Again there are two cases:

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

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

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

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

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

 $e_T < M$. Thus, equation (33) becomes:

$$V_{T,j}^{(1-0)} = \mathbb{E}\left[\left(d^{(T-j)}M - \delta^{(T-j)}e_T\right)\mathbb{1}\left\{e_T < M\right\} + \delta^{(T-j)}e_T\mathbb{1}\left\{e_T < 0\right\}\Big|e_1, \dots, e_j\right] \quad j = 1, \dots, T \quad (35)$$

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, $(d^{(T-j)}M \delta^{(T-j)}e_T) \mathbb{1}\{e_T < M\} + \delta^{(T-j)}e_T \mathbb{1}\{e_T < 0\} = (d^{(T-j)}M \delta^{(T-j)}e_T) \mathbb{1}\{e_T < M\}$, which is weakly decreasing in $\delta^{(T-j)}$.
- 2. $e_T \leq 0$: In this case, $\left(d^{(T-j)}M \delta^{(T-j)}e_T\right) \mathbb{1}\left\{e_T < M\right\} + \delta^{(T-j)}e_T \mathbb{1}\left\{e_T < 0\right\} = \left(d^{(T-j)}M \delta^{(T-j)}e_T\right) + \delta^{(T-j)}e_T = d^{(T-j)}M$, 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 (34) becomes:

$$V_{t,j}^{(1-0)} = \mathbb{E}\left[\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_t\right) \mathbb{1}\left\{e_t < V_{t+1,t}^{(1-0)}\right\} + \delta^{(t-j)}e_t \mathbb{1}\left\{e_t < 0\right\} \middle| e_1, \dots, e_j\right] \quad j = 1, \dots, t \quad (36)$$

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 (36) and the same reverse induction argument as for naïfs to show this implies that $V_{t,j}^{(1-0)}$ is decreasing in all $\delta^{(T-j)}, \ldots, \delta^{(t-j)}$ for all $j \leq t \leq T$.⁵⁹ By the same argument used for naïfs, this then implies overall compliance C is weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(T-1)}$ for sophisticates.

Proposition 6 (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 separable contract will be constant with $\delta^{(t)}$. In contrast, compliance and effectiveness in the threshold contract will be weakly decreasing in $\delta^{(t)}$. Hence, compliance and effectiveness in the threshold relative to separable contract will be 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 $e \ge m'$. In contrast, we now show that when people are impatient over effort, they often will comply even when e > m'.

⁵⁹Again the induction hypothesis is that $V_{t+1,j}^{(1-0)}$ is weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(t)}$. One can then use equation (36) to show that this implies that $V_{t,j}^{(1-0)}$ is weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(t)}$ because the argument, $\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_T\right) \mathbb{1}\left\{e_T < V_{t+1,j}^{(1-0)}\right\} + \delta^{(t-j)}e_T \mathbb{1}\left\{e_T < 0\right\}$, is weakly decreasing in $\delta^{(1)}, \ldots, \delta^{(t)}$ for all e_t . There are two cases::

1. $e_T > 0$: In this case, $\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_T\right) \mathbb{1}\left\{e_T < V_{t+1,j}^{(1-0)}\right\} + \delta^{(t-j)}e_T \mathbb{1}\left\{e_T < 0\right\} =$

 $\left(V_{t+1,j}^{(1-0)} - \delta^{(t-j)}e_T\right) \mathbb{1}\left\{e_T < V_{t+1,j}^{(1-0)}\right\}$, which is weakly decreasing in $\delta^{(t-j)}$ under the induction hypothesis.

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

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

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' ($e \ge d^{(T-K+1)}m'$) and so the threshold "binds." In this case, note that agents will never comply more than K days total.⁶⁰

A naïf 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 naïf will comply if:

$$\sum_{t=T-K+1}^{T} \delta^{(t-(T-K+1))} e \le d^{(T-t)} Km'$$
(37)

Compliance on day T - K + 1 is thus decreasing in $\delta^{(t)}$ for all t from 1 to K. If the naïf complies on day T - K + 1, the naïf 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 naïf with the same discount rates would. In addition, the sophisticate may comply before the last K days as well.⁶¹

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.⁶² 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 hold:

$$\sum_{j=1}^{K} \delta^{(t_j - t_1)} e \leq d^{(T - t_1)} K m'$$

$$\sum_{j=2}^{K} \delta^{(t_j - t_2)} e \leq d^{(T - t_2)} K m'$$
....
$$\sum_{j=K}^{K} \delta^{(t_j - t_K)} e \leq d^{(T - t_K)} K m'$$
(38)

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 increasing 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

⁶⁰Once 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.

⁶¹For 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.

⁶²In our example with T = 3 and K = 2, the combinations would be 1, 3 and 2, 3.

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 7. Let T = 3. Let the cost of effort on each day be binary, taking on one of two values, e_H or e_L , with $e_H \ge e_L$. 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 $e_L \ge m'$, 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 $e_L \ge m'$ and so we next consider the cases where $e_H \ge m'$ 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 $e_H \ge m'$ 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 2).
- 2. Case 3: e_H, e_H, e_L : Again, neither sophisticates nor naïfs 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 $e_H < 2m'$. Naïfs 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 $(e_H < 2m')$. Naïfs walk in period 1 if $e_L + \delta^2 e_H < 2m'$. Neither type walks in period 2 since $e_H \ge m'$. 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 $e_H \ge m'$. Thus, the prediction (trivially) holds.

To prove the effectiveness part of the result, we examine sophisticates first and then naïfs 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, naïfs can sometimes not receive payment for a day on which they comply. In case 4, naïfs 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, naïfs 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 naïfs.

Hence, since we have shown that compliance is decreasing in δ whereas cost-effectiveness is nonincreasing (and in particular, flat for sophisticates and weakly decreasing for naïfs), 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 naïfs as well.

Proposition 8. 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 in the threshold contract is 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 :

$$w_3(2) = 1\{e_3 < m'\}$$

$$w_3(1) = 1\{e_3 < 2m'\}$$

$$w_3(0) = 1\{e_3 < 0\}.$$

We show that the prediction holds by showing that it holds under all potential cases for e_3 . Case 1: $m' \le e_3 < 2m'$ In this case, walking in period 3 is

$$w_3(2) = 0$$

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

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

$$w_2(1) = 1\{e_2 \le \delta e_3\}$$

$$w_2(0) = 1\{e_2 + \delta e_3 < 2m'\}$$

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. $e_2 + \delta e_3 \ge 2m'$: 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:

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

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. $e_2 \ge m'$: 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).

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

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 .

H.3 Proofs of Section E Propositions

We now provide the proofs for Propositions 9 and 10 and for Corollary 1. We define some additional notation for these. Let the realized day 2 payment be $m_2 = (1 - w_1)m_{2L} + w_1m_{2H}$. Denote the value an individual on day 1 places on the opportunity to have the day 2 payment m_2 go from payment level m to payment level m' as $V_m^{m'}$.

Proposition 10. Fix all parameters other than δ . For na $\ddot{i}fs$, compliance is weakly decreasing in δ for Option-only and Option + commitment contracts, but invariant to δ under Commitment-only and Inframarginal contracts.

Proof. Time-bundled contracts with a two-day payment period reward walking on day 1 with m_1 and an increase in the day-2 payment from m_{2L} to m_{2H} . Individuals thus comply on day 1 if

$$e_1 < m_1 + V_{m_{2L}}^{m_{2H}}. (39)$$

As a result, day 1 (and hence total)⁶³ compliance is decreasing in δ if and only if $V_{m_{2L}}^{m_{2H}}$ decreases in δ .

We now show that, for naïfs, $V_{m_{2L}}^{m_{2H}}$ only decreases in δ for Option-only and Option + commitment contracts. Because naïfs think their day 2 selves will comply when $m > \delta e_2$, the value they place on having their day 2 payment increase from 0 to m > 0 is

$$V_0^m |^{\text{Naïf}} = \max(m - \delta e_2, 0). \tag{40}$$

Since $V_{m_{2L}}^{m_{2H}} = V_0^{m_{2H}} - V_0^{m_{2L}}$, then for any $m_{2H} > m_{2L}$:

$$V_{m_{2L}}^{m_{2H}}|^{\text{Naif}} = \begin{cases} m_{2H} - m_{2L} & \text{if } \delta e_2 < m_{2L} < m_{2H} \\ m_{2H} - \delta e_2 & \text{if } m_{2L} \le \delta e_2 < m_{2H} \\ 0 & \text{if } m_{2L} < m_{2H} \le \delta e_2 \end{cases}$$
(41)

Equation (41) shows that $V_{m_{2L}}^{m_{2H}}|^{\text{Naïf}}$ is weakly decreasing in δ for all parameter values. It is strictly increasing in δ when $m_{2L} \leq \delta e_2 < m_{2H}$, which is the option case: from the day 1 perspective, walking on day 2 is not worth a payment of m_{2L} but is worth a payment of m_{2H} . As a result, increasing the payment from m_{2L} to m_{2H} creates a lucrative option that the naïf believes she will exploit on day 2. The more she discounts effort, the more valuable the option is. Since there exists a range of $\delta \leq 1$ for which $m_{2L} < \delta e_2 < m_{2H}$ for all Option-only and Option + commitment contracts, equation (41) implies that, for those contracts, there exists a range of e_1 , m_1 , and $\delta \leq 1$ for which $V_{m_{2L}}^{m_{2H}}$ (and hence compliance) is strictly decreasing in δ .

To see that compliance is not decreasing in δ under Commitment-only or Inframarginal contracts, note that equation (41) shows that for those contracts $V_{m_{2L}}^{m_{2H}} = m_{2H} - m_{2L}$, which is not decreasing in δ . The intuition is that, in these contracts, the naïf believes on day 1 that she will comply on day 2 regardless of her day 1 action. She thus values the increase in m_2 from m_{2L} to m_{2H} at exactly its cash value, which is invariant to δ .

Proposition 11. Fix all parameters other than δ . For sophisticates, compliance is weakly decreasing in δ for Commitment-only and Option + commitment contracts, but invariant to δ under Option-only and Inframarginal contracts.

Proof. Sophisticates know they will not comply on day 2 for a payment less than e_2 , yielding the following valuation of a day 2 payment m > 0:

$$V_0^m |^{\text{Soph}} = (m - \delta e_2) \times 1\{e_2 < m\}.$$
(42)

The value the sophisticate places on an increase in the day 2 payment level from m_{2L} to m_{2H} is thus

$$V_{m_{2L}}^{m_{2H}}|^{\text{Soph}} = \begin{cases} m_{2H} - m_{2L} & \text{if } e_2 < m_{2L} < m_{2H} \text{ (Inframarginal)} \\ m_{2H} - \delta e_2 & \text{if } m_{2L} \le e_2 < m_{2H} \text{ (Option + Comm. or Comm. only)} \\ 0 & \text{if } m_{2L} < m_{2H} \le e_2 \text{ (Option-only)} \end{cases}$$
(43)

⁶³Total compliance follows from day 1 compliance since day 2 compliance is increasing in day 1 compliance (because $m_{2H} > m_{2L}$) and is not otherwise affected by δ .

The critical cutoff for day 2 payment over which it is differentially valuable for more impatient sophisticates is thus e_2 , not δe_2 as it was for naïfs. Even though the day 2 contract appears to be a lucrative option as soon as m_2 surpasses δe_2 , sophisticates know their day 2 selves will not follow through on the option if $m_2 < e_2$. Thus, Option-only contracts are not more effective for low- δ sophisticates. In contrast, in contracts with commitment, sophisticates know that day 1 compliance is pivotal to day 2 compliance, and the more they discount future effort, the higher the net benefits of compliance.

To complete the proof, note that compliance with Inframarginal contracts is invariant to δ for the same reason as for naïfs.

Corollary 1. Fix all parameters other than δ . If a sophisticate with a given $\delta < 1$ complies on day 1 of a time-bundled contract when neither a naïf with the same δ nor a patient person with $\delta = 1$ would comply, then two things must be true:

(1) The Commitment Value condition must hold (equation (25)) but the Option Value condition must not (equation (24)), which implies the following holds:

Commitment Value Without Option Value Condition:
$$\delta e_2 < m_{2L} \le e_2 < m_{2H}$$
 (44)

(2) The sophisticate must be complying on day 1 even though $e_1 \ge m_1 + m_{2H} - m_{2L}$. That is, the sophisticate complies on day 1 even though the cost of day 1 effort exceeds the maximum potential financial benefit from day 1 compliance.

Proof. If a sophisticate complies on day 1 when a naïf does not, it means that $V_{m_{2L}}^{m_{2H}}|^{\text{Naïf}} < V_{m_{2L}}^{m_{2H}}|^{\text{Soph}}$. Comparison of equations (41) and (43) show that this means that the Commitment Value condition must hold (equation (25)) but the Option Value condition must not (equation (24)), i.e., which implies that Equation (26) must hold. This condition always holds in Commitment-only contracts and can sometimes hold for sufficiently low δ in Option + commitment contracts that have $m_{2L} > 0$. Under these circumstances, $V_{m_{2L}}^{m_{2H}}|^{\text{Naïf}} = m_{2H} - m_{2L}$ and $V_{m_{2L}}^{m_{2H}}|^{\text{Soph}} = m_{2H} - \delta e_2$. The naïf's condition for complying on day 1, as specified in equation (39), then becomes: $e_1 < m_1 + m_{2H} - m_{2L}$. If the naïf does not comply, this means:

$$e_1 \ge m_1 + m_{2H} - m_{2L}. \tag{45}$$

Thus, when sophisticates comply when naïfs do not, equation (45) must hold.⁶⁴ If the naïf does not comply, this also means that a patient person would not comply, since compliance is weakly decreasing in δ (Proposition 9 and 10), and so the result is shown.

⁶⁴Note that the sophisticate can still comply even when equation (45) holds, since the sophisticate's condition for complying is $e_1 < m_1 + m_{2H} - \delta e_2$, which can hold even when equation (45) holds since $\delta e_2 < m_{2L}$ (equation (26)).

I Impatience Index Validation

In late 2021, we collected data to examine the relationship between our impatience index and incentivized measures of impatience over effort and of impatience over recharges. In this section, we first describe the measurement. We next show that, reassuringly, our impatience index correlates well with the incentivized measure of impatience over effort and does not correlate with the incentivized measure of impatience over recharges.

I.1 Measurement

We recruited participants who had participated in a follow-on project in Tamil Nadu that had similar recruitment criteria as this project (Dizon-Ross and Zucker, 2022).⁶⁵ For each participant, we measured the same impatience index measured in this study (which we refer to throughout as the *impatience index*), as well as two incentivized impatience measures, one over effort and one over recharges. We enrolled and consented 71 participants.⁶⁶

Effort Impatience Measure We use the methodology of Augenblick (2018), which is similar to the methodology of Augenblick and Rabin (2019) and which was previously adapted by John and Orkin (2021) to a field setting. Individuals are asked to perform an effort task. Our incentivized effort task was to call into a toll-free automated phone line, listen to a 30-second useless recording, and answer a simple question at the end to confirm that they listened.⁶⁷ On the survey date (day 0), individuals choose how many calls they want 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 $0.^{68}$ One choice is then chosen to be incentivized.

The basic 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 high levels of respondent understanding.

The data are also consistent with positive discounting of future effort and some present bias. Figure I.1 shows the average number of tasks chosen by task date. Consistent with positive discounting, the number of tasks chosen on days with t > 0 are all significantly greater than on t = 0. Consistent with present bias, the biggest difference appears to be between "today" and "tomorrow," not between days in the future. That is, the tasks chosen do not monotnically

⁶⁵The primary difference was that, in addition to enrolling participants with diabetes and prediabetes, Dizon-Ross and Zucker (2022) also enrolled participants who had hypertension.

⁶⁶This sample size is in the same ballpark as the samples enrolled in the original papers using the methodology on which our work is based, with 99 participants completing the choices in Augenblick (2018) and 79 in Augenblick and Rabin (2019). That said, it is smaller than our target sample size, as we had aimed for a larger sample to be able to identify correlations between measures instead of aggregate and individual parameters. The reason the sample is smaller than planned is that COVID cut our data collection short.

 $^{^{67}}$ The task from Augenblick (2018) was infeasible due to the lack of computer and smartphone access. The task from John and Orkin (2021) was also not viable in our setting. There, respondents sent 30-digit random number strings via text to a toll-free number, which was infeasible for us because (a) there were no *toll-free* text options available, and (b) our participants' ability to send texts is very low, as we discovered when piloting options for our step reporting system.

 $^{^{68}}$ We include a 0 INR piece rate following guidance from John and Orkin (2021) that a zero piece rate helps fit the data better. All respondents, regardless of the chosen piece rate, receive 100 INR if they complete all the tasks they sign up for (in addition to 1 "mandatory task"). However, in our case, our structural measure does not converge when we include the 0 INR piece rate choice in the data and so we exclude that measure when estimating. the structural parameters.

increase by day, and we cannot reject that the tasks chosen on days 7 and 8 equal the tasks chosen on day 1. Interpreted from the lens of the "beta-delta" model, wherein $\delta^{(t)} = \beta \delta^t$ with β the present-bias parameter and δ the exponential discount rate, the data appear most consistent with a model with $\beta < 1$ and $\delta \approx 1$.

Appendix Figure I.1: Respondents Exhibit Positive Effort Impatience, Allocating More Tasks to Future Dates than "Today"



Notes: This figure displays the average number of tasks chosen for day 0, day 1, day 7, and day 8. The 95% confidence intervals on the bars for days 1, 7, and 8 represent tests of equality between average choices on those days and day 0. The dotted line represents the average for day 0 which implies there are significant differences when the confidence intervals do not intersect with this line.

Given these patterns, we use these data to construct two proxies—one reduced form and one structural—for a single parameter capturing the average discount factor over effort on days 1, 7, and 8. We label this parameter β . Our reduced-form measure follows Augenblick (2018) in examining the excess number of tasks chosen on future dates relative to day 0. Specifically, for all choices made regarding future days (t > 0), we construct a measure (hereafter, the *effort-impatience measure*) at the individual × choice level equal to the number of tasks chosen on day t for piece rate w minus the number of tasks chosen on day 0 for the same piece rate w. People who are more impatient (lower β) will assign relatively more tasks to future days than today, and should thus have higher average values of this measure.

To create our structural measure, we follow John and Orkin (2021) (who follow Augenblick (2018) in assuming quasi-linear utility and a power cost of effort function and who follow DellaVigna and Pope (2018) in allowing for a non-monetary reward s which participants receive for each task in addition to the piece rate). See the notes to Table I.2 for the estimating equation. We estimate group-level (as opposed to individual-level) structural estimates of the β parameter.⁶⁹ We present two β estimates, one from the full sample and one from the sample with below-median values of our impatience index. Unfortunately our estimates do not converge for the sample with above-median values of our impatience index, but we believe the estimates are still informative.⁷⁰

⁶⁹As is also the case in John and Orkin (2021), individual-level structural estimates are not feasible as they converge for less than half of the sample.

⁷⁰Issues with convergence when estimating parameters at the individual level are common in these models (see, e.g., Augenblick et al. (2015); Augenblick (2018)), and our sample sizes once we are analyzing subsamples appear to not be large enough to overcome these issues even at the pooled level.

Recharge Impatience Measure We use a conventional multiple price list (MPL) approach to measure impatience over recharges (Andreoni and Sprenger, 2012; John and Orkin, 2021). Participants were asked to make 10 choices between receiving a recharge today and at a later date.⁷¹ The payment today was always 50 INR, while the later date payments were always larger: 60, 70, 90, 100, and 150 INR. We used two later dates: 7 days from today and 14 days from today. We construct a reduced form measure of impatience (hereafter: the *recharge impatience measure*) at the individual × choice level which is equal to an indicator that the individual chose the smaller recharge on the sooner date for that choice.

I.2 Results

The impatience index and effort impatience Figure I.2(a) summarizes the reduced-form effort-impatience measure separately for individuals who have above-median and below-median values for the impatience index. Those who have above-median values of the impatience index have substantially higher values of the effort-impatience measure, on average. Those with above-median values of the impatience index allocate roughly 1.5 more tasks to the future dates relative to today, whereas those with below-median values of the impatience index only allocate approximately 0.5 more tasks to future days relative to today.

Appendix Figure I.2: Higher Values of Impatience Index Predict Higher Effort Impatience But No Higher Recharge Impatience



Notes: Panel A displays the average difference between the number of tasks chosen on all future dates minus the number of tasks chosen on day 0 (the same day as the survey) for the same payment amount separately for the below- and above-median impatience index samples. In Panel B, we display the average proportion of 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 explore the significance of these results, we estimate the following regression:

$$EffortImpatience_{itw} = \beta_0 + \beta_1 ImpatienceIndex_i + \beta_2 y_{i0w} + \tau_w + \tau_t + \varepsilon_{itw}$$
(46)

where i indexes individuals, t indexes the day the tasks would be completed, and w indexes the

⁷¹Since we are interested in average discount rates over future events, not time inconsistency per se, we do not include choices where the sooner date is not today.

piece rate. $EffortImpatience_{itw}$ is our effort-impatience measure (day t tasks by individual i for piece rate w - day 0 tasks by individual i for the same piece rate). ImpatienceIndex_i is our impatience index, either an indicator for having an above-median value or the continuous index itself. y_{i0w} is the number of tasks chosen by individual i at piece rate w on day 0; controlling for this allows for the possibility that the effort-impatience measure varies with the level of chosen tasks and improves precision.⁷² τ_w and τ_t are fixed effects for the piece rate and task day, respectively.

Table I.1 presents the results. Consistent with Figure I.2, Column (1) shows that the difference in effort-impatience between those with above- and below-median values of the impatience index is roughly 1 task, significant at the 10% level. Following Augenblick (2018), in column (2), we remove all individuals with "little or no variation in their effort choices," as their choices do not well identify their time preferences.⁷³ In this sample, we see the relationship even more strongly: those with above-median values of the impatience index have 1.6 tasks higher effortimpatience than those with below-median values, significant at the 5% level. Columns (3) and (4) show that the patterns are qualitatively similar when we use the continuous impatience index as the regressor, but precision goes down (*p*-values of 0.23 and 0.15), potentially reflecting some outliers in the impatience index in our relatively small sample.

Table I.2 presents the results of the structural estimation. We estimate a β of 0.90 for the full sample, significantly different from 1 (*p*-value <.01), consistent with moderate levels of effort-impatience. However, in the subsample of individuals with below-median levels of the impatience index, β is near 1, equal to 0.997, and statistically indistinguishable from 1 (*p*-value 0.45). Thus, although the estimates did not converge in the subsample with above-median levels of the impatience index, these estimates suggest that the individuals with above-median values of the impatience index are the only ones with $\beta < 1$.

The impatience index and recharge impatience Figure I.2(b) summarizes our rechargeimpatience measure separately for individuals with above-median and below-median values of the impatience index. The levels of recharge impatience (i.e., of choosing a smaller, sooner recharge over a larger, later recharge) are very similar across the subsamples; if anything, those with higher values of the impatience index have slightly lower levels of recharge impatience.

To explore the significance of these results, we estimate the following regression:

$$RechargeImpatience_{itw} = \beta_0 + \beta_1 ImpatienceIndex_i + \tau_w + \tau_t + \varepsilon_{itw}$$

$$\tag{47}$$

where *i* indexes individuals, *t* indexes the later recharge date, and *w* indexes the later recharge amount. *RechargeImpatience_{itw}* is our measure of recharge impatience, and τ_w and τ_t are fixed effects for the later recharge amount and later recharge date.

Table I.3 confirms that there is no meaningful or significant relationship between recharge

⁷²Define 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 those from a regression where we replace $EffortImpatience_{itw}$ with y_{itw} as the dependent variable and include the same controls. We run as specified in equation (47) for comparability of the mean value of the dependent variable with Figure I.2.

 $^{^{73}}$ Specifically, we remove respondents who never adjust their effort choice in response to a change in the piece rate, which removes 26/71 respondents. Augenblick (2018) remove 21/99 respondents for this and similar reasons. Our slightly higher rate of removal may reflect higher confusion in a field setting. There is no significant difference in the likelihood of being removed between those with below-median and above-median values of the impatience index.

	Dep. var.: Future tasks - day 0 tasks				
	(1)	(2)	(3)	(4)	
Above-median impatience index	0.946^{*}	1.805^{***}			
	[0.529]	[0.651]			
Impatience index			0.705	1.082	
			[0.573]	[0.738]	
Sample	All	Changers	All	Changers	
P-value on impatience	0.078	0.008	0.222	0.150	
Dep. var. mean on					
below-median impatience sample	0.552	0.171	0.552	0.171	
# Individuals	71	45	71	45	
# Observations	852	540	852	540	

Appendix Table I.1: Impatience index correlates with effort impatience

Notes: Each observation is an individual \times task choice; choices differ in the date of the task (t > 0) and the payment amount. Controls are day and payment amount fixed effects. The dependent variable is the difference between the number of tasks chosen in that choice minus the number of tasks chosen on day 0 (the same day as the survey) for the same payment amount; we exclude choices with task date t = 0. The impatience index is our standardized index of questions on impatience and self-control from the psychology literature. "Changers" represent respondents who, for at least one day t, adjust their effort choice in response to a change in the piece rate, w. That is, it removes people who, for every t, choose the same number of tasks across all w for t. The regressor in columns 1 and 2 is an above-median impatience index dummy, while in columns 3 and 4 the regressor is the continuous measure. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table 1.2: Structural Model Est	Estimates
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Parameter	Full sample	Below-median impatience
		sample
β	$0.898 \\ (0.037)$	$0.997 \\ (0.004)$

Notes: This table displays estimation of structural parameters from the model presented by Augenblick (2018) using an approach similar to John and Orkin (2021). 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 participants receive for each task in addition to the monetary payment, w is the piece rate (i.e. payment amount), $d^{(11)}$ captures the monetary discounting of the payment in 11 days, and ϕ is a slope parameter. We parametrize $\delta^{(t)} = \beta$ (equivalent to setting $\delta = 1$ in a standard "beta-delta" model) and estimate s, ϕ , β , and γ . We present results using data from the full sample and the below-median impatience index sample. To enable convergence, we only included observations where w > 0. However, our model did not converge for the above-median impatience index sample. The p-value for the test that $\beta = 1$ is 0.006 in the full sample and 0.45 in the below-median impatience sample.

impatience and the impatience index. Columns (1) and (3) show that there is no relationship in the full sample. With MPLs, it is standard to see people switch more than once in a given price list, a sign of potential subject confusion (Andreoni and Sprenger, 2012). Columns (2) and (4) remove people who ever switch more than once, with similar results.

	Dep	endent variable:	Chose ear	rlier date
	(1)	(2)	(3)	(4)
Above-median impatience index	-0.0838	-0.0818		
	[0.0981]	[0.106]		
Impatience index			-0.0402	-0.0546
			[0.0862]	[0.110]
Sample	All	No violations	All	No violations
P-value on impatience	0.396	0.445	0.643	0.623
Dep. var. mean on				
below-median impatience sample	0.484	0.471	0.484	0.471
# Individuals	71	64	71	64
# Observations	710	640	710	640

Appendix Table I.3: Impatience index does not correlate with recharge impatience

Notes: Each observation is an individual \times recharge choice. The dependent variable is an indicator for choosing to receive recharges today rather than in the future. Controls are 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 "No violations" sample represents people who do not switch multiple times on either price list. The regressor in columns 1 and 2 is an above-median impatience index dummy, while in columns 3 and 4 the regressor is a continuous measure. Significance levels: * 10%, ** 5%, *** 1%.

Recharge impatience and effort impatience To correlate recharge impatience and effort impatience, we calculate the person-level averages of the recharge impatience and effort impatience measures and then correlate them. We find that the correlation is near 0 (correlation of -0.05; *p*-value = 0.67).

Recharge proxies Finally, we correlate the proxies for impatience over recharges that are available from our main sample—recharge usage and recharge balances—with our incentivized measure of recharge impatience. While these correlations are only suggestive as we only have measures of usage and balances from 14 subjects,⁷⁴ we find that the correlations are -0.65 (*p*-value = 0.114) and -0.25 (*p*-value = 0.589), respectively. Thus, as one would expect, those with higher usage and higher balances may be less impatient over recharges.

J CTB Time Preference Measurement

We adapted the convex time budget (CTB) methodology of Andreoni and Sprenger (2012) to try to measure time preferences in two domains, walking and mobile recharges. Here we discuss why we believe our measurement was not a reliable measure of time preferences in this setting.

First, the methodology is complicated. It was difficult to explain to our sample, 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 suggest that participant understanding was limited. First, law of demand

⁷⁴The other subjects have a different type of unlimited plan, which has become more popular since our original data collection for this study, which does not have the same balances and usage measures. Note that the correlations between recharge impatience, the impatience index, and effort impatience look similar in this small sample as well.

violations are far more common than in previous studies.⁷⁵ As shown in Table J.4, 57% of the sample violated the law of demand at least once. For reference, participants in the 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%.

	# of violators	% of sample
	(1)	(2)
Violates $0/7$	1,337	41.4
Violates 7/14	1,515	46.9
Violates at least once	1,830	56.7
Violates both	1,022	31.6
Total:	3,232	100

Appendix Table J.4: Law of demand violations in effort allocations

Notes: 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.

Second, 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.

Third, 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.

Fourth, 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.

⁷⁵We 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.

Fifth, the CTB parameter estimates themselves are not robust and are inconsistent with typical priors. First, 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: 26% for steps and 30% for recharges. This is more than the usual rate of negative individual-level estimates.

Finally, 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). 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.

To try to measure time preferences in two domains, walking and mobile recharges, we adapted the convex time budget (CTB) methodology of Andreoni and Sprenger (2012). Below we discuss the methodology used and then show that the measures do not correlate with the behaviors that we would expect.

J.1 Estimation Methodology

We follow earlier work and use a Convex Time Budget (CTB) methodology. In each CTB choice of the time-preference survey, 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

$$c_t + \frac{1}{r}c_{t+k} = m \tag{48}$$

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.3.

We asked participants to make six allocations in the recharge domain, and eight allocations in the step domain, as summarized in Table J.5. We assume a time-separable and goodseparable CRRA utility function with quasi-hyperbolic discounting. In the domain of recharges, individuals will then seek to maximize utility,

$$U(c_t, c_{t+k}) = \frac{1}{\alpha} (c_t - \omega)^{\alpha} + \beta \delta^k \frac{1}{\alpha} (c_{t+k} - \omega)^{\alpha}$$
(49)

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

$$C(c_t, c_{t+k}) = \frac{1}{\alpha} (c_t + \omega)^{\alpha} + \beta \delta^k \frac{1}{\alpha} (c_{t+k} + \omega)^{\alpha}$$
(50)

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


Appendix Figure J.3: Sample decision screen for mobile recharges. 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 ($c_t = 70, c_{t+k} = 82$).

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.

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).

$$\log\left(\frac{c_t + \omega}{c_{t+k} + \omega}\right) = \frac{\log(\beta)}{\alpha - 1} \mathbf{1}_{t=0} + \frac{\log(\delta)}{\alpha - 1} k - \frac{1}{\alpha - 1} \log\left(r\right)$$
(51)

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 timeconsistent, 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.⁷⁶ However, the results we describe next are similar if we do not allow α to vary at the individual-level for steps.

Summary of Convex Time Budget allocations								
Question no.	t	k	r	Recharge domain	Step domain			
1	7	$\overline{7}$	1	Х	Х			
2	0	7	1	Х	Х			
3	0	5	1	Х	Х			
4	0	3	1	Х	Х			
5	0	2	1	Х	Х			
6	0	1	1	Х	Х			
7	7	7	1.25		Х			
8	0	7	1.25		Х			

Appendix Table J.5: CTB allocation parameters

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 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.⁷⁷ To try to ensure that participants complete 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

⁷⁶Indeed, 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.

⁷⁷This 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.

of measurement.⁷⁸ 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.2 CTB Estimates

Table J.6 displays the summary statistics as well as the convergence statistics diussed in more detail in the upfront text.

Tables J.7 and J.8 show that the estimated CTB parameters do not correlate in the expected direction with measured behaviors. In particular, Table J.7 shows that the CTB estimates in the steps/effort domain do not correlate with exercise and health,⁷⁹ and Table J.8 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 10% 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.

 $^{^{78}}$ 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: individualspecific 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 presentbias 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.

⁷⁹Table J.7 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.

	Full s	ample	α <	: 1	
Parameters estimated:	$eta\delta$	δ	$\beta\delta$	δ (4)	
	(1)	(2)	(3)		
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	33.8	37.9	
# Individuals:	2,494	1,821	1,092	1,225	
B. Recharges					
Beta	1.004	_	_	_	
Delta	0.997	0.997	_	_	
% of sample:	55.9	62.2	_	_	
# Individuals:	1,808	2,010	-	_	

Appendix Table J.6: Summary Statistics For CTB Parameters

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 (2012), 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%.

Covariate type:	Exe	ercise					
Dependent variable:	Daily steps	Daily exercise (min)	Health index	Negative vices index	Healthy diet index	# Individuals	
Delta	-0.019	0.009	-0.040	0.010	0.027	1,225	
Beta-delta	0.016	0.018	0.014	0.010	0.027	1,092	

Appendix Table J.7: CTB Estimates of Discount Factors Over Steps Do Not Correlate with Measured Behaviors

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.8: CTB Estimates of Discount Factors Over Recharges Do Not Correlate with Other Proxies for Impatience over Recharges

Covariate type:		Recharge	variables	Credit const			
Dependent variable:	Negative mobile balance	Negative yesterday's talk time	Prefers daily payment (=1)	Prefers monthly payment (=1)	Negative wealth index	Negative monthly household income	# Individuals
Delta	0.026	0.008	-0.120***	0.053	-0.016	1,853	
Beta-delta	-0.012	-0.004	0.130***	-0.017	-0.001	$1,\!669$	

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: * 10%, ** 5%, *** 1%.

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 prepost 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 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:

$$y_{it} = \alpha + \beta_1 Intervention Period_{it} + \beta_2 incentives_i + \beta_3 (Intervention Period_{it} \times incentives_i) + \mathbf{X}'_i \gamma + \boldsymbol{\mu}_m + \varepsilon_{it},$$
(52)

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, X_i is a vector of individual-specific controls, and μ_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.

	Acl	nieved 10K st	ceps	Daily steps			
	(1)	(2)	(3)	(4)	(5)	(6)	
Incentives	0.012 [0.024]	0.013 [0.024]	0.012 [0.014]	66.7 $[268.1]$	66.4 $[266.9]$	48.9 [112.3]	
Intervention period	0.057^{***} [0.020]	0.073^{***} [0.020]	0.064^{***} [0.020]	-130.4 [237.8]	108.0 [240.8]	-18.5 [234.1]	
Intervention period X Incentives	0.19^{***} [0.021]	0.19^{***} [0.021]	0.19^{***} [0.021]	1270.9^{***} [248.6]	1258.9^{***} [249.2]	$\begin{array}{c} 1212.7^{***} \\ [243.4] \end{array}$	
Monitoring phase-in mean	0.24	0.24	0.24	6904.80	6904.80	6904.80	
Year-month FEs Individual controls	No No	Yes No	Yes Yes	No No	Yes No	Yes Yes	
# Monitoring # Incentives # Individuals	203 2,401 2,604	203 2,401 2,604	203 2,401 2,604	203 2,401 2,604	203 2,401 2,604	203 2,401 2,604	
Observations	221,214	221,214	221,214	221,214	221,214	221,214	

Appendix Table K.1: Impacts of monitoring (pre-post) and incentives (difference-in-differences) on exercise outcomes.

Notes: This table shows coefficient estimates from regressions of the form specified in equation (52). 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 selected by lasso in Table 2. The omitted category in all columns is the monitoring group 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%.

L Lifestyle Modification Guidelines



TRANSLATING RESEARCH INTO ACTION



Lifestyle Modification Guidelines

The Ministry of Health and Family Welfare, Government of Tamil Nadu recommends that all hypertensives, diabetics *and* people at risk for those diseases follow the life style modifications below. These modifications will help to maintain blood pressure, blood sugar, and body weight at healthy and controlled levels. This is essential for the prevention and management of diabetes and hypertension.

PHYSICAL ACTIVITY

- Do moderate physical activity, such as brisk walking for 30-40 minutes per day at least 4 times every week, but preferably daily. Brisk walking is walking at a pace where you find speaking difficult, but not impossible.
- Do some sort of physical exercise every day.
- Try to reach a total of 10,000 steps, or about 8km of walking, per day.
- In order to increase your level of physical activity, perform all your household chores such as cleaning, washing clothes, gardening etc. yourself.

MAINTAIN AN IDEAL WEIGHT FOR HEIGHT

- Avoid being overweight for your height.
- Being overweight increases the risks of diabetes, hypertension, heart attacks, stroke, and paralysis.

HEALTHY DIET

- Reduce your consumption of sugar, salt, cooking oil, and dairy fat.
- If you are a non-vegetarian, replace fatty meats with skinless chicken and fish.
- Increase your consumption of fruit and green leafy vegetables. As a general rule, you must have at least 500 grams of fruits and vegetables in a day.
 - o However, avoid vegetables like potatoes and tubers.
 - In addition, some diabetics should avoid fruits. If you are diabetic you should check with a medical professional about your fruit intake.
- Stop smoking and avoid alcohol.

DECREASE YOUR STRESS LEVELS

 Physical activities from walking to dancing, listening to and playing music, meditation, and yoga can all reduce stress.

MAINTAIN NORMAL BLOOD PRESSURE AND BLOOD SUGAR

- Uncontrolled blood pressure can lead to cardiovascular diseases such as hypertension, stroke, and heart attack.
- Uncontrolled blood sugar can lead to diabetes.
- Maintaining healthy and controlled blood sugar levels is the best way to prevent complications of diabetes such as foot and nerve damage, loss of eyesight, and kidney failure.
- If you are at risk for NCDs, check your blood sugar and blood pressure at least once a year at an NCD screening location. The nearest location is: Coimbatore Government Hospital [address]

Appendix Figure L.1: Lifestyle Modification Advice Delivered at Baseline