Online Supplement for:

Incentivizing Behavioral Change: The Role of Time Preferences

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S1 Heterogeneity in frequency effects by impatience

This appendix explores the possibility that immediate incentive delivery is a driver of incentive effectiveness among the subset of more impatient participants. If so, we expect a positive interaction between more immediate incentive delivery and our (imperfect) proxies for impatience over mobile recharges. We test this interaction using both between-treatment and within-treatment variation in immediacy of payment.

Our first test is whether daily incentives are relatively more effective, and monthly relatively less effective, than the base case of weekly payments for those who display more impatience. For simplicity, we restrict the sample to those who were in the daily, weekly, and monthly groups, and run the following regression:

$$\begin{aligned} y_{it} = &\alpha + \beta_0 Impatience_i + \beta_1 daily_i + \beta_2 monthly_i \\ &+ \beta_3 Impatience_i \times daily_i + \beta_4 Impatience_i \times monthly_i + \boldsymbol{X}_i'\gamma + \varepsilon_{it}, \end{aligned} \tag{35}$$

where y_{it} is a daily walking outcome; $Impatience_i$ is one of our three proxies for impatience in the recharge domain (the negative of someone's baseline balances, the negative of someone's baseline daily recharge usage, or a measure of constraints), all normalized so that higher values proxy for higher levels of impatience; and $daily_i$ and $monthly_i$ are indicators for being assigned to the daily and monthly treatments, respectively. β_1 and β_2 represent the effects of daily and monthly relative to the base case weekly payment (respectively). The coefficients of interest are β_3 and β_4, showing whether the effects of daily or monthly relative to Weekly are differentially large for those who are more impatient. If impatience over recharges is a mechanism through which more immediate incentive delivery increases effectiveness, then we expect the daily treatment to be more effective ($\beta_3 > 0$) and the monthly treatment to be less effective ($\beta_4 < 0$) for more impatient individuals.

We report our results in Table S1.1. We find no evidence that suggests that sooner payments work better for those we would expect to be more impatient, with no clear pattern across measures and the one result significant at the 5% level going the wrong way.

Our second test is whether individuals who display more impatience are more likely to increase step-target compliance on their payday. We perform this test among individuals in the base case incentive and monthly incentive groups. Following Kaur et al. (2015), we define individual-specific walking "payday effects" as the difference in the probability of exceeding 10,000 steps on paydays compared to all other days. The walking payday effect is a revealed-preference measure of impatience over payments. We estimate the interaction between individual payday effects and our baseline proxies for impatience over recharges using regressions of the following form:

$$\begin{aligned} y_{it} = &\alpha + \beta_0 \left(Impatience\,Measure\right)_i + \beta_1 \left(Payday\right)_{it} \\ &+ \beta_2 \left(Payday\right)_{it} \times \left(Impatience\,Measure\right)_i + \boldsymbol{X}_i'\gamma + \varepsilon_{it}, \end{aligned} \tag{36}$$

where y_i, $(Impatience\,Measure)$, and $\boldsymbol{X}_i$ are defined as in equation 35; and $(Payday)_{it}$ is an indicator for whether day t is a payday for individual i. To test whether more impatient

Appendix Table S1.1: High-frequency treatments are not more effective for those who are more impatient

Dependent variable:	Met step target		
Impatience measure:	Negative mobile balance	Negative yesterday's usage	Marginal talk time, if gifted
	(1)	(2)	(3)
Daily × Impatience	-0.057**	-0.030	-0.013
	[0.026]	[0.034]	[0.015]
Monthly × Impatience	-0.026	-0.046*	-0.017
	[0.020]	[0.025]	[0.013]
Daily	-0.0068	0.0095	0.0012
	[0.026]	[0.028]	[0.028]
Monthly	-0.025	-0.025	-0.031
	[0.025]	[0.025]	[0.026]
Impatience	0.0033	-0.038	-0.0019
	[0.011]	[0.026]	[0.010]
Base case mean	0.50	0.50	0.51
# Individuals	2,558	2,450	2,388

Notes: This table shows heterogeneity in the effect of the frequency subtreatments by treatment effects of each incentive non-threshold treatment, interacted with measures of impatience; the base case incentive group is omitted. We use three mobile recharge variables collected at baseline as proxies for impatience over recharges: mobile balance, yesterday's talk time in INR, and unconstrained recharge usage if we were to gift individuals recharges. Variables are normalized by the standard deviations of the control group. 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. Controls are the same as Table 2. Larger values of each impatience measure indicates more impatience. The unit of observation is a respondent × day. Standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

individuals respond more to more immediate payment, we test whether $\beta_2 > 0$.

Our results are shown in Table S1.2. We find no evidence that even those individuals who are most impatient over payments react to more immediate reward delivery over the payment cycle.

Appendix Table S1.2: Payday effects are not bigger for those who are more impatient

Dependent variable:	Met step target		
Impatience measure:	Negative mobile balance	Negative yesterday's usage	Marginal talk time, if gifted
	(1)	(2)	(3)
Impatience × Payday	0.0047	0.0059	0.0017
	[0.0050]	[0.0064]	[0.0033]
Payday	0.028	0.025	0.036
	[0.025]	[0.025]	[0.025]
Impatience	-0.010	0.016	-0.014**
	[0.0088]	[0.011]	[0.0066]
Base case mean	0.50	0.50	0.51
# Base case	890	845	826
# Monthly	163	160	155
# Individuals	1,053	1,005	981

Notes: This table shows heterogeneity in the "payday" effects for those in the base case incentive and the monthly incentive groups, by proxies for impatience over recharges. Our proxies include baseline measures for mobile balance, yesterday's usage, and unconstrained usage, which is a self-reported estimate of usage in INR if recharges were gifted. Impatience measures are normalized by the standard deviations of the control group, and such that higher values correspond to greater impatience. We normalize the proxies so that higher values correspond to higher expected discount rates. Payday effects are defined as the difference in a daily exercise behavior on paydays compared to all other days. Standard errors are in brackets. Significance levels: * 10%, ** 5%, *** 1%.

S2 Additional Program Evaluation

Appendix Table S2.1: Impacts of incentive program and monitoring on diet and addictive consumption.

A. Healthy diet	Healthy Diet Index	Wheat meals	Meals with vegetables	Servings of fruit	Negative of rice meals	Negative of junkfood pieces	Negative of spoons sugar in coffee	Negative of sweets yesterday)	Avoid unhealthy food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Incentives	0.046	0.023	0.058*	0.040	0.026	-0.026	-0.024	-0.025	0.0045
	[0.046]	[0.030]	[0.031]	[0.038]	[0.033]	[0.064]	[0.047]	[0.038]	[0.017]
Monitoring	0.018	0.011	0.076	0.063	-0.010	0.13	-0.030	-0.039	-0.040
	[0.081]	[0.053]	[0.054]	[0.066]	[0.059]	[0.11]	[0.083]	[0.067]	[0.031]
Control mean	0.00	0.49	0.58	0.53	-2.34	-0.91	-1.12	-0.35	0.83
P-value: M = I	0.69	0.80	0.70	0.70	0.49	0.14	0.94	0.82	0.11
# Individuals	3,063	3,063	3,063	3,063	3,063	3,063	3,063	3,063	3,063

B. Addictive consumption	Addictive good consumption index	Average daily areca	Average daily alcohol	Average daily cigarettes
	(1)	(2)	(3)	(4)
Incentives	-0.013	0.034	-0.034*	-0.061
	[0.037]	[0.042]	[0.020]	[0.11]
Monitoring	-0.00021	0.019	-0.014	-0.027
	[0.065]	[0.074]	[0.036]	[0.19]
Control mean	0.00	0.13	0.11	1.02
P-value: M = I	0.83	0.82	0.52	0.84
# Individuals	3,063	3,063	3,063	3,063

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 are the same as Table 2, 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%.

S2.1 SMS Treatment Impacts

We here present the effects of the SMS treatment, which we included in the experiment to appease our government partners who were interested in its efficacy. We estimate regressions of the following form, using the same outcome variables as in Table 2 and Section 6:

$$y_i = \alpha + \beta_1 \times incentives_i + \beta_2 \times monitoring_i + \beta_3 \times SMS_i + \boldsymbol{X}_i'\gamma + \varepsilon_i \tag{37}$$

where y_i is a health or lifestyle outcome at endline for individual i; $incentives_i$ is an indicator for being in the incentive group; $monitoring_i$ is an indicator for being in the monitoring group; SMS is an indicator for being in the SMS treatment; and $\boldsymbol{X}_i$ is a vector of controls, shown in the table notes. β_1 is the ITT effect of incentives relative to the pure control group, β_2 is the ITT effect of the monitoring relative to the pure control group, and β_3 is the ITT effect of being in the SMS reminders group. Note that, when the outcome variable represents exercise measured using pedometer data, we omit the $monitoring_i$ dummy and the reference group becomes the monitoring group (since that data is unavailable for the control group).

Table S2.2 contains the walking impacts of the SMS treatment and Table S2.3 contains the health impacts. We do not see any significant effects on either. While the coefficients for exercise are near 0, the coefficient for health impacts is non-trivial in magnitude and, if anything, positive; recall that here positive coefficients are associated with *worse* health. Although this coefficient is not signiificant and likely reflects statistical noise, to probe further, in Table S2.4, we estimate a model including all of the interaction effects between the SMS treatment and the monitoring or incentive treatments. Although these estimates should be interpreted as suggestive since (a) we did not plan to run this specification ex ante, and (b) the interaction effects are only marginally significant, it appears that one reason for the SMS Treatment's marginally negative average effect may be that the SMS treatment does not interact well with incentives.

Appendix Table S2.2: Impacts of SMS treatments and incentives on exercise

	Pedometer data (intervention period)		
	Fraction days achieved 10K Steps	Daily steps	Daily steps (conditional on positive)
	(1)	(2)	(3)
A. Pooled incentives			
Incentives	0.197***	1266.0***	1156.2***
	[0.0179]	[208.7]	[186.2]
SMS	0.00616	123.2	67.86
	[0.0217]	[208.2]	[179.6]
B. Unpooled incentives			
Base case	0.208***	1388.4***	1197.8***
	[0.0196]	[222.1]	[197.2]
Daily	0.202***	1122.0***	1196.0***
	[0.0301]	[331.5]	[273.4]
Monthly	0.181***	1274.2***	1210.9***
	[0.0282]	[307.5]	[264.9]
5-Day threshold	0.208***	1306.6***	1229.0***
	[0.0250]	[264.0]	[230.0]
4-Day threshold	0.188***	1181.0***	1112.6***
	[0.0203]	[229.8]	[202.9]
Small payment	0.125***	731.2*	511.6
	[0.0382]	[386.2]	[328.4]
SMS	0.00416	111.0	55.69
	[0.0217]	[208.3]	[179.8]
Monitoring mean	0.294	6774.469	7985.923
Controls	Yes	Yes	Yes
P-value for Base case vs			
Daily	0.82	0.35	0.99
Monthly	0.27	0.65	0.95
4-Day threshold	0.98	0.68	0.85
5-Day threshold	0.20	0.17	0.50
Small payment	0.02	0.06	0.02
# Individuals	2,559	2,559	2,557
Observations	205,732	205,732	180,018

Notes: We report pooled incentive effects in Panel A, and separately by incentive treatment group in Panel B. The sample includes the incentive and monitoring groups. Controls are the same as Table 2. The omitted category in all columns is the monitoring group. Standard errors, in brackets, are clustered at the individual level. A number of individuals in the incentive and monitoring groups immediately withdrew from the contract period, but there was no statistically significant difference in likeliness to withdraw by group (p-val > 0.7).

Appendix Table S2.3: Impacts of incentives, monitoring, and SMS treatments on health

	Health risk index	HbA1c	Random blood sugar	Mean arterial BP	Body mass index	Waist circum-ference
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	-0.045*	-0.072	-5.67*	0.081	-0.049	-0.18
	[0.025]	[0.070]	[3.42]	[0.42]	[0.042]	[0.27]
Monitoring	0.014	-0.13	1.63	1.08	0.064	0.00080
	[0.044]	[0.12]	[6.07]	[0.75]	[0.074]	[0.48]
SMS treatment	0.043	0.12	-4.05	0.66	0.099*	0.43
	[0.032]	[0.090]	[4.41]	[0.55]	[0.054]	[0.35]
Control mean	0.00	8.44	193.83	103.02	26.45	94.44
# Control	561	560	561	560	559	559
P-value: M = I	0.13	0.57	0.18	0.14	0.09	0.67
P-value: SMS = I	0.03	0.10	0.77	0.40	0.03	0.16
P-value: SMS = M	0.59	0.10	0.45	0.65	0.70	0.47
# Individuals	3,063	3,061	3,062	3,051	3,053	3,054

Notes: Standard errors in brackets. For the Health Risk Index, controls are the same as Table 2, along with second order polynomials of all health variables underlying the index at baseline. Controls for all other outcomes are the same as Table 2. The Health Risk Index is an index created by the average of endline Hba1c, RBS, MAP, BMI, and waist circumference standardized by their average and standard deviation in the control group. Hba1c is the average plasma glucose concentration (%), RBS is the blood glucose level (mg/dL), MAP is the mean arterial blood pressure (mm Hg), and BMI is the body mass index. The omitted category in all columns is the pure control group. Significance levels: * 10%, ** 5%, *** 1%.

Appendix Table S2.4: Impacts of incentives, monitoring, SMS treatments, and their interactions on health risk factors

	Health risk index	HbA1c	Random blood sugar	Mean arterial BP	Body mass index	Waist circumference
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	-0.061**	-0.13*	-7.72**	0.12	-0.056	-0.20
	[0.026]	[0.074]	[3.61]	[0.45]	[0.044]	[0.29]
Monitoring	0.018	-0.16	2.27	1.04	0.084	0.068
	[0.047]	[0.13]	[6.39]	[0.79]	[0.078]	[0.50]
SMS treatment	-0.068	-0.34*	-18.5*	0.93	0.064	0.32
	[0.073]	[0.20]	[10.0]	[1.24]	[0.12]	[0.79]
Incentives × SMS	0.15*	0.60***	20.0*	-0.40	0.065	0.20
	[0.082]	[0.23]	[11.2]	[1.39]	[0.14]	[0.88]
Monitoring × SMS	-0.040	0.21	-7.47	0.39	-0.20	-0.66
	[0.15]	[0.41]	[20.2]	[2.46]	[0.24]	[1.57]
Control mean	0.00	8.44	193.83	103.02	26.45	94.44
# Control	561	560	561	560	559	559
P-value: M = I	0.06	0.84	0.08	0.19	0.05	0.55
P-value: SMS = I	0.92	0.29	0.26	0.49	0.31	0.49
P-value: SMS = M	0.28	0.41	0.06	0.94	0.88	0.77
# Individuals	3,063	3,061	3,062	3,051	3,053	3,054

Notes: Standard errors in brackets. All definitions and controls follow Table 2. The omitted category in all columns is the pure control group. Significance levels: * 10%, ** 5%, *** 1%.

S3 Monitoring treatment impacts on walking

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

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

One can visualize the variation used for our pre-post estimate in Figure 11, panels A and B. Walking increases immediately during the intervention period for the monitoring group, although the effects decay over time.

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

$$\begin{aligned} y_{it} = &\alpha + \beta_1 Intervention\,Period_{it} + \beta_2 incentives_i + \beta_3(Intervention\,Period_{it} \times incentives_i) \\ &+ \boldsymbol{X}_i'\gamma + \boldsymbol{\mu}_m + \varepsilon_{it}, \end{aligned} \tag{38}$$

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

Table S3.1 presents the results. Column 2 shows that the monitoring group achieves the 10,000 step target on approximately 7% more days in the intervention period than in the phase-in period, an effect significant at the 1% level and equal to roughly 36% of the estimated impact of incentives. In contrast, the estimated effect on steps is very small in magnitude, varies across specifications, and is in fact sometimes negative (columns 4-6). Thus, the monitoring treatment, if anything, appears to do more to make walking consistent across days than it does to increase total steps.

Appendix Table S3.1: Impacts of monitoring (pre-post) and incentives (difference-in-differences) on exercise outcomes.

	Achieved 10K Steps			Daily Steps		
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	0.012	0.013	0.0074	66.7	66.8	41.1
	[0.024]	[0.024]	[0.011]	[268.1]	[266.8]	[111.3]
Intervention Period	0.057***	0.073***	0.064***	-130.3	109.2	-70.4
	[0.020]	[0.020]	[0.020]	[237.8]	[240.8]	[235.4]
Intervention Period X	0.19***	0.19***	0.19***	1263.4***	1251.1***	1220.9***
Incentives	[0.021]	[0.021]	[0.021]	[248.6]	[249.2]	[244.7]
Monitoring phase-in mean	0.24	0.24	0.24	6904.84	6904.84	6904.84
Year-month FEs	No	Yes	Yes	No	Yes	Yes
Individual controls	No	No	Yes	No	No	Yes
# Monitoring	203	203	202	203	203	202
# Incentives	2,401	2,401	2,388	2,401	2,401	2,388
# Individuals	2,604	2,604	2,590	2,604	2,604	2,590
Observations	221,568	221,568	220,406	221,568	221,568	220,406

Notes: This table shows coefficient estimates from regressions of the form specified in Equation 38. The outcomes are from daily panel data from the pedometers. Standard errors, in brackets, are clustered at the individual level. Individual controls are the same as Table 2. The omitted category 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%.

S4 CTB Time Preference Measurement

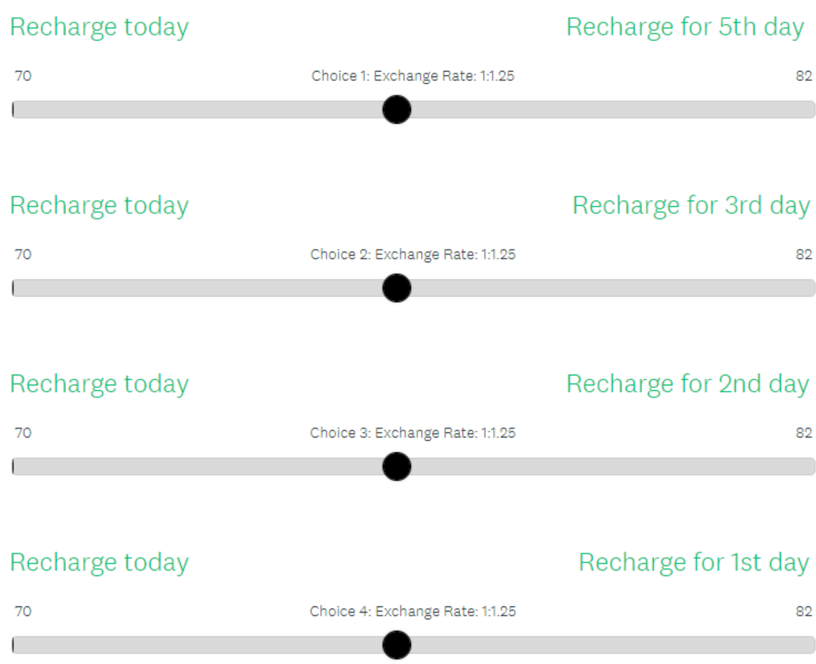
To try to measure time preferences in two domains, walking and mobile recharges, we adapted the convex time budget (CTB) methodology of Andreoni and Sprenger (2012). Below we discuss the methodology used and then show that the measures do not correlate with the behaviors that we would expect.

S4.1 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

$$c_t + \frac{1}{r} c_{t+k} = m \tag{39}$$

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 S4.1.



Appendix Figure S4.1: 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 ($c_t = 70, c_{t+k} = 82$).

We asked participants to make six allocations in the recharge domain, and eight allocations in the step domain, as summarized in Table S4.1. We assume a time-separable and good-separable CRRA utility function with quasi-hyperbolic discounting. In the domain of recharges, individuals will then seek to maximize utility,

$$U(c_t, c_{t+k}) = \frac{1}{\alpha}(c_t - \omega)^{\alpha} + \beta\delta^k \frac{1}{\alpha}(c_{t+k} - \omega)^{\alpha} \tag{40}$$

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

$$C(c_t, c_{t+k}) = \frac{1}{\alpha}(c_t + \omega)^{\alpha} + \beta\delta^k \frac{1}{\alpha}(c_{t+k} + \omega)^{\alpha} \tag{41}$$

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

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

$$\log\left(\frac{c_t+\omega}{c_{t+k}+\omega}\right) = \frac{\log(\beta)}{\alpha-1}1_{t=0} + \frac{\log(\delta)}{\alpha-1}k - \frac{1}{\alpha-1}\log(r) \tag{42}$$

Details on the estimation strategy can be found in the Online Appendix of Augenblick et al. (2015). Because our predictions concern overall impatience, not whether an individual is time-consistent, on the time preference side we want one single summary measure capturing impatience. To do so, we estimate two different variants. In one, we set $\beta = 1$ for everyone at the estimation stage and simply estimate δ at the individual level. In the second, we estimate the equation as above, allowing both β and δ to vary at the individual level, and use $\beta \times \delta$ as our measure of individual-level impatience. In both estimation procedures, we allow α to vary at the individual-level in the steps domain, since we considered individual-level convexity of the step function to be an important potential confound.[39] However, the results we describe next are similar if we do not allow α to vary at the individual-level for steps.

Appendix Table S4.1: CTB allocation parameters

Summary of Convex Time Budget allocations					
Question no.	t	k	r	Recharge domain	Step domain
1	7	7	1	X	X
2	0	7	1	X	X
3	0	5	1	X	X
4	0	3	1	X	X
5	0	2	1	X	X
6	0	1	1	X	X
7	7	7	1.25		X
8	0	7	1.25		X

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

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

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.[40] To try to ensure that participants complete the allocated steps, we offer a large cash completion bonus of 500 INR in the step domain if the allocation is selected to be implemented, and the steps are completed as allocated, with the bonus to be delivered 15 days from the date of the survey (which is 1 day after the latest "later" day used).

We also take a number of precautions to avoid various potential confounds, including confounds reflecting fixed costs or benefits of taking an action, or confounds due to the time of day of measurement.[41] However, we were not able to fully address one potential confound to our estimates of time-preferences across individuals: variation across people in the cost of walking over time, or in the benefit of receiving a recharge over time. For

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

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

Appendix Table S4.2: Summary Statistics For CTB Parameters

	Full sample		$\alpha < 1$	
Parameters estimated:	$\beta\delta$	δ	$\beta\delta$	δ
	(1)	(2)	(3)	(4)
A. Effort				
Beta	2.066	–	1.573	–
Delta	0.883	0.997	1.015	0.999
Alpha	0.244	0.723	1.673	1.576
% of sample:	77.2	56.3	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	–	–

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%.

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.

S4.2 CTB Estimates

Table S4.2 displays the summary statistics as well as the convergence statistics diussed in more detail in the upfront text.

Tables S4.3 and S4.4 show that the estimated CTB parameters do not correlate in the expected direction with measured behaviors. In particular, Table S4.3 shows that the CTB estimates in the steps/effort domain do not correlate with exercise and health,[42] and Table S4.4 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.

[42] Table S4.3 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.

Appendix Table S4.3: CTB Estimates of Discount Factors Over Steps Do Not Correlate with Measured Behaviors

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

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 S4.4: CTB Estimates of Discount Factors Over Recharges Do Not Correlate with Other Proxies for Impatience over Recharges

Covariate type:	Recharge variables			Credit constraint proxies			
Dependent variable:	Negative mobile balance	Negative yesterday's talk time	Marginal talk time, if gifted	Wealth index	Monthly household income	Monthly personal income	# Individuals
Delta	0.026	0.008	-0.042*	-0.016	0.002	0.008	1,892
Beta-delta	-0.012	-0.004	0.044*	-0.001	0.018	-0.012	1,701

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

S5 Lifestyle Modification

ABDUL LATIF JAMEEL Poverty Action Lab — TRANSLATING RESEARCH INTO ACTION

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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.
    - 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 S5.1: Lifestyle Modification Advice Delivered at Baseline