Why Do We Procrastinate? Present Bias and Optimism

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Abstract

Research has shown that procrastination has significant adverse effects on individuals, including lower savings and poorer health. Procrastination is typically modeled as resulting from present bias. In this paper we study an alternative: excessively optimistic beliefs about future demands on an individual's time. The models can be distinguished by how individuals respond to information on their past choices. Experimental results refute the hypothesis that present bias is the sole source of dynamic inconsistency, but they are consistent with optimism. These findings offer an explanation for low takeup of commitment and suggest that personalized information on past choices can mitigate procrastination.

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1 Introduction

Procrastination is an important feature of everyday life. It is a common topic of conversation at work and at home, and economists have documented it in consequential settings including retirement saving, exercise, and education (Thaler and Benartzi, 2004, DellaVigna and Malmendier, 2006, Ariely and Wertenbroch, 2002). Procrastination is commonly modeled as originating from present biased discounting that favors the present at the expense of the future (Strotz, 1955, Laibson, 1997, O'Donoghue and Rabin, 1999, Barro, 1999, Ashraf et al., 2006, Heidhues and Kőszegi, 2010, Augenblick et al., 2015).[1] We study an alternative model in which dynamic inconsistency arises from excessive optimism about future demands on an individual's time. While both models predict dynamically inconsistent choices, they predict different responses to information about past procrastination. We test these predictions experimentally and reject the hypothesis that present bias is the sole source of dynamic inconsistency. Instead we find evidence that both discount rates and beliefs matter. Our results suggest that the typical policy prescription—offering people the chance to tie themselves to the mast, committing to decisions in advance—is incomplete and that personalized historical information is an important additional tool for people making decisions over time.

Biased beliefs about future time shocks can cause choices made ahead of time to differ from choices made in the moment. Consider an agent who does not accurately anticipate the arrival of a time-consuming task. Colloquially we say that such an agent is optimistic about her time shocks. Once the task arrives, the agent will need to defer planned time use to accommodate the unanticipated shock.[2] If the agent has systematically biased beliefs over future time shocks, then such procrastination can occur even with neoclassical discounting. We refer to this as *belief-based dynamic inconsistency*.[3]

Discounting-based dynamic inconsistency, in contrast, models dynamically incon-

[1] In the quasi-hyperbolic model of Laibson (1997), the agent discounts at rate δ between future periods, but between the current period and the next period at rate $\beta\delta$ with $\beta < 1$. This heavier discounting leads to "present biased" allocative choices.

[2] As this suggests, the model shares features with Kahneman and Tversky's (1982) "planning fallacy." In Section 2 we discuss some of the models of and empirical evidence for biased beliefs. One of our key theoretical insights is showing the link between biased beliefs and dynamic inconsistency.

[3] In contrast to Halevy (2008) and Andreoni and Sprenger (2012b), this inconsistency is a result of the decision maker having incorrect beliefs rather than a utility function which does not take the expected utility form.

sistent choices as originating from a utility function that places lower weight on the more distant future relative to the present or the immediate future. This leads the agent to exhibit present-biased dynamic inconsistency because choices made far enough in advance will be governed by geometric discounting, while choices made about the immediate future will not. If an agent is naïve about her own present-biased discounting, she believes that she will behave more consistently than she actually does.[4]

Because these two models lead to similar dynamically inconsistent choices, a research design seeking to distinguish discounting-based from belief-based dynamic inconsistency cannot rely solely on revealed procrastination.[5] However, making past time-inconsistent decisions salient to decision makers resolves this identification problem. Both models rely on agents not incorporating information about their prior choices when forming beliefs, but discounting- and belief-based models make different predictions about how agents will respond to information about those choices.

First, the two models give different predictions for how effort allocation will change in response to information. Discounting-based dynamically inconsistent agents have a clear idea of the time shocks that they face, but have trouble committing to time use choices. Such agents will not change effort allocation decisions in response to information. In contrast, belief-based dynamically inconsistent agents have erroneous expectations about time shocks. Correcting these beliefs will cause them to change their effort allocation decisions to better conform to the true state of the world.

Second, information can cause naïve discounting-based dynamically inconsistent agents to learn about their own present bias. For instance, an agent might learn that her discounting is more present-biased than she previously thought.[6] This will increase commitment demand for time-use choices made far enough in advance. If agents have biased beliefs over time shocks, however, this prediction need not hold. Information on past dynamically inconsistent decisions should help belief-based dynamically inconsistent agents bring their beliefs in line with the true state, but this does not necessarily lead them to demand costly commitment (Laibson, 2015).

[4]Again, in the quasi-hyperbolic model of Laibson (1997), partially naïve agents have true discounting parameters β and δ but believe their present-bias parameter is $\hat{\beta}$ where $\beta < \hat{\beta} \leq 1$ (O'Donoghue and Rabin, 2001).

[5]The working paper by Browning and Tobacman (2015) makes a similar theoretical argument. Gabaix and Laibson (2017) show that a similar identification problem can occur due to imperfect (but unbiased) forecasting of the future.

[6]More formally, the agent might learn that she has a lower β than she previously thought.

We tested these predictions in an experiment over two weeks. The first week allowed us to measure baseline dynamically inconsistent behavior for each subject. On the first morning of the experiment, subjects divided required tasks between a period later that day and a period two days in the future. Subjects were able to pay a price in terms of additional tasks to commit to their morning choice.[7] On the evening of day 1 subjects could revise their task allocation, conditional on the morning commitment decision. Procrastination was indicated by the subject moving tasks to the later date. In addition, we gathered information on routine time use and subjects' predicted and actual bedtimes so we could compare task allocation behavior to real-world decisions.

At the beginning of week 2, treated subjects were presented with information on their own task procrastination. They were also given information on how well they were able to forecast their own bedtimes, a real-world decision where subjects routinely exhibit procrastination. Because we only treated some subjects with this information, the experiment allows us to identify the effect of changing beliefs while controlling for other determinants of procrastination and commitment demand such as background shocks or learning outside our treatment. To reduce the probability of experimenter demand effects, the information treatment was cast as a neutral reporting of past behavior.[8] All subjects then engaged in the same task decisions as in week 1.

Our experimental results indicate that both beliefs and discounting are important determinants of time inconsistency. Evidence on beliefs comes from testing the effect of treatment on task allocation. Among subjects who deferred work in week 1, the treatment caused a practically large and statistically significant reduction in deferral of tasks in week 2. This is inconsistent with discounting-based dynamic inconsistency and consistent with belief-based dynamic inconsistency. In additional results we show that the treatment affected subjects' reported expectations about being busy and that the changes in expectations were concentrated among subjects who exhibited behavior consistent with the belief-based model.

Evidence on present-biased utility comes from testing the effect of treatment on commitment demand in week 2 for individuals who were procrastinators in week 1.

[7] The in-kind price could take on both positive and negative values.

[8] In Section 5.4 we test for the presence of experimenter demand effects and do not find evidence for them.

For these individuals, treatment increased week 2 commitment demand, consistent with discounting-based dynamic inconsistency. Heterogeneity analysis using baseline measures of naïvete about time preferences and time shocks shows that these results are stronger for naifs.

Next we assess the prevalence of belief- and discounting-based dynamic inconsistency. Each treatment-group subject who reallocated tasks in week 1 is statistically matched with a control-group peer. If such a subject reduced the number of tasks deferred in week 2 more than her control group peer, we classify her behavior as belief-based. If such a subject increased commitment demand more than her control group peer, we classify her behavior as discounting-based. Under this taxonomy, 25% of subjects exhibited discounting-based inconsistency, 39% exhibited belief-based inconsistency, and 21% exhibited both. The remaining 15% exhibited behavior inconsistent with either model.

We then show that subjects' dynamic inconsistency extended to consequential, real-world time use. At baseline, subjects systematically mis-predicted their own bedtimes, going to bed later than planned on average. Treated subjects reduced their forecast error in week two. Consistent with the task-based results, this reduction was larger for those with larger week-one forecast errors. This is evidence that the treatment affected subjects' decision problem in the time domain.[9] To investigate further, we collected a panel of time use data from subjects over the course of the experiment. Panel data on time use is rare (Frazis and Stewart, 2012), and it allows us to examine how our treatment affected behavior outside the experiment. We show that when subjects were randomly induced to spend more time on our experiment, on average they spent less time studying and working, but more time watching television.

This study provides evidence on the sources of dynamically inconsistent behavior and the real-world consequences of such behavior. We show that a purely discounting-based model is incomplete. Instead, subjects exhibited nuanced dynamic behavior consistent with the presence of both discounting- and belief-based mechanisms. The presence of both mechanisms matters for policy aimed at dynamically inconsistent behavior. The prescription from the time inconsistency literature has primarily been to encourage commitment by sophisticated present-biased agents. Workers are often

[9] Firms may be exploiting time inconsistency in sleep decisions and elsewhere. For instance, Netflix CEO Reed Hastings has argued that tempting services like streaming video may affect sleep decisions, saying in an earnings call that "We're competing with sleep, on the margin." If individuals want to avoid such lures, the appropriate action depends on the source of their time inconsistency.

urged to contribute to retirement plans with early withdrawal penalties or commit to smoking cessation through a website like stickK.com. Our results suggest that such tools are inappropriate for some people. If procrastination stems from overestimation of future earnings or underestimation of how difficult it will be to quit smoking, then organizations and individuals seeking to correct dynamic inconsistency should provide personalized, salient information as well. This hypothesis is consistent with the widespread sale of goods—like fitness trackers and planners—that help consumers reflect on execution of their own plans.[10] Models of long-run forecasting errors due to bounded rationality, as in Gabaix (2014), similarly suggest that targeted information may correct consistent planning errors.

In addition, our study makes two contributions to research on demand for costly commitment. First, our findings help explain the widely observed, low take-up of such commitment. Subjects whose dynamic inconsistency originates solely from optimism will not demand costly commitment. Schilbach (2019) observes that in the majority of past experiments, subjects were either unwilling to pay for commitment or were willing to pay only very small amounts.[11] In significant exceptions, Schilbach does find high demand for commitment in the domain of alcohol consumption, and Casaburi and Macchiavello (2019) find demand for costly commitment in the Kenyan dairy sector. Second, our experimental design makes a methodological contribution in its elicitation of commitment demand. Our design begins from the convex time budget techniques of Andreoni and Sprenger (2012a).[12] Specifically, it uses real-effort tasks similar to those employed by Augenblick et al. (2015) and hews closely to the overall experimental design of that paper to clarify the importance of belief-based dynamic inconsistency. In contrast to Augenblick et al. (2015), our commitment price is denominated in tasks rather than money.[13] By keeping all choices in the task domain, we reduce the tendency of commitment demand to spike sharply at a zero

[10]Paul Krugman has made this point when reflecting on his own fitness tracker use, writing that "what fitness devices do, at least for me, is make it harder to lie to myself" (Krugman, 2015).

[11]While commitment demand is typically low, Carrera et al. (2019) show that misperception of contracts can lead an agent to demand too much commitment. In their context, the authors find that information on own and peers' past choices reduces commitment demand.

[12]The approach here differs from previous convex time budget experiments in that it does not vary the rate at which subjects trade off between present and future consumption. This simplifies the experiment and its instructions but does so at the cost of not being able to estimate discounting parameters. For an overview of designs used to estimate time preferences, see Frederick et al. (2002).

[13]To the best of our knowledge Toussaert (2018) is the only other experiment that elicits commitment demand with prices denominated in tasks.

price. We find that about one quarter of subjects were willing to commit to their time use choices at positive task-denominated prices.

Finally, our results contribute to a growing body of research demonstrating the importance of a decision maker's beliefs for how they make choices involving time. There is evidence that decision makers are subject to the "planning fallacy" when forming beliefs about future events (Kahneman and Tversky, 1982, Roy et al., 2005). DellaVigna and Malmendier (2006) and Acland and Levy (2015) both study gym membership and attendance, showing that consumers systematically overestimate how often they will go to the gym in the future even when this choice entails monetary costs. Avery et al. (2019) show that students routinely sleep less than the medically recommended amount due to both impatience and over-confidence. Börsch-Supan et al. (2018) demonstrate that a much larger portion of regret about not having saved more earlier in life is explained by positive and negative financial shocks than present bias. Consistent with the common lack of commitment demand in experimental subjects, Augenblick and Rabin (2018) find that individuals' *predictions* about the choices they will make in the future suggest that they do not understand their own present bias. Furthermore, subjects who make choices for the future immediately after completing tasks volunteer for less work in the future than those asked just before completing tasks. While the authors interpret this as evidence of projection bias, it is also consistent with decision makers who are optimistic about their desire to complete future tasks but who update after getting information. Our paper experimentally tests the link between time inconsistency and a planning fallacy that occurs due to biased beliefs.

The paper proceeds as follows. Section 2 presents the models of time inconsistency due to discounting and beliefs, then explains the model predictions. Section 3 gives the experimental design. Section 4 describes the data. Section 5 presents tests of our theoretical hypotheses. Section 6 links our experimental and theoretical results to real-world behavior. Section 7 concludes.

2 Theory and Hypotheses

This section provides a theoretical model that yields the hypotheses we test in the experiment. We begin by discussing more formally the discounting- and belief-based sources of dynamic inconsistency that motivate our study. We then create a general framework that embeds the possibilities of dynamic consistency, belief-based dynamic inconsistency, and discounting-based dynamic inconsistency. Both sources of dynamic inconsistency lead to procrastination. We then show that decision makers with these sources of dynamic inconsistency may react differently to information about their previous decisions.

2.1 Sources of Dynamic Inconsistency

A popular way to model present bias is through β-δ preferences, in which δ captures the standard "exponential" part of discounting, while β is the "present bias" parameter, which places a lower weight on all future sources of utility (Laibson, 1997). The β-δ model is used in part because it generates the dynamic inconsistency that is often seen in choice data. In a two-period model, the inconsistency arises from the difference in how the decision maker trades off utility coming from periods 1 and 2 when the decision is made at or before period 1. In the former case, the rate of discount between the two periods is $\beta\delta$, while in the latter it is δ.

In models where decision makers are aware of their present bias, they have an incentive to seek out commitment. While previous work has found evidence of present bias, evidence for commitment demand has been more elusive. One explanation is that individuals are unaware of their present bias, or are "naïve" in the sense that they believe that their β parameter is closer to 1 than it actually is.

While choice revisions can arise from present-biased discounting, they can also arise from biased beliefs over time shocks. Consider a decision maker solving an effort allocation problem over periods 1 and 2. She may do so under the belief that the distribution of period-one shocks, $F(\theta)$, is more favorable than it truly is. In this case, the time shocks in period 1 will tend to be surprisingly adverse, and the decision maker will want to complete less work than originally planned.

We do not model the source of incorrect beliefs, instead taking them as given and studying their implications. However, a number of existing models could lead to these optimistic beliefs. Kahneman and Tversky (1982) coined the term "planning fallacy" and provided an intuitive model in which decision makers neglect distributional information, leading to optimistic beliefs about outcomes like task duration or earnings. Beliefs and updating rules have also been modeled as a choice variable from the point of view of the decision maker (Bénabou and Tirole, 2002, Brunnermeier and Parker, 2005, Brunnermeier et al., 2016). Agents in these models trade off between the distortions caused by incorrect beliefs and their benefits, such as improved self-esteem or higher motivation.[14] Although these different models are important for the welfare implications of an intervention to reduce bias in beliefs, they are not important for the purpose of this study: to assess the roles of beliefs and discounting in dynamically inconsistent behavior.

There is a great deal of empirical evidence suggesting that decision makers exhibit optimistic beliefs about the future. Roy et al. (2005) survey the literature on the planning fallacy, which is observed in situations as diverse as predicting the amount of time it takes to fill out tax forms and predicting the amount of time one will have to wait in line for gas. Similarly, people have a tendency to think they are more likely than their peers to experience positive events and less likely to experience negative events (Taylor and Brown, 1988). Overconfidence is an especially consequential form of optimism. Research has shown that individuals are systematically overconfident, with the vast majority thinking they are smarter (Larwood and Whittaker, 1977) or better drivers (Svenson, 1981) than their average peer. This overconfidence continues to be experimentally observable even when it is costly to the decision makers (Camerer and Lovallo, 1999, Niederle and Vesterlund, 2007). It also manifests in decisions outside of the lab, such as changing patterns in corporate investment and mergers (Malmendier and Tate, 2005, 2008) and job search behavior (Mueller et al., 2018). These sorts of excess optimism and overconfidence might lead decision makers to start a task later than they expected, or fail to save sufficiently, in a way that makes them appear present biased.

2.2 Model

We study a decision maker whom we observe over two weeks. Each week, the decision maker is given tasks and decides how to allocate them between two predetermined days. When the tasks are to be completed, a time shock is realized (imagine that this is a problem set to finish or unexpected car trouble). The decision maker suffers costs that are quadratic in the sum of the time shock and the number of tasks to be completed.

In week t, the decision maker has w tasks to to complete, which she is allowed to split between two days as w_t and $w - w_t$. The time shocks that the decision maker

[14] In accord with these theoretical results, there is evidence that subjects do not update their beliefs according to Bayes' rule (Falk et al., 2006, Eil and Rao, 2011).

faces on the first and second days are denoted $\theta_{t,1}$ and $\theta_{t,2}$ respectively, both of which are weakly positive. For $i \in \{1, 2\}$, $\theta_{t,i}$ has a distribution $F(\theta_{t,i}; \alpha_i)$, where α_i is an index on the distribution such that if $\alpha > \alpha'$, then $F(\theta_{t,i}; \alpha)$ first order stochastically dominates $F(\theta_{t,i}; \alpha')$. These shocks are independent with the same mean and have distributions that are symmetric.

The decision maker may be present biased with $\beta \leq 1$. For simplicity, she has a long-term discount rate of $\delta = 1$. When the decision maker is offered the chance to split up the tasks well in advance of the tasks being completed, we refer to this choice as being "committed" (denoted by subscript C). If instead the choice is being made just before the tasks are completed, we refer to the choice as being "not committed" (denoted by subscript NC).

When deciding how to split up these tasks before the tasks must be completed, the decision maker solves

$$\min_{w_{t,C}} \; \beta \mathbb{E}\left[(w_{t,C} + \theta_{t,1})^2 + \delta(w - w_{t,C} + \theta_{t,2})^2\right], \tag{1}$$

which given our assumptions on shocks and $\delta = 1$ has solution

$$w^*_{t,C} = \frac{1}{2} w \tag{2}$$

When given the same decision after the first time shock is observed and when the work has to be done, the decision maker instead chooses a workload to solve

$$\min_{w_{t,NC}} \; (w_{t,NC} + \theta_{t,1})^2 + \beta\delta \mathbb{E}\left[(w - w_{t,NC} + \theta_2)^2\right], \tag{3}$$

which has the solution

$$w^*_{t,NC}(\theta_{t,1}) = \min\left\{\max\left\{0, \frac{\beta}{1+\beta} w + \frac{\beta}{1+\beta}\mathbb{E}[\theta_{t,2}] - \frac{1}{1+\beta}\theta_{t,1}\right\}, w\right\} \tag{4}$$

Previous research has demonstrated that individuals often make choices that appear present biased: they allocate more work to the earlier date when the decision is made for the future compared to when it is made in the present. We refer to the number of tasks that are deferred in period t as

$$D_t(\theta_{t,1}) = w_{t,C} - w_{t,NC}(\theta_{t,1})$$

Both belief-based and discounting-based dynamic inconsistency are consistent with behavior that looks like procrastination: planning to do work, then putting it off to a later date when given the chance. With belief-based dynamic inconsistency, this is a result of the average value of $\theta_{t,1}$ being higher than the decision maker's perception of the expected value of $\theta_{t,1}$. With discounting-based dynamic inconsistency, this is a result of β being less than one. An agent exhibiting either or both forms of dynamic inconsistency will procrastinate. Formally, for such an agent $\mathbb{E}[D_t] > 0$. An agent with $\beta = 1$ and unbiased beliefs over time shocks will not procrastinate ($\mathbb{E}[D_t] = 0$).

In what follows, we discuss both belief-based and discounting-based dynamically inconsistent decision makers. When we refer to a belief-based dynamically inconsistent decision maker, specifically we mean a decision maker in the above model who has $\beta = 1$ but an overly optimistic belief distribution ($\hat{\alpha} < \alpha$). Alternatively, when we refer to a discounting-based dynamically inconsistent agent, specifically we mean a decision maker who has $\beta < 1$ and is naïve about this present bias, but has correct beliefs about time shocks ($\hat{\alpha} = \alpha$).

2.3 Information Provision and Testable Hypotheses

Models of biased beliefs and present bias generate the same predictions for effort allocation behavior. But because both models rely on incorrect beliefs (either over α or β) to generate patterns seen in the data, one way to differentiate between the two models is to observe the effects of information provision on subsequent choice. Suppose the decision maker is given the information that when making past choices, the amount of work she agreed to complete earlier in the day was higher than the amount of work she chose when the tasks actually had to be completed. This could cause the decision maker to update to a higher belief about α or a lower belief about β. The predicted responses of work allocations and commitment to these two types of updating differ.

In the experimental design that follows (see Section 3), we treat some subjects with information about their week one choices before they make choices in week two. We use a tilde to refer to choices made by a decision maker who received this information treatment. Thus, $\tilde{D}_2(\theta_{2,1})$ refers to the number of tasks deferred by a decision maker in the second week after she has received a reminder of her previous choices, while $D_2(\theta_{2,1})$ is the same task deferral, but by someone who did not receive the information treatment.

In the naïve β-δ model, the decision maker has incorrect beliefs about the present bias parameter that will govern her decisions in the future. Because she cares about what her future self will choose, a change in these beliefs might lead to changes in her willingness to commit to her present actions (O'Donoghue and Rabin, 1999). In prior work, researchers have typically interpreted a lack of commitment demand by individuals as evidence that $\hat{\beta}$, the individual's belief about her own present bias, is closer to one than to β, the "true" present bias.

In the experiment, we observe the information subjects are given and the choices they make. We make the following assumptions about how decision makers update their beliefs when provided with information.[15]

Assumption DB. For discounting-based dynamically inconsistent decision makers, high observations of D_1 (more tasks deferred) cause the decision maker to believe she is more present biased (lower $\hat{\beta}$) than low observations.

Assumption BB. For belief-based dynamically inconsistent decision makers, high observations of D_1 (more tasks deferred) cause the decision maker to believe shocks on the first day are worse ($\hat{\alpha}$ is higher) than low observations.[16]

Assumptions DB and BB describe how beliefs are expected to change in each model when decision makers are reminded of their past choices.[17] This sort of reminder may offset the effects of a "selective memory" (Bénabou and Tirole, 2002). One reason why we may see effects on short run decision making while biases remain persistent is that information treatments have been shown to be more effective in changing short-run than long run beliefs (Zimmermann, 2020). If beliefs do not change in response to information provision, we would expect information to have no effect for any type of decision maker.

[15]Empirical evidence for these assumptions is discussed in Sections 5 and 6.

[16]Here we assume that information causes the decision maker to update about *only* the distribution of the first state. This is a reasonable simplification, because the information that the decision maker receives is generated by choices that were made before the value of the second state was observed. However, similar results hold if the decision maker updates beliefs about the distributions of both states but shifts beliefs about the first state more.

[17]In Section 5 we show that the main empirical tests, discussed below, hold for subjects who are naïve along relevant dimensions at baseline. We also show that treated subjects updated their beliefs about time use to move closer to the truth and that this updating was concentrated among subjects we classify as being belief-biased.

When applied to the decision problem described above, the $\hat{\beta}$ of a discounting-based dynamically inconsistent decision maker only affects her willingness to commit—not the allocative choices that she makes when either committed or uncommitted. Thus the information treatment should not have any effect on procrastination.

Hypothesis DB1. Information provision will have no effect on work allocations for discounting-based dynamically inconsistent agents. Formally, $\tilde{D}_2(\theta_{2,1}) - D_2(\theta_{2,1}) \perp D_1(\theta_{1,1})$.

While β affects the solution to the problem in equation (3), beliefs about β do not. Intuitively, changing beliefs about β affects what the decision-maker believes she will do in the future, but these beliefs do not affect the decision maker's incentives in the present.

While changing beliefs about present bias will not affect work allocations, changing beliefs about the distribution of time shocks will.

Hypothesis BB1. Information provision will decrease procrastination for belief-based dynamically inconsistent agents. Formally, $\mathbb{E}[\tilde{D}_2(\theta_{2,1}) - D_2(\theta_{2,1})]$ is decreasing in $D_1(\theta_{1,1})$.

A decision maker whose beliefs become more pessimistic modifies her choices so that her earlier decisions are more consistent with the decisions she makes later.

Our model of discounting-based dynamic inconsistency also has implications for how commitment demand changes after receiving information. Suppose this decision maker is choosing between the payoffs from equations (3) and (1). To match the experimental design given in Section 3, we model commitment demand as the number of extra tasks a subject is willing to complete in order to have her committed decision implemented. Mathematically, this is the k such that

$$\begin{aligned}
&\mathbb{E}\left[(w_{t,C} + k(\hat{\beta}) + \theta_1)^2 + \delta(w - w_{t,C} + k(\hat{\beta}) + \theta_2)^2\right] \\
&\quad = \mathbb{E}\left[(w_{t,NC}(\theta_1; \hat{\beta}) + \theta_1)^2 + \delta(w - w_{t,NC}(\theta_1; \hat{\beta}) + \theta_2)^2\right].
\end{aligned}$$

The decision maker's beliefs $\hat{\beta}$ about her present bias affect her beliefs about what she will choose just before the tasks need to be completed.[18] Therefore, they affect demand for commitment.

[18] The equation determines demand for full commitment. In the experiment, commitment demand is stochastic to preserve incentive compatibility of all choices. Subjects choose whether to be

Hypothesis DB2. Information provision will increase commitment demand for discounting-based dynamically inconsistent individuals. Formally, $\tilde{k} - k$ is increasing in $D_1(\theta_{1,1})$.

The reasoning behind this result is standard. As $\hat{\beta}$ falls, the payoffs that the decision maker expects to receive from the non-committed choice also fall. Since $\hat{\beta}$ only affects committed payoffs through the demand for commitment, $k(\hat{\beta})$ must increase proportionately to the signal the decision maker receives about her own level of procrastination.

The effect of information provision on the commitment demand of an individual with biased beliefs over time shocks is more ambiguous; in general a clear prediction cannot be made without stronger assumptions either on the structure of decision makers' cost functions, or how beliefs are updated. Even a first order stochastic dominant shift in the distribution can increase or decrease demand.

3 Experimental Design

We implemented a longitudinal experiment to test these hypotheses. Undergraduate subjects were recruited through an online system to four different sessions across a school term. Each session lasted for two weeks. To complete the study, subjects were required to complete eight surveys on the mornings of Monday through Thursday of each week and four sets of tasks in the evenings of Monday and Wednesday of each week. All surveys and tasks were distributed through Qualtrics. The experimental instructions and surveys can be found in Appendix C.

To begin, subjects completed an introductory session in a lab. Subjects were first read an overview of the timeline and requirements of the study. They then logged on to the computer to complete a survey that included basic demographic information as well as a measure of present bias. The survey then presented five sample tasks for subjects to complete, and explained how the allocation and commitment decisions would be made. Finally, subjects were required to complete a comprehension quiz before advancing.

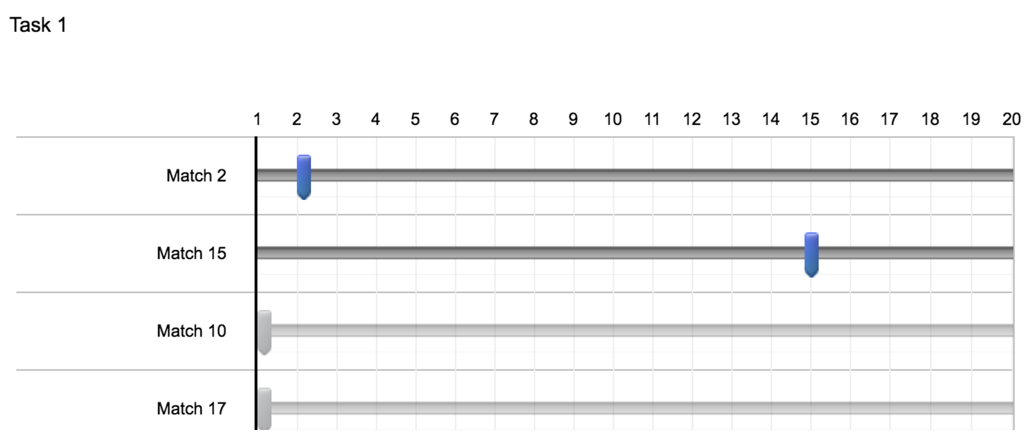
After the introductory session, all surveys and tasks were completed outside the

committed with a probability of 4/5. Adding this complication does not alter the main conclusion that beliefs about $\hat{\beta}$ affect commitment demand, and we present the full commitment version for legibility.

lab on subjects' own devices.[19] Subjects were required to complete surveys and tasks at particular times. A link to each survey was sent out at 6 a.m., and subjects were instructed to complete the survey before noon that day. At noon, subjects who had not completed the task were sent a reminder and had two hours to complete the survey. If they did not complete the survey by 2 p.m., they were dropped from the study.[20] Morning surveys contained no incentivized elicitation of beliefs about α (time shocks) because of anchoring concerns. Augenblick and Rabin (2018) show that such anchoring can itself act as a commitment device. As an alternative, we asked an indirect question about subjects' expected busyness that day, and results using this measure are discussed in Section 5.4. A link to the tasks was sent out at 9 p.m. and tasks had to be completed before 4 a.m. the next morning.

3.1 Tasks

Figure 1: The first four sliders of a task



Notes: The figure shows the beginning of a task, showing the first four required sliders. Two of the sliders have been aligned and two remain to be aligned.

The tasks that subjects were required to complete consisted of moving sliders to match particular, predetermined levels. Slider tasks have proved useful in experimental settings as tasks that require real effort and focus from subjects (Gill and Prowse,

[19] While it was possible to complete the surveys on a smartphone, the task interface was easier to use on a computer.

[20] We analyze attrition in Section 4 and find no evidence of selection on observables. Including subjects who missed a deadline, but eventually completed all surveys, does not substantially change the empirical estimates in Section 5.

2012). In other experimental work, subjects have been required to set each slider to its midpoint, with the sliders offset to make the task more difficult. The software we employed did not allow sliders to be offset, so the required level of each slider was varied to increase difficulty.[21] The order of the sliders was randomized for the same reason.

A single task consisted of moving nineteen sliders. Each page included no more than 10 tasks, and subjects were unable to proceed to the next page if the current page was incomplete or if there were any errors. If subjects tried to proceed in these cases, they were informed that the task had a problem, but were not told which slider was incorrect. Figure 1 presents the first four sliders of an example task. The tasks were designed so that each would take about one minute to complete. The actual median time spent per task by subjects was 1 minute and 20 seconds.

3.2 Allocation Decisions and Commitment

Subjects made two allocation decisions each week. Each allocation decision consisted of dividing 10 tasks between Monday and Wednesday evenings. On each of these evenings, subjects had to complete the tasks allocated to that evening in addition to a number of mandatory tasks, which are described below. The first allocation was made when completing a survey on Monday morning, imposing at least a seven hour delay between when the allocative decision was made and when the tasks were actually carried out. The second allocation was made immediately before completing the tasks on Monday evening.

In addition to allocating tasks across evenings, subjects were also offered the chance to "commit," increasing the probability that the morning allocation would be the one implemented. If the subjects did not commit they had a one-in-five chance of the morning allocation being implemented. If the subjects did commit this probability rose to four out of five. The commitment was probabilistic rather than deterministic to preserve the incentive compatibility of the evening choices.[22]

To elicit subjects' demand for commitment, they were given the choice of whether

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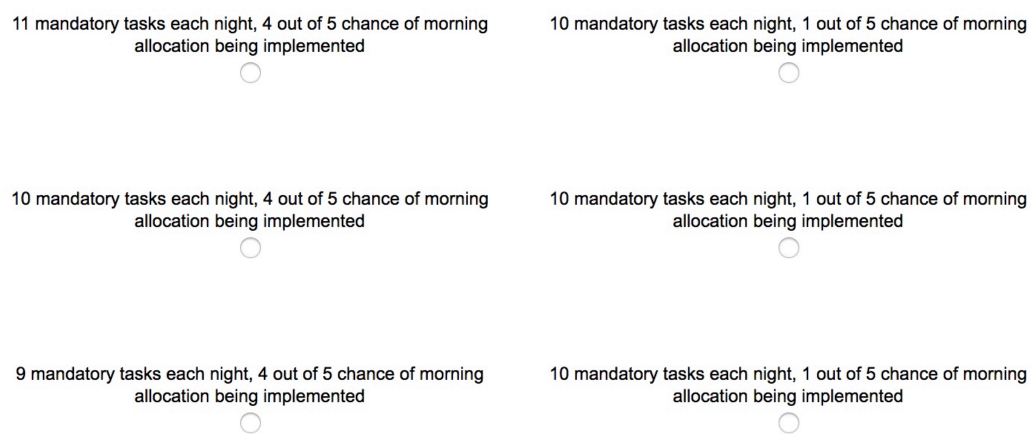
[21]Each slider was initialized at the number one, but had to be clicked before it became active. To avoid subjects becoming confused by their tasks not being accepted due to an inactive slider, the number one was omitted from the potential target levels.

[22]In principle, commitment devices outside the experiment could have undermined the incentive compatibility of the evening allocation. In Section 5.4 we test for such external commitment and find no evidence for it.

or not to commit at a variety of prices, both positive and negative. Due to previous work, including Augenblick et al. (2015), suggesting that many subjects' money-denominated willingness to pay for commitment is near 0, the prices were denominated in terms of mandatory tasks that would have to be done each night in addition to the tasks that were allocated to that night. Mandatory tasks could potentially vary between 4 and 16, depending on a subject's choices and which choice was implemented. A portion of the price list subjects faced can be seen in Figure 2.

Figure 2: Commitment price list



Notes: Subjects were asked to choose between pairs of task allocations. The choices in the left column also committed the subjects (with a higher probability) to carrying out their morning allocations.

3.3 Bedtime and Time Use Measurements

Both expected and actual bedtimes were elicited from subjects. In each morning survey, subjects were asked when they went to sleep the night before. Additionally, in both the morning and evening surveys subjects were asked at what time they expected to go to sleep that night. Again these predictions were deliberately not incentivized. An incentivized prediction could have functioned as a commitment device, and we wanted to make observable reductions in dynamic inconsistency by uncommitted agents. Intuitively, we wanted to see whether subjects would take the treatment to heart and apply its lessons outside the task domain.

Subjects also filled out diaries each morning describing their time use each hour for the previous day. The diaries allowed subjects to choose up to five activities for each hour from a menu.[23] In the data analysis, we allocate time uniformly over activities within the hour, yielding 12-minute resolution. This method of eliciting diaries balances precision against the limits of subjects' recall and the burden of completing the diaries. It is similar to the American Time Use Survey (ATUS) in that all subjects were asked for a sequential list of activities performed during the diary period, with responses constrained to total 24 hours. This method has been shown to yield high-quality estimates of time use (Hamermesh et al., 2005).

3.4 Information Treatment

Within each study wave, N subjects were randomly sorted and the first $N/2$ subjects were assigned to treatment.[24] In the second week of the study, treated subjects were given information about their own past choices. The treatment—a real example of which can be seen in Figure 3—consisted of three main parts. The first described the allocation choices that the individual made the week before. Subjects were told whether or not any tasks were reallocated on Monday evening. The second part reported the subject's average actual and predicted bedtimes and gave the difference between them in minutes. Finally, treated subjects were asked why someone's choices and predictions might change throughout the day. Subjects were given a blank space in which they had to type something to proceed.

The treatment information was intentionally neutral to avoid experimenter demand effects. In particular, we did not use judgmental language when describing the change in task allocation. The message was presented within the survey without an experimenter present, ruling out any physical or vocal suggestions (de Quidt et al., 2019). We provided subjects with information that they could have recorded for themselves had they chosen to do so. Finally, we did not mention commitment.

This information was given to treated subjects (and only treated subjects) on Monday morning of the second week. They were shown the information after they reported their bedtime for the previous night and made a prediction for Monday night but before they made the commitment and allocation decisions.

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[23]The activity menu included the following: class, exercising, other, sleeping, socializing, studying, TV, and working.

[24]Random sorting was based on a single draw for each subject from a uniform distribution on $[0, 1]$. No re-randomization was performed, nor was any blocking or stratification employed within study wave. For N odd, the first $(N/2) + .5$ subjects were assigned to treatment.

Figure 3: Treatment

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do 15 tasks on Monday evening and 7 tasks on Wednesday. When you were asked in the evening, you decided to do 16 on Monday, and 6 on Wednesday. **Thus, you moved 1 task from Wednesday to Monday.**

Also, on average you predicted that your bedtime would be 12:30 AM, and your actual average bedtime was 1:42 AM, **so you missed your predicted bedtime by about 72 minutes.**

Why might someone's choices and predictions change throughout the day?

There may be unforeseen things that pop up throughout the day that keep them busier than they thought or they miscalculate how long something will take

Notes: An example of an actual message that one of the treated subjects received at the beginning of week 2 of the experiment, along with the response they entered. The information was provided to subjects just before they made commitment and allocation decisions. The text given in the box is an example of a response that a subject gave to the open-ended question about why someone's choices and predictions might change. The box was empty when subjects were presented with the message.

3.5 Payments

Subjects received $40 total for completing the full study. An initial payment of $10 was made to all subjects on Thursday or Friday of the first week. The second payment of $30 was made to the subjects on Thursday or Friday of the second week, conditional on all portions of the experiment being completed on time.

4 Data

A total of 274 undergraduate subjects were recruited through an online system and completed the introductory session. Twenty-six of these subjects did not complete some surveys and left the experiment having received only the initial payment of $10. The vast majority of those who dropped out of the experiment did so in the first week of their participation. Another 39 subjects missed the completion deadlines for at least one survey, though they eventually did answer all surveys. These subjects are excluded from the primary sample, leaving a final baseline sample of 209 subjects.

Table 1 tests baseline covariate balance. Tests of differences between the two groups are corrected for multiple hypothesis testing using the procedure from List et al. (2019). We find no statistically significant differences across the two groups. The difference in gender, however, could be practically important. We control for

Table 1: Treatment-control balance

	Control Mean/(SD)	Treatment Mean/(SD)	Diff./[p-value]
Commitment demand week 1	-0.92	-0.13	-0.79
	(3.04)	(3.27)	[0.30]
Tasks deferred week 1	0.030	0.0092	0.021
	(2.84)	(1.87)	[0.95]
Bedtime difference from plan (minutes)	36.1	39.8	-3.68
	(68.7)	(53.9)	[0.89]
GPA	3.22	3.31	-0.085
	(0.49)	(0.47)	[0.59]
Female (indicator)	0.54	0.70	-0.16
	(0.50)	(0.46)	[0.12]
Study wave	2.47	2.56	-0.090
	(1.13)	(1.09)	[0.91]
Observations	100	109	

Notes: The significance of the differences is assessed using the procedure from List et al. (2019). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

gender in the regression analyses below and test for treatment heterogeneity by gender in Section 5.4.

The bottom row of Table 2 shows that about 17% of untreated subjects deferred work, consistent with discounting-based inconsistency ($\beta < 1$), belief-based inconsistency ($\hat{\alpha} < \alpha$), or both. Another 15% of subjects pulled work forward. The remaining two thirds of subjects did not alter their initial work allocation. These values lie between the monetary choice and effort choice behavior observed in Augenblick et al. (2015). In that study, 13% of monetary choices were present biased and 9% were future biased. For effort decisions (similar to our study), 33% of choices were present-biased and 21% of choices were future-biased.

We do not observe significant procrastination on average, with the means of deferred tasks near zero in both the treatment and control groups. Note however that our assumptions and hypotheses are based on differences in tasks deferred in week one—not average procrastination. Variation in week 1 task deferral allows us to estimate the effect of the information treatment conditional on different levels of procrastination.

The other rows of Table 2 show a breakdown of commitment demand among

Table 2: Untreated Task Reallocation and Commitment Demand

Price paid for commitment	(1) Proportion deferring work	(2) Proportion dynamically consistent	(3) Proportion pulling work forward
Negative	9.1	34.0	5.5
Zero	2.9	19.4	4.9
Positive	4.5	15.2	4.5
Overall	16.5	68.6	14.9

Notes: The table shows task reallocation and commitment demand behavior for all untreated subjects (all subjects in week 1 and control subjects in week 2). Figures are percentages.

untreated subjects. As noted in Section 3, subjects were able to choose between: (a) doing 10 tasks and having a low chance of being committed; or (b) doing a different number of tasks and having a high chance of being committed. The in-kind price indicated the maximum amount a subject was willing to pay in order to be commited. For instance, a commitment demand of one indicated that the subject was willing to do one extra task to be committed but was unwilling to do two. The in-kind price could take on both positive and negative values. Denominating the commitment price in tasks generated a larger spread in commitment demand than has been seen in otherwise similar experiments. Over 24% of subjects were willing to do at least one extra task in order to be committed in the first week, while 49% were willing to do at least one extra task to be flexible. 27% of subjects chose to commit at a price of zero. The full distributions of commitment demand in weeks 1 and 2 are shown in Appendix Figure 5.

The observed level of commitment demand is higher than in previous experiments that relied on monetary payments.[25] To see this, one can calculate a rough equivalent cash price as follows. The travel cost literature has estimated that people value time at roughly 72 (Lam and Small, 2001) to 93 percent (Small et al., 2005) of the wage rate. The median wage among subjects was $14, so the implied range of values was $10 to $13. As median task completion time was 1 minute 20 seconds, the cash value

[25] The experiment of Sadoff et al. (2020) offered commitment to a grocery choice at zero cost and 53% of subjects took it up. For comparison, a modestly smaller share (41%) of subjects in our experiment exhibited non-negative willingness to pay for commitment.

of one task was approximately 22 to 29 cents. In contrast, Augenblick et al. (2015) find only 9% of subjects were willing to pay $0.25 to be committed (the lowest price offered), and 10% of subjects were willing to pay $0.25 for flexibility. We find that more than twice as many subjects were willing to be committed when paying in terms of tasks.

Table 3: Observables by attrition status

	Did not finish Mean/(SD)	Finished study Mean/(SD)	Diff./[p-value]
Treat	0.44	0.52	-0.084
	(0.50)	(0.50)	[0.67]
Age	20.2	20.5	-0.23
	(1.55)	(1.83)	[0.68]
GPA	3.19	3.27	-0.074
	(0.49)	(0.48)	[0.74]
Female (indicator)	0.56	0.62	-0.060
	(0.50)	(0.49)	[0.64]
Study wave	2.38	2.52	-0.14
	(1.15)	(1.11)	[0.41]
Observations	64	209	

Notes: The significance of the differences is assessed using the procedures in List et al. (2019). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 3 summarizes attrition in the experiment. Column 1 presents means for the 64 subjects (23%) who did not complete the experiment and column 2 presents means for those who did, pooling treatment and control. There are no statistically significant differences across the two groups. Appendix Table 11 reports results from a regression of a study completion dummy on observable characteristics from our baseline survey. Point estimates are uniformly small, and in a joint test of the null hypothesis that all coefficients are zero, we fail to reject. Subjects who attrited were also no more likely to change their work allocations or display costly commitment demand. Conducting our primary empirical analyses on the baseline sample plus subjects who eventually finished all surveys yields estimates substantially similar to those reported below.

5 Primary Results: Testing Model Hypotheses

In this section, we present tests of our main hypotheses from Section 2. In Section 5.1, we test whether the information treatment caused individuals who were dynamically inconsistent in week 1 to change their work allocation decision in week 2 relative to the control group. In Section 5.2, we test whether the treatment caused individuals who were dynamically inconsistent in week 1 to change their commitment demand in week 2. The results provide evidence that is inconsistent with discounting-based models as the sole source of dynamically inconsistent behavior observed in the experiment. Instead, the evidence is consistent with some subjects behaving according to a model of discounting-based dynamic inconsistency, others behaving according to a model of belief-based dynamic inconsistency, and a third group behaving in accord with both.[26] In Section 5.3 we provide evidence supporting the assumptions underlying each of our hypothesis tests. We then classify subject behavior according to which—if any—of the models predicts it. Finally, Section 5.4 provides robustness checks for the main results.

5.1 Hypotheses DB1 and BB1 on Work Allocation Behavior

In this section we examine Hypothesis DB1 and Hypothesis BB1 by estimating variants of the following equation.

$$\text{Deferred}_{i2} = \gamma_0 + \gamma_1 \text{Treat}_{i2} + \gamma_2 \text{Deferred}_{i1} + \gamma_3 \text{Treat}_{i2}\text{Deferred}_{i1} + \mathbf{x}_i' \gamma_4 + \varepsilon_{i2} \quad (5)$$

where $\textit{Deferred}_{it}$ is the number of tasks deferred by subject i in week t, $\textit{Treat}_{i2}$ is an indicator for being in the treatment group in week 2, $\mathbf{x}_i$ is a vector of control variables, and ε_{i2} is the stochastic error term associated with this regression. We test robustness to alternative control sets across different specifications. First, we report results conditional on experimental design covariates: indicators for study wave (randomization into treatment was conditional on wave) and an indicator for whether we needed to issue a reminder for the subject to complete a morning survey during week 1.[27] Second, we add demographic variables gender, age, and age squared. Finally, we

[26]While our discussion emphasizes the differences between these mechanisms, the theory of Section 2 accommodates agents who behave consistent with both sources of time-inconsistent behavior.

[27]Subjects were instructed to complete the surveys by noon and were given one reminder before being dropped from the experiment if they had not completed the survey by 2 p.m.

add covariates for academic performance and busyness: grade point average (GPA) prior to the experiment, employment, as well as week 1 measures of study time and social time. Here and in subsequent regressions, we report heteroskedasticity-robust standard errors (White, 1980).

We account for multiple testing over our primary hypotheses using the procedure from List et al. (2019), modified by Barsbai et al. (2020) for use in regression settings. The procedure asymptotically controls the familywise error rate. In particular, we adjust for multiple testing of estimated coefficients on our regressor of interest, $Treat_{i2}Deferred_{i1}$, using 10,000 bootstrap replications.[28]

Table 4: Effect of treatment on work deferred

	(1) Work deferred week 2	(2) Work deferred week 2	(3) Work deferred week 2
Treat	0.24	0.29	0.095
	(0.37)	(0.37)	(0.38)
Work deferred week 1	0.32**	0.32**	0.32**
	(0.14)	(0.14)	(0.12)
Treat × Work deferred week 1	-0.42**	-0.44**	-0.44**
	(0.21)	(0.21)	(0.18)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
MHT-adjusted p-value on interaction	0.035	0.037	0.039
Observations	209	209	209

Notes: The table shows results from estimating equation 5. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance based on these standard errors indicated by: *** p<0.01, ** p<0.05, * p<0.1. The p-value is a multiple hypothesis-adjusted test of the null hypotheses of 0 for the coefficients on $Treat_{i2}Deferred_{i1}$ here and in Table 5 using a modified version of the procedure from List et al. (2019).

[28]The procedure was implemented in Stata 14.2 using `mhtreg` (version distributed on 2020-01-06). Because the procedure relies on bootstrap resampling, both initial and MHT-adjusted p-values typically differ from p-values derived from the heteroskedasticity-robust standard errors.

Table 4 presents results based on estimating equation (5), in which the dependent variable is work deferred in week 2. Estimates are similar across the specifications, though the inclusion of additional time use and prior academic performance covariates improves precision. The estimated treatment effect on subjects who did not defer work in week 1 is positive, but we fail to reject a zero null hypothesis at conventional significance levels. The second coefficient in each column shows that deferring work in week 1 was positively and significantly associated with deferring work in week 2.

The third coefficient is of primary interest. It shows that, for subjects who deferred work in week 1, the treatment caused a statistically significant and practically substantial reduction in deferral of tasks in week 2. This effect provides evidence against Hypothesis DB1, that information provision will have no effect on work allocation. Because a naïve β-δ agent's allocation depends on present bias, Hypothesis DB1 implies a zero coefficient on the interaction of treatment and work deferred in week one. The agent may update her belief $\hat{\beta}$, but this affects commitment demand, not task allocation. The estimates are consistent with Hypothesis BB1 that information provision will decrease the difference between present and future allocations.

This is our first empirical evidence that biased beliefs influence time-inconsistent behaviors. Underlying this test is assumption BB, that the information treatment causes subjects who are optimistic about their time shocks to update toward being less optimistic. We report evidence supporting this assumption in Section 5.3.

5.2 Hypothesis DB2 on Commitment Demand

We next test Hypothesis DB2, that treated procrastinators in week 1 will increase their commitment demand in week 2. If subjects do behave consistent with DB2, that provides evidence for discounting-based dynamic inconsistency. The estimating equation models the difference in commitment demand across weeks as a function of treatment interacted with procrastination.

$$\Delta\text{Commitment}_i = \xi_0 + \xi_1\text{Treat}_{i2} + \xi_2\text{Deferred}_{i1} + \xi_3\text{Treat}_{i2}\text{Deferred}_{i1} + \mathbf{x}_i'\xi_4 + \nu_i \quad (6)$$

In the equation above, $\Delta\mathit{Commitment}_i$ is the change in commitment for subject i (week 2 minus week 1) and ν_i is the stochastic error term for this regression. All other right-hand side variables are the same as in equation (5). Again we adjust for multiple hypothesis testing of coefficients on $\mathit{Treat}_{i2}\mathit{Deferred}_{i1}$ using the procedure from List et al. (2019), modified by Barsbai et al. (2020) for use in a multivariate regression setting.

Table 5: Effect of treatment on commitment demand

	(1) Change in commitment demand	(2) Change in commitment demand	(3) Change in commitment demand
Treat	-0.0051	0.0074	0.00019
	(0.42)	(0.43)	(0.44)
Work deferred week 1	0.016	0.020	0.0024
	(0.10)	(0.10)	(0.11)
Treat × Work deferred week 1	0.45**	0.44**	0.47**
	(0.18)	(0.18)	(0.18)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
MHT-adjusted p-value on interaction	0.047	0.046	0.026
Observations	209	209	209

Notes: The table shows results from estimating equation 6. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance based on these standard errors indicated by: *** p<0.01, ** p<0.05, * p<0.1. The p-value is a multiple hypothesis-adjusted test of the null hypotheses of 0 for the coefficients on $Treat_{i2}Deferred_{i1}$ here and in Table 4 using a modified version of the procedure from List et al. (2019).

The corresponding estimates appear in Table 5. Consistent with theory, the first row shows that the effect of treatment on subjects who deferred zero tasks in week 1 was small. Similarly the second row shows that for control subjects, week 1 procrastination was not associated with the change in commitment demand.

The third row shows the interaction of treatment with tasks deferred in week one. The estimated coefficient on the interaction term is positive and statistically significant at the five percent level. This is consistent with Hypothesis DB2. Our model predicts that when a present-biased individual receives information, she will update her belief over her present bias and increase her commitment demand in response.[29]

[29] As noted in Section 4, some of the dynamic inconsistency in our experiment is future-biased.

The addition of demographic and other controls produces minimal changes in the estimated coefficients.

5.3 Corroborating Evidence and Classification

The results presented above refute the hypothesis that discounting alone drives dynamic inconsistency. Both beliefs and discounting are important for explaining behavior in our setting. In this section we present more direct evidence that the treatment message changed beliefs and that estimated treatment effects are driven by subjects who were naïve *ex ante*. Finally we classify dynamically inconsistent, treated subjects' behavior as belief-based, discounting-based, both, or neither.

The first piece of evidence indicating that treatment affected beliefs appears in Table 6. As discussed in Section 3, incentivized elicitation of beliefs over time shocks would have been inadvisable due to the potential for responses to act as a commitment device (Augenblick and Rabin, 2018). Instead, each morning survey asked an indirect question about how busy subjects expected to be during the day. Table 6 reports changes in expected busyness on day 10 (post-treatment) as a function of treatment.[30] Control-group subjects who deferred more work in week 1 did not change their beliefs in week 2. Treated subjects who deferred more work in the first week reported that they expected to be busier in the second week. In other words, treated procrastinators updated their beliefs in the direction of expecting larger time shocks. In contrast, procrastinators who did not receive timely, targeted information did not change their reported expectations.

Second, Appendix Table 16 presents estimates similar to Table 4, but the dependent variables are tasks chosen Monday morning of week 2 or tasks chosen Monday evening of week 2. Estimated effects on morning choices are similar to the headline effects found in Table 4, though not significant. Subjects who were reminded that they deferred work in the prior week planned less work for the first night when asked in the morning but more when asked just before doing the work. This behavior indi-

If our treatment caused $\hat{\beta}$ to be greater than one for these subjects, we might expect that they would *increase* their commitment demand. We investigate this possibility in Table 12 but do not find evidence for it. Instead, we find that the treatment caused these subjects to decrease their commitment demand.

[30] All regressions are conditional on reported busyness the week before (day 3) and just before the information treatment was given to the treated group (day 8). The estimates showing these two coefficients are Table 26.

Table 6: Changes in expected busyness

	(1) Busyness day 10	(2) Busyness day 10	(3) Busyness day 10
Treat	-0.20	-0.19	-0.19
	(0.23)	(0.24)	(0.24)
Work deferred week 1	-0.026	-0.027	-0.037
	(0.069)	(0.069)	(0.068)
Treat × Work deferred week 1	0.22**	0.22**	0.22**
	(0.098)	(0.097)	(0.098)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: The table shows results from estimating equation 5, but with the dependent variable replaced by self-reported expected busyness on day 10 (after treatment). Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

cates that treatment operated primarily by affecting work plans.[31] This evidence is inconsistent with Hypothesis DB1 but consistent with Hypothesis BB1.

Third, related evidence on how treatment affected beliefs can be found in Section 6. There, we show that treated subjects revised their bedtime predictions in week 2, leading to much higher accuracy compared to control subjects. Both of these results indicate that the treatment had an impact on beliefs.

We now turn to the question of whether beliefs changed in the directions implied by assumptions DB and DB. Using commitment demand and task allocation behavior, we can classify individual subjects as time consistent, discounting-based time inconsistent, belief-based time inconsistent, both discounting- and belief-based time inconsistent, or neither discounting- nor belief-based time inconsistent. The procedure works as follows. First, we match each treatment group subject to a single, nearest

[31]These results are consistent with the information treatment affecting beliefs about shocks on *both* nights, although with a stronger effect on beliefs about Monday night.

neighbor control group subject using propensity score. Matching depends on commitment demand and tasks deferred as well as GPA, gender, and experiment wave. Next, we use individual-level versions of the regressions above to classify subjects' behavior.[32] If a time inconsistent treatment group member increased her commitment demand more than her control group peer, we classify the subject's behavior as discounting-based. If a time inconsistent treatment group member reduced the amount of work she deferred in week 2 more than her control group peer, then we classify the subject's behavior as belief-based.

Using this taxonomy, we find that conditional on changing their work allocation in week 1, 85% of subjects exhibited behavior consistent with one or both mechanisms. 25% of subjects behaved consistent with discounting-based dynamic inconsistency. 39% of subjects behaved consistent with belief-based dynamic inconsistency, and 21% of subjects behaved consistent with both.[33]

This classification allows us to estimate changes in beliefs by subgroup. Results appear in Appendix Table 26. Self-reported busyness increased among subjects classified as belief-based, and among those classified as belief- and discounting-based. These estimates are consistent with Assumption BB, though they are not statistically significant. The estimate for discounting-based subjects is substantially smaller and negative. This is inconsistent with the intuitive hypothesis that discounting-based subjects with ex ante unbiased beliefs over time shocks might have mistakenly revised their beliefs in response to treatment, leading to reduced procrastination.

Finally, our model implies that the treatment effect will be driven by naifs along at least one dimension: beliefs about present biased discounting ($\hat{\beta}$) or about time shocks ($\hat{\alpha}$). For belief-based agents, the model assumes that the information treatment will affect those who are optimistic about their time shocks. To test this mechanism directly, we estimate regressions in which treatment interacts not only with task de-

[32]In addition to the individual-level regressions, we also estimate a finite mixture model with four latent classes. These results are reported in Table 17 of Appendix A.

[33]Under the null hypothesis that our data were generated by uniform random noise over the possible levels of work deferral and commitment demand, we would expect that 28% of subjects would be classified as not acting consistently with either model, 25% of subjects would be classified as being discounting-based dynamically inconsistent, 25% would be classified as belief-based dynamic inconsistent, and the remaining 22% would be classified as both. We reject a test of equality of this distribution and our empirical distribution ($p = 0.03$, $\chi^2 = 4.0$ with 1 degree of freedom). Due to the fact that we classify treated subjects based on their behavior being strictly different than their untreated peer's behavior, this test is more conservative than one based on a null hypothesis where a higher proportion of subjects choose to defer 0 tasks or leave their commitment demand unchanged.

ferral, but also with beliefs over time shocks.[34] Table 7 presents results from these regressions.[35] Recall that subjects who deferred work in week 1 might have been motivated by belief-based dynamic inconsistency, discounting-based dynamic inconsistency, or both. In column 1, effects on task deferral are driven by the coefficient on *Treat* $\times$ *Work deferred week 1* (-0.82). That is, the most responsive subjects were those who deferred tasks (consistent with either mechanism, or both) and did not expect to be busy (consistent with lower $\hat{\alpha}$ relative to other subjects). This evidence provides additional support for Hypothesis BB1 and fails to falsify Assumption BB.

In column 2, effects on commitment demand are driven by the coefficient on *Treat* $\times$ *Work deferred week 1* $\times$ *Expect busy day* (0.59). That is, the most responsive subjects were those who deferred tasks (consistent with either mechanism, or both) and expected to be busy (consistent with higher $\hat{\alpha}$ relative to other subjects and discounting-based dynamic inconsistency). In contrast, the effect of treatment on subjects who deferred tasks but did not expect a busy day was small and positive (0.10). This evidence provides additional support for Hypothesis DB1.

In Appendix Table 15, we similarly test whether treatment effects on commitment demand were larger for subjects with *ex ante* naïve beliefs about their own procrastination. These regressions employ two proxies for sophistication. One is an indicator that equals 1 if, at baseline, subjects responded "yes" to a question asking whether they tend to procrastinate. The second proxy is the number of nights the subject reported that they wake up wishing they had gone to bed earlier. Results are consistent with the model prediction that the treatment effect will be stronger for naifs.

5.4 Robustness Checks

The initial checks of robustness to specification changes appear in Tables 4 and 5. As noted in Section 4, our sample displays a subjectively large treatment-control difference on one potentially important demographic characteristic: gender. The specifications with gender controls in Tables 4 and 5, however, show that this imbalance did not have a substantial effect on the estimates. Appendix Tables 18 and 19 interact treatment variables with gender. We fail to reject a null of zero difference

[34] These regressions employ an indicator that equals one for subjects who reported expected busyness below the median on the first day of the experiment.

[35] The reported coefficients highlight interactions of treatment with work deferral and self-reported busyness. Appendix Table 14 reports complete results, including all levels and interactions of treatment, busyness, and work deferred in week 1.

Table 7: Treatment interactions with naïvete over time shocks

	(1) Work deferred week 2	(2) Change in commitment demand
Treat	-0.061	-0.61
	(0.49)	(0.58)
Treat × Work deferred week 1	-0.82**	0.10
	(0.41)	(0.16)
Treat × Expect busy day	0.50	1.02
	(0.75)	(0.81)
Treat × Work deferred week 1 × Expect busy day	0.59	0.59**
	(0.45)	(0.28)
Level and other interaction terms	Yes	Yes
Design controls	Yes	Yes
Observations	209	209

Notes: The table shows coefficients from estimating modified versions of equations 5 and 6 that include interactions with work deferral and self-reported busyness. The reported coefficients highlight interactions of treatment with these variables. Appendix Table 14 reports complete results, including all levels and interactions of treatment, busyness, and work deferred in week 1. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

in the marginal effects of treatment on females and males. In addition, we report variants of our primary specifications without any control variables in Table 13. As we discuss in Section 3, the study was randomized independently within study wave, so these uncontrolled regressions give estimates almost identical to those from Tables 4 and 5.

To evaluate whether our results arise from outliers, we Winsorize the upper and lower 2.5% of observations on tasks deferred and commitment demand in both weeks, then re-estimate our primary specifications. Appendix Tables 20 and 21 show that under this procedure point estimates are strongly similar and they remain statistically significant at conventional levels.

Appendix Section B reports the main results, excluding subjects who switched multiple times when choosing from the commitment price list. (In the results reported in the body of the paper, we include all subjects.) If at any point the subject violated the law of demand, we classify the subject as a multiple switcher. Appendix Section B shows that whether or not these subjects are excluded, the results remain substantively unchanged.

We assess potential experimenter demand effects using three different empirical tests. The first is based on the *Treat* coefficients in our primary results (Tables 4 and 5). As treatment effects on non-procrastinators potentially reflect multiple mechanisms and are imprecisely estimated, we do not emphasize them. Note, however, that these point estimates are inconsistent with what one would expect based solely on experimenter demand: receipt of the treatment message by a non-procrastinator increases work deferred and has a negligible effect commitment demand. This rules out cases in which experimenter demand dominates the responses of non-procrastinators. Our second test takes advantage of the fact that treatment messages reported both work reallocation and bedtime. Assuming that experimenter demand effects would have been stronger when both reports were consistent and larger—for instance, when the treatment message indicated that the subject both deferred tasks and delayed bedtime—then we can assess experimenter demand by interacting bedtime prediction error with the other variables in equations (5) and (6). We report these interaction models in Table 22. In neither case does the interaction between the bedtime error message and tasks deferred in week 1 substantially change the conclusions from the baseline analysis. These results rule out intensive-margin experimenter demand responses. Our third test takes advantage of over-reporting of study time, defined as the average difference over eight days between study time reported in response to a direct question and study time as measured by the subject's time diary (in hours).[36] This is plausibly a good proxy for responsiveness to experimenter demand because subjects were undergraduate students, being asked about study time in a survey conducted by graduate students and a professor. The over-report is interacted with the other variables in equations (5) and (6); results appear in Table 23. Again the coefficients of interest from our primary analysis are substantially unchanged, suggesting that experimenter demand is not a first-order driver of our primary results.

It is possible that our treatment caused subjects to show a preference for consistency. In prior work, Augenblick and Rabin (2018) show that subjects make effort choices in a previously predicted window almost 100 percent of the time, and Falk and Zimmermann (2011) find that more than half of the subjects in a dictator game

[36]Research in survey methods has found that diaries reduce bias in time use measurement (Hamermesh et al., 2005, Frazis and Stewart, 2012). Such diaries are used in high-quality surveys like the American Time Use Survey. In our setting, study time was 1 of 8 possible activities and subjects could report up to 5 activities per hour, so study time was not made particularly salient.

exactly match an incentivized choice to an earlier unincentivized choice for the same game. For this to generate our results, the treatment would have had to increase subjects' tendency to match the decision they were about to make to the decision they would make in the future. Subjects might have viewed this consistency as a way to signal ability to themselves or the experimenter (Falk and Zimmermann, 2017). To assess this narrative empirically, we estimate the effect of treatment on the likelihood that a subject made consistent choices in the second week and report results in Table 24. Estimated treatment effects are all near zero and insignificant, suggesting that our treatment did not induce the sort of preference for consistency seen in previous work. Investigating further, Table 25 shows estimates of equation 5 based only on subjects who were inconsistent in the second week. If anything, the effect of treatment is larger on subjects who were not consistent in the second week, again suggesting that a preference for consistency is not responsible for our results.

Finally, it is possible that subjects sought out commitment devices external to the experiment. *A priori* we view this as unlikely because the experiment offered internal commitment at such low cost (see Section 4). Nonetheless we take advantage of our randomized commitment price to test for the hypothesized behavior. A subject facing a high internal (within-experiment) commitment price may commit externally and so will not defer work. A subject facing a low internal commitment price will commit internally, but may still defer work in the evening allocation because internal commitment is probabilistic. This theory predicts a negative relationship between the randomized price of internal commitment and tasks deferred. Appendix Table 27 reports the corresponding regression results. Coefficient estimates on commitment price are very close to zero and the associated t statistics are substantially less than one, inconsistent with the prediction based on external commitment.

6 Secondary Results: Effects on Real-World Behavior

To this point we have focused on testing theoretical predictions about task choices and commitment. These analyses allow for a close connection between theory and data, but our experimental design also allows us to study procrastination behavior in another domain—real-world time use. In particular, we study bedtime, which has important effects on sleep duration and well-being (Gibson and Shrader, 2018). Because wakeup times are constrained by work and school start times, going to bed later typically results in less sleep. A large literature in has found sleep deprivation causes significant performance deficits (Van Dongen et al., 2003) and impairs health (Cappuccio et al., 2010). Delayed and irregular bedtimes are also positively associated with difficulty falling asleep and poor sleep quality (Kang and Chen, 2009, Ohayon and Sagales, 2010). Despite these deleterious effects, people frequently revise their initial plans and delay bedtime (Kroese et al., 2014, Kühnel et al., 2018). More generally, our panel time-use data allow evaluation of how changes in commitment price alter the entire time allocation.

6.1 Effect of Treatment on Bedtime Forecast Error

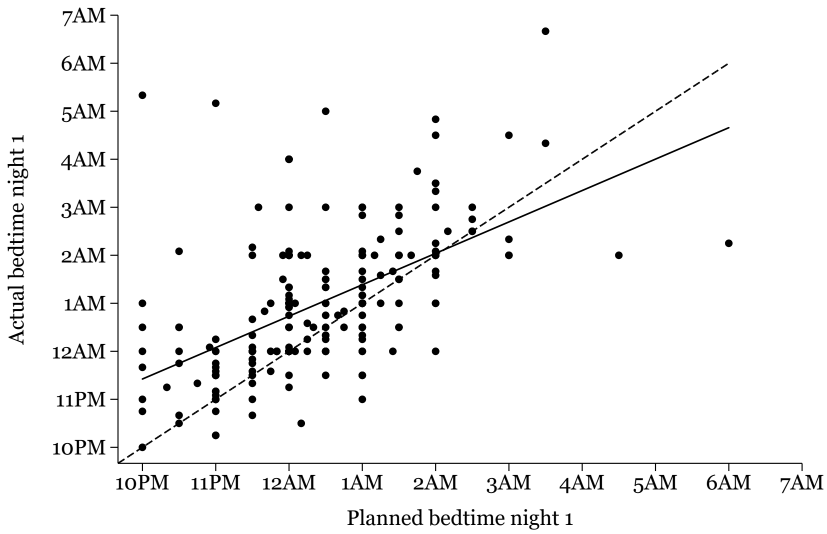
Table 1 provides initial evidence that subjects procrastinate around bedtime in our setting. The average values for "Bedtime difference from plan" in Table 1 indicate that subjects missed their planned bedtimes by 36 to 40 minutes. In a regression of this variable on a constant, the pooled mean of 38 minutes is significantly different from zero (the robust standard error is 4.24). Subjects were not incentivized to meet their bedtime plans, so this behavior plausibly reflects subjects' decision making in a non-experimental setting. It is possible in principle that asking for bedtime predictions generated anchoring effects (Tversky and Kahneman, 1992), but such effects would have reduced forecast errors for both treatment and control subjects.

Delving deeper, Figure 4 compares individual planned bedtimes (horizontal axis) to self-reported actual bedtimes (vertical axis) for the first night of the study.[37] The majority of subjects missed their planned bedtime and appear as points above the 45-degree line, corroborating the earlier finding that subjects are generally optimistic about their bedtime plans. A linear fit (solid, black line) shows that, on average, subjects underestimated their bedtime earlier in the evening and tended to overestimate it later in the evening, though the overestimation is supported by relatively few observations.

Figure 4 displays a single night's planned and actual bedtimes in order to highlight three types of noise in the data. First, there is considerable round-number heaping in both planned and actual bedtimes. Planned bedtimes in particular were likely to fall on the hour or half hour. Second, 35 subjects reported actual bedtime exactly equal

[37] Our choice of the first night is arbitrary; analogous figures for other nights look strongly similar. Comparison of bedtime plans, self-reports and readings from sleep monitors suggests subjects frequently failed to correctly enter a.m. or p.m. For both planned and self-reported bedtimes, we assume reports in the range from 10 a.m. to 3 p.m. reflect this type of error.

Figure 4: Planned and actual bedtimes, first night



Notes: The figure shows planned bedtime (x-axis) versus actual bedtime (y-axis) for all observations on the first night of the study. Points above the 45 degree line (dashed), indicate that subjects went to bed later than their stated plan. A linear fit (solid, black line) shows that, on average, subjects underestimated their planned bedtime when going to bed earlier in the evening and overestimated later in the evening.

to planned bedtime. Although these subjects might have gone to bed around the time that they planned, an exact match between plan and realization could reflect misreporting. Finally, some subjects reported extreme bedtime plans and realizations. On this night, for instance, 9 subjects reported going to bed later than 4 a.m. Although extreme bedtime values were not necessarily in error, erroneous extreme bedtimes could exert disproportionate influence on regression analysis.

As previously described in Section 3.4, treated subjects were given information about their own time use decisions at the beginning of week 2 of the experiment. To test whether this reduced procrastination, we re-estimate equation (5) using forecast error as the outcome.[38] To reduce the influence of noise in the bedtime data highlighted above, we employ a trichotomized measure of forecast error. Bedtime forecast error is classified into one group if the subject underestimated her bedtime, a second group if her prediction exactly matched her actual bedtime, and a third group if she

[38]A new piece of evidence such as our treatment can affect beliefs even in cases where the subject has received substantial prior evidence if the subject's model is misspecified (Heidhues et al., 2020).

overestimated her bedtime. Table 8 shows results from estimating the effect of treatment on this trichotomized bedtime error variable, using an ordered logit model.[39] On average, treated subjects moved to lower categories, representing bedtime equal to or earlier than forecast. There are multiple potential mechanisms for this change, including changes in beliefs over time shocks. Movement across categories was larger for those with larger week 1 forecast errors.

Table 8: Effect of treatment on bedtime forecast error

	(1) Discrete forecast error week 2	(2) Discrete forecast error week 2	(3) Discrete forecast error week 2
Treat	-0.54*	-0.53*	-0.51
	(0.30)	(0.31)	(0.31)
Forecast error week 1	0.74**	0.73**	0.95***
	(0.29)	(0.30)	(0.27)
Treat × Forecast error week 1	-0.88**	-0.87**	-1.19***
	(0.37)	(0.38)	(0.36)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	202	202	202

Notes: Subjects who do not go to sleep at all are excluded from the sample. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Marginal effects of treatment, reported in Table 9, allow us to assess the magnitudes of these responses and see whether subjects moved into the group with bedtime equal to forecast, or the group with bedtime before forecast. Column 1 gives marginal effects of treatment with week 1 error at its mean, which is equal to zero because this variable is standardized. Treatment reduced the probability of going to bed later than

[39] Subjects who do not go to sleep at all are excluded from the sample, as their forecast errors are not well defined. Most of these subjects report studying through the night. Assigning these subjects to the first group does not meaningfully change the estimates. Appendix Table 28 reports results without the adjustment of planned bedtimes between 10 a.m. and 3 p.m.. Estimates are strongly similar in both magnitude and statistical significance.

forecast by 12 percentage points, a large change compared to the predicted probability of 62 percent with all variables at their means. The offsetting increases were split roughly equally over the other two categories. Column 2 gives marginal effects of treatment with week 1 error equal to 1 (1 standard deviation above the mean). Comparing to column 1, two differences are apparent. The reduction in the probability of going to bed late is greater, and the increase in the probability of going to bed on time is greater than the increase in the probability of going to bed early (though the latter two are not statistically distinguishable). That is, week 1 procrastinators reduced week 2 procrastination more than did other subjects, and the principal mechanism was going to bed on time. Finally column 3 gives marginal effects of treatment with week 1 error equal to 2 (2 standard deviations above the mean). Here the pattern of column 2 becomes still more pronounced. The probability of procrastination falls by 44 percentage points, and the offsetting increase in the probability of going to bed on time is 10 percentage points greater than the increase in the probability of going to bed early. Again this suggests procrastinators responded more strongly to treatment, and that they did so primarily by going to bed on time.

Table 9: Marginal effect of treatment, varying week 1 forecast error

	(1) Week 1 forecast error at mean	(2) Week 1 forecast error 1 s.d. higher	(3) Week 1 forecast error 2 s.d. higher
Pr(Bedtime $<$ forecast)	0.057	0.13***	0.17**
	(0.035)	(0.046)	(0.071)
Pr(Bedtime $=$ forecast)	0.059	0.17***	0.27***
	(0.037)	(0.067)	(0.10)
Pr(Bedtime $>$ forecast)	-0.12*	-0.31***	-0.44***
	(0.070)	(0.10)	(0.16)
Observations	202	202	202

Notes: Subjects who do not go to sleep at all are excluded from the sample. Each column of the table shows the marginal effects of treatment, estimated based on column 1 of Table 8, on the probabilities of being in each of the three error categories: bedtime prior to forecast, bedtime equal to forecast, and bedtime after forecast. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Taken together, our bedtime results indicate that the type of dynamic inconsistency we study matters for consequential real-world decisions. They corroborate our task-based results showing that information provision can alter procrastination behavior, suggesting a purely discounting-based model of dynamic inconsistency is incomplete.

6.2 Effect of Treatment on Other Activities

Bedtime is not the only element of an agent's time allocation problem that may be influenced by time shocks and beliefs over those shocks. Indeed time shocks may influence an agent's entire time allocation, and optimizing responses to such shocks are substantially understudied. These responses plausibly matter for welfare, for example because shifts in within-day time use can affect health and productivity (Bessone et al., 2019).

Using time budget recall data, we are able to investigate the effect of an experimentally administered time shock on other time use choices. The randomly assigned shock that subjects face is the price at which they are offered commitment (i.e. the line of the price list which is implemented). Empirically, a one-unit increase in the price of commitment is associated with a subject needing to complete an additional two-thirds of a task. Each task took about 1.3 minutes for the median participant and 2.2 minutes for the mean participant in the study, so each one-unit change in commitment price is equivalent to about 1 to 1.5 minutes of induced experimental time. This randomization allows for a series of tests on realized time use. While these are interesting reduced-form exercises based on random variation, they are not theoretically founded and results should be interpreted cautiously.

We estimate equations of the form

$$\Delta\text{Time}_i = \alpha_0 + \alpha_1 \Delta\text{Time Shock}_i + \mathbf{x}_i'\alpha_2 + \upsilon_i \tag{7}$$

In the above equation $\Delta\, Time_i$ is the change in time spent on a given activity between weeks 1 and 2, $\Delta\, Time\ Shock_i$ is the change in commitment price between the two weeks, υ_i is the stochastic error term associated with this regression, and all other variables are the same as in equation (5).

Table 10 reports the estimated effects of a marginal increase in commitment price on time use, measured in minutes per day. Within this table, we correct for multiple testing using the procedure of List et al. (2019), as modified by Barsbai et al.

Table 10: Time use and commitment price

	Class	Exercising	Other	Sleeping
Time Shock diff.	-0.73	0.38	5.08	-1.28
	(1.31)	(0.54)	(2.31)	(1.46)
Observations	209	209	209	209
MHT-adjusted p-value	0.84	0.96	0.29	0.95

	Socializing	Studying	TV	Working
Time Shock diff.	1.26	-5.17	2.12	-1.65
	(1.94)	(2.54)	(1.23)	(0.92)
Observations	209	209	209	209
MHT-adjusted p-value	0.66	0.19	0.30	0.20

Notes: The table shows results from estimating equation 7. Each column is a separate regression. Time use changes are in minutes per day. All regressions contain the study design controls: an indicator for receiving a submission reminder in week 1 and indicators for study wave. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Multiple hypothesis testing adjustment is carried out on all tests and the p-values are based on 10,000 bootstrap replications. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

(2020), with 10,000 bootstrap replications. After adjustment, none of the results are statistically significant at conventional levels, but point estimates are nonetheless instructive. When the price of commitment increased by one task (roughly 1 to 1.5 minutes), studying decreased by roughly five minutes. “Other time” and television time increased by roughly 5 and 2 minutes, respectively; the latter result is consistent with TV being a time use luxury, as found in Aguiar et al. (2017). This pattern of results is potentially consistent with present-biased preferences, as subjects substituted toward immediately pleasurable time uses and away from a time use with largely deferred payoffs.

7 Conclusion

This paper models agents whose dynamic inconsistency potentially arises from two sources: discounting and beliefs. Agents with optimistic beliefs about future time shocks will exhibit dynamically-inconsistent choices over effort that are observationally equivalent to those driven by present bias. An informational intervention that tells agents about their past time inconsistency, however, will yield different behavior for these two biases. Optimistic agents will change effort allocations, but agents with present bias will not. Present biased agents will increase commitment demand, while optimistic agents will not necessarily do so.

We test these different predictions experimentally and find that both preferences and beliefs matter for time inconsistency. The results help explain puzzlingly low take-up of costly commitment. Perhaps more importantly, they offer an alternative policy prescription to help overcome time-inconsistent behavior—providing information on agents' own past execution of their plans just prior to a new decision.

The welfare effects of an informational treatment on behaviorally biased agents are unclear. To the extent that information pushes a naïvely present-biased decision maker toward sophistication, she will be better able to make plans that account for her present-biased future self. On the other hand, if optimistic beliefs are directly valuable to decision makers, giving them clear evidence their beliefs are biased could make them worse off. One avenue for future research is to identify situations in which subjects demand this information, and how it can be structured to reduce time inconsistency with minimal associated welfare losses.

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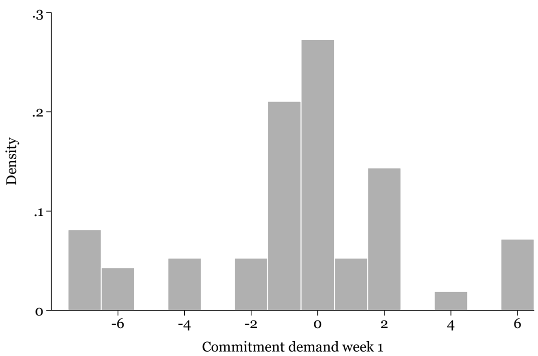
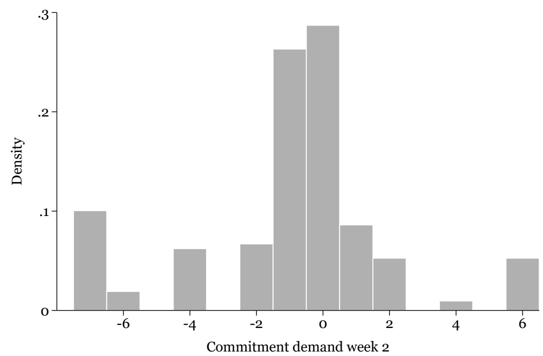
Appendix for online publication

A Appendix figures and tables

Figure 5: Distribution of commitment demand



(a) Commitment demand in week 1



(b) Commitment demand in week 2

Notes: The figure shows the distribution of commitment demand. Panel (a) shows week 1, before treatment. Panel (b) shows week 2, after treatment. The x-axis shows the *maximum* price the subject was willing to pay for commitment in terms of extra tasks. A commitment demand of one indicates that the subject was willing to do one extra task to be committed, but was unwilling to do two. The in-kind price could take on both positive and negative values. Subjects who were unwilling to commit even if it lowered the number of tasks they had to do by six were assigned a commitment demand of negative seven.

Table 11: Regression of study completion dummy on observables

	Finished study
Treat	0.052
	(0.053)
Age	0.018
	(0.013)
GPA	0.070
	(0.055)
Female (indicator)	0.035
	(0.054)
Study wave	0.024
	(0.023)
F	1.21
p-value	0.30
Observations	273

Notes: Sample includes all subjects who completed our baseline survey instrument: 64 who did not complete the study and 209 who did. Estimates are from a regression of a study completion dummy on the listed variables. No other variables are included. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 12: Effect of treatment on commitment demand: Comparing effects by week 1 reallocation behavior

	(1) Change in commitment demand	(2) Change in commitment demand	(3) Change in commitment demand
Treat	-0.19	-0.18	-0.21
	(0.48)	(0.48)	(0.49)
Wk. 1 work hastened	0.16	0.16	0.15
	(0.15)	(0.15)	(0.16)
Treat × Wk. 1 work hastened	0.29	0.29	0.27
	(0.33)	(0.33)	(0.35)
Wk. 1 work delayed	-0.11	-0.095	-0.12
	(0.14)	(0.13)	(0.15)
Treat × Wk. 1 work delayed	0.58***	0.57***	0.62***
	(0.21)	(0.21)	(0.21)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: The table shows results from estimating a linear spline with a knot at 0 work deferred week 1 based on equation 6. The equation allows a different slope for the effect of work delayed versus brought forward (hastened). Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Baseline estimates without any controls

	(1) Work deferred week 2	(2) Change in commitment demand
Treat	0.32	-0.10
	(0.37)	(0.41)
Work deferred week 1	0.33**	0.0063
	(0.14)	(0.10)
Treat × Work deferred week 1	-0.41**	0.43**
	(0.20)	(0.19)
Design controls	No	No
Demographic controls	No	No
Additional controls	No	No
Observations	209	209

Notes: The table shows results from estimating versions of equations 5 and 6 that include no control variables. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Treatment interactions with naïvete over time shocks

	(1) Work deferred week 2	(2) Change in commitment demand
Treat	-0.061	-0.61
	(0.49)	(0.58)
Work deferred week 1	0.45	-0.019
	(0.28)	(0.069)
Treat × Work deferred week 1	-0.82**	0.10
	(0.41)	(0.16)
Expect busy day	-0.17	-1.16*
	(0.55)	(0.64)
Treat × Expect busy day	0.50	1.02
	(0.75)	(0.81)
Work deferred week 1 × Expect busy day	-0.19	0.061
	(0.32)	(0.15)
Treat × Work deferred week 1 × Expect busy day	0.59	0.59**
	(0.45)	(0.28)
Design controls	Yes	Yes
Observations	209	209

Notes: The table shows results from estimating modified versions of equations 5 and 6 that include interactions with self-reported measures of procrastination and busyness. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 15: Treatment interactions with naïvete over procrastination and regret

	(1) Change in commitment demand	(2) Change in commitment demand
Treat	-1.63*	-0.41
	(0.84)	(0.81)
Work deferred week 1	0.0075	-0.079
	(0.27)	(0.17)
Treat × Work deferred week 1	0.75**	1.00***
	(0.30)	(0.34)
Tend to procrastinate	-1.31*	
	(0.68)	
Treat × Tend to procrastinate	2.13**	
	(0.97)	
Work deferred week 1 × Tend to procrastinate	-0.0098	
	(0.29)	
Treat × Work deferred week 1 × Tend to procrastinate	-0.43	
	(0.39)	
Bedtime regret		0.075
		(0.16)
Treat × Bedtime regret		0.12
		(0.21)
Work deferred week 1 × Bedtime regret		0.021
		(0.035)
Treat × Work deferred week 1 × Bedtime regret		-0.20**
		(0.097)
Design controls	Yes	Yes
Observations	209	209

Notes: The table shows results from estimating modified versions of equation 6 that include interactions with self-reported measures of procrastination and busyness. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 16: Effect of treatment on planned tasks

	(1) Morning tasks week 2	(2) Evening tasks week 2
Treat	-0.26	-0.51
	(0.43)	(0.50)
Work deferred week 1	0.13	-0.19
	(0.11)	(0.15)
Treat × Work deferred week 1	-0.31	0.12
	(0.21)	(0.26)
Design controls	Yes	Yes
Observations	209	209

Notes: The table shows results from estimating equation 5, but using planned tasks or executed tasks as the dependent variable. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 17: Finite Mixture Model

	Class 1	Class 2	Class 3	Class 4
Work deferred week 2				
Treat	1.87	1.55	-0.08	0.08
	(2.39)	(1.87)	(0.54)	(0.42)
Work deferred week 1	-0.10	0.53***	-0.06	1.02***
	(0.09)	(0.10)	(0.06)	(0.07)
Treat × Work deferred week 1	-0.42	-0.14	1.27***	-1.07***
	(0.36)	(0.38)	(0.19)	(0.09)
Change in commitment demand				
Treat	4.05**	-0.82**	0.09	-1.14
	(2.01)	(0.33)	(1.20)	(1.14)
Work deferred week 1	0.73**	0.63***	-0.09	0.05
	(0.34)	(0.03)	(0.13)	(0.15)
Treat × Work deferred week 1	-0.09	-0.62***	0.36	0.72**
	(0.36)	(0.07)	(0.25)	(0.31)
Latent Class Proportion	0.060	0.083	0.415	0.442

Notes: The table shows results from estimating equations 5 and 6 in a seemingly unrelated regression with Gaussian errors using maximum likelihood. Sets of coefficients are estimated for four classes. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 18: Effect of treatment on work deferred, by gender

	(1) Work deferred week 2	(2) Work deferred week 2	(3) Work deferred week 2
Treat	0.98	1.00	0.93
	(0.74)	(0.75)	(0.77)
Work deferred week 1	0.55**	0.55**	0.46*
	(0.25)	(0.25)	(0.24)
Treat × Work deferred week 1	-0.75***	-0.75***	-0.64**
	(0.27)	(0.27)	(0.25)
Female	0.22	0.24	0.39
	(0.55)	(0.56)	(0.53)
Female × Treat	-1.06	-1.09	-1.26
	(0.84)	(0.84)	(0.85)
Female × Work deferred week 1	-0.40	-0.40	-0.25
	(0.27)	(0.27)	(0.27)
Female × Treat × Work deferred week 1	0.52	0.52	0.31
	(0.44)	(0.44)	(0.41)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: The table shows results from estimating equation 5. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 19: Effect of treatment on commitment demand, by gender

	(1) Change in commitment demand	(2) Change in commitment demand	(3) Change in commitment demand
Treat	-0.43	-0.38	-0.39
	(0.80)	(0.81)	(0.82)
Work deferred week 1	0.049	0.051	0.059
	(0.11)	(0.11)	(0.12)
Treat × Work deferred week 1	0.57**	0.56**	0.59***
	(0.22)	(0.22)	(0.23)
Female	-0.28	-0.24	-0.29
	(0.64)	(0.65)	(0.70)
Female × Treat	0.61	0.54	0.58
	(0.93)	(0.94)	(0.98)
Female × Work deferred week 1	-0.061	-0.056	-0.11
	(0.19)	(0.19)	(0.22)
Female × Treat × Work deferred week 1	-0.23	-0.23	-0.25
	(0.36)	(0.36)	(0.41)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: The table shows results from estimating equation 6. Each column is a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 20: Effect of treatment on work deferred, Winsorized data

	(1) Work deferred week 2	(2) Work deferred week 2	(3) Work deferred week 2
Treat	0.23	0.25	0.097
	(0.32)	(0.32)	(0.32)
Work deferred week 1	0.39***	0.39***	0.41***
	(0.13)	(0.13)	(0.13)
Treat × Work deferred week 1	-0.49**	-0.50**	-0.50**
	(0.22)	(0.23)	(0.21)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: Variables for tasks deferred are Winsorized at the 2.5% level in both weeks. The table shows results from estimating equation 5. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 21: Effect of treatment on commitment demand, Winsorized data

	(1) Change in commitment demand	(2) Change in commitment demand	(3) Change in commitment demand
Treat	-0.16	-0.12	-0.11
	(0.37)	(0.39)	(0.41)
Work deferred week 1	-0.014	-0.0083	-0.031
	(0.13)	(0.13)	(0.14)
Treat × Work deferred week 1	0.46**	0.44**	0.47**
	(0.22)	(0.21)	(0.21)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: Variables for commitment demand are Winsorized at the 2.5% level in both weeks. The table shows results from estimating equation 6. Each column is a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 22: Assessing experimenter demand using treatment message consistency

	(1) Work deferred week 2	(2) Change in commitment demand
Treat	0.49	-0.73
	(0.48)	(0.50)
Work deferred week 1	0.45**	-0.062
	(0.23)	(0.18)
Treat × Work deferred week 1	-0.70**	0.53**
	(0.32)	(0.26)
Bedtime error 1	0.35	-0.54*
	(0.27)	(0.29)
Treat × Bedtime error 1	-0.17	0.70**
	(0.33)	(0.35)
Work deferred week 1 × Bedtime error 1	-0.078	0.046
	(0.12)	(0.069)
Treat × Work deferred week 1 × Bedtime error 1	0.33	-0.045
	(0.38)	(0.23)
Design controls	Yes	Yes
Observations	209	209

Notes: The table shows results from estimating triple-difference variants of equations 5 and 6. "Bedtime error 1" is the subject's week 1 bedtime prediction error (which was reported to treatment group subjects), divided by its own standard deviation. Controls and sample are the same as in Tables 4 and 5. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 23: Assessing experimenter demand using over-reporting of study time

	(1) Work deferred week 2	(2) Change in commitment demand
Treat	0.059	-0.043
	(0.41)	(0.43)
Work deferred week 1	0.31**	0.0071
	(0.13)	(0.12)
Over-report	-0.18	-0.79
	(0.39)	(0.61)
Treat × Work deferred week 1	-0.48**	0.50**
	(0.20)	(0.21)
Treat × Over-report	0.65	0.83
	(0.49)	(0.67)
Over-report × Work deferred week 1	-0.022	-0.0061
	(0.097)	(0.13)
Treat × Work deferred week 1 × Over-report	-0.090	0.034
	(0.12)	(0.16)
Design controls	Yes	Yes
Demographic controls	Yes	Yes
Additional controls	Yes	Yes
Observations	209	209

Notes: The table shows results from estimating triple-difference variants of equations 5 and 6. "Over-report" is constructed as follows. For each day a subject's over-reporting is defined as study time reported in response to a direct question minus study time as measured by the subject's time diary. Over-reports are then averaged over the 8 days for which time diaries and direct reports of study time are available. Controls and sample are the same as in Tables 4 and 5. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 24: Effect of treatment on preference for consistency

	(1) Consistent Week 2	(2) Consistent Week 2	(3) Consistent Week 2
Treat	0.0035	-0.0062	0.015
	(0.066)	(0.068)	(0.072)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: The table shows results from regressing an indicator for making consistent choices in the second week on a treatment indicator and various controls. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 25: Effect of treatment on commitment demand for inconsistent subjects

	(1) Work deferred week 2	(2) Work deferred week 2	(3) Work deferred week 2
Treat	0.37	0.46	-0.016
	(1.12)	(1.15)	(1.12)
Work deferred week 1	0.72***	0.74***	0.57*
	(0.22)	(0.21)	(0.32)
Treat $\times$ Work deferred week 1	-0.63	-0.64	-0.84*
	(0.41)	(0.43)	(0.46)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	71	71	71

Notes: The table shows results from estimating equation 5 while restricting the sample to subjects who were inconsistent in the second week. Each column shows the results of a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. Demographic controls are gender, age, and age squared. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 26: Changes in expected busyness by treatment and subject type

	(1) Full Sample: Busyness day 10	(2) Treated Sample: Busyness day 10
Busyness day 3	0.41***	0.40***
	(0.072)	(0.083)
Busyness day 8	0.20**	0.44***
	(0.081)	(0.11)
Treat	-0.20	
	(0.23)	
Work deferred week 1	-0.026	0.29***
	(0.069)	(0.068)
Treat $\times$ Work deferred week 1	0.22**	
	(0.098)	
Inconsistency: Discounting		-0.24
		(0.34)
Inconsistency: Beliefs		0.51
		(0.42)
Inconsistency: Disc. and beliefs		0.41
		(0.68)
Design controls	Yes	Yes
Observations	209	109

Notes: Each column is a separate regression. The dependent variable self-reported, expected busyness on day 10 (after treatment). Column (1) uses the full sample to show that treated subjects who deferred more work during week 1 (pre-treatment) reported that they would be busier during the second week. Column (2) uses the treatment group, classified by the source of their dynamic inconsistency (see Section 5) to show that belief-based inconsistent subjects updated toward thinking they were busier. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 27: Assessing external commitment using random price variation

	(1) Work deferred week 2	(2) Work deferred week 2	(3) Work deferred week 2
Commitment price wk. 2	-0.019	-0.020	0.0082
	(0.070)	(0.071)	(0.070)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	209	209	209

Notes: The table shows results from estimating a variant of equation 5, with treatment variables replaced by randomized commitment price in week 2. Controls and sample are the same as in Table 4. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 28: Effect of treatment on bedtime forecast error, raw planned bedtimes

	(1) Discrete forecast error week 2	(2) Discrete forecast error week 2	(3) Discrete forecast error week 2
Treat	-0.57*	-0.62*	-0.67**
	(0.31)	(0.32)	(0.32)
Forecast error week 1	1.16**	1.13**	1.49***
	(0.48)	(0.48)	(0.45)
Treat × Forecast error week 1	-1.25**	-1.26**	-1.63***
	(0.56)	(0.56)	(0.56)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	202	202	202

Notes: Forecast error based on raw self-reported planned bedtime, without adjusting planned bedtimes 10 a.m.–3 p.m. for likely a.m.-p.m. entry error. Each column of the table shows results from a separate regression. Design controls are indicators for study wave and receipt of a survey completion reminder in week 1. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

B Tables Without Multiple Switchers

Table 29: Treatment-control balance without multiple switchers

	Mean/(SD)	Mean/(SD)	Diff./(SE)
Commitment demand week 1	-1.03	-0.24	-0.79
	(3.20)	(3.43)	(0.52)
Commitment demand week 2	-1.63	-0.71	-0.92**
	(2.79)	(3.04)	(0.46)
Work deferred week 1	-0.082	-0.011	-0.071
	(3.15)	(1.92)	(0.40)
Work deferred week 2	0.22	0.56	-0.34
	(2.74)	(2.86)	(0.44)
Bedtime difference from plan (minutes)	42.9	44.6	-1.74
	(67.7)	(53.4)	(9.46)
GPA	3.26	3.32	-0.062
	(0.47)	(0.45)	(0.072)
Female (indicator)	0.58	0.68	-0.11
	(0.50)	(0.47)	(0.076)
Study wave	2.48	2.55	-0.070
	(1.16)	(1.11)	(0.18)
Observations	73	91	

Notes: The significance of the differences is assessed using a t-test. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 30: Effect of treatment on work deferred without multiple switchers

	(1) Work deferred week 2	(2) Work deferred week 2	(3) Work deferred week 2
Treat	0.21	0.29	0.11
	(0.43)	(0.42)	(0.49)
Work deferred week 1	0.26*	0.26*	0.22
	(0.16)	(0.15)	(0.14)
Treat × Work deferred week 1	-0.43**	-0.46**	-0.36*
	(0.21)	(0.22)	(0.20)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	164	164	164

Notes: The table shows results from estimating equation 5 estimated on the sample that excludes multiple switchers. Each column shows the results of a separate regression. Controls are indicated at the bottom of each regression. Demographic controls are an indicator for gender, indicators for study wave, age, age squared, and an indicator for whether the subject was employed at the time of the study. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 31: Effect of treatment and task procrastination on commitment demand without multiple switchers

	(1) Change in commitment demand	(2) Change in commitment demand	(3) Change in commitment demand
Treat	0.25	0.25	0.083
	(0.47)	(0.47)	(0.45)
Treat × Work deferred week 1	0.53***	0.54***	0.61***
	(0.19)	(0.19)	(0.22)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	164	164	164

Notes: The table shows results from estimating equation 6 estimated on the sample that excludes multiple switchers. Each column is a separate regression. Demographic controls are an indicator for gender, indicators for study wave, age, age squared, and an indicator for whether the subject was employed at the time of the study. Additional controls are GPA, GPA squared, an employment indicator, and week 1 measures (days 1, 2, 3 and 4) of study time (hrs) and social time (hrs). In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 32: Effect of treatment on planned tasks without multiple switchers

	(1) Morning tasks week 2	(2) Morning tasks week 2	(3) Evening tasks week 2	(4) Evening tasks week 2
Treat	-0.0069	-0.0073	-0.22	-0.30
	(0.51)	(0.52)	(0.58)	(0.59)
Work deferred week 1	0.14	0.14	-0.12	-0.12
	(0.12)	(0.12)	(0.16)	(0.16)
Treat × Work deferred week 1	-0.30	-0.30	0.12	0.16
	(0.23)	(0.24)	(0.28)	(0.28)
Design controls	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	No	Yes
Observations	164	164	164	164

Notes: The table shows results from estimating equation 5, but using planned tasks or executed tasks as the dependent variable, on the sample without multiple switchers. Each column shows the results of a separate regression. All regressions contain the following controls: an indicator for gender, indicators for study wave, age, age squared, GPA, GPA squared, and an indicator for whether the subject was employed at the time of the study. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 33: Effect of treatment on bedtime forecast error without multiple switchers

	(1) Discrete forecast error week 2	(2) Discrete forecast error week 2	(3) Discrete forecast error week 2
Treat	-0.53	-0.59	-0.79*
	(0.35)	(0.36)	(0.44)
Forecast error	0.25	0.21	0.45
	(0.29)	(0.28)	(0.33)
Treat × Forecast error	-0.54	-0.52	-0.85*
	(0.38)	(0.37)	(0.44)
Design controls	Yes	Yes	Yes
Demographic controls	No	Yes	Yes
Additional controls	No	No	Yes
Observations	158	158	158

Notes: Each column of the table shows results from a separate regression estimated on the sample that excludes multiple switchers. All regressions contain the following controls: an indicator for gender, indicators for study wave, age, age squared, and an indicator for whether the subject was employed at the time of the study. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table 34: Time use and commitment price without multiple switchers

	Class	Exercising	Other	Sleeping
Time Shock diff.	-0.78	0.53	4.87*	-1.13
	(1.43)	(0.57)	(2.54)	(1.53)
Observations	164	164	164	164

	Socializing	Studying	TV	Working
Time Shock diff.	1.97	-6.70**	2.45*	-1.20
	(2.28)	(2.67)	(1.38)	(0.93)
Observations	164	164	164	164

Notes: The table shows results from estimating equation 7, without multiple switchers. Each column is a separate regression. All regressions contain the following controls: an indicator for gender, indicators for study wave, age, age squared, and an indicator for whether the subject was employed at the time of the study. In parentheses are heteroskedasticity-robust standard errors (White, 1980). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

C Experimental Instructions and Surveys

This appendix includes a representative portion of our experimental instructions and surveys. The surveys rely on Qualtrics' internal logic and referencing system; subjects only see one version of any question and any reference fields would be replaced with the information which was appropriate for that subject. The full set of surveys are omitted for space reasons and because they do not differ significantly from the materials provided here. They will be provided by the authors upon request.

Appendix C.1 provides the introductory script with the associated presentation that was read to subjects. Appendix C.2 provides the survey that subjects completed at the introductory session.

Appendix C.3 provides the morning survey that subjects completed on Monday of the second week of the experiment. The survey given on Monday of the first week looked identical except for the treatment, which was omitted. The survey given on every other Monday looked identical except that it did not include the treatment, the commitment demand elicitation, or the allocation decision.

Appendix C.3 provides the evening survey and first task that subjects completed on Monday of the second week of the experiment. The evening survey subjects completed on Monday of the first week of the experiment looked identical. The evening survey subjects completed on Wednesday of both weeks looked identical except that it did not include the allocation decision.

C.1 Introductory Script and Slides

Time Use Study Script

Hello everyone, my name is Zachary. Thank you for your participation in this study about sleep and time use.

Slide

This study requires participation over two weeks. To participate, you must be willing to:

- wear the Fitbit wristband on Sunday, Monday, Tuesday, and Wednesday nights of this coming week,
- to complete a series of 8 surveys on Monday through Thursday morning of the next two weeks,
- to complete a series of 4 tasks on Monday and Wednesday evenings of the next two weeks,
- to return the Fitbit wristband to the economics department on Thursday or Friday of next week, and
- to pick up your payment on Thursday or Friday of the second week.

Surveys and tasks will be completed online, and the link will be sent to you when it is time to complete the survey.

Slide

1

This is a picture of the Fitbit wristband that you will be required to wear to bed Sunday through Thursday of this coming week. You should only wear it when you're in bed, not during daytime hours. If the band gets wet, dry it off. You are required to return the band to the Economics Department on Thursday or Friday of next week, between the hours of 8AM and 6PM.

Slide

The tasks you must complete in the evenings consist of moving sliders to a predetermined level, which is given to the left of the slider. You will be unable to move on until you match each slider to the given level. While the morning surveys can be completed on a phone or tablet, it is recommended that you complete these tasks on a computer. You'll complete several example tasks during the initial survey at the end of this session to see what they're like.

Slide

Participation in the study requires completing the surveys and tasks at particular times. Morning surveys must be completed before noon, and evening tasks must be completed between 9PM and 2AM. Furthermore, the Fitbit wristband must be returned during business hours on Thursday or Friday next week, and the final payment must be picked up on Thursday or Friday in two weeks. If you are unable to complete the requirements of this study, you are free to leave at this point, as payments will be forfeit if the requirements are not met.

Slide

You have been given a consent form that describes your rights. Please look it over now while we prepare the remainder of this session. If you have questions, please raise your hand and I'll come address them. We'll pick up the forms before continuing.

Unplug projector and prepare survey

You may open your computers now. The link for the initial survey should now have been sent to the email address you gave us, although it may take a few minutes to arrive. Please log in to your email account and follow the directions. When you have completed the initial survey, you may come to me to receive your Fitbit wristband.

Give band and payment

Here is your sleep tracker. Remember to wear it when you sleep Sunday night through Wednesday night. Your first survey will arrive on Monday morning.

Time Use Study

Requirements

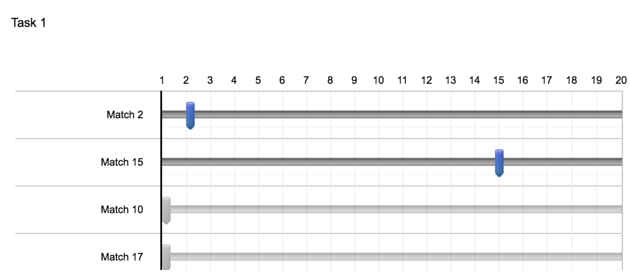
- Wear the Fitbit wristband on Sunday, Monday, Tuesday, and Wednesday nights of the first week
- Complete eight morning surveys, and 4 sets of evening tasks, all of which will be emailed to you
- Return the Fitbit wristband and pick up $10 payment at the economics department on Thursday or Friday of the first week
- Pick up your $30 payment from the economics department on Thursday or Friday of the second week

Fitbit Wristband

- Wear the band on Sunday, Monday, Tuesday, and Wednesday nights of the first week, *only when you are in bed*
- Return them to the economics department next Thursday or Friday

Tasks

- You'll be asked to move sliders to a particular level



Timing

To participate in the study, you must be able to do all of the following:

- eight surveys completed at home, lasting 5-10 minutes, **to be completed before noon** on Monday-Thursday of next week and the week after
- four surveys completed at home, lasting 15-20 minutes, **to be completed between 9PM and 2AM** on Monday and Wednesday night of next week and the week after
- return the Fitbit wristband during business hours (8AM to 6PM) on Thursday June 1st or Friday June 2nd.
- pick up your payment during business hours (8AM to 6PM) on Thursday June 8th or Friday June 9th.

Informed Consent

- You received a consent form after you entered that describes your rights as a subject. Please read it.
- If you would no longer like to be part of this study, you are free to leave at this point.

C.2 Introductory Survey

Intro Survey

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First Click: **0 seconds**
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Welcome to the experiment. Before explaining what will be happening for the rest of the experiment, please answer this short survey about yourself and your sleep and work habits.

What is your first name?

What is your last name?

What is your email address?

What is your phone number?

What is your PID?

What is your college major?
If you do not have a college major, write "undeclared".

What is your current GPA?

What is your gender?
If you do not wish to provide gender information, you may leave this question blank.

What is your current age?

Are you employed?

- Yes
- No

If you are employed, what is your hourly wage?
If you are not employed, please leave blank.

How many hours per day did you usually spend studying last quarter?

Would you say that you tend to procrastinate?

- Yes
- No

How much do you generally sleep on weekdays?

How much do you generally sleep on weekends?

How many hours would you like to sleep per night?

Please choose the response that best represents about how many nights per week, on average, you had trouble sleeping last quarter.

- 0
- 1
- 2
- 3
- 4
- 5
- 6
- 7

How many hours do you feel like you would need to sleep every night for 1 week to feel completely rested?

What is your usual bedtime on weekdays?

	Hour	Minute	AM/ PM
Weekday Bedtime			

What is your usual bedtime on weekends?

	Hour	Minute	AM/ PM
Weekend Bedtime			

What time do you usually wake up on weekdays?

	Hour	Minute	AM/ PM
Weekday wake-up			

What time do you usually wake up on weekends?

	Hour	Minute	AM/ PM
Weekend wake-up			

Do you live in UC San Diego on-campus housing?

- Yes
- No

Please select your residence hall from the following list

What is the name of the neighborhood where you live?

From the following items, select any that you generally keep in the room where you most often sleep.

- Television
- Desktop computer
- Video game console
- iPad or other tablet
- Laptop computer

Do you take naps?

- Yes
- No

If you take naps, how long do you usually nap?
If you do not usually take naps, please leave blank.

How often do you wake up and feel like you wish you had gone to bed earlier?

- Never
- Rarely
- 1-2 times per week
- 3-4 times per week
- 5-6 times per week
- Every day

How often do work or studying make it hard to go to bed when you'd like?

- Never
- Rarely
- 1-2 times per week
- 3-4 times per week
- 5-6 times per week
- Every day

Personality

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Here are a few more questions about your attitude and behavior. Please be as honest and accurate as you can throughout. Try not to let your response to one statement influence your responses to other statements. There are no "correct" or "incorrect" answers. Answer according to your own feelings, rather than how you think "most people" would answer.

In uncertain times, I usually expect the best.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly Disagree

It's easy for me to relax.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

If something can go wrong for me, it will.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I'm always optimistic about my future.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I enjoy my friends a lot.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

It's important for me to keep busy.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I hardly ever expect things to go my way.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I don't get upset too easily.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree

C.3 Week 2 Monday Morning Survey

Default Question Block

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First Click: **0 seconds**
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Click Count: **0 clicks**

Hi! This is a quick survey to better understand your daily sleep habits. Most of the questions ask you about your sleep last night. Please answer them as honestly as possible. You'll make decisions about tonight's tasks on the next page. Thanks!

How long did you study yesterday (in hours)?
Note: Study time does not include class time.

I want to ask you about how you spent your time yesterday. For each hour of the day, please select the activities you did.

	Sleeping	Socializing	Class	Studying	Exercising	Working at a job	Watching TV	Other
12:00 a.m. (Midnight)								
1:00 a.m.								
2:00 a.m.								
3:00 a.m.								
4:00 a.m.								
5:00 a.m.								
6:00 a.m.								
7:00 a.m.								
8:00 a.m.								
9:00 a.m.								
10:00 a.m.								
11:00 a.m.								
12:00 p.m. (noon)								
1:00 p.m.								
2:00 p.m.								
3:00 p.m.								
4:00 p.m.								
5:00 p.m.								
6:00 p.m.								
7:00 p.m.								
8:00 p.m.								
9:00 p.m.								
10:00 p.m.								
11:00 p.m.								

What time do you plan on turning off the light to go to sleep tonight?

	Hour	Minute	AM/PM
Planned bedtime			

Which of the following did you do in the hour before you went to bed (select all that apply)?

- Watch TV
- Play video or computer games
- Use a computer or smart phone (other than for games)
- Exercise
- Read a book or non-backlit e-reader
- Other:

What time did you turn off the light intending to go to sleep last night?

	Hour	Minute	AM/PM
Light off last night			

How long did it take you to fall asleep last night (in minutes)?

How many times did you wake up last night?

What time did you wake up this morning (for the last time)?

	Hour	Minute	AM/PM
Wake-up time this morning			

What time did you get out of bed this morning (for the last time)?

	Hour	Minute	AM/PM
Out of bed this morning			

How well do you feel like you slept last night?

Slept very badly — Slept very well

0	1	2	3	4	5	6	7	8	9	10

How tired did you feel today?

Not tired at all — Extremely tired

0	1	2	3	4	5	6	7	8	9	10

How busy are you today?

Less busy than usual				About as busy as usual						More busy than usual
0	1	2	3	4	5	6	7	8	9	10

Block 3

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} task from Wednesday to Monday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} tasks from Wednesday to Monday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} task from Monday to Wednesday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} tasks from Monday to Wednesday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, your choices did not change.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Commitment

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Choosing the Implemented Allocation

Here is a series of choices that can affect the probability that the morning allocation will be the one that is chosen. To affect the probability, you may have to agree to do more baseline tasks. **You will never have to do tasks in the morning - these decisions just affect which allocation is implemented.**

This decision will measure the strength of your preference for which decision is implemented.

We'll randomly select which one of these decisions we implement. When you make these decisions, **treat every decision as if it is the one that counts because each decision is the one that could be implemented.**

16 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented ○	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented ○
14 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented ○	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented ○
12 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented

11 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
10 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
9 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
8 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
6 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
4 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented

Allocation Decision

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 1 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 16 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 14 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 12 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 11 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 9 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 8 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 6 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 4 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

C.4 Week 2 Monday Evening Survey

Default Question Block

These page timer metrics will not be displayed to the recipient.
First Click: **0 seconds**
Last Click: **0 seconds**
Page Submit: **0 seconds**
Click Count: **0 clicks**

Hi! This is a quick survey to better understand your daily sleep habits. Most of the questions ask you about your sleep last night. Please answer them as honestly as possible. You'll make decisions about tonight's tasks on the next page. Thanks!

How long did you study today (in hours)?
Note: Study time does not include class time.

What time do you plan on turning off the light to go to sleep tonight?

	Hour	Minute	AM/PM
Planned bedtime			

What time do you plan on waking up tomorrow?

	Hour	Minute	AM/PM
Planned wake-up			

What time do you plan on getting out of bed tomorrow?

	Hour	Minute	AM/PM
Getting out of bed			

How tired did you feel today?

Extremely tired — Not tired at all

0	1	2	3	4	5	6	7	8	9	10

How busy were you today?

Less busy than usual — More busy than usual

0	1	2	3	4	5	6	7	8	9	10

Allocation Decision

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 1 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 16 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 14 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 12 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 11 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 9 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 8 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 6 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 4 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

Tasks 1-10

Completing Tasks

We've randomly selected that your **morning allocation** be the one implemented, so **tonight you will complete ${e://Field/Tasks21} tasks**, and Wednesday you will complete ${e://Field/Tasks22} tasks.

You may complete tonight's tasks below.

Completing Tasks

We've randomly selected that your **evening allocation** be the one implemented, so **tonight you will complete ${e://Field/Tasks21} tasks**, and Wednesday you will complete ${e://Field/Tasks22} tasks.

You may complete tonight's tasks below.

Task 1

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Match 2																				
Match 15																				
Match 10																				
Match 17																				
Match 19																				
Match 11																				
Match 9																				
Match 13																				
Match 12																				

Match 6																				
Match 3																				
Match 20																				
Match 16																				
Match 4																				
Match 7																				
Match 5																				
Match 18																				
Match 8																				
Match 14																				

Task 2

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Match 14																				
Match 4																				
Match 5																				
Match 8																				
Match 17																				
Match 7																				