Working Over Time: Dynamic Inconsistency in Real Effort Tasks *

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Abstract

Experimental tests of dynamically inconsistent time preferences have largely relied on choices over time-dated monetary rewards. Several recent studies have failed to find the standard patterns of time inconsistency. However, such monetary studies contain often-discussed confounds. In this paper, we sidestep these confounds and investigate choices over consumption (real effort) in a longitudinal experiment. We pair those effort choices with a companion monetary discounting study. We confirm very limited time inconsistency in monetary choices. However, subjects show considerably more present bias in effort. Furthermore, present bias in the allocation of work has predictive power for demand of a meaningfully binding commitment device. Therefore our findings validate a key implication of models of dynamic inconsistency, with corresponding policy implications.

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1 Introduction

Models of dynamically inconsistent time preferences (Strotz, 1956; Laibson, 1997; O'Donoghue and Rabin, 1999) are a pillar of modern behavioral economics, having added generally to economists' understanding of the tensions involved in consumption-savings choices, task performance, temptation, and self-control beyond the standard model of exponential discounting (Samuelson, 1937). Given the position of present-biased preferences in the behavioral literature, there is clear importance in testing the model's central falsifiable hypothesis of diminishing impatience through time. Further, testing auxiliary predictions such as individuals' potential to restrict future activities through commitment devices can deliver critical prescriptions to policy makers. In this paper we present a test of dynamic inconsistency in consumption and investigate the demand for a meaningfully binding commitment device.

To date, a notably large body of laboratory research has been generated focused on identifying the shape of time preferences (for a comprehensive review to the early 2000s, see Frederick, Loewenstein and O'Donoghue, 2002).[1] The core of this experimental literature has identified preferences from time-dated monetary payments. A paradigmatic example would have a subject state the monetary payment received today, $\$X$, that makes her indifferent to \$50 received in one months' time, then would have her state the monetary payment received in one months' time, $\$Y$, that makes her indifferent to \$50 received in two months' time.[2] Non-equivalence in the stated indifferent values is often taken as evidence of dynamic inconsistency, and $\$X < \$Y$ is taken as evidence of a present-biased shape of discounting. Though conducted experiments differ along many dimensions including payment horizons, methods, subject pools, and potential

[1]Though much of the literature has focused on laboratory samples, there is also a growing body of research attempting to identify the shape and extent of discounting from real world choices and aggregate data such as durable goods purchase, annuity choice, and consumption patterns (Hausman, 1979; Lawrance, 1991; Warner and Pleeter, 2001; Gourinchas and Parker, 2002; Cagetti, 2003; Laibson, Repetto and Tobacman, 2003, 2005).

[2]A popular methodolgy for eliciting such indifferences is the Multiple Price List technique (Coller and Williams, 1999; Harrison, Lau and Williams, 2002) asking individuals a series of binary choices between time dated payments, identifying intervals in which \$X and \$Y lie. Psychology has often relied on an alternative method to identify dynamic inconsistency, asking subjects a series of questions involving increasing delay lengths and examining whether the implied discount factors nest exponentially (see, for example Kirby, Petry and Bickel, 1999; Giordano, Bickel, Loewenstein, Jacobs, Marsch and Badger, 2002).

transaction costs, a stylized fact has emerged that many subjects are dynamically inconsistent and the majority of inconsistencies are in the direction of present bias (Frederick et al., 2002).[3]

Several confounds exist for identifying the shape of time preferences from experimental choices over time-dated monetary payments, muddying the strict interpretations of behavior provided above. Critically, issues of payment reliability and risk preference suggest that if information is to be gleaned from such choices, it may be linked to the subject's assessment of the experimenter's reliability.[4] Recent work validates this suspicion. Andreoni and Sprenger (2012a), Gine, Goldberg, Silverman and Yang (2010), and Andersen, Harrison, Lau and Rutstrom (2012) all document that when closely controlling transactions costs and payment reliability, dynamic inconsistency in choices over monetary payments is virtually eliminated on aggregate. Further, when payment risk is added in an experimentally controlled way, non-expected utility risk preferences deliver behavior observationally equivalent to present bias as described above (Andreoni and Sprenger, 2012b).[5]

Beyond these operational issues, there is reason to question the use of potentially fungible monetary payments to identify the parameters of models defined over time-dated consumption. Clear arbitrage arguments exist indicating that nothing beyond the interval of subjects' borrowing and lending rates should be revealed in choices over monetary payments.[6] Chabris,

[3]For example, Ashraf, Karlan and Yin (2006) find that roughly 47% of their subjects are dynamically inconsistent over hypothetical time-dated monetary payments and around 60% of the inconsistencies are in the direction of present bias. Similarly, Meier and Sprenger (2010) find that roughly 45% of their subjects are dynamically inconsistent over incentivized time dated payments and 80% of the inconsistencies are in the direction of present bias.

[4]This point was originally raised by Thaler (1981) who, when considering the possibility of using incentivized monetary payments in intertemporal choice experiments noted 'Real money experiments would be interesting but seem to present enormous tactical problems. (Would subjects believe they would get paid in five years?)'

[5]Specifically, Andreoni and Sprenger (2012b) show that when sooner payments are certain while future payments are delivered only with 80%, subjects prefer the certain sooner payment. When payments at both time periods are made uncertain, occurring with 50% sooner and 40% in the future, subjects appear more patient, violating discounted expected utility. The observation that non-expected utility risk preferences generate dynamic inconsistencies was previously thoughtfully analyzed theoretically by Machina (1989). Halevy (2008) makes the link between prospect theory probability weighting and diminishing impatience through time citing psychology experiments conducted by Keren and Roelofsma (1995) and Weber and Chapman (2005) who show in an original experiment and a partial reproduction, respectively, that when payment risk is added to binary choices over monetary payments, dynamic inconsistency is reduced in some experimental contexts.

[6]This point has been thoughtfully taken into account in some studies. For example, Harrison et al. (2002) explicitly account for potential arbitrage in their calculations of individual discount rates by measuring individual borrowing and saving rates and incorporating these values in estimation. Cubitt and Read (2007) provide

Laibson and Schuldt (2008) describe the difficulty in mapping experimental choices over money to corresponding model parameters, casting skepticism over monetary experiments in general. The model is one of consumption, so falsifying the key prediction of diminishing impatience through time may be more convincing when done in the relevant domain, consumption.[7] There are only a few experimental tests of dynamic inconsistency for consumption. Key contributions include Read and van Leeuwen (1998) who identify dynamic inconsistency in the surprise reallocations of snack choices, and McClure, Laibson, Loewenstein and Cohen (2007) and Brown, Chua and Camerer (2009), who document dynamic inconsistency in brief intertemporal choices over squirts of juice and soda.

In this paper we attempt to move out of the domain of monetary choices and into the domain of consumption, while maintaining a portable design that allows individual parameters of dynamic inconsistency to be estimated. With 102 UC Berkeley Xlab subjects, we introduce a seven week longitudinal experimental design asking subjects to allocate and subsequently reallocate units of effort (i.e., negative leisure consumption) over time at various gross interest rates. Subject responses are incentivized by requiring completion of the tasks from either one initial allocation or one subsequent reallocation. Subjects receive a one-time completion bonus of $100 in the seventh week of the experiment, fixing the monetary dimension of their effort allocation choices. The tasks over which subjects make choices are transcription of meaningless greek texts and completion of partial tetris games. Allocations are made in a convex decision environment permitting identification of both cost function and discounting parameters. Differences between initial allocations and subsequent reallocations allow for the identification of dynamic inconsistency.

The repeated interaction of our seven-week study allows us to complement measures of effort

excellent recent discussion of the arbitrage arguments and other issues for choices over monetary payments. One counterpoint is provided by Coller and Williams (1999), who present experimental subjects with a fully articulated arbitrage argument and external interest rate information and document only a small treatment effect.

[7]Though our objective in the present study is the exploration of present bias separate from issues of fungibility, recent developments in the field have led to another important facet of the debate: why and when do monetary discounting studies deliver measures of present bias with predictive validity despite their potential flaws? This question lies outside the scope of this paper but clearly represents an important avenue for future research.

discounting with measures of monetary discounting taken from Andreoni and Sprenger (2012a) Convex Time Budget (CTB) choices over cash payments received in the laboratory. In these choices, subjects allocate money across time at various gross interest rates. We can compare dynamic inconsistency measured over work and money at both the aggregate and individual level.

Finally, once subjects have experienced the tasks for several weeks, we elicit their demand for a commitment device. Specifically, we allow subjects to probabilistically favor their initial allocations over their subsequent reallocations of work. We investigate the aggregate demand for our offered commitment device and correlate identified dynamic inconsistency over both effort and money with commitment demand.

We document three primary findings. First, in the domain of money we find virtually no aggregate evidence of present bias using immediate in-lab cash payments. Second, in the domain of effort we find significant evidence of present bias. Allocations of tasks made one week in advance exceed those made on the date of actual effort by approximately 9%, on average. Corresponding parameter estimates corroborate these non-parametric results. Third, we find that the elicited demand for commitment is limited to price zero, at which price 59% of subjects would prefer a higher likelihood of implementing one of their initial allocations over their subsequent reallocations. More importantly, we show that subjects we identified as present biased choose the commitment device, while others do not. We show that the choice of commitment is binding and meaningful in the sense that initial preferred allocations differ significantly from subsequent reallocations. This provides key validation and support for our experimental measures and well-known theoretical models of present bias.

Despite recent negative findings for models of dynamic inconsistency with time-dated payments, we find support for the model's central prediction of diminishing impatience through time in the domain of consumption. Further, the auxiliary predictions of both the potential demand for commitment and the link between commitment demand and present bias are also validated.

The paper proceeds as follows: Section 2 provides details for our longitudinal experimental design. Section 3 describes identification of intertemporal parameters based on experimental choices over both effort and money. Section 4 presents results. Section 5 is a discussion and section 6 concludes.

2 Design

To examine dynamic inconsistency in real effort, we introduce a longitudinal experimental design conducted over seven weeks. In the experiment, subjects are asked to allocate, subsequently reallocate and complete tasks for two jobs. If all elements of the experiment are completed satisfactorily, subjects receive a completion bonus of $100 in Week 7 of the study. Otherwise they receive only $10 in Week 7. The objective of the completion bonus is to fix the monetary dimension of subjects' effort choices. Subjects are always paid the same amount for their completed work, the question of interest is *when* they prefer to exert effort.

Having individuals make intertemporal choices over effort allows us to circumvent many of the key concerns that plague monetary discounting experiments. First, subjects cannot borrow, save or substitute units of tasks outside of the experiment, removing opportunities of arbitrage.[8] Second, the precise date of consumption is known to both the researcher and the subject at the time of decision, allowing for precise identification of discounting parameters. Third, individuals select into a seven week experiment with a $100 completion bonus in the seventh week, reducing issues of payment reliability. This also separates effort allocation decisions from payment. And lastly, we implement a minimum work requirement. This equalizes transaction costs over time as subjects are forced to participate and complete minimum effort on all dates.

We present the design in five subsections. First, we describe the Jobs to be completed. Second, we present a timeline of the experiment and the convex decision environment in which allocations were made. The third subsection describes the design of the commitment device

[8]Though this removes substitutability of the task at hand, subjects may alter their allocations of other, extra-lab consumption. As a first pass we ignore this possibility and the possibility that subjects subcontract their experimental tasks. Section 6 provides additional discussion.

for which demand was elicited once subjects had gained experience with the tasks. The fourth subsection addresses design details including recruitment, selection and attrition. The fifth subsection presents the complementary monetary discounting study facilitated by the repeated interaction with subjects during the experiment.

2.1 Jobs

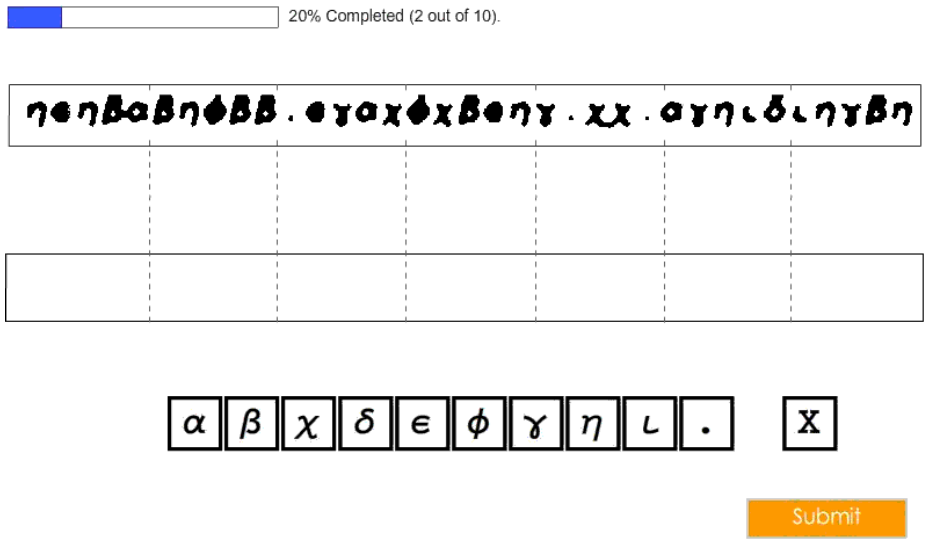
The experiment focuses on intertemporal allocations of effort. Subjects are asked to allocate, subsequently reallocate and complete tasks of two jobs. In Job 1, subjects transcribe a meaningless greek text through a computer interface. Figure 1, Panel A demonstrates the paradigm. Greek letters appear in random order, slightly blurry, in subjects' transcription box. By pointing and clicking on the corresponding keyboard below the transcription box, subjects must reproduce the observed series of Greek letters. One task is the completion of one row of Greek text with 80 percent accuracy as measured by the Levenshtein Distance.[9] In the first week, subjects completed a task from Job 1 in an average of 54 seconds. By the final week, the average was 46 seconds.

In Job 2, subjects are asked to complete four rows of a standard tetris game. Figure 1, Panel B demonstrates the paradigm. Blocks of random shapes appear at the top of the tetris box and fall at fixed speed. Arranging the shapes to complete a horizontal line of the tetris box is the game's objective. Once a row is complete, it disappears and the shapes above fall into place. One task is the completion of four rows of tetris. If the tetris box fills to the top with shapes before the four rows are complete, the subject begins again with credit for the rows already completed. In the first week, subjects completed a task from Job 2 in an average of 55 seconds. By the final week, the average was 46 seconds.

[9] The Levenshtein Distance is commonly used in computer science to measure the distance between two strings and is defined as the minimum number of edits needed to transform one string into the other. Allowable edits are insertion, deletion or change of a single character. As the strings of Greek characters used in the transcription task are 35 characters long our 80 percent accuracy measure is equivalent to 7 edits or less or a Levenshtein Distance ≤ 7.

Figure 1: Experimental Jobs

Panel A: Job 1- Greek Transcription



Panel B: Job 2- Partial Tetris Games

2.2 Experimental Timeline and Allocation Environment

2.2.1 Timeline

The seven weeks of the experiment are divided into two blocks. Weeks 1, 2, and 3 serve as the first block. Weeks 4, 5, and 6 serve as the second block and mirror the first block with the addition of a commitment decision discussed below. Week 7 occurs in the laboratory and is only used to distribute payment to the subjects. Subjects always participate on the same day of the week throughout the experiment. That is, subjects entering the lab on a Monday allocate tasks to be completed on future Mondays. Therefore, the time frame over which effort choices are made is exactly seven days in all choices.

Weeks 1 and 4 occur in the laboratory and subjects are reminded of their study time the night before. Weeks 2, 3, 5, and 6 are completed online. For Weeks 2, 3, 5, and 6, subjects are sent an email reminder at 8pm the night before with a (subject-unique) website address. Subjects are required to log in to this website between 8am and midnight of the day in question and complete their work by 2am the following morning.

At each point of contact, subjects are first given instructions about the decisions to be made and work to be completed that day, reminded of the timeline of the experiment, given demonstrations of any unfamiliar actions, and then asked to complete the necessary actions.

In each week, subjects are required to complete 10 tasks of each Job prior to making allocations decision or completing allocated tasks. The objective of this pair of 10 tasks, which we call "minimum work," is two-fold. First, minimum work requires a few minutes of participation at each date, forcing subjects to incur the transaction costs of logging on to the experimental website at each time.[10] Second, minimum work, especially in Week 1, provides experience for subjects such that they have a sense of how effortful the tasks are when making their allocation decisions. We require minimum work in all weeks before all decisions and provide this information to subjects to control for issues related to projection bias (Loewenstein, O'Donoghue and Rabin, 2003). This ensures that subjects have experienced and can forecast having experienced the same amount of effort when making their allocation decisions at all points in time.

[10]A similar technique is used in monetary discounting studies where minimum payments are employed to eliminate subjects loading allocations to certain dates to avoid transaction costs of receiving multiple payments or cashing multiple checks (Andreoni and Sprenger, 2012a).

2.2.2 Allocation Environment

In Week 1, subjects allocate tasks between Weeks 2 and 3. Hence, subjects are choosing how much work to complete at two future dates. In Week 2, subjects also allocate tasks between Weeks 2 and 3. Note that in Week 1 subjects are making decision involving two future dates, whereas in Week 2, subjects are making decisions involving the present and a future date. Before making the choice in Week 1, subjects are told of the Week 2 decisions and are aware that exactly one of all Week 1 and Week 2 allocation decisions will be implemented.[11]

Initial allocations and subsequent reallocations for Jobs 1 and 2 are made in a convex decision environment. Using slider bars, subjects allocate tasks to two dates, one earlier and one later, under different gross interest rates.[12] Figure 2 provides a sample allocation screen. To motivate the intertemporal tradeoffs faced by subjects, decisions are described as having different 'task rates' such that every task allocated to the sooner date reduces the number of tasks allocated to the later date by a stated number. For example, a task rate of 1:0.5 implies that each task allocated to Week 2 reduces by 0.5 the number allocated to Week 3. It is important to note that the minimum 10 tasks required for each job detailed in the previous section are separate from this allocation decision and are *not counted* toward the allocations.

The subjects' decision can be formulated as allocating tasks e over times t and $t+k$, e_t and e_{t+k}, subject to the present-value budget constraint,

$$e_t + \frac{1}{p} \cdot e_{t+k} = m, \tag{1}$$

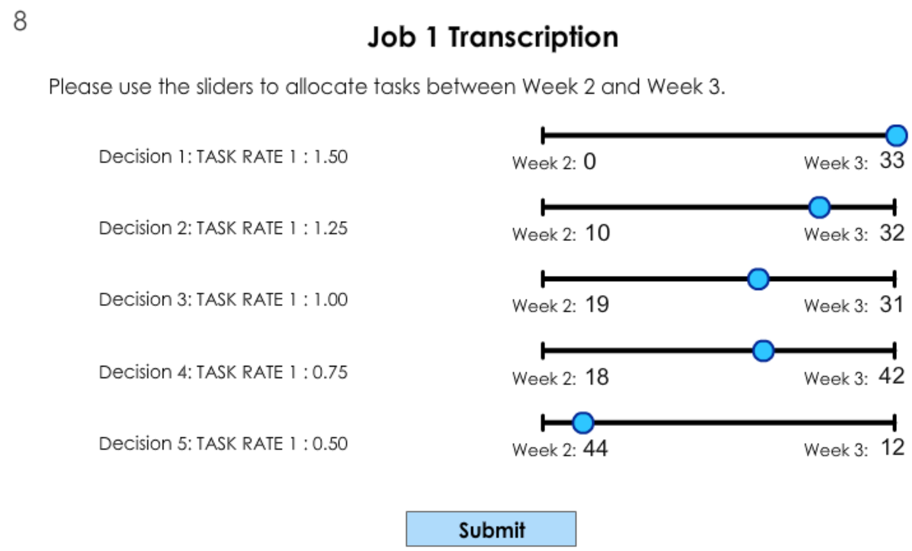
where $1/p$ represents the provided task rate. For each task and for each date where allocations were made, subjects faced five task rates, $1/p \in \{0.5, 0.75, 1, 1.25, 1.5\}$. The number of tasks that subjects could allocate to the sooner date was fixed at fifty such that $m = 50$ in every decision in the experiment. Note that as the task rate falls, the relative cost of a task in Week

[11] Subjects were not shown their initial allocations when making their subsequent reallocations.

[12] Passive allocations are avoided in the design as the sliders' initial location was in the middle of the slider bar and subjects were required to click on every slider before submitting their answers.

2 (the earlier week) falls, altering intertemporal incentives.

Figure 2: Convex Allocation Environment



In Weeks 1 and 2 each subject makes 20 allocation decisions: five for each Job in Week 1 and five for each Job in Week 2. After the Week 2 decisions, one of these 20 allocations is chosen at random as the 'allocation-that-counts' and subjects have to complete the allocated number of tasks to ensure successful completion of the experiment. However, the randomization device probabilistically favors the Week 2 allocations over the Week 1 allocations. In particular, subjects are told (from the beginning) that their Week 1 allocations will count with probability 0.1, while their Week 2 reallocations will count with probability 0.9. Within each week's allocations, every choice is equally likely to be the allocation-that-counts.[13] This randomization process effectively favors flexibility while maintaining incentive compatibility in a comprehensible manner. This design choice was made for two reasons. First, it increased the chance that subjects experienced their own potentially present-biased reallocations. Second, it provides a greater symmetry to the decisions in the second block of three weeks that elicit demand for commitment.

[13]For a complete description of the randomization process please see instructions in Appendix C.

The second block of the experiment, Weeks 4, 5, and 6, mimics the first block of Weeks 1, 2, and 3, with one exception. In Week 4, subjects are offered a probabilistic commitment device, which is described in detail in the following subsection.

2.3 Commitment Demand

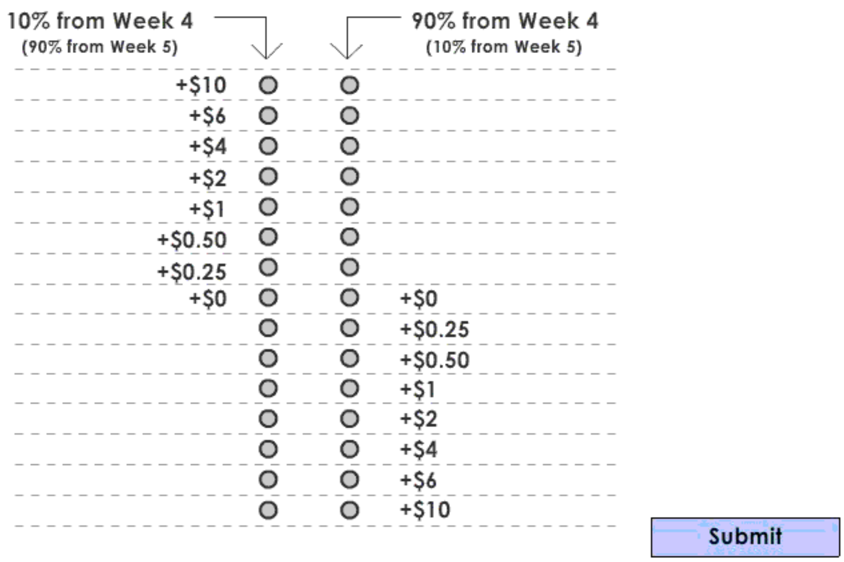
In the second block of the experiment, Weeks 4, 5, and 6, once subjects have gained experience with the tasks and the experimental design, they are offered a probabilistic commitment device. In the first block of the experiment, the allocation-that-counts is taken from the Week 1 allocations with probability 0.1 and from the subsequent Week 2 reallocations with probability 0.9, favoring the later reallocations. In Week 4, subjects are given the opportunity to choose which allocations will be probabilistically favored. In particular, they can choose whether the allocation-that-counts comes from Week 4 with probability 0.1 (and Week 5 with probability 0.9), favoring flexibility, or from Week 4 with probability 0.9, favoring commitment. This form of commitment device was chosen because of its potential to be meaningfully binding. Subjects who choose to commit and who differ in their allocation choices through time can find themselves constrained by commitment with high probability.

In order to operationalize our elicitation of commitment demand, subjects are asked to make 15 multiple price list decisions between two options. In the first option, the allocation-that-counts will come from Week 4 with probability 0.1. In the second option, the allocation-that-counts will come from Week 4 with probability 0.9. In order to determine the strength of preference, an additional payment of between \$0 and \$10 is added to one of the options for each decision.[14] Figure 3 provides the implemented price list. One of the 15 commitment decisions is chosen for implementation, ensuring incentive compatibility. Subjects are told that the implementation of the randomization for the commitment decisions will occur once they submitted their Week 5 allocation decisions.

Our commitment demand decisions, and the second block of the experiment, serve three

[14] We chose not to have the listed prices ever take negative values (as in a cost) to avoid subjects viewing paying for commitment as a loss.

Figure 3: Commitment Demand Elicitation



purposes. First, they allow us to assess the demand for commitment and its extent. If individuals demand commitment, it is important to know both how much they are willing to pay for the opportunity to restrict their future activities and to help separate commitment demand from simple decision error. Second, a key objective of our study is to explore the theoretical link, under the assumption of sophistication, between present bias and commitment demand. Are subjects who are present biased comparing initial allocations to subsequent reallocations more likely to demand commitment? With the exception of Ashraf et al. (2006) and Kaur, Kremer and Mullainathan (2010) virtually no research tests this critical implication of models of dynamic inconsistency. We will compare our results to those papers in subsection 4.4. Finally, a correlation between time inconsistency and commitment validates the interpretation of present bias over other explanations for time inconsistent effort choices.

To summarize our longitudinal effort experiment, Table 1 contains the major events in each week.

Table 1: Summary of Longitudinal Experiment

	Minimum Work	10 Effort Allocations	Allocation-That-Counts Chosen	Complete Work	Commitment Choice	Receive Payment
Week 1 (In Lab):	x	x				
Week 2 (Online):	x	x	x	x		
Week 3 (Online):	x			x		
Week 4 (In Lab):	x	x			x	
Week 5 (Online):	x	x	x	x		
Week 6 (Online):	x			x		
Week 7 (In Lab):						x

2.4 Design Details

102 subjects from the UC Berkeley Xlab subject pool were initially recruited into the experiment across 4 experimental sessions on February 8th, 9th and 10th, 2012 and were told in advance of the seven week longitudinal design and the $100 completion bonus.[15] Subjects did not receive an independent show up fee. 90 subjects completed all aspects of the working over time experiment and received the $100 completion bonus. The 12 subjects who selected out of the experiment do not appear different on either initial allocations, comprehension or a small series of demographic data collected at the end of the first day of the experiment.[16] One more subject completed initial allocations in Week 1, but due to computer error did not have their choices recorded. This leaves us with 89 subjects.

One critical aspect of behavior limits our ability to make inference for time preferences based on experimental responses. In particular, if subjects have no variation in allocations in response to gross interest rate changes in some weeks, then attempting to point identify both discounting and cost function parameters is difficult or impossible, yielding imprecise and unstable estimates. Similar to multiple price list experiments, if a subject always chooses a

[15]This is a potentially important avenue of selection into the experiment. Our subjects were willing to put forth effort and wait seven weeks to receive $100. Though we have no formal test, this suggests that our subjects may be a relatively patient selection.

[16]3 of those 12 subjects dropped after the first week while the remaining 9 dropped after the second week. Including data for these 9 subjects where available does not qualitatively alter the analysis or conclusions.

specific option, only one-sided bounds on parameters can be obtained. Here, the problem is compounded by our efforts to identify both discounting and cost function parameters. In our sample, nine subjects have this issue for one or more weeks of the study. For the analysis, we focus on the primary sample of 80 subjects who completed all aspects of the experiment with positive variation in their responses in each week. In Appendix Table A2, we re-conduct the aggregate analysis including these nine subjects and obtain very similar findings.

2.5 Monetary Discounting

Subjects were present in the UC Berkeley X-Laboratory in the first, fourth, and seventh weeks of the experiment. This repeated interaction facilitates a monetary discounting study that complements our main avenue of analysis. In Weeks 1 and 4 of our experimental design, once subjects complete their allocation of tasks, they are invited to respond to additional questions allocating monetary payments to Weeks 1, 4, and 7. In Week 1, we implement three Andreoni and Sprenger (2012a) Convex Time Budget (CTB) choice sets, allocating payments across: 1) Week 1 vs. Week 4; 2) Week 4 vs. Week 7 (Prospective); and 3) Week 1 vs. Week 7. Individuals are asked to allocate monetary payments c across the two dates t and $t+k$, c_t and c_{t+k}, subject to the intertemporal budget constraint,

$$r \cdot c_t + c_{t+k} = m. \tag{2}$$

The experimental budget is fixed at $m = \$20$ and five gross interest rates are implemented in each choice set, $r \in \{0.99, 1, 1.11, 1.25, 1.43\}$. These gross interest rates were chosen for comparison with prior work (Andreoni and Sprenger, 2012a).[17] Such questions permit identification of monetary discounting parameters following Andreoni and Sprenger (2012a). In Week 4, we ask subjects to allocate in a CTB choice set over Week 4 and Week 7 under the same five gross interest rates. We refer to these choices made in Week 4 as Week 4 vs. Week 7 and those made in Week 1 over these two dates as Week 4 vs. Week 7 (Prospective). Hence, subjects complete

[17] Additionally, $r = 0.99$ allows us to investigate the potential extent of negative discounting.

a total of four CTB choice sets.

The CTBs implemented in Weeks 1 and 4 are paid separately and independently from the rest of the experiment with one choice from Week 1 and one choice from Week 4 chosen to be implemented. Subjects are paid according to their choices. Subjects are not told of the Week 4 choices in Week 1. As in Andreoni and Sprenger (2012a), miniminum payments of $5 at each payment date are enacted to eliminate transaction cost issues similar to those discussed above. Appendix C provides the full experimental instructions.

The implemented monetary discounting experiments have two nuances relative to Andreoni and Sprenger (2012a). First, Andreoni and Sprenger (2012a) implement CTBs with payment by check. Our design implements payment by cash with potentially lower transaction costs. Second, Andreoni and Sprenger (2012a) implement CTBs with present payment received only by 5:00 p.m. in a subject's residence mailbox. Here we provide payment immediately in the laboratory limiting arguments about the relevant epoch of the present.

In both Weeks 1 and 4, the monetary allocations are implemented after the more central effort choices. The monetary choices were not announced in advance and subjects could choose not to participate; five did so in either Weeks 1 or 4. In our analysis of monetary discounting, we focus on the 75 subjects from the primary sample with complete monetary choice data.

3 Identification

In the intertemporal allocation of effort and money, discounting and additional parameters can be identified at either the aggregate or individual level under various structural assumptions. In the following two subsections we describe which experimental variation provides identification of specific parameters of interest and lay out methodology for estimation at both the aggregate and individual level.

3.1 Effort Discounting

In the working over time experiment, subjects allocate effort to an earlier date, e_t, and a later date, e_{t+k}, subject to the intertemporal budget constraint described in (1). Hence, the subject's decision problem is

$$min_{e_t,e_{t+k}}\ C(e_t, e_{t+k}) \quad s.t. \quad e_t + \frac{1}{p}e_{t+k} = m,$$

where $C(e_t, e_{t+k})$ is a general cost function, assumed to be globally convex such that standard constrained optimization yields meaningful first order conditions. We assume that the cost function is time separable, that the instantaneous cost function is stationary and takes an exponential form, and that discounting follows the quasi-hyperbolic form proposed by Laibson (1997). Under these structural assumptions we can write

$$C(e_t, e_{t+k}) = (e_t + \omega)^{\gamma} + \beta^{\mathbf{1}_{t=0}}\delta^k(e_{t+k} + \omega)^{\gamma}, \tag{3}$$

where $\gamma > 1$ represents the stationary parameter on the convex instantaneous cost of effort function. The present-bias parameter, β, activated when the time period t is the present, $\mathbf{1}_{t=0}$, captures the extent to which individual's disproportionately discount the future when viewed from the present. The parameter δ captures the daily discount factor over the $k = 7$ days of each considered allocation. The additive term ω in the cost function could be interpreted as a Stone-Geary minimum or as some background level of required work. Such parameters are used in monetary discounting studies (Andersen, Harrison, Lau and Rutstrom, 2008; Andreoni and Sprenger, 2012a), and are either taken from some external data source on background consumption or estimated from experimental choices. For simplicity, we interpret ω as the required minimum work of the experiment and set $\omega = 10$.

Minimizing (3) subject to (1) yields the intertemporal Euler equation

$$(\frac{e_t + \omega}{e_{t+k} + \omega})^{(\gamma-1)}(\frac{1}{\beta^{(\mathbf{1}_{t=0})}\delta^k}) = p.$$

Rearranging and taking logs yields

$$log(\frac{e_t+\omega}{e_{t+k}+\omega}) = \frac{log(\beta)}{\gamma-1}\cdot(\mathbf{1}_{t=0}) + \frac{log(\delta)}{\gamma-1}\cdot k + (\frac{1}{\gamma-1})\cdot log(p), \quad (4)$$

which is linear in the key experimentally varied parameters of whether allocations involve the present, $\mathbf{1}_{t=0}$, and a log transform of the task rate, $log(p)$.

From the intertemporal Euler equation above, identification of discounting and the cost function is straightforward. The task rate delivers identification of the cost function, γ; the choice being made in the present (Week 2 decision) rather than the future (Week 1 decision) delivers identification of present bias, β; and the delay length gives identification of the discount factor, δ.[18]

In order to estimate discounting and cost function parameters from aggregate data, we assume an additive error structure and estimate the linear regression implied by (4). The parameters of interest can be recovered from non-linear combinations of regression coefficients with standard errors calculated via the delta method.[19] One important issue to consider in the estimation of (4) is the potential presence of corner solutions. We provide estimates from two-limit tobit regressions designed to account for the possibility that the tangency condition implied by (4) does not hold with equality (Wooldridge, 2002).

Estimating (4) is easily extended to the study of individual parameters. To begin, (4) can be estimated at the individual level.[20] However, with limited numbers of individual choices it is helpful to consider alternative, more structured approaches. In particular, we allow for heterogeneous discounting across individuals, but assume all individuals have the same cost

[18] Of course, with only one delay length of seven days considered in the experiment, we have limited confidence that our estimate of δ can be extrapolated to arbitrary delay lengths.

[19] To be specific, the regression equation is, for $k = 7$,

$$log(\frac{e_t+\omega}{e_{t+k}+\omega})_i = \eta_0 k + \eta_1\cdot(\mathbf{1}_{t=0})_i + \eta_2\cdot log(p)_i + \epsilon_i,$$

and we recover the parameters of interest as $\hat{\beta} = exp(\hat{\eta}_1/\hat{\eta}_2)$ and $\hat{\gamma} = 1 + 1/\hat{\eta}_2$. Note that $\hat{\delta} = exp(\hat{\eta}_0/\hat{\eta}_2)$ is recovered from the constant as only one delay length was used in the experimental design.

[20] Broadly similar conclusions are reached when estimating (4) at the individual level, however, parameter precision is greatly reduced and substantial estimate instability is uncovered in some cases.

function. Consider a vector of fixed effects $(\mathbf{1}_j)_i$ which take the value 1 if observation i was contributed by individual j. This leads to the fixed effects formulation

$$log(\frac{e_t+\omega}{e_{t+k}+\omega})_i = \frac{log(\overline{\delta})}{\gamma-1}\cdot k + \frac{(log(\boldsymbol{\delta}_j)-log(\overline{\delta}))}{\gamma-1}\cdot(\mathbf{1}_j)_i\cdot k + \frac{log(\overline{\beta})}{\gamma-1}\cdot(\mathbf{1}_{t=0})_i$$
$$+\frac{(log(\boldsymbol{\beta}_j)-log(\overline{\beta}))}{\gamma-1}\cdot(\mathbf{1}_{t=0})_i\cdot(\mathbf{1}_j)_i + \frac{1}{\gamma-1}\cdot log(p)_i,$$

where $\overline{\delta}$, $\overline{\beta}$ refer to sample means, and δ_j, β_j refer to individual-specific discounting parameters. With an additive error structure this is easily estimable.[21] The individual fixed effect interacted with the decision being made in the present provides identification of the individual-specific β_j. In Appendix A we conduct simulation exercises under various correlation structures for the true parameters of interest and document that the implemented estimation methods perform well both at the aggregate and individual level.

3.2 Monetary Discounting

Our methods for recovering monetary discounting parameters at both the aggregate and individual level closely follow those for effort. Following most of the literature, we abstract from standard arbitrage arguments for monetary discounting and assume laboratory administered rates are the relevant ones.[22] In particular, for monetary payments, c_t and c_{t+k}, allocated subject to the constraint (2), we assume a quasi-hyperbolic constant relative risk averse utility function,

$$U(c_t, c_{t+k}) = (c_t+\omega)^\alpha + \beta^{\mathbf{1}_{t=0}}\delta^k(c_{t+k}+\omega)^\alpha. \tag{5}$$

Here, the utility function is assumed to be concave, $\alpha < 1$, such that first order conditions provided meaningful optima. Here, the parameter ω is a background parameter that we take

[21] We allow both β and δ to vary across individuals such that the implemented regression is a standard interaction with both level and slope effects.

[22] One prominent exception to this tradition is Harrison et al. (2002), who measure and account for extra-lab borrowing and savings opportunities.

to be the \$5 minimum payment of the monetary experiment.[23]

Maximizing (6) subject to the intertemporal budget constraint (2) yields an intertemporal Euler equation similar to that above for effort. Taking logs and rearranging we have

$$log(\frac{c_t + \omega}{c_{t+k} + \omega}) = \frac{log(\beta)}{\alpha - 1} \cdot (\mathbf{1}_{t=0}) + \frac{log(\delta)}{\alpha - 1} \cdot k + (\frac{1}{\alpha - 1}) \cdot log(r), \tag{6}$$

which can again be estimated at the aggregate or individual level via two-limit Tobit. Discounting and utility function parameters can be recovered via non-linear combinations of regression coefficients as above with standard errors estimated again via the delta method.

4 Results

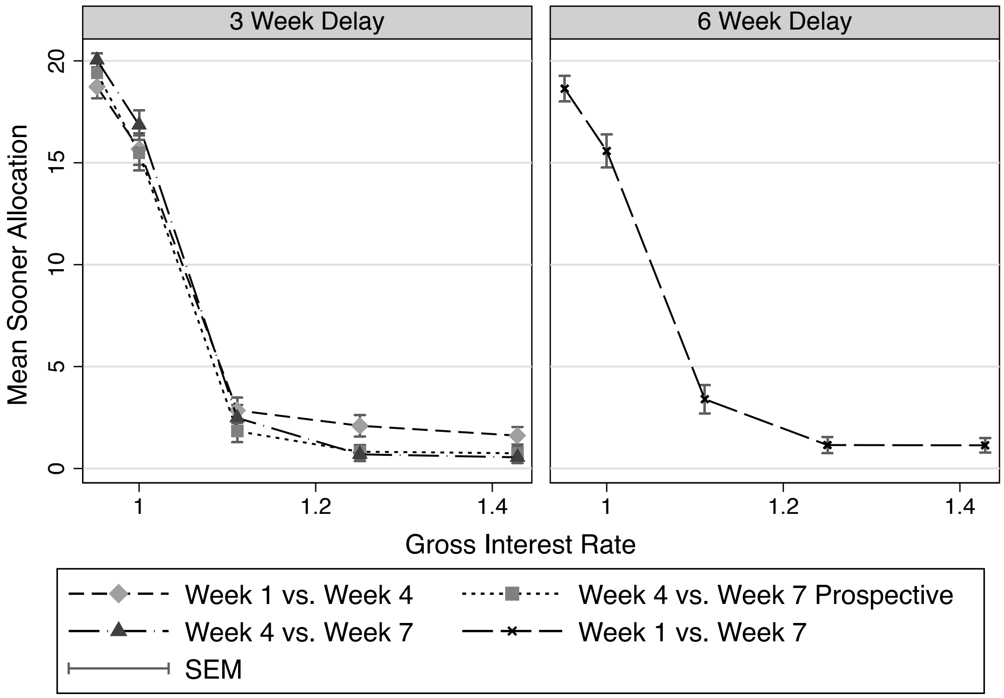
The results are presented in three subsections. First, we present results from the monetary discounting study and compare our observed level of limited present bias with other recent findings. Second, we move to effort related discounting and provide both non-parametric and parametric evidence of present bias. In a third subsection we present results related to commitment demand and document correlations between identified present bias over work and money with the demand for our lab-offered commitment device.

4.1 Monetary Discounting

Figure 4 presents the data from our monetary discounting experiment. The mean allocation to the sooner payment date at each interest rate is reported for the 75 subjects from the primary sample for whom we have all monetary discounting data. Four data series are provided separated by delay length corresponding to the four payment sets over which subjects made allocations. Standard error bars are provided, clustered at the individual level.

[23] Andreoni and Sprenger (2012a) provide detailed discussion of the use of such background parameters and provide robustness tests with differing values of ω and differing assumptions for the functional form of utility in CTB estimates. The findings suggest that though utility function curvature estimates may be sensitive to different background parameter assumptions, discounting parameters, particularly present bias, are virtually unaffected by such choices.

Figure 4: Monetary Discounting Behavior



We highlight two features of Figure 4. First, note that as the gross interest rate increases the average allocation to the sooner payment decreases, following the law of demand. Indeed, at the individual level 98% of choices are monotonically decreasing in interest rate, and only 1 subject exhibits more than 5 non-monotonicities in demand in their monetary choices.[24] This suggests that subjects as a whole understand the implied intertemporal tradeoffs and the decision environment.

Second, Figure 4 allows for non-parametric investigation of present bias in two contexts.[25] First, one can consider the static behavior, often attributed to present bias, of subjects being

[24]Subjects have 16 opportunities to violate monotonicity comparing two adjacent interest rates in their 20 total CTB choices. 63 of 75 subjects have no identified non-monotonocities. Andreoni and Sprenger (2012a) provide a detailed discussion of the extent of potential errors in CTB choices. In particular they note that prevalence of non-monotonicities in demand are somewhat less than the similar behavior of multiple switching in standard Multiple Price List experiments.

[25]Though the six-week delay data are used in estimation, our non-parametric tests only identify present bias from choices over three-week delays. We ignore here the method of identifying present bias frequently used in psychology where short horizon choices are compared to long horizon choices.

more patient in the future than in the present by comparing Week 1 vs. Week 4 to Week 4 vs. Week 7 (Prospective). In this comparison, controlling for interest rate fixed effects, subjects do allocate on average \$0.54 ($s.e = 0.31$) more to the sooner payment when it is in the present $F(1, 74) = 2.93,\ (p = 0.09)$. A second measure of present bias is to compare Week 4 vs. Week 7 (Prospective) made in Week 1 to the Week 4 vs. Week 7 choices made in Week 4. This measure is similar to the recent work of Halevy (2012). Ignoring income effects associated with having potentially received prior experimental payments, this comparison provides a secondary measure of present bias. In this comparison, controlling for interest rate fixed effects, subjects allocate on average \$0.47 ($s.e = 0.32$) more to the sooner payment when the sooner payment is in the present, $F(1, 74) = 2.08,\ (p = 0.15)$.[26]

Over monetary payments, we find limited non-parametric support for the existence of a present bias. In Table 2, columns (1) and (2) we provide corresponding parameter estimates implementing two-limit Tobit regressions of (6), with standard errors clustered at the individual level. In column (1) we use all 4 CTB choice sets. In column (2) we use only the choice sets which have three-week delays for continuity with both our non-parametric evidence and the comparisons generally made in experimental economics. In both cases we estimate $\hat{\alpha}$ of around 0.975 indicating limited utility function curvature over monetary payments. Further, we identify daily discount factors of around 0.998. The 95% confidence interval in column (1) for the daily discount factor implies annual discount rates between 40% and 140%.[27]

In column (1) of Table 2 we estimate $\hat{\beta} = 0.974$ ($s.e. = 0.009$), close to dynamic consistency. Though we do reject the null hypothesis of $\beta = 1$, $\chi^2(1) = 8.77,\ (p < 0.01)$, our estimated value for β is economically close to 1. In column (2), focusing only on three week delay data, we find $\hat{\beta} = 0.988$ (0.009) and are unable to reject the null hypothesis of dynamic consistency,

[26] Additionally, this measure is close in spirit to our effort experiment where initial allocations are compared to subsequent reallocations. To get a sense of the size of potential income effects, we can also compare the Week 1 vs. Week 4 choices made in Week 1 to the Week 4 vs. Week 7 choices made in Week 4. Controlling for interest rate fixed effects, subjects allocate on average \$0.07 ($s.e = 0.31$) more to the sooner payment in Week 1, $F(1, 74) = 0.05,\ (p = 0.82)$, suggesting negligible income effects.

[27] Admittedly, our ability to precisely identify aggregate discounting was not a focus of the experimental design and is compromised by limited variation in delay length and interest rates. In monetary discounting experiments it is not unusual to find implied annual discount rates in excess of 100%.

Table 2: Parameter Estimates

	Monetary Discounting		Effort Discounting		
	(1) All Delay Lengths	(2) Three Week Delay Lengths	(3) Job 1 Greek	(4) Job 2 Tetris	(5) Combined
Present Bias Parameter: $\hat{\beta}$	0.974 (0.009)	0.988 (0.009)	0.900 (0.037)	0.877 (0.036)	0.888 (0.033)
Daily Discount Factor: $\hat{\delta}$	0.998 (0.000)	0.997 (0.000)	0.999 (0.004)	1.001 (0.004)	1.000 (0.004)
Monetary Curvature Parameter: $\hat{\alpha}$	0.975 (0.006)	0.976 (0.005)			
Cost of Effort Parameter: $\hat{\gamma}$			1.624 (0.114)	1.557 (0.099)	1.589 (0.104)
# Observations	1500	1125	800	800	1600
# Clusters	75	75	80	80	80
Job Effects					Yes
$H_0 : \beta = 1$	$\chi^2(1) = 8.77$ ($p < 0.01$)	$\chi^2(1) = 1.96$ ($p = 0.16$)	$\chi^2(1) = 7.36$ ($p < 0.01$)	$\chi^2(1) = 11.43$ ($p < 0.01$)	$\chi^2(1) = 11.42$ ($p < 0.01$)
$H_0 : \beta(Col.\ 1) = \beta(Col.\ 5)$	$\chi^2(1) = 6.37$ ($p = 0.01$)				
$H_0 : \beta(Col.\ 2) = \beta(Col.\ 5)$		$\chi^2(1) = 8.26$ ($p < 0.01$)			

Notes: Parameters identified from two-limit Tobit regressions of equations (6) and (4) for monetary discounting and effort discounting, respectively. Parameters recovered via non-linear combinations of regression coefficients. Standard errors clustered at individual level reported in parentheses, recovered via the delta method. Effort regressions control for Job Effects (Task 1 vs. Task 2). We use Chi-squared tests for the null hypotheses in the last three rows.

$\chi^2(1) = 1.96$, ($p = 0.16$). For a fixed delay length and interest rate, subjects make virtually identical allocations in the present and the future.

Our non-parametric and parametric results closely mirror the aggregate findings of Andreoni and Sprenger (2012a) and Gine et al. (2010).[28] A potential concern of these earlier studies that carefully control transaction costs and payment reliability, is that a payment in the present was implemented by a payment in the afternoon of the same day, e.g. by 5:00 pm in the subjects' residence mailboxes in Andreoni and Sprenger (2012a). In this paper, because subjects

[28]In both of these prior exercises substantial heterogeneity in behavior is uncovered. In subsection 4.3 we conduct individual analyses, revealing similar findings.

repeatedly have to come to the lab, a payment in the present is implemented by an immediate cash payment. The fact that we replicate the earlier studies that carefully control for transaction costs and payment reliability alleviates the concerns that payments in the afternoon are not treated as present payments.

Confirming the finding of limited present bias in the domain of money motivates our exploration of choices over effort. Clear confounds exist for identifying or rejecting models of dynamic inconsistency from monetary choices. In the next section we attempt to test the central hypothesis of diminishing impatience without these confounds in the domain of effort.

4.2 Effort Discounting

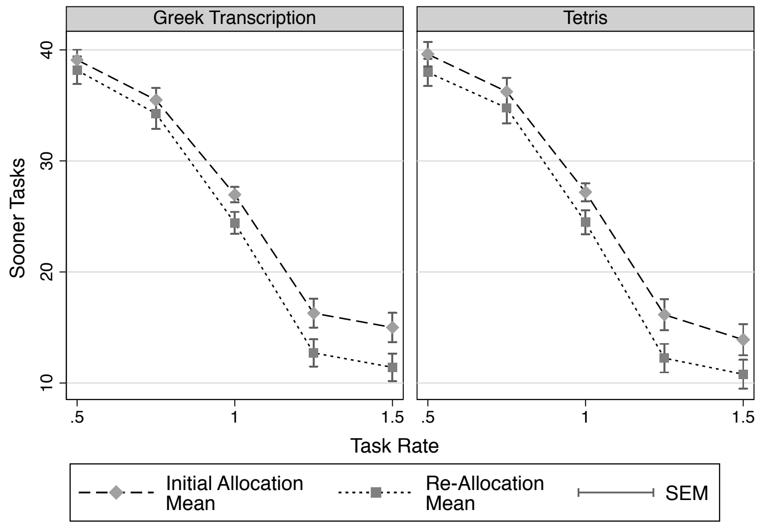
Subjects make a total of 40 allocation decisions over effort in our seven week experiment. Twenty of these decisions are initial allocations and reallocations made in the first block of the experiment. The other twenty are made in the second block. Our design is focused on testing whether participants identified as being present biased (in Block 1) demand commitment in Week 4 (in Block 2). Hence, we opt to present here allocation data from only the first block of the experiment, Weeks 1 and 2. This allows the prediction of Week 4's commitment demand to be conducted truly as an out-of-sample exercise. In Appendix B.3, we present results of present bias from both blocks of the experiment and document very similar results.

In Figure 5 we show for each task rate the amount of tasks allocated to the sooner date, Week 2, which could range from 0 to 50. We contrast initial allocations of effort made in Week 1 at each task rate with subsequent reallocations made in Week 2 for the 80 subjects of the primary sample. Standard errors bars are provided, clustered at the individual level.

As with monetary discounting, subjects appear to have understood the central intertemporal tradeoffs of the experiment as both initial allocations and subsequent reallocations decrease as the task rate is increased. At the individual level 95% of choices are monotonically decreasing in interest rate, and only 5 subjects exhibit more than 5 non-monotonicities in their effort choices.[29]

[29]Subjects have 32 opportunities to violate monotonicity comparing two adjacent interest rates in their 40 total CTB choices. 41 of 80 subjects are fully consistent with monotonicity and only 5 subjects have more

Figure 5: Real Effort Discounting Behavior



This suggests that subjects as a whole understand the implied intertemporal tradeoffs and the decision environment.

Apparent from the observed choices is that at all task rates average subsequent reallocations lie below average initial allocations. Controlling for all task rate and task interactions, subjects allocate 2.47 fewer tasks to the sooner work date when the sooner work date is the present $F(1,79) = 14.78$, ($p < 0.01$). Subjects initially allocate 9.3% more to the sooner date than they subsequently reallocate (26.59 initial vs. 24.12 reallocation).[30]

Motivated by our non-parametric analysis we proceed to estimate intertemporal parameters. Table 2 columns (3) through (5) present two-limit Tobit regressions based on (4). In column (3) the analyzed data are the allocations for Job 1, Greek Transcription. We find an estimated

than 5 non-monotonicities. Deviations are in general small with a median required allocation change of 3 tasks to bring the data in line with monotonicity. Three subjects have more than 10 non-monotonicities indicating upward sloping sooner effort curves. Such subjects may find the tasks enjoyable such that they prefer to do more tasks sooner to fewer tasks later. We believe the increased volume of non-downward sloping behavior in effort relative to money has several sources. Subjects may actually enjoy the tasks, they make more choices for effort than for money, and half of their allocations are completed outside of the controlled lab environment. Importantly, non-monotonicities decrease with experience such that in the second block of the experiment 97 percent of choices satisfy monotonicity while in the first block, only 93 percent do so, $F(1,79) = 8.34$ ($p < 0.01$).

[30] The behavior is more pronounced for the first block of the experiment. For both blocks combined subjects allocate 25.95 tasks to the sooner date, 1.59 more tasks than they subsequently reallocate (24.38 tasks), representing a difference of around 6%, $F(1,79) = 15.16$, ($p < 0.01$). See Appendix B.3 for detail.

cost parameter $\hat{\gamma} = 1.624$ (0.114). Abstracting from discounting, a subject with this parameter would be indifferent between completing all 50 tasks on one experimental date and completing 32 tasks over two experimental dates.[31] The daily discount factor of $\hat{\delta} = 0.999$ (0.004) is similar to our findings for monetary discounting.

In column (3) of Table 2 we estimate an aggregate $\hat{\beta} = 0.900$ (0.037), and easily reject the null hypothesis of dynamic consistency, $\chi^2(1) = 7.36,\ (p < 0.01)$. In column (4), we obtain broadly similar conclusions for Job 2, the partial tetris games. We aggregate over the two jobs in column (5), controlling for the job, and again document that subjects are significantly present-biased over effort.[32]

Finally, our implemented analysis allows us to compare present bias across effort and money with χ^2 tests based on seemingly unrelated estimation techniques. We reject the null hypothesis that the β identified in column (5) over effort is equal to that identified for monetary discounting in column (1), $\chi^2(1) = 6.37,\ (p = 0.01)$, or column (2), $\chi^2(1) = 8.26,\ (p < 0.01)$. Subjects are significantly more present-biased over effort than over money.[33]

4.3 Individual Analysis

On aggregate, we find that subjects are significantly more present-biased over work than over money. In this sub-section we investigate behavior at the individual level to understand the extent to which present bias over effort and money is correlated within individual.

In order to investigate individual level discounting parameters we run fixed effect versions of the regressions provided in columns (2) and (5) of Table 2.[34] As discussed in section 3, we

[31]In many applications in economics and experiments, quadratic cost functions are assumed for tractability and our analysis suggests that at least in our domain this assumption would not be too inaccurate.

[32]For robustness, we run regressions similar to column (5) separately for each week and note that though the cost function does change somewhat from week to week, present bias is still significantly identified as individuals are significantly less patient in their reallocation decisions compared to their initial allocation decisions. Appendix Table A3 provides estimates.

[33]In Appendix B.3 we conduct identical analysis using both Blocks 1 and 2 and arrive at the same conclusions. See Appendix Table A4 for estimates.

[34]We choose to use the measures of present bias based on three week delay choices for the monetary discounting for continuity with our non-parametric tests of present bias. Further, when validating our individual measures, we focus on reallocations over three week delay decisions as in the presentation for the aggregate data. Very similar results are obtained if we use the fixed effects versions of Table 2, column (1).

identify discounting parameters at the individual level assuming no heterogeneity in cost or utility function curvature. Individual parameter estimates of $\hat{\beta}_e$, present bias for effort, and $\hat{\beta}_m$, present bias for money, are recovered as non-linear combinations of regression coefficients as described in section 3.

One technical constraint prevents us from estimating individual discounting parameters with two-limit Tobit as in the aggregate analysis. In order for parameters to be estimable at the individual level with two-limit Tobit, some interior allocations are required. As suggested by our curvature estimates in Table 2, 86% of monetary allocations are at budget corners and 61% of the sample has zero interior allocations. This issue is not as severe for effort discounting as only 31% of allocations are at budget corners and only 1 subject has zero interior allocations. For individual-level discounting, we therefore use ordinary least squares for both money and effort.[35]

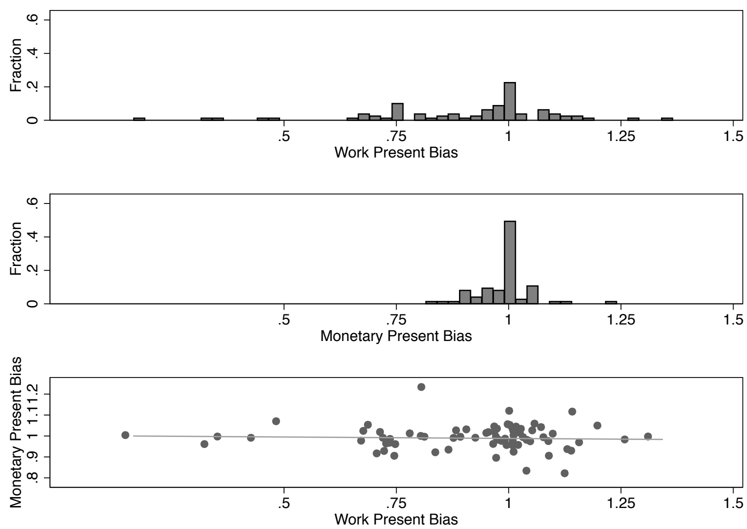
Figure 6 presents individual estimates and their correlation. First, note that nearly 60% of subjects have an estimated $\hat{\beta}_m$ close to 1, indicating dynamic consistency for monetary discounting choices. This is in contrast to only around 25% of subjects with $\hat{\beta}_e$ close to 1. The mean value for $\hat{\beta}_m$ is 0.99 ($s.d. = 0.06$), while the mean value for $\hat{\beta}_e$ is 0.91 ($s.d. = 0.20$). The difference between these measures is significant, $t = 3.09$, $(p < 0.01)$. Second, note that for the majority of subjects when they deviate from dynamic consistency in effort, they deviate in the direction of present bias.

Since correlational studies (e.g., Ashraf et al., 2006; Meier and Sprenger, 2010) often use binary measures of present bias, we define the variables 'Present Biased'$_e$ and 'Present Biased'$_m$ which take the value 1 if the corresponding estimate of β lies strictly below 0.99 and zero otherwise. We find that 56% of subjects have a 'Present-Biased'$_e$ of 1 while only 33% of subjects have a 'Present-Biased'$_m$ of 1. The difference in proportions of individuals classified

[35]Nearly identical aggregate discounting estimates are generated when conducting ordinary least squares versions of Table 2. Curvature estimates, however, are sensitive to estimation techniques that do and do not recognize that the tangency conditions implied by (4) and (6) may be met with inequality at budget corners. Andreoni and Sprenger (2012a) provides further discussion.

as present-biased over work and money is significant, $z = 2.31,\ (p = 0.02)$.[36]

Figure 6: Individual Estimates of Present Bias



Two important questions with respect to our individual measures arise. First, how much do these measures correlate within individual? The answer to this question is important for understanding both the validity of studies relying on monetary measures and the potential consistency of preferences across domains. If significant correlations are obtained it suggests that there may be some important preference-related behavior uncovered in monetary discounting studies.[37] Figure 6 presents a scatterplot of $\hat{\beta}_m$ and $\hat{\beta}_e$. In our sample of 75 subjects with both complete monetary and effort discounting choices, we find that $\hat{\beta}_e$ and $\hat{\beta}_m$ have almost zero correlation, $\rho = -0.05,\ (p = 0.66)$. Additionally, we find that the binary measures for present

[36]Further, one can define future bias in a similar way. 17% of subjects are future biased in money while 29% of subjects are future biased over effort. Similar differing proportions between present and future bias have been previously documented (see, e.g., Ashraf et al., 2006; Meier and Sprenger, 2010). Two important counter-examples are Gine et al. (2010) who find almost equal proportions of present and future biased choices and Dohmen, Falk, Huffman and Sunde (2006) who find a greater proportion of future-biased than present-biased subjects.

[37]Indeed psychology provides some grounds for such views as money generates broadly similar rewards-related neural patterns as more primary incentives (Knutson, Adams, Fong and Hommer, 2001), and in the domain of discounting evidence suggests that discounting over primary rewards, such as juice, produces similar neural images to discounting over monetary rewards (McClure, Laibson, Loewenstein and Cohen, 2004; McClure et al., 2007).

bias, 'Present Biased'$_e$ and 'Present Biased'$_m$ are also uncorrelated, $\rho = 0.11,\ (p = 0.33)$.[38]

Table 3: Validation of Individual Parameter Estimates

Dependent Variable:	*Budget Share Distance*			
	Effort Discounting		Monetary Discounting	
	(1)	(2)	(3)	(4)
Real Effort Present Bias Parameter: $\hat{\beta}_e$	0.532***			
	(0.053)			
Present Biased$_e$ (=1)		-0.123***		
		(0.020)		
Monetary Present Bias Parameter:$\hat{\beta}_m$			2.393***	
			(0.052)	
Present Biased$_m$ (=1)				-0.201***
				(0.026)
Constant	-0.531***	0.020***	-2.391***	0.044***
	(0.052)	(0.007)	(0.049)	(0.015)
Job Effects	Yes	Yes	-	-
Choice Set Effects	-	-	Yes	Yes
# Observations	800	800	750	750
# Uncensored Observations	798	798	731	731
# Clusters	80	80	75	75

Notes: Coefficients from tobit regressions of budget share distance $\in [-1, 1]$ on identified individual discounting parameters. 10 reallocations per individual for effort decisions and 10 reallocations per individual for monetary decisions. Standard errors clustered on individual level in parentheses. Job fixed effects for effort and choice set fixed effects for monetary discounting included but not reported. Discounting parameters identified from OLS regressions for monetary discounting and real effort discounting with individual specific effects for both $\hat{\delta}$ and $\hat{\beta}$. Curvature parameter, α, and cost parameter, λ, assumed constant across individuals. Effort regressions identifying parameters control for Job Effects (Job 1 vs. Job 2). Monetary Present Bias (=1) and Effort Present Bias (=1) calculated as individuals with estimated $\hat{\beta} < 0.99$ in the relevant domain. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The second question concerning our estimated parameters is whether they can be validated

[38]Interestingly, when using both Blocks 1 and 2 of the data, we come to a slightly different conclusion. Though $\hat{\beta}_m$ and $\hat{\beta}_e$ remain virtually uncorrelated, with the additional data we uncover a substantial and significant correlation between Present Biased'$_e$ and 'Present Biased'$_m$ $\rho = 0.24,\ (p = 0.03)$. Further, 'Present Biased'$_m$ is also significantly correlated with the continuous measure $\hat{\beta}_e$, $\rho = -0.27,\ (p = 0.02)$. More work is needed to understand the relationship between monetary and effort present bias parameters.

in sample. That is, given that $\hat{\beta}_e$ and $\hat{\beta}_m$ are recovered as non-linear combinations of regression coefficients, to what extent do these measures predict present-biased reallocations of tasks and money? In order to examine this internal validity question, we generate distance measures for reallocations. For effort choices we calculate the budget share of each allocation for Week 2 effort. The difference in budget shares between reallocation and initial allocation is what we term a 'Budget Share Distance.'[39] As budget shares are valued between $[0, 1]$, our budget share distance measure takes values on the interval $[-1, 1]$, with negative numbers indicating present-biased reallocations. Each subject has 10 such effort budget share distance measures in Block 1. A similar measure is constructed for monetary discounting choices. Taking only the three week delay data, at each gross interest rate we take the difference between the future allocation (Week 4 vs. Week 7 (Prospective)) budget share and the present allocation (Week 1 vs. Week 4 or Week 4 vs. Week 7) budget share. This measure takes values on the interval $[-1, 1]$, with negative numbers indicating present-biased reallocations. Each subject has 10 such monetary budget share distance measures.

Table 3 presents a validation table with tobit regressions of 'Budget Share Distance' for effort and money on our corresponding parameter estimates.[40] Individuals who are identified as present-biased by our parameter measures do indeed have more present-biased reallocations for both work and money. Columns (1) and (3) demonstrate that individuals with lower values of $\hat{\beta}_e$ and $\hat{\beta}_m$ make more present-biased reallocations, and those subjects with $\beta = 1$ would be predicted to make virtually identical allocations through time. Columns (2) and (4) demonstrate that subjects who are present-biased over effort allocate 12 percent less of their budget to the sooner date when the sooner date is the present. Subjects who are present-biased over money allocate around 20 percent less of their budget to the sooner payment when the sooner payment is in the future.

In the next section we move out-of-sample to investigate commitment demand. The inves-

[39]Specifically, given an initial Week 1 allocation of e_2 of work to be done in Week 2 and a reallocation of e_2' in Week 2 of work to be done in week 2, the budget share distance is $\frac{e_2'-e_2}{m}$.

[40]Tobit regressions are implemented to account for the dependent variable being measured on the interval $[-1, 1]$.

tigation of commitment demand is critical to ruling out potential alternative explanations for time inconsistency in effort allocations. Our preferred explanation is the existence of a present-bias in individual decision-making. Many alternative explanations exist, which are considered in detail in Section 5. Importantly, under none of these alternative hypotheses would we expect a clear link between the behavioral pattern of reallocating fewer tasks to the present and commitment demand. This is in contrast to a model of present bias under the assumption of sophistication. Present-biased individuals should have demand for commitment. In the next section we document commitment demand on the aggregate level and link commitment demand to measured present-bias at both the aggregate and individual level.

4.4 Commitment Demand

In Week 4 of our experiment, subjects are offered a probabilistic commitment device. Subjects are asked whether they prefer the allocation-that-counts to come from their Week 4 allocations with probability 0.1 (plus an amount \$X) or with probability 0.9 (plus an amount \$Y), with either \$X=0 or \$Y=0. The second of these choices represents commitment and \$X - \$Y is the price of commitment.[41] Virtually nobody is willing to pay more than \$0.25 for commitment, with 91 percent of subjects preferring flexibility when the cost of commitment (\$X - \$Y) is \$0.25. Likewise, nobody is willing to pay more than \$0.25 for flexibility, with 90 percent of subjects preferring commitment when the price of commitment (\$X - \$Y) is -\$0.25. It appears that most subjects simply followed the money in the elicitation, preferring either commitment or flexibility depending on which option provided additional payment.[42]

We obtain some heterogeneity in behavior at price zero (\$X=0 and \$Y=0) where 59% (47/80) of subjects commit, indicating substantial commitment demand. We define the binary

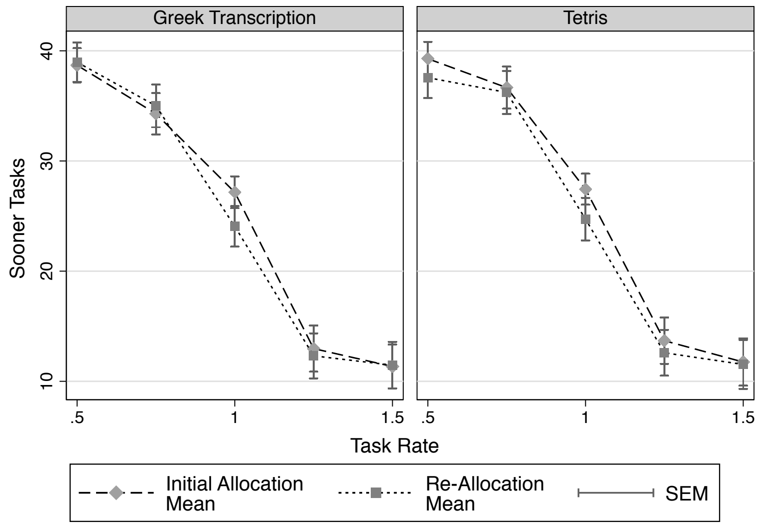
[41] To avoid cutting the sample further, here we consider all 80 subjects in the primary sample. 4 of 80 subjects switched multiple times in the commitment device price list elicitation. Identical results are obtained excluding such individuals.

[42] We are hesitant to draw strong conclusions based on price sensitivity for two reasons. First, the elicitation procedure may not have been optimized for fine price differentiations. Second, given the observed reallocations, and the overall payment subjects received for units of work, it is not clear how much the option to reallocate should be worth.

variable 'Commit (=1)' which takes the value 1 if a subject chooses to commit at price zero and use this measure in the following analysis.

Figure 7: Commitment Demand

Panel A: Commit (=0)



Panel B: Commit (=1)

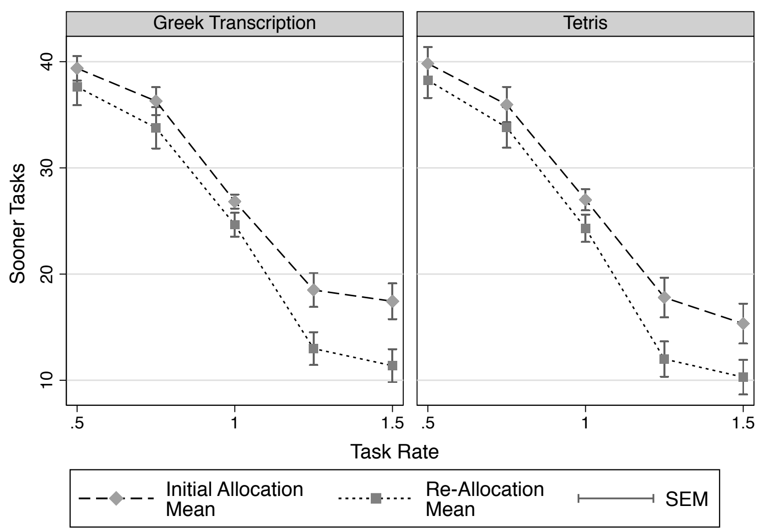


Figure 7 separates Figure 5 showing the real effort discounting behavior by commitment demand at price zero. Immediately apparent from Figure 7 is that experimental behavior separates along commitment demand. Subjects who demand commitment in Week 4 made substantially present biased reallocations in Week 2 given their initial Week 1 effort allocations. Controlling for all task rate and task interactions, subjects who demand commitment allocate 3.58 fewer tasks to the sooner date when it is the present, $F(1,46) = 12.18$, ($p < 0.01$). Subjects who do not demand commitment make more similar initial allocations and subsequent reallocations of effort. Controlling for all task rate and task interactions, they only allocate 0.89 fewer tasks to the sooner date when it is the present, $F(1,32) = 4.01$, ($p = 0.05$). Furthermore, subjects who demand commitment in Week 4 reallocated significantly more tasks than subjects who did not demand commitment, $F(1,79) = 5.84$, ($p = 0.02$).

Table 4 generates a similar conclusion with parametric estimates. In columns (3) and (4), we find that subjects who demand commitment in Week 4 are significantly present biased over effort in Weeks 1-3, $\chi^2(1) = 9.00$, ($p < 0.01$). For subjects who do not demand commitment, we cannot reject the null hypothesis of $\beta = 1$ at conventional levels, $\chi^2(1) = 2.64$, ($p = 0.10$). Further, we reject the null hypothesis of equal present bias across committers and non-committers, $\chi^2(1) = 4.85$, ($p = 0.03$).[43]

In columns (1) and (2) of Table 4 we repeat this exercise, predicting commitment demand for effort using present bias parameters from monetary decisions. While subjects who demand commitment also seem directionally more present-biased for monetary decisions than subjects who do not demand commitment, the difference is not significant, ($p = 0.26$).

In Table 5 we assess whether present bias identified at the individual level predicts commitment demand. We show logit regressions with 'Commit(=1)' as the dependent variable and measures of present bias over work and money as independent variables. In column (1) we find that the continuous value $\hat{\beta}_e$ significantly predicts commitment demand. Column (2) shows that while the binary measure, Present Biased$_e$, has the right sign, it fails to be significant.[44] There

[43]These results are stronger for the first block of the experiment prior to the offering of the commitment device, though the general patterns holds when we use both blocks of data. Appendix Table A6 provides analysis including the data from both blocks.

[44]In Appendix Table A7, we use parameter estimates from Blocks 1 and 2 combined and find that both the continuous and the binary measure are predictive. Indeed, marginal effects indicate that individuals who are present biased are 33 percentage points more likely to demand commitment, an increase of 56% from the mean. That binary present bias identified from the combined data set has increased predictive power may be related to the arbitrary cutoff ($\hat{\beta}_e < 0.99$) employed for the measure. For example, identifying binary present bias with

Table 4: Monetary and Real Effort Discounting by Commitment

	Monetary Discounting		Effort Discounting	
	Commit (=0)	Commit (=1)	Commit (=0)	Commit (=1)
	(1) Tobit	(2) Tobit	(3) Tobit	(4) Tobit
Present Bias Parameter: $\hat{\beta}$	0.999	0.981	0.965	0.835
	(0.010)	(0.013)	(0.022)	(0.055)
Daily Discount Factor: $\hat{\delta}$	0.997	0.997	0.988	1.009
	(0.000)	(0.001)	(0.005)	(0.005)
Monetary Curvature Parameter: $\hat{\alpha}$	0.981	0.973		
	(0.009)	(0.007)		
Cost of Effort Parameter: $\hat{\gamma}$			1.553	1.616
			(0.165)	(0.134)
# Observations	420	705	660	940
# Clusters	28	47	33	47
Job Effects	-	-	Yes	Yes
$H_0 : \beta = 1$	$\chi_2(1) = 0.01$	$\chi_2(1) = 2.15$	$\chi_2(1) = 2.64$	$\chi_2(1) = 9.00$
	$(p = 0.94)$	$(p = 0.14)$	$(p = 0.10)$	$(p < 0.01)$
$H_0 : \beta(Col.\ 1) = \beta(Col.\ 2)$	$\chi_2(1) = 1.29$			
	$(p = 0.26)$			
$H_0 : \beta(Col.\ 3) = \beta(Col.\ 4)$			$\chi_2(1) = 4.85$	
			$(p = 0.03)$	

Notes: Parameters identified from OLS regressions of equations (1) and (2) for monetary discounting and real effort discounting. Parameters recovered via non-linear combinations of regression coefficients. Standard errors clustered at individual level reported in parentheses, recovered via the delta method. Commit (=1) or Commit (=0) separates individuals into those who did (1) or those who did not (0) choose to commit at a commitment price of zero dollars. Effort regressions control for Job Effects (Job 1 vs. Job 2). Tested null hypotheses are zero present bias, $H_0 : \beta = 1$, and equality of present bias across commitment and no commitment, $H_0 : \beta(Col.\ 1) = \beta(Col.\ 2)$ and $H_0 : \beta(Col.\ 3) = \beta(Col.\ 4)$.

are again some suggestive, but statistically insignificant results, delivered by our measures of monetary present bias in columns (3) and (4). In columns (5) and (6) we combine present bias measures and note that where significant relations are achieved, present bias over effort has substantially more predictive power than present bias over money for explaining commitment

lower values of $\hat{\beta}_e$ yields significance with only Block 1 data.

demand.

Table 5: Predicting Commitment Demand

	Dependent Variable : Commit (=1)					
	(1)	(2)	(3)	(4)	(5)	(6)
$\hat{\beta}_e$	-2.595** [-0.625]				-3.157** [-0.725]	
	(1.170)				(1.333)	
Present Biased$_e$ (=1)		0.031 [0.008]				0.301 [0.070]
		(0.459)				(0.488)
$\hat{\beta}_m$			-3.146 [-0.735]		-3.841 [-0.883]	
			(4.140)		(4.008)	
Present Biased$_m$ (=1)				0.622 [0.140]		0.588 [0.133]
				(0.533)		(0.537)
Constant	2.746**	0.336	3.635	0.323	7.262*	0.180
	(1.087)	(0.340)	(4.092)	(0.288)	(4.259)	(0.369)
# Observations	80	80	75	75	75	75
Log-Likelihood	-52.031	-54.218	-49.280	-48.838	-46.529	-48.646
Pseudo R^2	0.040	0.000	0.006	0.014	0.061	0.018
Mean of Dependent Variable	0.59	0.59	0.63	0.63	0.63	0.63

Notes: Coefficients from logistic regression of demand for commitment on identified individual discounting parameters. Marginal effects in brackets. Robust standard errors in parentheses. Commit (=1) or Commit (=0) separates individuals into those who did (1) or those who did not (0) choose to commit at a commitment price of zero dollars. Discounting parameters identified from OLS regressions of equations (1) and (2) for monetary discounting and real effort discounting with individual specific effects for both $\hat{\delta}$ and $\hat{\beta}$. Curvature parameter, α, and cost parameter, λ, assumed constant across individuals. Effort regressions identifying parameters control for Job Effects (Job 1 vs. Job 2). Present Biased$_m$ (=1) and Present Biased$_e$ (=1) calculated as individuals with estimated $\hat{\beta} < 0.99$ in the relevant domain. Levels of significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

These findings indicate that present bias in effort is significantly related to future commitment demand. Individuals who are present biased over effort are substantially more likely to demand commitment at price zero.

It is important to ensure that the desired commitment is meaningful in that it imposes a binding constraint. Specifically, we ask whether individuals who demand commitment actually restrict their own activities through commitment, forcing themselves to complete more work than they instantaneously desire.[45] Given the nature of our commitment device, this seems at

[45]Though our offered commitment contract allows individuals only to meaningfully restrict themselves, this need not be the case. One example would be to have individuals commit to completing at least 1 task at the sooner work date. As virtually all initial allocations and subsequent reallocations satisfy this condition anyways,

least ex-ante plausible, and will be the case as long as initial allocations differ from reallocations. Two such comparisons are considered. First, we consider the first block of the experiment when no commitment contract is available. How many more tasks would subjects have been required to complete in Week 2 had the commitment contract been in place? To answer this question we examine budget share distances for Block 1. Non-committers have a mean budget share distance of -0.018 (clustered s.e. $= 0.009$) allocating about 2 percentage points less of each budget to Week 2 when deciding in the present. In contrast, committers have a mean budget share distance of -0.072 (0.020), allocating 7 percentage points less to Week 2 when deciding in the present. While both values are significantly different from zero ($F(1, 79) = 4.14$, ($p = 0.05$), $F(1, 79) = 12.39$, ($p < 0.01$), respectively), the difference between the two is also statistically significant, $F(1, 79) = 5.88$, ($p = 0.02$). Hence, had commitment been available in Week 2 and had subjects made the same choices, committers would have been required to complete significantly more work than they instantaneously desired and would have been more restricted than non-committers. The same analysis can be done for Block 2 focusing on required work in Week 5. Non-committers have a mean budget share distance of 0.011 (0.017) while committers have a mean distance of -0.030 (0.013). The difference for committers remains significantly different from zero, $F(1, 79) = 5.57$, ($p = 0.02$), and the difference between the two remains significant at the 10% level $F(1, 79) = 3.68$, ($p = 0.06$).[46] Hence, in the presence of commitment in Week 5, committers are required to complete significantly more work than they instantaneously desire and are more restricted than non-committers.

We are aware of two prior exercises exploring the potential extent of present bias and its correlation with commitment demand. Kaur et al. (2010) link the apparently present-biased behavior of working harder on paydays with demand for a dominated wage contract wherein individuals choose a work target. If the work target is not met, an individual receives a low piece-rate wage, while if it is met or exceeded the individual receives a higher piece rate

such commitment is not very meaningful.

[46]The difference for non-committers is no longer significantly different from zero $F(1, 79) = 0.39$, ($p = 0.53$).

wage. As the dominated wage contract can be viewed as a commitment to complete a certain amount of work, this represents a potential link between commitment and present bias. Though compelling, the commitment levels are chosen by individuals themselves and are set around 1/6 of average productivity.[47] Ashraf et al. (2006) consider hypothetical intertemporal choices over money, rice and ice cream and link those to take-up of a savings commitment device. The authors show that present bias in hypothetical monetary decisions is correlated with take-up for women.

5 Discussion

Our effort discounting data address several key confounds present in monetary studies, such as fungibility and arbitrage issues. However, some discussion is required before one can attribute the observed behavior for effort choices to dynamic inconsistency. Clearly, the ability to predict commitment demand based on present-biased reallocations gives a degree of confidence. In this section we discuss four additional hypotheses which can generate time inconsistent effort allocations. Though none of these explanations would predict a correlation between time inconsistency and commitment demand, we can also address these hypotheses directly.

First, subjects may reallocate fewer tasks to the present because of time constraints. Subjects who log on to the experimental website just prior to midnight may have limited opportunity to complete their tasks. Importantly, we recorded allocation times. On average, subjects completed their allocations in Weeks 2 and 5 with around 8 hours remaining before the imposed midnight deadline. Only 8 of 80 subjects completed their allocations in Weeks 2 or 5 with less than 2 hours remaining before the imposed midnight deadline and only 2 of 80 completed their allocations with less than 1 hour remaining. Of course, subjects that log in later may be more likely to be present-biased, but the physical time constraint appears not to be a driving force in the allocations.[48]

[47] The commitment may serve other purposes than constraining behavior that may still be desirable to individuals who work harder on paydays.

[48] Interestingly, in unreported post-hoc results, when splitting the sample at the median allocation completion

Second, subjects may make reallocations not because they are present-biased but because they find the task to be more or less difficult than they previously envisioned.[49] Though we do attempt to give subjects a sense of the tasks, this is a plausible and critical confound. Importantly, our environment is potentially able to address this confound as changes to perceived cost functions are separable from time preferences. The shape of the cost function is identified from changes in the experimental task rates. Because both initial allocations and subsequent reallocations are made at various task rates, the cost function is identified at both points and time. In Appendix Table A3, we estimate cost functions and discounting parameters at each point in time, lending credence to the notion that changes in cost functions are not driving the observed behavior.[50]

Third, and relatedly, attention must be paid to the role of uncertainty in delivering a present-biased pattern of behavior. When making initial allocations, subjects do so under a different informational environment than when making their subsequent reallocations. There could be uncertainty for initial allocations, which is partially resolved when reallocations are made one week later. Several aspects of uncertainty warrant attention. First, individuals may carry preferences for the resolution of uncertainty (Kreps and Porteus, 1978; Epstein and Zin, 1989; Chew and Epstein, 1989). Unlike monetary designs, in our effort experiment such a preference may more naturally lead to a future bias. Subjects desiring to resolve uncertainty in their reallocation choices could, in principle, choose to complete their tasks immediately when the present is available. Second, our discounting estimates do not account for subjects' potential uncertainty on their own parameters, such as uncertainty with regards to the future costliness of the task. Though the weekly parameter estimates provided in Table A3 help to alleviate some concerns, a deeper problem may exist. Subjects may make allocations in Week

time we do find that subjects who log in later are estimated to be more present-biased.

[49]We see this channel as distinct from the role of uncertainty, as such changes in difficulty need not have been forecasted.

[50]The non-parametric analysis above also gives some sense of the validity of this concern. If changing cost functions were to drive the behavior, one would observe a different pattern of reallocations. If an individual moved from having an extremely convex to an almost linear cost function, the corresponding allocations would shift from having limited price sensitivity to being very price sensitive. Hence, one would expect reallocations to be higher than initial allocations at some prices and lower at others.

1 that minimize their discounted *expected* cost in future weeks given the potential realizations of future parameters. This uncertainty may be subsequently resolved in Week 2, such that subjects minimize their discounted cost at specific realizations of key parameters. As the minimizer of the expectation need not be the expectation of the minimizer, such issues can lead to inconsistencies between initial allocations and subsequent reallocations. To explore the extent to which this issue hampers identification of present bias, we conduct simulations under a variety of uncertainty structures in Appendix A. Uncertainty, unresolved at initial allocation and realized at the time of reallocation, does bias our estimates of β both at the aggregate and individual level. However, the direction of bias is generally upward in the parameter regions of interest, leading to less estimated present bias.[51] Importantly, a subject with future uncertainty would benefit from flexibility, such that even if present bias was delivered by uncertainty of some form one would not expect a correlation between present bias and commitment demand.

Fourth, present-biased reallocations of effort may be a simple decision error. Hence, present bias, or any dynamic inconsistency, may be an unstable phenomenon. The two blocks of our experiment speak to this possibility. Subjects have two opportunities to exhibit present-biased reallocations. Indeed, present-biased behavior in Block 1 and Block 2 is significantly correlated.[52] At the allocation level, a subject who is present biased in Block 1 is 58% more likely than others to be present biased in Block 2, $F(1, 79) = 6.94$, $(p = 0.010)$.[53] Additionally, an individual who is dynamically consistent in Block 1 is 85% more likely to be dynamically consistent in Block 2 than others $F(1, 79) = 50.88$, $(p < 0.01)$.[54]

[51] Intuitively, subjects with unresolved uncertainty on future parameters seek to avoid the extreme possibilities of working under a very convex cost structure that is only rarely realized. This leads initial allocations to be frequently lower than subsequent reallocations, particularly at higher task rates. Appendix A provides greater detail.

[52] Though the behavior is significantly correlated when examined as indicators for present bias, future bias and dynamic consistency; the budget share distances are not significantly correlated through time. This may be due to the sheer volume of data with budget share distances equal to zero and the relative lack of stability for future-biased behavior.

[53] Test statistic from OLS regression of binary indicator for a present-biased reallocation in Block 2 on matched indicator for present-biased reallocation in Block 1 with standard errors clustered on the subject level. The estimated constant is 0.218 ($s.e. = 0.030$) and the coefficient on Block 1 present bias is 0.128 ($s.e. = 0.049$).

[54] Test statistic from OLS regression of binary indicator for a dynamically consistent reallocation in Block 2 on matched indicator for a dynamically consistent reallocation in Block 1 with standard errors clustered on the subject level. The estimated constant is 0.400 ($s.e. = 0.041$) and the coefficient on Block 1 dynamic consistency is

This discussion helps to clarify some of the potential confounds for our observed effects. We view it as unlikely that present-biased reallocations of effort are driven by time constraints, unforeseen difficulty or uncertainty. Further, that present bias over effort exhibits stability and predicts commitment demand gives confidence that our observed effects are generated by dynamic inconsistency.

6 Conclusion

Present biased time preferences are a core of behavioral research. The key hypothesis of diminishing impatience through time is able to capture a number of behavioral regularities at odds with standard exponential discounting. Further, the possibility of sophistication provides an important channel for policy improvements via the provision of commitment devices. With the exception of only a few pieces of research, most evidence of dynamic inconsistency is generated from experimental choices over time-dated monetary payments. Such methods are subject to a variety of critical confounds, muddying the resulting inference for a model defined over streams of consumption.

The present study recognizes the key shortcomings of monetary discounting experiments and attempts to identify dynamic inconsistency for choices over real effort. We introduce a longitudinal design asking subjects to allocate and subsequently reallocate units of effort through time. A complementary monetary study is conducted for comparison. We document three key findings. First, in choices over monetary payments, we find limited evidence of present bias. This finding supports recent work demonstrating that when transactions costs and payment risk are closely controlled, monetary choices generate only limited present bias. Second, in choices over effort, we find substantial present bias. Subjects reallocate about 9% less work to

0.342 ($s.e. = 0.048$). Interestingly, somewhat less precision is found for future biased reallocations. An individual who is future-biased in Block 1 is 54% more likely to be future-biased in Block 2 than others $F(1,79) = 3.07$, ($p = 0.08$). Test statistic from OLS regression of binary indicator for a future-biased reallocation in Block 2 on matched indicator for a future-biased reallocation in Block 1 with standard errors clustered on the subject level. The estimated constant is 0.162 ($s.e. = 0.025$) and the coefficient on Block 1 future bias is 0.088 ($s.e. = 0.050$).

the present than their initial allocation. Corresponding parameter estimates generate a similar conclusion. Individuals are estimated to be substantially present-biased in effort choices and significantly closer to dynamically consistent in choices over money. Third, we study commitment demand, documenting that at price zero roughly 60% of subjects prefer commitment to flexibility. A key result is that these commitment decisions correlate significantly with previously measured present bias. Individuals who demand commitment are significantly more present-biased in effort than those who do not. This provides validation for our experimental measures and helps to rule out a variety of potential confounds. Importantly, in our design commitment meaningfully restricts activities. Committed subjects are required to complete more effort than they instantaneously desire. By documenting the link between experimentally measured present bias and commitment demand, we provide support for models of dynamic inconsistency with sophistication. Subjects are apparently aware of their present bias and take actions to limit their future behavior.

We view our paper as providing a portable experimental method allowing tractable estimation of intertemporal preferences over consumption (effort) and correlating such preferences with a meaningful, potentially constraining, commitment device. Though the implementation here is with American undergraduates, we feel the design is suitable for field interventions.

We draw one conclusion and several words of caution from our findings. Our results indicate that present bias is plausibly identified in choices over effort and, furthermore, is linked to effort-related commitment demand. However, we caution using the estimated parameters at face value as they are for a specific subject pool (self-selected to work for six weeks for final payment in week seven) and a specific task. There may be other decision environments wherein behavior may not be well captured by models of dynamic inconsistency. For example, subjects may wish to get a painful single experience over with immediately or postpone a single pleasure (Loewenstein, 1987).[55] Additionally, though fungibility issues may be mediated in the present design, the natural problems of arbitrage will still exist if subjects substitute effort in the

[55]This suggests a key anticipatory component of intertemporal behavior, potentially mediated by our design's use of minimum effort requirements and convex decisions.

lab with their extra-lab behavior. The existence and use of such substitutes, like avoiding doing laundry or homework in response to the experiment, may bias our discounting estimates. Together these cautions suggest important benefits to future research exploring the correlation between our measures and ecologically relevant decisions.

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