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Essays in Experimental Economics: Intertemporal and Social Choice

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by

Adrian Wolanski

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2024

The dissertation of Adrian Wolanski is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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2024

DEDICATION

To my parents, Mark and Susan, for their continuous support of lifelong learning.

To my wonderful partner Mary Anne, for her company on this journey.

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ABSTRACT OF THE DISSERTATION

Essays in Experimental Economics: Intertemporal and Social Choice

by

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Doctor of Philosophy in Economics

University of California San Diego, 2024

Professor Isabel Trevino, Chair

This dissertation examines how people make choices over time and for other people.

In the first chapter, we study when it is possible to link a social planner's preferences across groups of different agents. We propose a preference consistency criterion that relates members of a family of social preferences across domains of different agents; this criterion requires preferences to be identical on domains differing only by adding agents with choice-independent payoffs. We derive additional domain changes for which consistent preferences are invariant, and test adherence to these predictions in an online laboratory experiment. While consistency rates are reasonably high, we document significant differences in consistency across the different types of domain changes. Additionally, we find that participants tend to choose

options with higher inequality/lower inefficiency as domain size increases.

In the second chapter, we provide a theoretical approach for investigating attitudes towards intertemporal inequality. We generalize the Pigou-Dalton principle to intertemporal settings by formulating several partial orders on the space of income streams. Three of the partial orders account for payments received over the lifespan of the stream and differ only by how intensely one stream dominates another. A fourth partial order only accounts for the level of inequality experienced in a specific period, rather than over the lifespan of the stream. We then perform a laboratory experiment to distinguish the empirical relevance of these different partial orders and inequality rankings. We find that orders that rank whole streams accurately reflect how participants view streams for themselves, but these views do not translate into how they choose for others. Instead, many of our participants display inequality aversion based on period-wise outcomes.

In the third chapter, we present a laboratory experiment designed to measure both actual and perceived dynamic inconsistency using a novel convex commitment device. We find that participants' demand a great deal of commitment, implying they believe they are significantly dynamically inconsistent, despite evidence of little to no dynamic inconsistency. The results suggest caution when employing commitment devices, as their usage may be unrelated to the problem they are trying to solve.

Chapter 1

Consistent Social Choice

1.1 Introduction

Many individuals make choices that have consequences for others. This chapter examines whether social decision-makers make similar choices for different groups of others. Knowing when a social decision-maker's preferences are related across groups can be critical when the decision-maker is chosen through some selection process. Consider the case of a politician running for a higher office. Besides touting their record while serving in lower office, candidates often highlight how they interact with their family, friends, and other members of their community in an effort to convey how they will make choices for all of their constituents. But can we draw inferences about the candidate's behavior on a larger social domain from their choices on a smaller social domain? Such inferences would be useful, but require the presence of a link between preferences on these different social domains.

In this chapter, we provide an axiom that characterizes the internal consistency of a family of preference relations across domains of different agents followed by an experimental test of the relevance of this condition. This axiom, which we call *joint consistency*, requires preferences to remain the same when agents receiving choice independent outcomes are added to or removed from the domain of agents the decision-maker chooses for. Combined with the usual axioms of completeness, transitivity, and continuity, joint consistency implies an additively separable utility representation with domain-invariant Bernoulli functions. This representation yields two additional domain changes for which preferences are invariant: 1) changing the level of a choice independent outcome and 2) adding an agent with an identical choice-dependent outcome for each existing agent with a choice-dependent outcome.

To demonstrate these domain changes more concretely, consider the example in table 1.1. Dave is the mayor of a small town and is constructing a public park[1]. The facility can include either a dog park or a playground, in addition to the standard park features. In the first choice environment, Dave's decision only affects two other agents—Anne and Betty. In the second environment, a new constituent named Charles moves to town. Charles cares only about the general park and not about whether it has a dog park or playground, so his outcome is independent of which product Dave chooses to construct. In the third environment, the domain of agents is the same as in the second environment; the only difference is Charles experiences a different level of choice independent outcome. Finally, the fourth environment replaces Charles with two new agents, Evelyn and Fiona. Evelyn experiences the same outcome as Anne while Fiona experiences the same outcome as Betty. In order for Dave's preferences to be jointly consistent, he must choose to build the same project in each of these four choice environments[2].

Joint consistency and its implications are similar to some existing properties, namely the sure-thing principle in Savage (1954) and coordinate independence in Wakker (1988). In the example from table 1.1, Dave obeys the sure-thing principle if he builds the same project in choice 1 and choice 2, and obeys coordinate independence if he builds the same project in choice

[1]Construction of public goods is a scenario where joint consistency is more likely to arise naturally. Granting new agents access to a public good has a small impact on the experience of previous users, so the choice of which public good to construct should not depend on the presence of people who are indifferent between the different projects.

[2]The planner making the same choice in environment 4 requires an additional assumption of anonymity; that agents who receive the same outcome are viewed the same. This property naturally arises in laboratory settings.

2 and choice 3. Joint consistency is stronger than these existing properties because it requires that neither the level *nor the presence* of an agent's choice independent outcome affects the planner's decision. In the example from table 1.1, this results in Dave choosing the same project in each choice environment.

We investigate the empirical relevance of our criterion using an online laboratory experiment with 200 participants. Each participant acts as a social planner making binary decisions for groups of participants, across the various domain types described above[3]. Overall rates of joint consistency are high, including among choices where the planner's outcome is unaffected by their decision. Interpreting joint consistency as a measure of attention, this result indicates that participants often still behave purposefully when their choices only affect other people. Additionally, we find limited differences in consistency across the types of domain changes. Despite this success, we find a few substantive departures from the theory.

First, there is more consistency between choices that differ by exactly one of the three domain changes described in table 1.1 than between choices differing by compounded domain changes. This suggests inconsistency stems from a cognitive difficulty in parsing the domain changes. Second, we find adding or removing the *planner* results in lower consistency than adding or removing another agent (i.e. there is less consistency among choice 1 and choice 2 when Charles is replaced with Dave). This demonstrates that social planners make systematically different choices depending on their presence in the domain of agents. Additionally, we analyze the direction of inconsistencies and find that: 1) inconsistent planners are less inequality averse than consistent planners, especially over more complex domain changes, and 2) inconsistent planners exhibit diminishing inequality aversion as domain size increases. We propose two possible causes for this result: 1) inconsistent preferences over efficiency and inequality of outcomes and 2) cognitive complexity[4]. Our current design limits our ability to distinguish these

[3]The example in table 1.1 involves the decision-maker only choosing for others. We consider both this type of environment, as well as environments where the decision-maker is one of the agents affected by their decision.

[4]The literature studying when choices may differ from underlying preferences due to mistakes and/or complexity contains recent work by Esponda and Vespa (2023), Nielsen and Rehbeck (2022), Oprea (2020), Oprea (2023), and

causes, but we believe this is a productive area for future research.

Many works explore the consistency of decision rules from a theoretical perspective. Polman and Wu (2020) provides a comprehensive overview of the literature on decisions under uncertainty made for oneself and others, and Thomson (2011) offers a thorough overview of which allocation rules adhere to different notions of consistency. While there have many theoretical treatments of preference consistency, there has been very limited empirical work on the topic. Zame et al. (2020) is the only empirical work we are aware of, which focuses on the differences between social and personal preferences in the face of uncertainty. We eliminate uncertainty to focus on the social choice component of these decisions, in addition to investigating choices on different sizes of social domains rather than comparing choices in personal and social domains.

While there is limited empirical work on consistency, there is a vast set of empirical work on social preferences[5]. One difference between our study and much of this literature is that we consider both environments, one where the decision-maker's choices affect their own outcome and one in which they do not. Two important chapters in this area are Traub et al. (2009) and Hong et al. (2015), which both find systematic differences in how planners choose between equity and efficiency when their choices affect their own outcome and when they do not. We find this same result in our data, which manifests as much higher consistency within domains that keep the planner's presence constant rather than across those domains.

The rest of the chapter is structured as follows. Section 2.2 provides the theoretical components of this chapter. Section 2.3 provides details on the experimental design. Section 2.4 shares our main findings from the experiment, and section 2.5 concludes the chapter.

Enke et al. (2023).

[5]Harsanyi (1961) pioneered this literature introducing the ultimatum game, which Kahneman et al. (1986), Forsythe et al. (1994), and Andreoni and Miller (2002) refined into the modified dictator game. See Engel (2011) for a comprehensive review of dictator game literature.

1.2 Theory

Let $\mathcal{N} = \{1, ..., n\}$ be the set of all agents a social planner makes decisions for. Then $A \in \mathcal{P}(\mathcal{N})$ and $B \in \mathcal{P}(\mathcal{N})$ are two domains that contain some of the agents from $\mathcal{N}$, where $\mathcal{P}(\mathcal{N})$ refers to the power set of $\mathcal{N}$. For each domain $A \in \mathcal{P}(\mathcal{N})$, $x_A \in \mathbb{R}^{|A|}$ refers to a social allocation in A. The social planner has a preference relation $\succeq_A$ over such social allocations that satisfies the following standard axioms:

1.2.1 Axioms and Definitions

Axiom 1.1. *A preference relation $\succeq_A$ is complete if for all $x_A, y_A \in \mathbb{R}^{|A|}$, either $x_A \succeq_A y_A$ or $y_A \succeq_A x_A$.*

Axiom 1.2. *A preference relation $\succeq_A$ is transitive if for all $x_A, y_A, z_A \in \mathbb{R}^{|A|}$, if $x_A \succeq_A y_A$ and $y_A \succeq_A z_A$, then $x_A \succeq_A z_A$.*

Axiom 1.3. *A preference relation $\succeq_A$ is continuous if both $E = \{x_A | x_A \succeq_A y_A\}$ and $F = \{x_A | y_A \succeq_A x_A\}$ are closed for all $y_A \in \mathbb{R}^{|A|}$.*

Given that Euclidean space is complete and separable, these axioms imply the existence of a real-valued utility function that represents $\succeq_A$ (Debreu (1954)).

For $A \in \mathcal{P}(\mathcal{N})$ and $B \in \mathcal{P}(\mathcal{N})$, s.t. $A \subset B$, by $(x_A; c_{B \setminus A})$ we denote a collection of payments $x \in \mathbb{R}^{|A|}$ to ordered agents in A, and a collection of payments $c \in \mathbb{R}^{|B|-|A|}$ to ordered agents that are in B, but not in A.

When it is clear from the context which domain we are referring to, we will drop the subscripts and write x, instead of x_A. Additional notation: for $x \in \mathbb{R}^n$ and $c \in \mathbb{R}$, we would denote by $(x_{-i}c_i)$ an n-dimensional vector x, whose i-th component was replaced by c.

An additional aspect of social preferences is how the decision-maker treats different agents receiving the same payment profiles. We define the following pair of anonymity conditions, which require (most) agents receiving the same payoff to be treated equally. In the second anonymity condition, we acknowledge the possibility that the decision-maker treats themselves differently from others but requires that they treat the rest equally when they receive the same payoff[6]. These axioms are not necessary for the main theoretical result, but they naturally arise in our experimental setting and provide a direct method for extending the domain of preferences.

Axiom 1.4. *A preference relation* $\succeq$ *that has an additively separable utility representation* $U(x) = \sum_i u_i(x_i)$ *satisfies anonymity among everyone if* $x_i = x_j \implies u_i(x_i) = u_j(x_j)$.

Axiom 1.5. *A preference relation* $\succeq$ *that has an additively separable utility representation* $U(x) = u_{dm}(x_{dm}) + \sum_{i \neq dm} u_i(x_i)$ *satisfies anonymity among others if* $x_i = x_j, i, j \neq dm \implies u_i(x_i) = u_j(x_j)$.

The final piece of background that we require before introducing our consistency condition is *coordinate independence*, which states that an alternative is preferred to another if and only if changing the level of one choice independent component by the same amount in both alternatives does not change the preference ranking. Wakker (1988) shows that a preference relation admits an additively separable utility function if and only if it satisfies coordinate independence, axioms 1-3, and some mild regularity conditions. We will use this property to construct the utility representation for our consistency property.

Definition 1.1. *A preference relation* $\succeq$ *satisfies coordinate independence, if for all* $x, y \in \mathbb{R}^n$, $1 \leq i \leq n$, $c, d \in \mathbb{R}$, *we have* $(x_{-i}c_i) \succeq (y_{-i}c_i) \iff (x_{-i}d_i) \succeq (y_{-i}d_i)$.

We now present the main definition of consistency which we develop in this chapter. Intuitively, this condition says that if an alternative is preferred on a smaller domain, then it should still be preferred when we expand the domain by adding additional agents whose outcome is independent of the decision-maker's choice.

[6]By construction, both anonymity axioms are equivalent whenever the decision-maker is not receiving a direct payment.

Definition 1.2. *For $A \in \mathcal{P}(\mathcal{N})$ and $B \in \mathcal{P}(\mathcal{N})$, s.t. $A \subset B$, a pair of preference relations $\succeq_A$ and $\succeq_B$ satisfies pair-wise consistency if $\forall c \in \mathbb{R}^{|B|-|A|}; \forall x, y \in \mathbb{R}^{|A|}$ we have $x \succeq_A y \iff (x_A; c_{B\setminus A}) \succeq_B (y_A; c_{B\setminus A})$.*

Definition 1.3. *A family of preference relations $\{\succeq_A\}_{A\in\mathcal{P}(\mathcal{N})}$, satisfies joint consistency if $\forall A, B \in \mathcal{P}(\mathcal{N}) : A \subset B;\quad \succeq_A$ and $\succeq_B$ satisfy pairwise consistency.*

While similar to coordinate independence, joint consistency refers to how different preference rankings relate to each other when adding an agent with a choice independent outcome. Coordinate independence only deals with how a single preference ranking behaves when an agent's choice independent outcome changes, not with the relationship between preferences over different sets of agents.

1.2.2 Examples of Jointly Consistent Preferences

One example of jointly consistent preferences is a variation of Rawlsian preferences, which we call *Rawlsian lexicographic preferences* and denote by $\succeq_{RL}$. Standard Rawlsian preferences aim to make the lowest agent's payoff as high as possible, and are mathematically given by $x \succeq_R y \iff \min_j x_j \geq \min_j y_j$. These preferences are not jointly consistent; if $\min_j x_j \neq min_j y_j$, then either $x_j \succ y_j$ or $y_j \succ x_j$, but if $c < \min\{\min_j x_j, \min_j y_j\}$ then $(x_j, c) \sim_R (y_j, c)$. In other words, a Rawlsian planner is indifferent between all options when we add an agent with a choice independent payoff lower than the previous minimum payoff. A planner with Rawlsian Lexicographic preferences aims to make the *lowest choice-dependent payment* as high as possible. Mathematically, these preferences are described by: $x \succeq_{RL} y \iff x_{(m)} > y_{(m)}$ where $m = \min k$ such that $x_{(k)} \neq y_{(k)}$, and $x_{(k)}$ is the kth order statistic of the allocation x. Like standard lexicographic preferences, Rawlsian lexicographic preferences are not continuous and do not admit a utility representation. Our next example, however, admits a familiar utility representation.

A second example of jointly consistent preferences is *consistent utilitarian preferences*, which we denote by $\succeq_{CU}$. A planner with such preferences maximizes a weighted sum of agent-domain-specific individual utilities where the marginal rate of substitution between any two agents is the same on any domain containing both agents. Mathematically, $x \succeq_{CU} y \iff \sum_{i \in A} u_{A,i}(x_i) \geq \sum_{i \in A} u_{A,i}(y_i)$ and $\frac{u'_{B,j}}{u'_{B,i}} = \frac{u'_{A,j}}{u'_{A,i}}$ whenever $i, j \in A \cap B$. The restriction, that the marginal rate of substitution between agents is the same across domains, prevents arbitrary changes in the Bernoulli functions which is critical for joint consistency. Equivalently, we could instead require that the Bernoulli functions be unique up to a *jointly cardinal transformation* (the multiplication of every Bernoulli function by the same positive constant). We provide this alternative characterization because we can then suppress the choice domain in the notion: $x \succeq_{CU} y \iff \sum_{i \in A} u_{A,i}(x_i) \geq \sum_{i \in A} u_{A,i}(y_i) \forall A \in \mathcal{P}(\mathcal{N})$ where the u_i are unique up to jointly cardinal transformation. It is reassuring that there are utilitarian preferences that are jointly consistent, given their importance in many economic models and empirical applications.

1.2.3 Theoretical Results

Under some mild regularity conditions, joint consistency has remarkable implications for the utility formulation of families of social preferences:

Proposition 1.1. *$\{\succeq_A\}_{A \in \mathcal{P}(\mathcal{N})}$ is jointly consistent and each $\succeq_A$ satisfies completeness, transitivity, and continuity, if and only if $\{\succeq_A\}_{A \in \mathcal{P}(\mathcal{N})}$ has a consistent additively separable representation: $U_A(x) = \sum_{i \in A} u_{A,i}(x_i)$ for all $A \in \mathcal{P}(\mathcal{N})$, where u_i are unique up to a jointly cardinal transformation.*

Proof. The 'if' direction is straightforward. To show the 'only if' direction, we first show that joint consistency implies coordinate independence for all $A \in \mathcal{P}(\mathcal{N})$.

First, choose any $A, B \in \mathcal{P}(\mathcal{N})$, in such a way that $A \subset B$, $|B| = |A| + 1$. Choose arbitrary vectors $(x_A; c_{B \setminus A}), (y_A; c_{B \setminus A}) \in \mathbb{R}^{|B|}$, and assume without loss of generality that $(x_A; c_{B \setminus A}) \succeq_B (y_A; c_{B \setminus A})$.

Consider the $|A|-$dimensional vectors x_A and y_A. Since $\succeq_A$ is complete, either $x_A \succeq_A y_A$ or $y_A \succeq_A x_A$. If $y_A \succeq_A x_A$, then by pairwise consistency it must be the case that $(y_A; c_{B\setminus A}) \succeq_B (x_A; c_{B\setminus A})$; $\forall c \in \mathbb{R}$. This is only possible when both options are indifferent. so we must have $(y_A; c_{B\setminus A}) \succeq_B (x_A; c_{B\setminus A})$. Alternatively, $x_A \succeq_A y_A$, which by pairwise consistency implies that $(x_A; c_{B\setminus A}) \succeq_B (y_A; c_{B\setminus A})$; $\forall c \in \mathbb{R}$.

We have demonstrated that $(x_A; c_{B\setminus A}) \succeq_B (y_A; c_{B\setminus A})$ implies $(x_A; d_{B\setminus A}) \succeq_B (y_A; d_{B\setminus A})$, which proves that joint consistency implies coordinate independence of each $\succeq_A$. Using a theorem from Wakker (1988), each preference relation $\succeq_A$ then has an additively separable utility representation that is unique up to a jointly cardinal transformation. Now, consider an additively separable representation for $\succeq_{\mathcal{N}}$, $U(x) = \sum_{i\in\mathcal{N}} u_{A,i}(x_i)$. Because $\succeq_A$ is a jointly consistent family, for any $A \in \mathcal{P}(\mathcal{N})$ and any $i, j \in A$ we must then have $\frac{u'_{\mathcal{N},j}}{u'_{\mathcal{N},i}} = \frac{u'_{A,j}}{u'_{A,i}}$. □

An alternative framing is that consistent utilitarian preferences are the unique preference that satisfying the decomposition in the following propositions and corollary, which demonstrate explicitly how to relate utility representations across domains.

Proposition 1.2. *If $\{\succeq_A\}_{A\in\mathcal{P}(\mathcal{N})}$ is jointly consistent and each $\succeq_A$ satisfies completeness, transitivity, and continuity; $\forall B \in \mathcal{P}(\mathcal{N})$ such that $B = i \cup k$, there exists U_i, U_j, U_B, that respectively represent $\succeq_i, \succeq_j$, and $\succeq_B$, such that $U_B = U_i + U_j$.*

Proof. By proposition 1.1, there exist u_i and u_j that represent $\succeq_i$ and $\succeq_j$. Additionally, there exists some function U_B that represents $\succeq_B$ and is additively separable. Therefore, $U_B = f(u_i) + g(u_j)$ for some increasing functions f and g.

Define $U_i = f(u_i)$ and $U_j = g(u_j)$; therefore $U_B = U_i + U_j$ where U_i, U_j, and U_B respectively represent $\succeq_i, \succeq_j$, and $\succeq_B$ □

Proposition 1.3. *If $\{\succeq_A\}_{A\in\mathcal{P}(\mathcal{N})}$ is jointly consistent and each $\succeq_A$ satisfies completeness, transitivity, and continuity; $\forall A \subset B \in \mathcal{P}(\mathcal{N})$ such that $B = A \cup \{k\}$, there exists U_A, U_B, U_k, that respectively represent $\succeq_A, \succeq_B$, and $\succeq_k$, such that $U_B = U_A + U_k$.*

Proof. Fix $i \in A$ and $k \in B \setminus A$, and define $C = i \cup k$. By proposition 1.1, $\succeq_C$ has an additively separable representation $U_C = u_i + u_k$ (note that u_k also represents $\succeq_k$ by proposition 1.2).

By proposition 1.1, there exists an additively separable function $U_A = u_i + \sum_{j \neq i \in A} u_j$ that represents $\succeq_A$ where u_i is the same in both U_A and U_C. Define $U_B = u_i + \sum_{j \neq i \in A} u_j + u_k = u_A + u_k$.

The above U_B is additively separable and is jointly consistent because $\frac{u'_{B,i}}{u'_{B,j}} = \frac{u'_{A,i}}{u'_{A,j}}$ for all $i, j \in A$ and $\frac{u'_{B,i}}{u'_{B,k}} = \frac{u'_{C,i}}{u'_{C,k}}$ (by construction we also have $\frac{u'_{B,j}}{u'_{B,k}} = \frac{u'_{D,j}}{u'_{D,k}}$ such that $j, k \in D$). U_B therefore also represents $\succeq_B$.

□

By repeated application proposition 1.3, we obtain the following corollary:

Corollary 1.1. *If $\{\succeq_A\}_{A \in \mathcal{P}(\mathcal{N})}$ is jointly consistent and each $\succeq_A$ satisfies completeness, transitivity, and continuity; $\forall A \subset B \in \mathcal{P}(\mathcal{N})$, there exists U_A, U_B, $U_{B \setminus A}$, that respectively represent $\succeq_A, \succeq_B$, and $\succeq_{B \setminus A}$, such that $U_B = U_A + U_{B \setminus A}$.*

In addition, if preferences satisfy either anonymity condition we can extend $\{\succeq_A\}$ to include additional agents not in $\mathcal{N}$ as long as the new agent's payoff profile is identical to an agent already in the domain.

Proposition 1.4. *Suppose that $\{\succeq_A\}_{A \in \mathcal{P}(\mathcal{N})}$ are jointly consistent, anonymous, continuous, transitive, and complete. Suppose that agent $k \notin \mathcal{N}$ but there exists some agent $j \in \mathcal{N}$ such that $x_k = x_j$ for all options x. Then the family of preferences$\{\succeq_B\}$ for $B \in \mathcal{P}(\mathcal{N} \cup k)$ described by $u_{B, \mathcal{N} \cup \{k\}} = u_{B, \mathcal{N}}$ for $B \in \mathcal{P}(\mathcal{N}) \cap \{k\}^c$ and $u_{B, \mathcal{N} \cup \{k\}} = u_{B, \mathcal{N}} + u_j$ for B in $\mathcal{P}(\mathcal{N} \cup k)$ where j satisfies $x_k = x_j$ for all x is a jointly consistent family of preferences over $\mathcal{P}(\mathcal{N} \cup k)$*

1.2.4 Additional Commentary

Corollary 1.1 and proposition 1.4 are the main theoretical contributions of this chapter, as they provide a method for extending a planner's preferences across domains of different agents. Still, there are two broader implications of these results. First, they present clear testable implications which form the basis of the experimental design presented in the following section. Second, they demonstrate how important consistency (or the lack of it) is for empirical applications. Joint consistency implies external validity of preference measurements—that the measurement of a planner's preferences do not depend on the particular set of agents included in the choices used for measurement. In particular, the failure of joint consistency has very strong implications for the class of planners with additively separable utility representations. Joint consistency guarantees that measured Bernoulli functions do not vary (with the possible exception of a common scale factor) with the domain of measurement. Any measured Bernoulli functions for an inconsistent planner are specific *only to the domain of measurement*, and carry no information about the planner's preferences on other domains. This limits the value of such preference measurement for conducting welfare analysis or policy analysis.

1.3 Experimental Design

We recruited 200 participants using Prolific.co for an online laboratory experiment to test for joint consistency of preferences. Each participant made decisions for 31 binary choices over allocations to themselves and to other people, resulting in 31,400 pairs of decisions[7]. Each participant received $2.50 for their participation; 10% received an additional bonus based on a decision selected at random. The median completion time was 10 minutes, and the average payment with the bonus was $4, yielding an average hourly wage of[8] $24.

Questions were presented in two blocks: one where the participant makes decisions that affect their payment and the payments of others, and a second where they only make decisions

[7]Due to a coding error, we are only able to use 26 of the 31 decisions for the first 60 of our participants.

[8]We find no evidence that the time a participant takes to complete the study affects joint consistency. On average, participants taking an additional minute to complete the study had a lower joint consistency rate by 0.15%. We fail to reject the null hypothesis that this effect is 0, with a p-value for the t-test of 0.528.

that affect the payments of others[9]. The order of the blocks and the order of questions within the blocks were randomized[10].

1.3.1 The Basic Choices

There were three ‘basic’ choices our participants made, which involved only the choice of allocating \$5 to person 1 and \$25 to person 2, or allocating \$10 to person 1 and \$15 to person[11] 2. The difference between these three choices is the payment to the decision-maker. In one variant of the choice, they are not receiving either of the payments they are allocating. In the other two variants, they are receiving one of the payments they are allocating (either the poorer payment profile or the richer payment profile). Table 1.2 provides a description of these decision environments. While none of the decisions made in these basic choices are comparable to each other to test for joint consistency, every other decision our participants made is comparable to the decision in one of these three choices.

1.3.2 Additional Terminology

We refer to option 1 in all of the choices as ‘inefficiency averse’, and option 2 as ‘inequality averse’[12]. We refer to the profile receiving \$5 or \$10 as the ‘poorer’ profile, and the profile receiving either \$25 or \$15 as the ‘richer’ profile. When the decision-maker is making choices involving their own monetary payments, we refer to the option that provides them with the higher monetary payment as either the ‘selfish’ option or the individually payment-maximizing option (i.e. option 2 is selfish when the decision-maker is poorer, but option 1 is selfish when the

[9]The role of the other players was assigned to other different participants in the study at random each round, so at no point did any participant know exactly who was receiving the payments they were allocating to other people.

[10]The order of the blocks was not randomized for the first 60 participants. Because of this and the aforementioned coding error, we repeat all of our analysis dropping these individuals in section 4.5 and we find largely the same results.

[11]These numbers were selected to generate a tradeoff between efficiency and equality; we use multiples of \$5 to make the choices clear for participants to understand and minimize rounding.

[12]We randomly presented the options to participants; we have standardized the order here for clarity of exposition.

decision-maker is richer)[13].

1.3.3 Simple Change 1: Addition of Players with Choice Independent Outcomes

For each of the three basic decisions, we change the domain of choices by adding agents who receive a choice independent outcome to each option. Under the assumption of joint consistency, a decision-maker chooses the same option in both choices under such a domain transformation. Table 1.3 illustrates this comparison explicitly; choice 1 is a basic decision, choice 2 adds an additional agent receiving a choice independent outcome, and choice 3 adds two additional agents receiving choice independent outcomes. A decision-maker who is jointly consistent chooses option 1 in one choice if and only if they choose option 1 in all choices. Examples of the interface participants saw can be found in 1.1 and 1.2.

1.3.4 Simple Change 2: Changing the Level of Choice Independent Outcomes

Besides changing the set of agents in the choice domain, we can change the outcomes the agents receive. Table 1.3 again illustrates these changes. A decision-maker who is jointly consistent chooses option 1 in choice 2 for one level of c if and only if they choose option 1 in choice 2 for any level of c.

1.3.5 Simple Change 3: Duplication of Choice Dependent Outcomes

In addition to changing the domain by adding agents receiving a choice independent outcome, we test for joint consistency by duplicating all choice-dependent outcome profiles and assigning them to new agents. Table 1.4 compares a basic choice to a choice with one duplication

[13] These attributions are not theoretical predictions, but are useful for describing the direction of inconsistencies.

and a to a choice with two duplications. A decision-maker who is jointly consistent and satisfies anonymity chooses option 1 in one choice if and only if they choose option 1 in all choices[14].

1.3.6 Combining the Domain Changes and Testable Predictions

Joint consistency of preferences implies that decisions are invariant under each of the domain expansions described above and under the composition of these changes. Table 1.5 demonstrates several environments formed by the combination of these domain changes. Joint consistency implies that the decision-maker chooses option 1 in one choice in table 1.5 if and only if they choose option 1 in all choices in that table.

In our experiment, this means there are three categories of choices based on the individual outcome experienced by the decision-maker: the poorer payment profile, the richer payment profile, or a choice independent profile (this includes when the decision-maker receives no payment). Each participant made 7 choices where they received the richer payment profile (generating 21 pairs of comparable decisions), 7 choices when they received the poorer payment profile, and 17 choices where their outcome was independent of their choice (generating 136 pairs of comparable decisions[15]). This results in 178 pairs of decisions per participant.

1.4 Results

1.4.1 Descriptive Statistics

The average proportion of consistent choices is 0.8141, and the median proportion of consistent pairs among participants is 0.8764. We find no difference in consistency from gender,

[14]Our experimental design enforces anonymity among others, so we can test for joint consistency using duplications whenever the decision-maker is not one of the agents receiving a choice-dependent outcome profile. We, therefore, design our experiment to have duplications only when the decision-maker is not one of the agents receiving a choice-dependent outcome profile.

[15]The coding error for 60 of our participants resulted in only 12 choices from this block appearing correctly, so this set of participants only has 66 comparable pairs.

age, employment status, length of time spent on the experiment, or survey experience[16]. We include the full sample-balance results in table 1.6. We next examine the proportion of consistent choice pairs across different types of domain changes and present the results in table 1.7. There are two primary results.

Result 1.1 Adding an agent with a choice independent outcome and changing the level of a choice independent outcome have the same proportion of consistent choices

Our first result is consistent with the theory—that the different types of domain changes should be associated with the same rate of consistency. While it appears that combined changes and duplications have lower consistency rates, there are additional experimental parameters that may be driving these results.

1.4.2 Regression Results

We next build upon the results of table 1.7 using a linear probability model (LPM) and a logit model. We present the result in table 1.8. The regressions contain indicator variables for each domain change analyzed in table 1.7, utilize both fixed effects and standard error clustering at the participant level, and include controls for addition parameters of the experimental design.

Result 1.2 There is less consistency among choices involving a combination of changes, but consistency among choices involving duplication is the same as the other simple changes when controlling for additional experimental parameters.

Choices involving duplications were more likely to have the decision-maker receiving payment on one choice but on the other, which has a significant negative effect on consistency. Additionally, choices where the decision-maker has a choice-dependent outcome only occur when duplications are not present. When controlling for these factors, duplications have the same effect

[16]Survey experience has a statistically significant but economically insignificant effect on consistency. A participant would need to complete an additional 100,000 surveys to have a 1.59% increase in their consistency percentage. The maximum number of surveys completed in our sample was 7,063.

on consistency as the other two basic types of domain changes. However, combining changes still has a significant negative effect on consistency. Additionally, the effects of the controls provide insight on the effect of an additional change.

Result 1.3 There are no differences in consistency when the decision-maker receives a choice independent payment on both choices when that payment is positive or when it is 0. However, there is a significant decrease in consistency when comparing choices where the decision-maker receives a choice independent positive outcome in one choice and 0 in another choice.

Our participants view adding themselves to a domain differently than adding another agent to the domain. This is inconsistent with the theory; joint consistency requires that adding an additional agent with a choice independent payoff does not change preferences regardless of the identity of the added agent. While this indicates that participants make systematically different choices depending on their inclusion in the domain, they make consistent choices within the regime.

Another result in table 1.8 is that decision-makers are more consistent when they receive a choice-dependent payment regardless of whether that payment is the richer or poorer profile, but that receiving the poorer profile has a much larger impact on consistency than the richer one. This difference may be due to the decision-maker choosing the individual payment-maximizing choice more frequently when they receive the poorer payment profile. We therefore conduct a secondary analysis on the subset of choice pairs where the decision-maker has a non-constant payment. The results are reported in table 1.9; this control indeed absorbs most of the difference in consistency between richer or poorer payment profiles, with the only statistical significance coming from the logit specification with fixed effects (p-value of 0.059). This further supports the conclusion that there are systematic differences in how participants choose between different payment regimes. Our theory, however, allows for such differences in these particular environments.

1.4.3 Directions of Inconsistencies

One conclusion we drew from the results in table 1.7 was that our participants made systematically different choices in different environments. To further support this conclusion, we compare the likelihood of selecting the inequality-averse choices on different environments between participants who are fully consistent and those who are not. We present the results of this analysis in table 1.10, which compares the proportions between these different groups for a variety of decision environments.

Result 1.4 Individuals who are not fully consistent display significantly lower levels of inequality aversion than individuals who are fully consistent.

Inconsistent individuals overwhelmingly exhibit lower inequality aversion than consistent individuals. Additionally, inconsistent individuals appear to choose the inequality-averse option less frequently as domain size increases.

1.4.4 Direction of Inconsistencies as Size Changes

To further investigate changes in inequality aversion with domain size, we report the proportion of choices where the participants selected the inequality-averse choice on the smaller domain but not on the larger domain. We perform this analysis for each choice category investigated above, and report the results in table 1.11.

Result 1.5 When the domain changes through duplication or combined changes, inconsistencies are associated with decreasing inequality aversion. When the domain changes through adding choice independent outcomes, there is no trend in the direction of inconsistencies.

Inconsistencies are systematic, which provides some insight into the mechanisms behind them. One possible explanation is mechanical; participants may have inconsistent preferences that are decreasing in both inefficiency and inequality. We attempt to test this using choices from the category "duplication without choice independent outcomes. Many standard inequality measures are *identical* under these types of choice comparisons[17]. The absolute efficiency loss (the difference between total payments received by all players in a choice from choosing the inequality-averse option), however, is higher in the inequality-averse option on the duplicated economy. A participant with decreasing preferences over efficiency loss and inequality measured by some index would therefore exhibit decreasing inequality aversion as the domain size increased in this choice category. While our observed probability is consistent with this explanation, our sample size is far too small to draw a firm conclusion about this hypothesis[18].

A second explanation centers on the complexity of the decision environments. Inconsistencies under duplication or combined changes appear to be systematic, while inconsistencies when adding choice independent outcomes appear random. Duplication and combined changes are fundamentally more complex domain transformations than adding an agent with a choice independent outcome, since the former entails adding and/or changing more than 2 numbers in each choice while the latter only adds 2 numbers to each choice. Additionally, it is far easier to determine which option pays a larger sum to all participants than to calculate an inequality index for each option. A decision-maker who experiences differential difficulty across these factors would be more likely to choose more efficient/higher inequality options as domain size increases.

Unfortunately, our current data and design are not well-equipped to completely disentangle these two explanations, nor the complexity costs of changing domain size versus composition. We believe, however, that this would be a productive area for future research.

[17]Examples of measures that are identical under duplication without adding choice independent outcomes include but are not limited to: Gini index, Thiel index, Hoover index, Atkinson index, Palma ratio, variance, and average absolute difference. These measures depend only on the *distribution* of each payment profile within a choice; duplicating all payment profiles changes only the count of each payment profile without changing the distribution of profiles.

[18]Given the number of violations and proportion of each direction in this data set, we estimate we would need to recruit around 3,500 participants to effectively test this hypothesis using our current design.

1.4.5 Direction of Inconsistencies When Size Does Not Change

There are two types of domain changes that do not change size—a simple change in the level of a choice independent payoff, or a combined change that adds and removes different agents. There are four components of the domain that can change—the number of agents receiving a choice independent payment of \$5, the number of agents receiving a choice independent payment of \$18, the number of agents receiving a choice independent payment of \$25, and the number of duplications of the choice-dependent payment profiles. Table 1.12 depicts the direction of inconsistencies for all possible change categories.

Result 1.6 There is no trend in the direction of inconsistencies when changing only the level of a choice independent outcome or when changing choice independent outcomes to duplications. When the decision-maker receives a positive choice independent outcome on one domain but a choice independent 0 on the other, inequality aversion decreases as the level of choice independent payments increases.

The first part of this finding agrees with our previous finding—that inconsistencies for simpler changes are less systematic than inconsistencies for complex changes. The second part of the result also agrees with our earlier finding that participants made systematically different choices depending on their inclusion in the domain. Ultimately, this result says that systematic inconsistencies remain when domain size does not change. If issues with cognitive complexity drive observed inconsistencies, this suggests complexity does not come solely from differences in the number of components the decision-maker has to evaluate.

1.4.6 Robustness Checks

Robustness of Including All Participants

We mentioned earlier that 60 of our participants had a slightly different experimental experience; they did not see the blocks of questions in a random order, and they did not see five of the choices displayed correctly. Since there may be concern about spillovers between the choices or other systematic differences between these participants and the others in our study, we repeat all of the analysis from section 4 dropping these participants from the sample. The results are qualitatively unchanged; while many of the numerical estimates of coefficients and proportions change slightly, none of the conclusions we drew above were due to this particular subset of participants.

Robustness Against Random Choice and Indifference

Joint consistency is a strong condition—choice data either satisfies or does not satisfy it, leaving no room to incorporate random choice or errors in decision-making. Since we are dealing with social choice data, it is also possible that some of our participants are choosing randomly because of indifference between options. The presence of any of these phenomena would bias our measures of inconsistency upwards, since any of them could cause inconsistent choices without inconsistent preferences. This section addresses these concerns.

Since joint consistency and anonymity imply an additively separable utility representation, one approach to dealing with the above ideas would be to make functional form assumptions on the Bernoulli functions and estimate a random utility model. Finding parameter values that reasonably fit the data would indicate participants follow a random variant of joint consistency rather than the deterministic version presented in section 2. However, we believe such a model is insufficiently parsimonious to yield a meaningful rejection of a random utility variant of joint consistency.

Instead, we propose a simple alternative random choice model. Suppose the decision-maker only chooses their preferred choice with probability $p \in [0.5, 1]$; with probability $1-p$ the decision-maker instead selects their less preferred choice. When $p = 1$, the decision-maker's behavior is fully deterministic, and we can verify whether they are consistent or not. When $p = 0.5$, the decision-maker's choices are made uniformly at random and provide no information about their underlying preferences. Equivalently, a decision-maker with $p = 0.5$ can be thought of as someone who is indifferent between all options[19]. Because this interval is continuous, we can interpret a p estimate closer to 0.5 as providing less meaningful information about underlying preferences than a p estimate closer to 1. We use this model to analyze the 17 choices where participants' decisions have no impact on their own payment[20].

We can estimate what value of p is most likely to generate the distribution of consistency rates (percentage of consistent choices) observed in the data. We derive the following equation that relates p, the probability of random choice being in line with deterministic choice, to the expected consistency rate: $E[\text{consistency rate}] = 1 - 2p + 2p^2$.

Each choice is a Bernoulli trial with probability p, so a collection of n realizations can then be described as a vector of length-n composed of 0's and 1's. For joint consistency, we are interested in the probability that any two components of this vector are equal. The probability that the vector contains k ones and $n-k$ zeroes is:

$$Pr(k \text{ ones and } n-k \text{ zeros}) = p^k \cdot (1-p)^{n-k} \cdot \binom{n}{k}$$

Assuming both n and k are greater than 1, we can calculate the resulting percentage of consistent choices conditional on n and k as:

$$1 - \text{consistency rate} = Pr(1 \text{ one and } 1 \text{ zero}) = 1 - \frac{n-k}{n} \cdot \frac{k}{n-1} \cdot 2$$

Combining the two terms together, we can calculate the total expected consistency rate given probability p to be:

[19]Our experimental interface randomizes the order of the questions and the order of the options presented. Therefore, a decision-maker who is indifferent between the options but is not actively choosing uniformly at random will appear to be choosing uniformly at random.

[20]We focus on these choices since we believe they are most likely to generate upward bias in inconsistency measures.

$$\text{Expected consistency rate} = 1 - \sum_{k=0}^{n} p^k (1-p)^{n-k} \cdot \frac{2 \cdot (n-2)!}{(k-1)! \cdot (n-k-1)!} = 1 - 2p + 2p^2$$

Using this equation and the delta method, we can obtain an estimator for p based on the observed sample mean and sample variance (computed via bootstrap) of the consistency percentage.

On the subset of choices where the participants are receiving a choice independent payment (including no payment), our average consistency rate is 0.7897 with a standard deviation[21] of 0.0144. This consistency rate data and the above equation then provide an estimated choice probability value of $p = 0.8805$ with a standard deviation of 0.0189; the associated 95% confidence interval for p is then $[0.8407.0.9183]$. This estimate is substantially closer to 1 than to 0.5, indicating that there is indeed much we can learn about the consistency of underlying preferences despite potential randomness/errors of choice. Combined with trends in the direction of inconsistencies, this indicates that simple random choice is not responsible for most of our inconsistencies.

In addition to estimating a sample-wide choice probability, we estimate individual choice probabilities to identify which participants might not be choosing purposefully. To do this, we simulate the distribution of consistency rates for $p = 0.5$ and identify which participants have consistency rates that fall close to the center of this distribution. A comparison of these distributions is presented in figure 1.3. Only about 30% of our population have consistency rates that lie in the lower 98% of the distribution generated if our population was choosing uniformly at random. The remaining participants have consistency rates unlikely to come from such choice patterns. We then repeat the analysis of section 4 including an additional control for agents we identify as not choosing purposefully, and present the results in section 7.2.3. Our primary results

[21]The standard deviation is estimated via bootstrap.

remain: compound changes are associated with lower consistency, inconsistent decision-makers are less inequality averse, and decision-makers become less inequality averse/more inefficiency averse as domain size increases. Our main conclusions are not driven by participants with low consistency rates, but rather by the participants who behave purposefully. Additionally, the fact that trends in the direction of inconsistencies remain after removing many noisy observations further indicates our conclusions are not driven by simple random choice. Instead, there are systematic reasons for observed inconsistencies which require further research to understand fully.

1.5 Conclusion

In this chapter, we proposed a criterion that relates families of social preferences across domains of different sizes and compositions. Combined with standard regularity assumptions, this criterion produces an additively separable utilitarian representation with domain-invariant Bernoulli functions. A planner following the criterion has invariant preferences under three types of domain changes, in addition to any combination or composition of these changes.

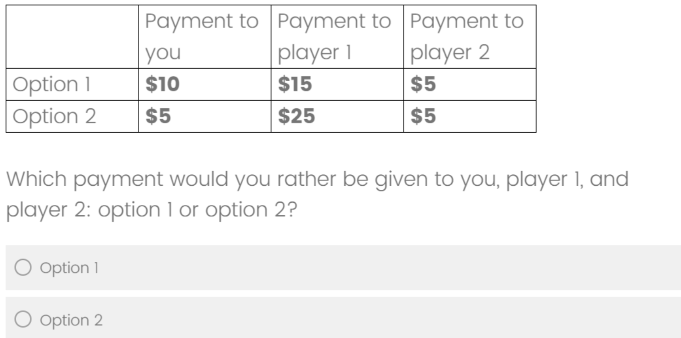
We then examine the empirical relevance of this criterion using an online laboratory experiment. While rates of joint consistency were generally high, there were systematic differences across types of domain changes. Importantly, participants were more consistent between domains differing by a single simple domain change than a combination or composition of changes, suggesting that the difficulty of decisions may play a role in inconsistent behavior. We also observe higher rates of consistency when the decision-maker's presence in the domain is constant between choices, indicating participants make systematically different choices depending on their inclusion. In addition, we document that inconsistent individuals are less inequality-averse than consistent individuals and that inequality aversion decreases as domain size increases. These trends indicate that inconsistencies are systematic, and again may be due to choice complexity rather than an accurate reflection of underlying preferences.

One promising direction for future research is a more thorough investigation of the mechanisms driving our observations. In our analysis above, we provided suggestive evidence that cognitive limitations play a substantial role in our observed inconsistencies, but this requires more experiments with different designs to confirm this hypothesis. Additionally, understanding precisely where cognitive limitations may enter the decision process may be critical for understanding choice more broadly. Recent work by Oprea (2023) and Enke et al. (2023) suggest that classical behavioral anomalies in many settings may come from similar cognitive limitations rather than a disparate set of non-standard preferences. If our results are also driven by cognitive limitations, disentangling how much is unique to our particular setting and how much could be generalized to other choice environments would further support this emerging literature.

1.6 Acknowledgements

Chapter 1 is currently being prepared for submission for publication of the material under the title “Consistent Social Choice”. Wolanski, Adrian; Baranov, Evgenii. The dissertation author, Adrian Wolanski, and Evgenii Baranov are the principal coauthors of this chapter.

1.7 Figures and Tables



	Payment to you	Payment to player 1	Payment to player 2
Option 1	**$10**	**$15**	**$5**
Option 2	**$5**	**$25**	**$5**

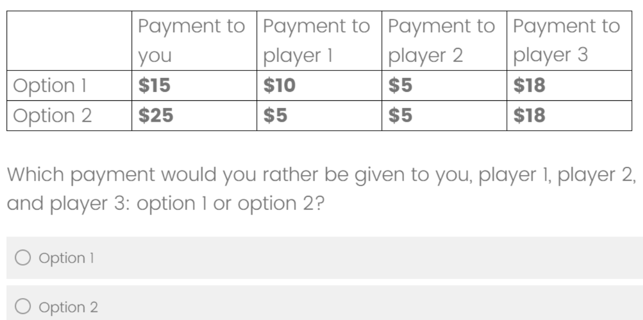
Which payment would you rather be given to you, player 1, and player 2: option 1 or option 2?

Option 1

Option 2

Figure 1.1. Screenshot of Interface

This figure displays an example of the interface participants saw during the experiment. This example presents a basic decision adding a single player with a choice independent outcome.



	Payment to you	Payment to player 1	Payment to player 2	Payment to player 3
Option 1	**$15**	**$10**	**$5**	**$18**
Option 2	**$25**	**$5**	**$5**	**$18**

Which payment would you rather be given to you, player 1, player 2, and player 3: option 1 or option 2?

Option 1

Option 2

Figure 1.2. Second Screenshot of Interface

This figure displays an example of the interface participants saw during the experiment. This example presents a basic decision adding two players with choice independent outcomes.

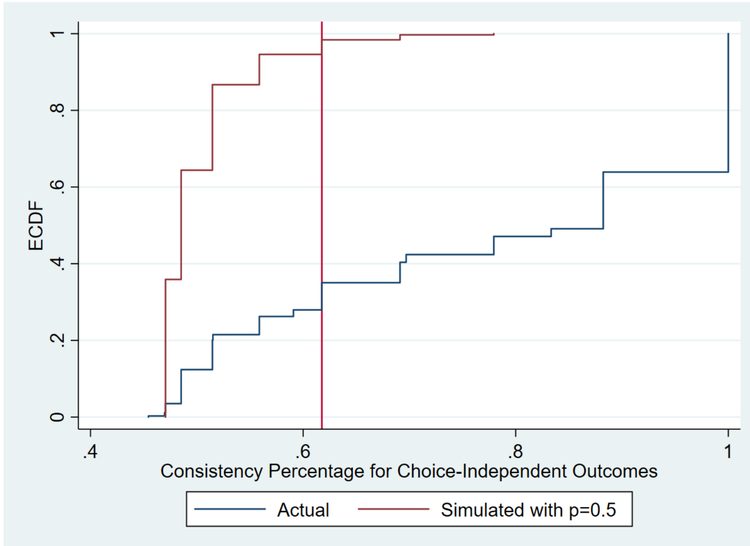


Figure 1.3. Comparison to Simulated Random Choice

This figure displays differences in the distribution of consistency rate between our data and simulated data with $p = 0.5$. The blue line is the empirical CDF of our actual data, while the red line is the empirical CDF of the simulated data. The vertical line indicates the lower 98% of consistency rates in the simulated distribution—only about 30% of our participants have a consistency rate that falls in this region.

Table 1.1. Example Choices

Choice 1	Outcome for Anne	Outcome for Betty		
Dog park	x	w		
Playground	y	z		
Choice 2	Outcome for Anne	Outcome for Betty	Outcome for Charles	
Dog park	x	w	c	
Playground	y	z	c	
Choice 3	Outcome for Anne	Outcome for Betty	Outcome for Charles	
Dog park	x	w	c'	
Playground	y	z	c'	
Choice 4	Outcome for Anne	Outcome for Betty	Outcome for Evelyn	Outcome for Fiona
Dog park	x	w	x	w
Playground	y	z	y	z

This table displays four different choices faced by Dave, our social planner. The first two choices differ only by the presence of Charles, whose outcome is independent of the choice Dave makes. The second and third choices differ only by the level of the choice independent outcome Charles receives. The first and fourth choices differ by the presence of Anne′ and Betty′, who each receive the same outcome as Anne and Betty respectively. We study when Charles selects the same option in each of these choices.

Table 1.2. Basic Decision

Choice 1	Payment to Player 1	Payment to Player 2
option 1	$5	$25
option 2	$10	$15

This table displays the most basic choice our participants made. There are three variants; the decision-maker could be player 1 (receiving the lower payment profile), player 2 (receiving the higher payment profile), or not a player and receive no payment from this decision.

Table 1.3. Addition of Choice Independent Outcomes

Choice 1	Payment to Player 1	Payment to Player 2		
option 1	$5	$25		
option 2	$10	$15		
Choice 2	Payment to Player 1	Payment to Player 2	Payment to Player 3	
option 1	$5	$25	$c	
option 2	$10	$15	$c	
Choice 3	Payment to Player 1	Payment to Player 2	Payment to Player 3	Payment to Player 4
option 1	$5	$25	$c	$$\hat{c}$
option 2	$10	$15	$c	$$\hat{c}$

This table presents choices that different only by inclusion of agents with choice independent outcomes. The decision-maker faces multiple variants where they are either player 1, player 2, player 3, or are not one of the players and receive no payment, and variants where the value of c varies between 5, 18, and 25.

Table 1.4. Addition of Duplications

Choice 1	Payment to Player 1	Payment to Player 2				
option 1	$5	$25				
option 2	$10	$15				
Choice 2	Payment to Player 1	Payment to Player 2	Payment to Player 3	Payment to Player 4		
option 1	$5	$25	$5	$25		
option 2	$10	$15	$10	$15		
Choice 3	Payment to Player 1	Payment to Player 2	Payment to Player 3	Payment to Player 4	Payment to Player 5	Payment to Player 6
option 1	$5	$25	$5	$25	$5	$25
option 2	$10	$15	$10	$15	$10	$15

This table presents choices that differ only by duplication of agents with variable payments. The decision-maker is not one of the players receiving payment here, as that would violate anonymity.

Table 1.5. Combined Additions

Choice 1	Payment to Player 1	Payment to Player 2				
option 1	$5	$25				
option 2	$10	$15				
Choice 2	Payment to Player 1	Payment to Player 2	Payment to Player 3			
option 1	$5	$25	$c			
option 2	$10	$15	$c			
Choice 3	Payment to Player 1	Payment to Player 2	Payment to Player 3	Payment to Player 4		
option 1	$5	$25	$5	$25		
option 2	$10	$15	$10	$15		
Choice 4	Payment to Player 1	Payment to Player 2	Payment to Player 3	Payment to Player 4	Payment to Player 5	
option 1	$5	$25	$5	$25	$c	
option 2	$10	$15	$10	$15	$c	
Choice 5	Payment to Player 1	Payment to Player 2	Payment to Player 3	Payment to Player 4	Payment to Player 5	Payment to Player 6
option 1	$5	$25	$5	$25	$c	$ĉ
option 2	$10	$15	$10	$15	$c	$ĉ

This table presents choices that differ by both the addition of agents with choice independent payments or the duplication of the choice-dependent payment profiles. The decision-maker faces variants where they are either player 6 or not one of the players and receive no payment, and where *c* varies between 5, 18, and 25.

Table 1.6. Sample-balance Table

Dependent Variable	Effect
male	-.0377 (.0552)
female	-.0499 (.0577)
age	-.0007 (.0010)
employed	-.0156 (.0246)
# of surveys completed	.0000164 (.00000769)**
time spent on experiment	-.00003 (.00004)
regression constant	.8720 (.0618)
R^2	.0310

This table presents linear regression results of the effect of different demographic variables on consistency percentage of the decision-maker, with standard errors clustered at the participant level. * is significant at 90%, ** is significant at 95%, *** is significant at 99%. We fail to reject the null hypothesis that the male and female coefficients are identical, with the F-test producing a p-value of 0.6427.

Table 1.7. Descriptive Statistics

condition	proportion consistent
Adding 1 choice-independent outcome	0.8672 (0.0040)
Changing level of 1 choice independent outcome	0.8662 (0.0051)
Duplication	0.8284 (.0094)
combined change	0.7789 (.0031)

This table presents the proportion of consistent decision pairs across different choice environments, with the more consistent option displayed on the left along with stars to indicate statistically significant differences in consistency. * is significant at 90%, ** is significant at 95%, *** is significant at 99%, **** is significant at 99.99%.

Table 1.8. Regression Results

	LPM	LPM	Logit	Logit	LPM	LPM	Logit	Logit
adding choice-independent outcome	.0009 (.0061)	.0006 (.0063)	.0087 (.0545)	.0053 (.0613)	-.0035 (.0059)	-.0036 (.0060)	-.0330 (.0529)	-.0383 (.0624)
duplication	-.0379 (.0133)***	-.0358 (.0133)***	-.2937 (.0966)***	-.3316 (.1163)***	.0065 (.0115)	.0087 (.0115)	.0357 (.0825)	.0651 (.0996)
combined change	-.0873 (.0120)***	-.0826 (.0114)***	-.6086 (.0774)***	-.6990 (.0871)***	-.0181 (.0071)**	-.0157 (.0070)**	-.1316 (.0531)**	-.1376 (.0632)**
DM paid on one only					-.0454 (.0128)***	-.0494 (.0129)***	-.2613 (.0783)***	-.3691 (.0966)***
DM paid on both					.0045 (.0199)	-.0038 (.0189)	.0290 (.1301)	-.0373 (.1517)
DM richer					.0611 (.0193)***	.0623 (.0192)***	.4801 (.1549)***	.5817 (.1799)***
DM poorer					.1020 (.0171)***	.1033 (.0170)***	.9410 (.1719)***	1.0978 (.1906)***
Regression constant	.8663 (.0106)	.8635 (.0075)	1.8681 (.0913)		.8185 (.0171)	.8219 (.0112)	1.5171 (.1146)	
Log Likelihood			-14,981.969	-11,071.051			-14,692.688	-10,843.016
(pseudo) R^2	.0116	.0116	.0125	.0178	.0226	.0226	.0257	.0380
N	31,400	31,400	31,400	23,010	31,400	31,400	31,400	23,010
# Clusters	200	200	200	145	200	200	200	145
Participant FE	No	Yes	No	Yes	No	Yes	No	Yes

This table presents multiple regression specifications comparing effect of domain changes on consistency. The baseline is changing only the level of a choice independent outcome when the decision-maker is not paid. All regressions computed with standard errors clustered at the participant level, along with participant-level fixed effects and additional controls where indicated. * is significant at 90%, ** is significant at 95%, *** is significant at 99%.

Table 1.9. Regression Results for Non-constant Payments

	LPM	Logit	LPM	Logit
DM makes selfish choice on smaller domain	.1258 (.0364)***	.9087 (.2679)***	.2011 (.0323)***	1.6732 (.2192)***
DM richer	-.0197 (.0150)	-.4230 (.2241)*	-.0076 (.0150)	-.0948 (.1873)
regression constant	.8074 (.0319)		.7403 (.0335)	1.0876 (.2158)
Log Likelihood		-1017.4788		-1789.0713
(pseudo) R^2	.0691	.0442	.0704	.0869
N	6,000	2,220	6,000	6,000
# clusters	200	74	200	200
Participant FE	Yes	Yes	No	No

This table presents multiple regression specifications comparing effects of different environments and choices on consistency, for the subset of choices in which the decision-maker has a non-constant payment. All regressions computed with standard errors clustered at the participant level, and with participant level fixed effects where indicated. * is significant at 90%, ** is significant at 95%, *** is significant at 99%.

Table 1.10. Breakdowns by Consistency Rate

Environment	Consistent	Inconsistent, Smaller	Inconsistent, Larger
Aggregate	.7383 (.0056)	.6009 (.0037)****	.5692 (.0038)****
Adding choice-independent outcome	.6648 (.0106)	.5735 (.0068)****	.5706 (.0069)****
Duplication	.7655 (.0203)	.6186 (.0141)****	.5781 (.0143)****
combined change	.7733 (.0067)	.6124(.0047)****	.5675 (.0048)****
DM choice-dependent outcome, aggregate	.6182 (.0120)	.5662 (.0075)***	.5501 (.0075)****
DM choice-dependent outcome, adding choice-independent outcome	.6182 (.0134)	.5629 (.0084)***	.5506 (.0084)****
DM choice-dependent outcome, combined change	.6182 (.0267)	.5793 (.0167)***	.5483 (.0169)**
DM choice-dependent outcome, richer, aggregate	.3272 (.0163)	.2497 (.0093)****	.2331 (.0091)****
DM choice-dependent outcome, richer, adding choice-independent outcome	.3273 (.0183)	.2483 (.0104)****	.2339 (.0101)****
DM choice-dependent outcome, richer, combined change	.3273 (.0365)	.2552 (.0209)*	.2299 (.0202)**
DM choice-dependent outcome, poorer, aggregate	.9091 (.0100)	.8828 (.0067)**	.8671 (.0073)***
DM choice-dependent outcome, poorer, adding choice-independent outcome	.9091 (.0112)	.8776 (.0079)**	.8672 (.0081)***
DM choice-dependent outcome, poorer, combined change	.9090 (.0223)	.9034 (.0142)	.8667 (.0163)***
DM choice-independent outcome, aggregate	.7816 (.0061)	.6127 (.0043)****	.5757 (.0044)****
DM choice-independent outcome, adding choice-independent outcome	.7588 (.0167)	.5943 (.0117)****	.6101 (.0116)****
DM choice-independent outcome, duplication	.7655 (.0203)	.6186 (.0141)****	.5781 (.0143)****
DM choice-independent outcome, combined change	.7880 (.0069)	.6153 (.0049)****	.5692 (.0050)****
DM paid on one domain, aggregate	.7838 (.0084)	.6127 (.0060)****	.5759 (.0061)****
DM paid on one domain, adding choice-independent outcome	.7702 (.0274)	.5985 (.0193)****	.6047 (.0193)****
DM paid on one domain, combined change	.7853 (.0089)	.6143 (.0063)****	.5727 (.0064)****
DM paid on both domains, aggregate	.6706 (.0089)	.5837 (.0057)****	.5640 (.0058)
DM paid on both domains, adding choice-independent outcome	.6363 (.0123)	.5658 (.0078)****	.5554 (.0078)****
DM paid on both domains, duplication	.7455 (.0240)	.6230 (.0164)****	.5724 (.0168)****
DM paid on both domains, combined change	.7005 (.0150)	.5991 (.0099)****	.5752 (.0010)****
DM paid on neither domain, aggregate	.8158 (.0118)	.6167 (.0086)***	.5675 (.0088)****
DM paid on neither domain, adding choice-independent outcome	.76 (.0302)	.6019 (.0211)***	.6444 (.0206)***
DM paid on neither domain, duplication	.8286 (.0368)	.6063 (.0275)****	.5937 (.0277)****
DM paid on neither domain, combined change	.8286 (.0133)	.6216 (.0101)****	.5459 (.0104)****

This table details the proportion of decision pairs where participants selected the inequality-averse choice, broken down by different choice environments. The left column is the proportion among individuals who are fully consistent in the sample, the middle and right columns are the proportion among individuals who are not fully consistent. The left and middle columns are the proportion where individuals selected the inequality-averse option on the choice with a smaller domain (regardless of their choice on the larger domain), and the left and right columns are the proportion where individuals selected the inequality-averse option on the choice with a larger domain (regardless of their choice on the smaller domain). Stars indicate significant differences against the left column. * is significant at 90%, ** is significant at 95%, *** is significant at 99%, **** is significant at 99.99%.

Table 1.11. Direction of Inconsistencies–Different Sizes

Environment	# Observations	Proportion Inequality ↓
Aggregate	4,450	.5611 (.0074)****
Adding choice-independent outcome	959	.5078 (.0161)
Duplication	278	.5863 (.0295)***
combined change	3213	.5749 (.0087)****
DM choice-dependent outcome, aggregate	604	.5779 (.0202)***
DM choice-dependent outcome, adding choice-independent outcome	473	.5454 (.0229)**
DM choice-dependent outcome, combined change	131	.6031 (.0427)**
DM choice-dependent outcome, richer, aggregate	362	.5497 (.0261)*
DM choice-dependent outcome, richer, adding choice-independent outcome	285	.5439 (.0295)
DM choice-dependent outcome, richer, combined change	77	.5715 (.0563)
DM choice-dependent outcome, poorer, aggregate	242	.5702 (.0318)**
DM choice-dependent outcome, poorer, adding choice-independent outcome	188	.5479 (.0363)
DM choice-dependent outcome, poorer, combined change	54	.6481 (.0650)**
DM choice-independent outcome, aggregate	3846	.5616 (.0080)****
DM choice-independent outcome, adding choice-independent outcome	486	.4712 (.0226)
DM choice-independent outcome, duplication	278	.5863 (.0295)***
DM choice-independent outcome, combined change	3082	.5736 (.0089)****
DM paid on one domain, aggregate	2235	.5545 (.0105)****
DM paid on one domain, adding choice-independent outcome	220	.4909 (.0337)
DM paid on one domain, combined change	2015	.5613 (.0111)****
DM paid on both domains, aggregate	769	.5488 (.0179)****
DM paid on both domains, adding choice-independent outcome	125	.4960 (.0447)
DM paid on both domains, duplication	224	.5982 (.0328)***
DM paid on both domains, combined change	420	.5381 (.0233)
DM paid on neither domain, aggregate	842	.5926 (.0169)****
DM paid on neither domain, adding choice-independent outcome	141	.4184 (.0415)*
DM paid on neither domain, duplication	54	.5370 (.0678)
DM paid on neither domain, combined change	647	.6352 (.0189)****
Duplication without choice-independent outcomes	54	.5370 (.0678)

This table presents the proportion of inconsistent choices which exhibit declining inequality aversion as size increase; that is, where the decision-maker selected the inequality-averse option on the smaller domain but not on the larger domain. ** is significant at 95%, *** is significant at 99% (significance is in terms of difference from 50%, which would indicate no trending direction).

Table 1.12. Direction of Inconsistencies–Same Sizes

	#Obs	Prop Inequality Av	Condition
total inconsistencies, same size	1,388		
Changing choice-independent level only	591		
Changing choice-independent only, replacing $25 with $18	173	.5607 (.0377)	more $18
Changing choice-independent only, replacing $25 with $5	175	.5371 (.0377)	more $5
Changing choice-independent only, replacing $18 with $5	243	.4733 (.0322)	more $5
combined change	797		
combined change, swapping DM without changing profiles	191	.4764 (.0361)	DM paid
combined change, swapping DM and $18 for $25	154	.6234 (.0390)***	more $18
combined change, swapping DM and$5 for $25	121	.6363 (.0437)***	more $5
combined change, swapping DM and $5 for $18	141	.5745 (.0416)*	more $5
combined change, swapping $18 for duplication	62	.5645 (.0629)	more dup
combined change, swapping $5 for duplication	45	.5556 (.0741)	more dup
combined change, swapping $18 and $25 for duplication	41	.5853 (.0769)	more dup
combined change, swapping $18 and $25 for $5	42	.5476 (.0769)	more $5
combined change, swapping DM aggregate	607	.4530 (.0202)**	DM paid
combined change, swapping DM without changing profiles	191	.4764 (.0361)	DM paid
combined change, swapping DM and $18 for $25	154	.3636 (.0387)***	DM paid
combined change, swapping DM and$5 for $25	121	.4711 (.0454)	DM paid
combined change, swapping DM and $5 for $18	141	.5035 (.0421)	DM paid

This table presents the proportion of inconsistent choices which exhibit declining inequality aversion as size stays the same buyt composition change; Prop Inequality Av is the proportion of choice pairs where the inequality averse choice is selected only after the indicated replacement is made. * is significant at 90%, ** is significant at 95%, *** is significant at 99% (significance is in terms of difference from 50%, which would indicate no trending direction).

Table 1.13. Robust Regression Results

	LPM	Logit	LPM	Logit
adding choice-independent outcome	.0004 (.0061)	.0044 (.0529)	-.0036 (.0059)	-.0332 (.0529)
duplication	-.0358 (.0134)***	-.2807 (.0983)***	.0087 (.0115)	.0522 (.0824)
combined change	-.0827 (.0114)***	-.5780 (.0751)****	-.0156 (.0070)**	-.1135 (.0527)**
DM paid on one only			-.0494 (.0129)***	-.2901 (.0777)***
DM paid on both			.0038 (.0189)	-.0303 (.1231)
DM richer			.0623 (.0192)***	.4904 (.1547)***
DM poorer			.1032 (.0170)****	.9519 (.1715)****
control for 60 participants	.0412 (.0239)*	.3031 (.1850)	.0356 (.0238)	.2672 (.1860)
regression constant	.8551 (.0129)	1.7928 (.1010)	.8146 (.0180)	1.4889 (.1203)
Log Likelihood		-14,861.112		-14,669.928
(pseudo) R^2	.0134	.0145	.0239	.0272
N	31,400	31,400	31,400	31,400
# Clusters	200	200	200	200

This table presents a version of table 1.8 which excludes the first 60 participants. The table presents multiple specifications comparing the effect of different choice environments on consistency. All regressions computed with with standard errors clustered at the participant level, and with participant-level fixed effects where indicated. * is significant at 90%, ** is significant at 95%, *** is significant at 99%. All conclusions are the same as in table 1.8.

Table 1.14. Robust Breakdowns by Consistency Rate

Environment	Consistent	Inconsistent, Smaller	Inconsistent, Larger
Aggregate	.7966 (.0059)	.5981 (.0041)****	.5624 (.0042)****
Adding choice-independent outcome	.7359 (.0123)	.5717 (.0079)****	.5624 (.0080)***
Duplication	.8286 (.0212)	.6190 (.0158)****	.5757 (.0161)****
combined change	.8188 (.0069)	.6070 (.0051)****	.5610 (.0052)****
DM choice-dependent outcome, aggregate	.6857 (.0143)	.5590 (.0088)****	.5419 (.0089)****
DM choice-dependent outcome, adding choice-independent outcome	.6857 (.0160)	.5560 (.0099)****	.5425 (.0099)****
DM choice-dependent outcome, combined change	.6857 (.0320)	.5714 (.0197)	.5397 (.0199)***
DM choice-dependent outcome, richer, aggregate	.4000 (.0213)	.2304 (.0106)****	.2229 (.0105)****
DM choice-dependent outcome, richer, adding choice-independent outcome	.4000 (.0239)	.2286 (.0118)****	.2230 (.0117)****
DM choice-dependent outcome, richer, combined change	.4000 (.0478)	.2381 (.0240)***	.2222 (.0234)***
DM choice-dependent outcome, poorer, aggregate	.9714 (.0073)	.8876 (.0080)****	.8610 (.0087)****
DM choice-dependent outcome, poorer, adding choice-independent outcome	.9714 (.0081)	.8833 (.0089)****	.8619 (.0097)****
DM choice-dependent outcome, poorer, combined change	.9714 (.0162)	.9047 (.0165)**	.8571 (.0197)****
DM choice-independent outcome, aggregate	.8286 (.0062)	.6093 (.0047)****	.5683 (.0047)****
DM choice-independent outcome, adding choice-independent outcome	.8286 (.0177)	.6007 (.0132)****	.5993 (.0133)****
DM choice-independent outcome, duplication	.8286 (.0212)	.6190 (.0158)****	.5757 (.0161)****
DM choice-independent outcome, combined change	.8286 (.0070)	.6096 (.0052)****	.5626 (.0053)****
DM paid on one domain, aggregate	.8286 (.0087)	.6047 (.0065)****	.5702 (.0066)****
DM paid on one domain, adding choice-independent outcome	.8286 (.0285)	.6038 (.0213)****	.6019 (.0214)****
DM paid on one domain, combined change	.8286 (.0091)	.6049 (.0068)****	.5670 (.0069)****
DM paid on both domains, aggregate	.7445 (.0103)	.5795 (.0067)****	.5544 (.0068)****
DM paid on both domains, adding choice-independent outcome	.7061 (.0146)	.5605 (.0092)****	.5463 (.0092)****
DM paid on both domains, duplication	.8286 (.0260)	.6254 (.0193)****	.5667 (.0197)****
DM paid on both domains, combined change	.7782 (.0170)	.5944 (.0116)****	.5636 (.0117)****
DM paid on neither domain, aggregate	.8286 (.0118)	.6183 (.0088)****	.5619 (.0090)****
DM paid on neither domain, adding choice-independent outcome	.8286 (.0319)	.6095 (.0238)****	.6262 (.0236)****
DM paid on neither domain, duplication	.8286 (.0368)	.6063 (.0275)****	.5937 (.0277)****
DM paid on neither domain, combined change	.8286 (.0136)	.6216 (.0101)****	.5459 (.0104)****

This table presents a version of table 1.10 which excludes the first 60 participants. This table details the proportion of decision pairs where participants selected the inequality-averse choice, broken down by different choice environments. The left column is the proportion among individuals who are fully consistent in the sample, the middle and right columns are the proportion among individuals who are not fully consistent. The left and middle columns are the proportion where individuals selected the inequality-averse option on the choice with a smaller domain (regardless of their choice on the larger domain), and the left and right columns are the proportion where individuals selected the inequality-averse option on the choice with a larger domain (regardless of their choice on the smaller domain). Stars indicate significant differences against the left column. * is significant at 90%, ** is significant at 95%, *** is significant at 99%, **** is significant at 99.99%.

Table 1.15. Direction of Inconsistencies–Different Sizes

Environment	# Observations	Proportion Inequality ↓
Aggregate	3782	.5664 (.0081)****
Adding choice-independent outcome	722	.5249 (.0186)
Duplication	221	.5928 (.0330)***
combined change	2839	.5749 (.0093)****
DM choice-dependent outcome, aggregate	428	.5631 (.0240)***
DM choice-dependent outcome, adding choice-independent outcome	338	.5503 (.0271)*
DM choice-dependent outcome, combined change	90	.6111 (.0514)**
DM choice-dependent outcome, richer, aggregate	242	.5248 (.0321)
DM choice-dependent outcome, richer, adding choice-independent outcome	193	.5181 (.0360)
DM choice-dependent outcome, richer, combined change	49	.5510 (.0711)
DM choice-dependent outcome, poorer, aggregate	186	.6129 (.0357)***
DM choice-dependent outcome, poorer, adding choice-independent outcome	145	.5931 (.0408)**
DM choice-dependent outcome, poorer, combined change	41	.6829 (.0191)**
DM choice-independent outcome, aggregate	3354	.5668 (.0086)****
DM choice-independent outcome, adding choice-independent outcome	384	.5026 (.0255)
DM choice-independent outcome, duplication	221	.5928 (.0330)***
DM choice-independent outcome, combined change	2749	.5737 (.0094)****
DM paid on one domain, aggregate	1980	.5495 (.0112)
DM paid on one domain, adding choice-independent outcome	185	.5027 (.0368)
DM paid on one domain, combined change	1795	.5543 (.0117)****
DM paid on both domains, aggregate	566	.5767 (.0208)***
DM paid on both domains, adding choice-independent outcome	92	.5435 (.0519)
DM paid on both domains, duplication	167	.6108 (.0377)***
DM paid on both domains, combined change	307	.5570 (.0284)**
DM paid on neither domain, aggregate	808	.6064 (.0172)****
DM paid on neither domain, adding choice-independent outcome	107	.4673 (.0482)
DM paid on neither domain, duplication	54	.5370 (.0678)
DM paid on neither domain, combined change	647	.6253 (.0189)****
Duplication without choice-independent outcomes	54	.5370 (.0679)

This table presents a version of table 1.11 which excludes the first 60 participants. Proportion Inequality ↓ is the proportion of choice pairs where we see decreasing inequality aversion as size increases; that is, where the decision-maker selected the inequality-averse option on the smaller domain but not on the larger domain. ** is significant at 95%, *** is significant at 99% (significance is in terms of difference from 50%, which would indicate no trending direction).

Table 1.16. Alternative Robust Regression Results

	LPM	Logit	LPM	Logit
adding choice-independent outcome	.0008 (.0061)	.0076 (.0589)	-.0035 (.0059)	-.0369 (.0595)
duplication	-.0373 (.0135)***	-.3277 (.1108)***	.0072 (.0115)	.0476 (.0945)
combined change	-.0861 (.0115)***	-.6854 (.0832)***	-.0173 (.0070)**	-.1444 (.0597)**
DM paid on one only			-.0467 (.0128)***	-.3163 (.0900)***
DM paid on both			.0019 (.0181)	.0133 (.1361)
DM richer			.0615 (.0192)***	.5458 (.1717)***
DM poorer			.1025 (.0169)****	1.0487 (.1849)****
control for low consistency	-.3075 (.0124)****	-1.8504 (.1071)****	-.3073 (.0124)****	-1.8794 (.1069)****
regression constant	.9511 (.0101)	2.6468 (.1206)	.9051 (.0146)	2.2770 (.1369)****
Log Likelihood		-13,078.505		-12855.812
(pseudo) R^2	.1371	.1327	.1480	.1475
N	31,400	31,400	31,400	31,400
# Clusters	200	200	200	200

This table presents a version of table 1.8 which accounts for individuals whose consistency is so low they are classified as choosing uniformly at random. Multiple specifications comparing effect of different choice environments. All regressions computed with with standard errors clustered at the participant level, and with participant level fixed effects where indicated. * is significant at 90%, ** is significant at 95%, *** is significant at 99%. All conclusions are the same as in table 1.8.

Table 1.17. Alternative Robust Breakdown by Consistency Rate

Environment	Consistent	Inconsistent, Smaller	Inconsistent, Larger
Aggregate	.7384 (.0056)	.6197 (.0047)****	.5987 (.0048)****
Adding choice-independent outcome	.6648 (.0106)	.5951 (.0086)****	.5855 (.0086)
Duplication	.7655 (.0203)	.6340 (.0178)****	.6245 (.0179)****
combined change	.7733 (.0068)	.6301 (.0059)****	.6023 (.0060)****
DM choice-dependent outcome, aggregate	.6182 (.0120)	.5867 (.0095)**	.5630 (.0095)***
DM choice-dependent outcome, adding choice-independent outcome	.6182 (.0134)	.5833 (.0106)**	.5639 (.0107)***
DM choice-dependent outcome, combined change	.6182 (.0267)	.6000 (.0211)	.5593 (.0214)*
DM choice-dependent outcome, richer, aggregate	.3273 (.0163)	.26 (.0119)***	.2319 (.0115)****
DM choice-dependent outcome, richer, adding choice-independent outcome	.3273 (.0183)	.2583 (.0133)***	.2333 (.0129)****
DM choice-dependent outcome, richer, combined change	.3273 (.0365)	.2667 (.0269)	.2259 (.0255)**
DM choice-dependent outcome, poorer, aggregate	.9091 (.0100)	.9133 (.0077)	.8941 (.0084)
DM choice-dependent outcome, poorer, adding choice-independent outcome	.9091 (.0112)	.9083 (.0088)	.8944 (.0093)
DM choice-dependent outcome, poorer, combined change	.9091 (.0224)	.9333 (.0152)	.8926 (.0188)
DM choice-independent outcome, aggregate	.7817 (.0061)	.6309 (.0054)****	.6108 (.0055)****
DM choice-independent outcome, adding choice-independent outcome	.7588 (.0167)	.6182 (.0147)****	.6283 (.0146)****
DM choice-independent outcome, duplication	.7655 (.0178)	.6340 (.0178)****	.6245 (.0179)****
DM choice-independent outcome, combined change	.7880 (.0069)	.6328 (.0062)****	.6061 (.0063)****
DM paid on one domain, aggregate	.7838 (.0085)	.6311 (.0075)****	.6078 (.0076)****
DM paid on one domain, adding choice-independent outcome	.7702 (.0274)	.6200 (.0243)****	.6250 (.0242)***
DM paid on one domain, combined change	.7853 (.0089)	.6323 (.0079)****	.6060 (.0080)****
DM paid on both domains, aggregate	.6706 (.0089)	.6050 (.0072)****	.5824 (.0073)****
DM paid on both domains, adding choice-independent outcome	.6364 (.0126)	.5877 (.0098)***	.5698 (.0099)****
DM paid on both domains, duplication	.7455 (.0240)	.6370 (.0206)***	.6167 (.0209)****
DM paid on both domains, combined change	.7005 (.0150)	.6222 (.0124)****	.5908 (.0126)****
DM paid on neither domain, aggregate	.8158 (.0118)	.6301 (.0109)****	.6179 (.0110)****
DM paid on neither domain, adding choice-independent outcome	.76 (.0302)	.6209 (.0265)***	.6567 (.0259)****
DM paid on neither domain, duplication	.8286 (.0368)	.6256 (.0347)***	.6462 (.0342)***
DM paid on neither domain, combined change	.8286 (.0136)	.6329 (.0127)****	.6049 (.0129)****

This table presents a version of table 1.10 which excludes participants with consistency rates so low they are classified as choosing uniformly at random. This table details the proportion of decision pairs where participants selected the inequality-averse choice, broken down by different choice environments. The left column is the proportion among individuals who are fully consistent in the sample, the middle and right columns are the proportion among individuals who are not fully consistent. The left and middle columns are the proportion where individuals selected the inequality-averse option on the choice with a smaller domain (regardless of their choice on the larger domain), and the left and right columns are the proportion where individuals selected the inequality-averse option on the choice with a larger domain (regardless of their choice on the smaller domain). Stars indicate significant differences against the left column. * is significant at 90%, ** is significant at 95%, *** is significant at 99%, **** is significant at 99.99%.

Table 1.18. Alternative Robust Direction of Inconsistencies–Different Sizes

Environment	# Observations	Proportion Inequality ↓
Aggregate	1697	.5657 (.0120)****
Adding choice-independent outcome	423	.5366 (.0242)
Duplication	117	.5299 (.0461)
combined change	1157	.5799 (.0145)****
DM choice-dependent outcome, aggregate	298	.6074 (.0283)***
DM choice-dependent outcome, adding choice-independent outcome	236	.5890 (.0320)***
DM choice-dependent outcome, combined change	62	.6774 (.0594)***
DM choice-dependent outcome, richer, aggregate	202	.5941 (.0346)***
DM choice-dependent outcome, richer, adding choice-independent outcome	159	.5849 (.0391)**
DM choice-dependent outcome, richer, combined change	43	.6279 (.0737)*
DM choice-dependent outcome, poorer, aggregate	96	.6354 (.0491)***
DM choice-dependent outcome, poorer, adding choice-independent outcome	77	.5974 (.0559)*
DM choice-dependent outcome, poorer, combined change	19	.7895 (.0935)**
DM choice-independent outcome, aggregate	1399	.5568 (.0132)***
DM choice-independent outcome, adding choice-independent outcome	187	.4706 (.0365)
DM choice-independent outcome, duplication	117	.5299 (.0461)
DM choice-independent outcome, combined change	1095	.5744 (.0149)****
DM paid on one domain, aggregate	777	.5611 (.0178)***
DM paid on one domain, adding choice-independent outcome	74	.4865 (.0581)
DM paid on one domain, combined change	703	.5690 (.0187)***
DM paid on both domains, aggregate	322	.5621 (.0276)**
DM paid on both domains, adding choice-independent outcome	53	.5283 (.0686)
DM paid on both domains, duplication	91	.5604 (.0520)
DM paid on both domains, combined change	178	.5730 (.0370)*
DM paid on neither domain, aggregate	300	.5400 (.0288)
DM paid on neither domain, adding choice-independent outcome	60	.4000 (.0632)
DM paid on neither domain, duplication	26	.4231 (.0969)
DM paid on neither domain, combined change	214	.5935 (.0336)
Duplication without choice-independent outcomes	26	.4231 (.0969)

This table presents a version of table 1.11 which excludes participants with consistency rates so low they are classified as choosing uniformly at random. This table details the proportion of decision pairs where participants selected the inequality-averse choice, broken down by different choice environments. The left column is the proportion among individuals who are fully consistent in the sample, the middle and right columns are the proportion among individuals who are not fully consistent. The left and middle columns are the proportion where individuals selected the inequality-averse option on the choice with a smaller domain (regardless of their choice on the larger domain), and the left and right columns are the proportion where individuals selected the inequality-averse option on the choice with a larger domain (regardless of their choice on the smaller domain). Stars indicate significant differences against the left column. * is significant at 90%, ** is significant at 95%, *** is significant at 99%, **** is significant at 99.99%.

1.8 Appendix–Participant Documentation

1.8.1 Consent Form

Please read the following consent form carefully.

Who is conducting the study, how you were selected, and what is the approximate number of participants in the study?

Adrian Wolanski, Evgenii Baranov, and Isabel Trevino are conducting a research study to find out more about collective choice. You have been asked to participate in this study because you are registered as a Participant on Prolific.co. There will be approximately 300 participants in this study.

Why is this study being done?

The purpose of this study is to understand how individuals make decisions that involve income distributed to other people.

What will happen to you in this study, and which procedures are standard of care and which are experimental?

If you agree to be in this study, you will experience the following: you will be randomly matched with one to three other participants in the study. You will be offered a series of pairs of payment options for those participants and yourself, and asked to select which option you would rather see paid out. Then, one of these decisions you make will be randomly selected to become the decision-that-counts, and randomly selected decisions-that-count will be paid out. Each participant in the study will make decisions about payment schedules for others, as well as be someone for whom others are making decisions. Thus, your payments will involve some chance and will depend on the choices of others, your own choices, and decisions-that-count.

How much time will each study procedure take, what is your total time commitment, and how long will the study last?

The entire study will take approximately 10 minutes.

What risks are associated with this study?

Participation in this study may involve some added risks or discomforts. These include the following:

Potential loss of confidentiality. Your choices will never be linked to your name. Each one of you has been given a Prolific ID. This ID will be used to register your choices and provide your payment. However, there does exist a record of your participation today. Research records will be kept confidential to the extent allowed by law. Research records may be reviewed by the UCSD Institutional Review Board.

Because this is a research study, there may also be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings.

What are the alternatives to participating in this study?

The alternative to participation in this study is to exit the study.

What benefits can be reasonably expected?

There may or may not be any direct benefit to you from participating in this study. If there is a direct benefit to you, it will be monetary and will depend on your choices and the choices of others. The average payment will be $3.80, which includes a $2.50 participation payment. The investigators may learn more about collective choice, and society may benefit from this research and knowledge.

Can you choose to not participate or withdraw from the study without penalty or loss of benefits?

Participation in this study is entirely voluntary. You may refuse to participate or withdraw or refuse to answer specific questions in an interview or on a questionnaire at any time without penalty or loss of benefits to which you are entitled. If you decide that you no longer wish to continue in this study, you will be required to contact the research staff immediately to request to leave the study. You will be told if any important new information is found during this study that may affect your desire to continue.

Can you be withdrawn from the study without your consent?

The PI may remove you from the study without your consent if the PI feels that doing so is in your best interest or the best interest of the study. You may also be withdrawn from the study if you do not follow the instructions given to you by the study personnel.

Will you be compensated for participating in this study?

In compensation for your time, you will be paid a participation payment of $2.50 and have a 10% chance to be paid based on the decision-that-counts (ranging from $5 to $25) from you or from another participant.

Are there any costs associated with participating in this study?

There will be no cost to you for participating in this study.

Who can you contact if you have questions? If at any time you have questions about the research, you may contact the investigator Adrian Wolanski who will answer all questions. His email address is awolansk@ucsd.edu.

Additionally, you may call the Human Research Protections Program Office at (858) 657-5100 to inquire about your rights as a research subject or to report research-related issues. You may also contact the University of California San Diego Institutional Review Board at IRB@ucsd.edu (the reference number for this project is IRB 805037).

Your consent and acknowledgement of consent: By clicking next, you acknowledge that you have read the consent form and agree to participate in this study.

1.8.2 Instructions

This study examines peoples' decisions about monetary payments received by themselves and other people, and takes place in two parts. You will be presented with a pair of options to be paid out to a randomly generated group of participants of this study, and asked which of the proposed options you would like to see paid out. Once we have collected data from all participants, we will select one decision at random to become the decision-that-counts. We will then randomly select 10% of participants to receive a bonus payment based on the decision-that-counts; this means that your decision-that-counts can affect the payment that you receive or that other people receive. As such, you should answer each question honestly, since your answers can affect your payment and the payments of other people. These bonus payments can range from $5 to $25. At no point will you see the choices other participants made for you, nor will any other participants see the choices you made for them.

Your total payment for the study consists of two components: your participation payment of $2.50, and a 10% chance for a bonus payment ranging from $5 to $25 which depends on your choices, the choices of others, and random chance.

Chapter 2

Attitudes Towards Intertemporal Inequality

2.1 Introduction

Measuring economic inequality has been a topic in the literatuer for more than 100 years, with many researchers developing measurements such as Gini (1921), Theil (1967), Atkinson (1970). These inequality measures offer slightly different interpretations of what contributes to higher levels of inequality. All of them, however, agree on the Pigou-Dalton[1] principle — that a rank-preserving transfer from a richer person to a poorer person reduces inequality. There have been approaches to generalize this principle from single-variable inequality to more general settings, starting with works of Kolm (1977), Maasoumi (1986), and Tsui (1999).

This chapter examines the particular multi-dimensional setting of intertemporal income. We define three different rankings for intertemporal income streams that differ by access to saving and borrowing technologies: one allows only for hand-to-mouth consumption, another allows for interest-free saving[2] but no borrowing, and the third one allows borrowing or lending

[1]Originally suggested by Pigou (1912) and Dalton (1920).

[2]The so-called "hiding money in the mattress" savings technology.

at a constant per-period interest rate. Using these income rankings, we establish criterion for inequality-reducing transfers which generalize the Pigou-Dalton principle to our intertemporal setting. We additionally provide a fourth variant of the Pigou-Dalton principle, which considers inequality levels in each period separately (rather than collectively over the lifetime of the stream.) We design a laboratory experiment to test the empirical relevance of these measures as measures of inequality. We also test the proposed income rankings for personal decisions and find that subjects exhibit strict preferences for receiving weakly more money, but when receiving the same total amount spread over the periods, their choices are indistinguishable from random. This suggests the lack of the use of borrowing/lending resources by student subjects.

In general, we consider the planner's problem of ranking social allocation streams of incomes. Every agent receives an income stream paid out across 3 time periods. Besides that, every agent is assumed to be identical. We study social preferences of the planner in this setting. The problem is identical to studying multidimensional inequality, but with additional structure imposed since each dimension represents the same good (money) at a different point in time. It is not obvious how the bundles (10 apples, 2 bananas) and (5 apples, 3 bananas) should be compared by an agent, but if instead those bundles were to be ($10 today, $2 tomorrow) and ($5 today, $3 tomorrow), then it is reasonable to claim that the former is better. Our income rankings can be viewed as a generalization of this simplistic example.

Pigou-Dalton transfer principle is a basis for measuring inequality, and there are multiple reasonable generalizations of it from the single good single period case. With our application to intertemporal income streams, there are multiple ways to define what it means to have a regressive transfer. Our proposed notions of stream dominance are designed to address that part of the generalization. We then provide generalizations of Pigou-Dalton principle to multi-period settings for within-period and whole-stream dominance rankings. Of particular interest is the fact that these notions need not always agree—one stream might have lower within-period inequality but higher whole-stream inequality than another.

Our weakest notion of stream dominance, vector dominance, is explored in Lasso de la Vega et al. (2010), Basili et al. (2017), which use standard vector rankings and majorization criteria for generalizing Pigou-Dalton principle to multidimensional inequality. Our findings confirm that it is indeed the most natural notion for ranking individual streams, commonly used by agents.

Bosmans et al. (2009) also proposes a multidimensional generalization of Pigou-Dalton, that uses the planner's trade-off rates between different attributes to define regressivity. In that regard, our work could be viewed as a special case of multidimensional generalization of Bosmans et al. (2009), but we have additional structure imposed from the fact that the agents are consuming the same good at every period. This allows us to propose more sensible dominance rankings that are unlikely to arise from individual trade-off rates between different goods. Additionally, individual trade-off rates between goods could differ drastically from one agent to the other, since it is unclear how to rank access to healthcare against a college degree. It is much clearer, however, how to rank money today versus money tomorrow. For the income streams, we find that a notion of cumulative dominance—comparing streams on the basis of accumulated sums at every period—is commonly spread.

While there are many possible rankings of streams and these rankings often produce agreeing definitions of regressive transfers, this is not always the case. It is possible for one transfer to be regressive under one ranking but progressive under another. As an example, consider two agents, Ann and Bob, with respective consumption streams $C_a = (6,1,6)$ and $C_b = (1,11,1)$ that denote \$ amounts consumed in each of the 3 periods. Our third notion of whole-stream inequality comes from a ranking using exponential discounting. Note that regardless of the discount factor, C_a always yields a higher discounted value than C_b, since: $6+\delta+6\delta^2 > 1+11\delta+\delta^2 \rightarrow 5-10\delta+5\delta^2 > 0 \rightarrow 5(1-\delta)^2 > 0$, which is true for $0 < \delta < 1$. An example of an inequality-increasing transfer would take \$2 from Bob in period 2 and give it to Ann, and then take \$2 from Ann in period 3 and give it to Bob. This results in new outcomes

$\hat{C}_a = (6,3,4)$ and $\hat{C}_b = (1,9,3)$, which is regressive under exponential discounting. However, note that in period 2, Ann has accumulated \$7 while Bob has \$12, while the new outcome gives Ann an accumulated income of \$9 in period 2 and Bob an accumulated income of \$10 in period 2, without changing the accumulated income in any other period. Such a transfer is then progressive in terms of reducing differences in period-wise accumulated income.

As this example illustrates, the same transfer may be viewed as both regressive and progressive depending on whether the decision-maker ranks streams period-wise or whole-stream-wise. This chapter argues that it is possible to have different agents express aversion to inequality, but still disagree on whether a particular transfer is regressive or progressive depending on the ranking they use. We show experimentally that one of these rankings, the period-wise ranking of cumulative payments, is empirically supported by far more participants than any whole-stream rankings of payments (even the most stringent ranking of vector dominance used frequently in the literature). To summarize, our contribution comes in three parts: we note the tension between whole-stream and period-wise dominance rankings used to generalize regressivity of the transfers, and thus leading to contradictory predictions for changes in inequality; we then take it to the laboratory experiment and show that there are indeed agents of different types: the majority cares about period-wise inequality, but some do care about life-time comparisons; finally, we observe individual decisions that are inconsistent with the commonly used exponential discounting.

Overall, we recruited 118 participants for a laboratory experiment in which each subject acts as a social planner selecting payment streams for a pair of other participants. In this environment, we find evidence that about 22% of the participants do not have any altruistic motives. Among the others, however, a much larger share of subjects' behavior is explained by period-wise inequality aversion rather than whole-stream inequality aversion. We also offer participants choices over streams for themselves that differ by various whole-stream rankings, and find that a large share of participants chooses dominated options (especially for our weakest ranking based on exponential discounting).

We also offer some choices that are slight variations of each other, where we introduce a very small inefficiency into the transfer. This allows us to see if some planners would be willing to forgo some efficiency to reduce inequality. While this is related to the literature on altruistic behavior and warm-glow Andreoni (1990), the main difference is that planners are giving up other people's payments (rather than their own) to reduce inequality. For an extensive overview of experimental work on inequality aversion, we refer the reader to Clark and d'Ambrosio (2015).

Zuber (2011) and Jackson and Yariv (2015) point out that if both individual and social preferences are represented by Exponential Discounted Utility with different discount factors, then a social criterion satisfying stationarity, time consistency, and the Pareto principle is dictatorial. There have been many different approaches to weakening some of these axioms to avoid the impossibility result: Billot and Qu (2022) and Feng and Ke (2018) weaken the Pareto Condition, Hayashi (2016) and Millner and Heal (2018) argue against time invariance, while Miyagishima (2022) weakens axioms on inequality aversion. Our chapter provides experimental evidence in favor of the latter two approaches; our participants often choose exponentially discounting dominanted options over dominant options, and display aversion to period-wise rather than whole-stream inequality.

The rest of the chapter is structured as follows. Section 2.2 formally defines our stream dominance relations and our generalizations of the Pigou-Dalton principle. Section 2.3 provides details on the experimental design. Section 2.4 shares our main findings from the experiment and section 2.5 concludes the chapter.

2.2 Theoretical Framework

2.2.1 Setup

A society consists of $n = 2$ agents [3], that live for $T \in \mathbb{N}$ periods. Let $I^j \in \mathbb{R}^T$ be the stream of income[4] received by agent j, with $I_t^j \in \mathbb{R}$ being the income received by agent j in period t.

2.2.2 Intertemporal Choice Problems

Each agent in society faces an intertemporal optimization problem of the form:

$$\max_{(c_1,\dots,c_T)} U(c_1,\dots,c_T)$$

$$\text{Subject to: } \sum_{t=1}^{T} c_t p_t \leq \sum_{t=1}^{T} I_t p_t$$

Where p_t measures relative prices of consumption in period t, given existing saving/borrowing techniques available to the consumers. To account for the value of flexibility, we will assume that $p_t \geq p_{t+1}$ for all t. There are many additional restrictions we can impose on the budget set. One example is when the agent does not have access to any saving or borrowing technology. In that case, the agent's constraint would be

setup 1

$$\forall t \leq T;\ c_t \leq I_t$$

Such a consumer would always be hand-to-mouth regardless of the level of income they receive.

[3] It is straightforward to generalize our structure to settings with more than two agents; we consider this case only for expositional simplicity.

[4] We focus on income rather than consumption. This means we will consider the indirect utility functions over income rather than utility function over consumption.

Another example is when the agent has access to a savings technology without interest or depreciation, but cannot borrow.

setup 2

$$\forall \tau \leq T;\ \sum_{t=1}^{\tau} c_t \leq \sum_{t=1}^{\tau} I_t$$

An additional constraint possibility is that the agent can save or borrow between any periods at a constant interest rate of r. Such a constraint would be

setup 3

$$\sum_{t=1}^{T} \frac{c_t}{(1+r)^{t-1}} \leq \sum_{t=1}^{T} \frac{I_t}{(1+r)^{t-1}}$$

It is important to note that we have assumed nothing about the agents preferences here; we have only provided a description of the intertemporal budget constraints the agent faces.

2.2.3 Partial Orders on Streams

The difficulty of discussing inequality in intertemporal settings is that it is not always obvious when one person's income stream is better than another's, since streams (vectors) of income are harder to rank than scalars. To this end, we define three variations of *stream dominance rankings*, which are partial orders over the space of income streams $I = (I_1, I_2, \ldots, I_T)$. The first of these is the standard partial order over $\mathbb{R}^T$, in which one stream is better than another if it pays a weakly larger amount in each period.

Definition 2.1. *The stream $c^A \in X$ is called vector dominant over the stream $I^B \in X$ if $\forall t\ \ I_t^A \geq I_t^B$, and we denote it as $I^A R^V I^B$.*

Proposition 2.1. *The indirect utility for an agent facing any of the three setups in section 2.2 is always weakly higher from stream I^A than from stream I^B whenever $I^A R^V I^B$.*

The second partial order we define makes use of the natural relationship between periods in an intertemporal budget: moving income from a later period to an earlier period provides greater flexibility

Definition 2.2. *The stream $I^A \in X$ is called cumulatively dominant over the stream $I^B \in X$ if $\forall \tau \ \sum_1^{\tau} I_t^A \geq \sum_1^{\tau} I_t^B$, and we denote it as $I^A R^C I^B$.*

Cumulatively dominant streams have sufficiently more consumption earlier in time such that there is a way to rearrange consumption only through savings that results in the previously cumulatively dominant stream now being vector dominant.[5] An example of a cumulatively dominant stream relationship is the example provided in section 1 of $I^A = (10,2)$ and $I^B = (5,3)$. While I^A does not vector dominate I^B, it does cumulatively dominate it since $I_1^A = 10 > I_1^B = 5$ and $I_1^A + I_2^A = 10 + 2 > I_1^B + I_2^B = 8$.

Proposition 2.2. *The indirect utility for an agent facing setup 2 or 3 in section 2.2 is always weakly higher from stream I^A than from stream I^B whenever $I^A R^C I^B$.*

The third partial order we define makes us of standard exponential discounting to rank streams, but uses an arbitrary discount factor.

Definition 2.3. *The stream $I^A \in X$ is called discounting dominant[6] over the stream $I^B \in X$ if $\forall \delta \in (0,1); \sum_1^T \delta^t I_t^A \geq \sum_1^T \delta^t I_t^B$, and we denote it as $I^A R^D I^B$.*

A discounting dominant stream has sufficiently more income earlier in time such that there is a way to rearrange both streams through saving and borrowing at an interest rate[7] of $1 - \delta$

[5]Let $t_1 = \min t$ such that $c_t^A < c_t^B$. Since $c^A R^C c^B$, $c_{t_1}^B - c_{t_1}^A \leq \sum_{t=1}^{t_1} c_t^A - c_t^B$. We can then define $c_t^D = c_t^B$ for $t < t_1$ and $c_{t_1}^D = c_\tau^A + \sum_{t=1}^{t_1} c_t^A - c_t^B$, and by construction $c^D|_{t_1} R^V c^B|_{t_1}$. Let $t_k = \min_{t > t_{k-1}} t$ such that $c_{t_k}^A < c_{t_k}^B$. We can then define $c_t^D = c_t^B$ for $t_{k-1} < t_k$ and $c_{t_K}^D = c_\tau^A + \sum_{t=1}^{t_1} c_t^A - c_t^B$, and by construction $c^D|_{t_k} R^V c^B|_{t_k}$, for all k. Then let $c_t^D = c^D|_{t_{\bar{k}}}$, for $t < t_{\bar{k}}$ where $\bar{k}$ is the maximal k such that $c_{t_k}^A < c_{t_k}^B$, and $c_t^D = c_t^A$ for $t > t_{\bar{K}}$. By construction $c^D R^V c^B$ and $c^D = c^A + \varepsilon$ where $\sum_\tau \varepsilon_\tau \leq 0 \forall \tau$.

[6]This definition is borrowed from Chambers and Echenique (2018), who use it to study when heterogeneity among individual discount factors may lead to agreement over social choices.

[7]This is merely a reinterpretation of the definition of discounting dominance, where the streams are rearranged to have positive income in the first period only and this consumption level is the present value of the stream with discount factor δ.

per period that results in a vector dominant stream regardless of the value of δ.

Proposition 2.3. *The indirect utility for an agent facing setup 3 in section 2.2 is always weakly higher from stream I^A than from stream I^B whenever $I^A R^D I^B$.*

While these rankings[8] capture many ways in which one stream can be better than another, they all describe the totality of the streams without describing what happens at various points in time. It may be reasonable to instead care about which stream is better *at some point in time* rather than in total. We propose the following notion of resource measurement:

Definition 2.4. *The cumulative income of stream I at time t is $w_t^I = \sum_{\tau=1}^{t} I_\tau$. We say that stream I^A is richer or wealthier than stream I^B at time t if $w_t^{I^A} \geq w_t^{I^B}$.*

Note that if one stream is cumulatively dominant over another, the cumulative consumption of the dominant stream is higher than that of the dominated stream at each period. In general, however, the rankings of cumulative income can change over time. This definition allows us to compare any two streams period-by-period without requiring a relationship across the whole stream.

2.2.4 Inequality Aversion

Proposed dominance notions serve to identify who is richer and who is poorer in an intertemporal setting. We use these to predict how a planner who dislikes inequality might rank pairs of streams. We construct generalized variants of the Pigou-Dalton principle[9], which utilize the different stream dominance rankings discussed above. We have:

[8] Note that these dominance rankings are nested: vector dominance implies cumulative dominance, and cumulative dominance implies discounting dominance.

[9] The standard (single period single good) Pigou-Dalton principle says that if $I^A > I^A - \varepsilon > I^B + \varepsilon > I^B$, then $(I^A - \varepsilon, I^B + \varepsilon) \succ (I^A, I^B)$. All of our results reduce to the standard Pigou-Dalton principle when $T = 1$.

Definition 2.5. *The generalized Pigou-Dalton principle with stream dominance ranking R: given* (I^A, I^B) *and* $(I^A - \varepsilon, I^B + \varepsilon)$*, for all* $\varepsilon \in \mathbb{R}^T$ *such*[10] *that* $I^A R(I^A - \varepsilon) R(I^B + \varepsilon) R I^B$ *then* $(I^A - \varepsilon, I^B + \varepsilon) \succeq (I^A, I^B)$.

That is, a transfer from the dominant to the dominated stream results in an allocation the planner prefers (provided that the transfer is sufficiently small to preserve the dominance ranking between the new streams). Proposition 2.2 states that such transfers can always be found.

Proposition 2.4. *For* $R = R^V, R^C, R^D$*, if* $I^A R I^B$ *and* $I^A \neq I^B$*, there exists* $\varepsilon \neq 0$ *such that* $I^A R(I^A - \varepsilon) R(I^B + \varepsilon) R I^B$.

Proof. Let $\varepsilon = \frac{1}{4}(c^A - c^B)$. We will show that $c^A R c^A - \varepsilon = \frac{3c^A + c^B}{4} R \frac{2c^A + 2c^B}{4} R c^B + \varepsilon = \frac{c^A + 3c^B}{4} R c^B$ for $R = R^V, R = R^C$, and $R = R^D$.

For $c^A R^V c^B$ we have $c_t^A \geq c_t^B$, which implies that $c_t^A \geq \frac{3c_t^A + c_t^B}{4} \geq \frac{2c_t^A + 2c_t^B}{4} \geq \frac{c_t^A + 3c_t^B}{4} \geq c_t^B$. For $c^A R^C c^B$ we have $\sum_{\tau=1}^t c_t^A \geq \sum_{\tau=1}^t c_t^B$ for all t, which implies that $\sum_{\tau=1}^t c_t^A \geq \frac{3\sum_{\tau=1}^t c_t^A + \sum_{\tau=1}^t c_t^B}{4} \geq \frac{2\sum_{\tau=1}^t c_t^A + 2\sum_{\tau=1}^t c_t^B}{4} \geq \frac{\sum_{\tau=1}^t c_t^A + 3\sum_{\tau=1}^t c_t^B}{4} \geq \sum_{\tau=1}^t c_t^B$ for all t. For $c^A R^D c^B$ we have $\sum_{t=1}^T \delta^T c_t^A \geq \sum_{t=1}^T \delta^t c_t^B$ for all $\delta \in (0,1)$, which implies that $\sum_{t=1}^T \delta^T c_t^A \geq \frac{3\sum_{t=1}^T \delta^T c_t^A + \sum_{t=1}^T \delta^T c_t^B}{4} \geq$
$$\geq \frac{2\sum_{t=1}^T \delta^T c_t^A + 2\sum_{t=1}^T \delta^T c_t^B}{4} \geq \frac{1\sum_{t=1}^T \delta^T c_t^A + 3\sum_{t=1}^T \delta^T c_t^B}{4} \geq \sum_{t=1}^T c_t^B \text{ for all } \delta \in (0,1).$$ □

We then provide a definition of the generalized Pigou-Dalton principle and an analog of proposition 2.4 for period-wise inequality rankings.

Definition 2.6. *The generalized Pigou-Dalton principle for period-wise inequality:* $\forall I^A, I^B, \varepsilon, \tau$ *such that* $w_\tau^{I^A} \geq w_\tau^{I^A - \varepsilon} \geq w_\tau^{I^B + \varepsilon} \geq w_\tau^{I^B}$*, or* $w_\tau^{I^B} \geq w_\tau^{I^B + \varepsilon} \geq w_\tau^{I^A - \varepsilon} \geq w_\tau^{I^A}$ *we have* $(I^A - \varepsilon, I^B + \varepsilon) \succeq (I^A, I^B)$

That is, a transfer that preserves who is richer at each period but (weakly) decreases the difference in cumulative income in each period generates a preferred allocation. Such transfers are always guaranteed to exist for any pair of (different) streams.

[10] The components of ε can be positive or negative; the only restriction on the transfer stream is that $I^A R(I^A - \varepsilon) R(I^B + \varepsilon) R I^B$ then $(I^A - \varepsilon, I^B + \varepsilon) \succeq (I^A, I^B)$.

Proposition 2.5. *For any streams I^A and I^B where $I^A \neq I^B$, there exists an* ε *such that a planner satisfying the generalized Pigou-Dalton principle for period-wise inequality prefers* $(I^A - \varepsilon, I^B + \varepsilon)$ *to* (I^A, I^B).

Proof. Let τ be the minimum of $t \geq 0$ such that c^A is richer than c^B at period τ. If no such τ exists, switch the roles of A and B. Let τ' be the minimum of $t > \tau$ such that B is weakly richer than A in period τ'. If $c^A R^C c^B$, instead define $\tau' = \tau + 1$. Let $\varepsilon_\tau = \frac{1}{4} \cdot \min_{\tau \leq t < \tau'} w_t^{c^A} - w_t^{c^B}$, $\varepsilon_{\tau'} = -\varepsilon_\tau$, and $\varepsilon_t = 0$ otherwise. By construction, $c^A - \varepsilon$ is still richer than $c^B + \varepsilon$ at $\tau \leq t < \tau'$ and $c^B + \varepsilon$ is still richer than $c^A - \varepsilon$ at τ', but the difference in wealth is smaller for $\tau \leq t < \tau'$ and unchanged for $t \geq \tau'$ and $t < \tau$, so a planner who follows generalized Pigou-Dalton principle for period-wise inequality prefers $(c^A - \varepsilon, c^B + \varepsilon)$ to (c^A, c^B). □

Both generalized Pigou-Dalton principles are consistent with the idea that moving income from richer individuals to poorer individuals reduces inequality, and that lower inequality is preferred to higher inequality. These versions differ only by how they determine who is richer and who is poorer. Both principles also agree that providing each agent with the same stream minimizes the level of inequality and yields the most preferred outcome.

Proposition 2.6. *If a planner satisfies either the generalized Pigou-Dalton principle for whole-stream or for period-wise inequality, there does not exist* $\varepsilon \neq 0$ *such that* $(I - \varepsilon, I + \varepsilon) \succeq (I, I)$.

Proof. For whole-stream inequality measures, if $cRc - \varepsilon Rc + \varepsilon Rc$, then $\varepsilon = 0$. Since all of the stream dominance relations we discussed are partial orders, they are transitive, and therefore $cRc - \varepsilon Rc + \varepsilon Rc$ implies $c + \varepsilon Rc - \varepsilon$ and therefore that $c - \varepsilon = c + \varepsilon$, so $\varepsilon = 0$

For period-wise inequality, if $(c^A, c^B) = (c, c)$, then $w_t^{c^A} = w_t^{c^B}$ for all t. Therefore if ε satisfies $w_\tau^{c^A} \geq w_\tau^{c^A - \varepsilon} \geq w_\tau^{c^B + \varepsilon} \geq w_\tau^{c^B}$, or $w_\tau^{c^B} \geq w_\tau^{c^B + \varepsilon} \geq w_\tau^{c^A - \varepsilon} \geq w_\tau^{c^A}$ for all τ then $\varepsilon = 0$ □

Transfers that are preferred by planners satisfying one generalized Pigou-Dalton principle may not always be preferred by planners satisfying the other. That is, there are pairs of streams and transfers from a discounting dominant stream to a discounting dominated stream that preserve the dominance ranking, but also weakly increase the difference between cumulative consumption in all periods. Consider the following example from section 1 between $I_a = (6,1,6)$ and $I_b = (1,11,1)$, and $\hat{I}_a = (6,3,4)$ and $\hat{I}_b = (1,9,3)$. Note that $\hat{I}_A R^D I_A R^D I_B R^D \hat{I}_B$, so a planner who is whole-stream inequality averse with discounting dominance would prefer (I_A, I_B) to $(\hat{I}_A, \hat{I}_b)$. However, note that the cumulative consumption vectors of the streams are $w_{I_A} = (6,7,13)$, $w_{I_B} = (1,12,13)$, $w_{\hat{I}_A} = (6,9,13)$ and $w_{\hat{I}_B} = (1,10,13)$. The pair $(\hat{I}_A, \hat{I}_b)$ has a smaller difference in cumulative consumption between Ann and Bob in period 2 than (I_A, I_B) does, with identical differences in cumulative consumption in the other periods. A planner who is period-wise inequality averse would therefore prefer $(\hat{I}_A, \hat{I}_b)$ to (I_A, I_B).

This example demonstrates that it is reasonable for people to be inequality-averse but disagree over the inequality effects of a particular policy, due differences in type of inequality considered. This feature is unique to the intertemporal setting, as neither the single period nor the general multidimensional setting admits multiple sensible ways of discussing inequality reduction. This then leads us to an empirical exercise to determine which, if any, of our notions of intertemporal inequality aversion describe empirical behavior.

2.3 Experimental Design

There are two goals of our laboratory experiments. The first is investigating if participants find our intertemporal inequality definitions sufficiently descriptive. The second purpose is attempting to disentangle which, if any, stream rankings participants find more important.

We recruited 118 UCSD students using the UCSD Economics Laboratory subject pool. Participants make two sets of decisions[11], one set of choices as social planners for pairs of other participants, and a second set of decisions only for themselves. All choices were binary over pairs

[11] The experiment is conducted using the oTree platform created by Chen et al. (2016)

of streams for the relevant agents. When choosing for others, the pair of participants remained the same across all decisions but the identity of these other participants is unknown to the planner. They were simply referred to as player 1 and player 2, with the planners do knowing which participant is assigned to which role. Figure 2.1 presents an example of the choices displayed to participants.

These social planner choices are designed to measure consistency with our various generalized Pigou-Dalton principles. The pairs of streams in many of these choices are related an inequality-reducing transfer (according to one of our generalized Pigou-Dalton principles). Eleven choices have a transfer that reduces both whole-stream (2 for vector dominance, 5 for cumulative dominance, and 4 for discounting dominance) and period-wise inequality. Another 5 choices involve a transfer that reduces period-wise inequality, but is unranked according to whole-stream inequality for any ranking above. An additional 5 choices involve a transfer that increases period-wise inequality while reducing stream-wise inequality under discounting dominance. The final 3 choices pay a positive amount in exactly one period, and this period is the same for each person and stream. This replicates the conditions of a static choice environment while keeping the decision interface the same as the other choices. The remaining 10 choices are what we call *perturbation choices*, which we construct by introducing a small inefficiency in the transfer (i.e. a transfer of $2 from player 1 to player 2 would become a transfer of $2 from player 1 and a transfer of $1.90 to player 2) in an existing choice. These perturbation choices allow us to determine if participants dislike inequality enough to trade (another player's) money to reduce inequality.

In addition to being social planners for other participants, we also asked participants to make several binary decisions about payment streams for themselves. These choices are designed to measure if the participants find that particular dominance notions adequately describe which streams are better. As such, the streams in these choices do not involve any transfers; instead, they simply present one dominant stream and one dominated stream according to either vector dominance, cumulative dominance, or discounting dominance.

The choices participants make for themselves and the single period choices both help us determine why participants' behavior might deviate from the predictions of model. Participants who choose dominant streams for themselves but make fail to select inequality-reducing options for others may not be inequality averse in general. Similarly, participants who choose the inequality-averse option in the single-period setting but fail to select dominant streams for themselves may not believe these rankings adequately rank streams.

2.4 Results

2.4.1 Choosing for oneself

We begin our analysis with the second part of the experiment, where subjects were choosing between different payment streams for themselves.

Descriptive Statistics

Table 2.1 describes the percentage of participants or percentage of choices where participants selected the dominant stream for themselves.

The results indicate that these dominance notions are indeed different in practice. Participants selected the vector dominant option essentially every time it was available, but selected the discounting dominant option only slightly[12] more frequently than half of the time.

We next build upon the results of table 2.1 using a linear probability model (LPM) and a logit model. We present the result in table 2.2. The regressions contain indicator variables for each dominance type, utilize both fixed effects and standard error clustering at the participant level, and can also disaggregate the effects of cumulative dominance by the sum of the streams.

Result 2.1 *Participants choose dominant options more often when they are vector dominant than cumulatively dominant, and more often when they are cumulatively dominant than discounting*

[12]The p-value of the z test against the null hypothesis of choosing this option exactly half of the time is 0.0702.

dominant. However, participants choose the cumulatively dominant option far more often when the total payment of the streams is different than when it is the same.

The first half of this result is not particularly surprising. The second part, however, indicates that cumulative dominance does not adequately capture how participants view streams. This result suggests that participants evaluated streams using a heuristic based on total payment received rather than a consideration of the distribution of payments across time. Additionally, this result suggests that participants can somewhat distinguish between the different types of streams. This indicates that a failure to choose streams with lower intertemporal inequality for others does not stem from a failure to identify which streams are better than others.

2.4.2 Choosing for others

We now move to the main section of the experiment, where the participants acted as social planners for others. This is a variant of a dictator game, where the dictator's monetary payoff is not determined by the dictators actions. Participants with no concerns for inequality are indifferent between the non-perturbation choices they face in this portion of the experiment, so they should attempt to complete this portion of the experiment as soon as possible. We find that behavior in this portion of the experiment is *not* affected by completion time. We also would like to examine behavior in unperturbed-perturbed pairs of choices to search for nondeliberate (random) choice. Figures 2.4 and 2.5 show an example of an unperturbed-perturbed pair of choices. Of our 118 participants, 22% make inequality averse choices more often in the perturbed choices than they do in the unperturbed variants. For example, these participants would pick option 2 in the choice depicted by figured 2.4 and switch to option 1 in the choice from figured 2.5. These participants make statistically similar choices to the other participants when choosing for themselves, but behave very differently when acting as social planners—where their choices are statistically indistinguishable from choosing uniformly at random. Our analysis below will include both the whole sample as well as the subsample of participants that excludes those identified as choosing uniformly at random. (henceforth referred to simply as 'subsample').

We first focus on the set of decisions in which inequality notions agreed. This means that each choice had a period-wise inequality averse option, while some choices had a discounting dominant, cumulatively dominant, or vector dominant whole-stream inequality averse option (when a choice had an inequality averse option, this option was the same across each type of inequality aversion). Recall that our dominance notions are nested—when a stream vector dominates another, it also cumulatively dominates it. When a stream cumulatively dominates another, it also discounting dominates it. Since our participants seemed to understand this nesting for their own outcomes, the natural prediction is that we should see declining inequality aversion as we move from static to period-wise to discounting dominant to cumulatively dominant to vector dominant. As table 2.3 demonstrates, we do not observe this monotonicty in the data.

While the type of dominance ranking reasonably predicted how participants would choose for themselves, it does not adequately predict how they will choose for others. This is confirmed by the results of table 2.4, which presents several specifications on both the full sample and subsample of participants who are choosing deliberately.

Result 2.2 *Participants select the inequality-averse option in the static settings and discounting dominant settings with the same frequency. They selected the inequality-averse option in vector dominant, cumulatively dominant, and period-wise settings with the same frequency as each other, but lower frequency than in static or discounting dominant settings.*

The previous analysis considered only cases where the lower inequality choice under discounting dominance was also the lower inequality choice under period-wise dominance. We also have choices for which the one option was lower inequality under discounting dominance but higher under period-wise dominance, and choices where one option has lower inequality under discounting dominance but the options are not ranked according to period-wise dominance. Regression results for these latter cases are presented in figures 2.6 and 2.7.

Result 2.3 *Participants choose the discounting dominant option more when it is also period-wise* *dominant than when it is just discounting dominant. They choose the discounting dominant option less when the other option is period-wise dominant.*

This result is puzzling. Participants choose the inequality averse option under discounting dominance the most frequently out of any dynamic setting, but will not choose it when it is not also period-wise dominant. Furthermore, participants appear to appreciate lower period-wise inequality when it does not coincide with lower inequality under discounting dominance than when it does.

2.5 Conclusion

The central theme of this chapter revolves around the complex nature of measuring inequality, especially when viewed through the lens of time. Our primary contribution lies in extending the defining principle of inequality changes to an intertemporal environment. The main difficulty for the generalization lies in the way future payoffs could be treated: we propose three ways that differ by the access to different saving/borrowing technologies.

One of the insights from our study is the prevalence of period-wise inequality aversion over stream-wise inequality aversion. This highlights a tendency that the subjects think of inequality on a period-by-period basis, rather than by aggregation of all incoming payments. This emphasis on period-wise inequality aversion suggests a more nuanced understanding of intertemporal income inequality, which has substantial implications for policy design and public perception. Our findings reveal that people have opinions and biases about policies even when they're not directly affected. This observation is crucial for policymakers who need to understand and anticipate public reactions to their decisions. One of the potential explanations for our findings on discounted-dominating streams could be the lack of use of borrowing/lending by students, who constitute the typical subject pool in many economic experiments.

Furthermore, our findings present a challenge to the conventional application of social

Exponential Discounted Utility (EDU) in intertemporal decision-making. The preference patterns observed in our experiments suggest that the standard EDU model might not accurately capture how individuals value future payments, particularly when it comes to inequality aversion. This discrepancy opens new avenues for resolving the 'impossibility result' noted by Zuber (2011) and Jackson and Yariv (2015). Moreover, our laboratory experiments have raised several intriguing questions, particularly concerning the distinct behavioral patterns among different groups. Understanding how these groups differ in their perception and valuation of intertemporal transfers remains an open question, requiring further investigation.

2.6 Acknowledgements

Chapter 2 is currently being prepared for submission for publication of the material under the title "Attitudes Towards Intertemporal Inequality". Wolanski, Adrian; Baranov, Evgenii. The dissertation author, Adrian Wolanski, and Evgenii Baranov are the principal coauthors of this chapter.

2.7 Figures and Tables

Choices: Part 1

	Player 1's payment in two weeks	Player 1's payment in four weeks	Player 1's payment in six weeks		Player 2's payment in two weeks	Player 2's payment in four weeks	Player 2's payment in six weeks
Option 1	$6	$3	$4		$1	$9	$3
Option 2	$6	$1	$6		$1	$11	$1

Please select which set of payment schedules you would rather be given to the other players, option 1 or option 2.

○ 1
○ 2

This is choice 1 out of 34 for part 1.

Next

Figure 2.1. Decision Interface

This figure demonstrates an example of the decision interface.

	Your payment in two weeks	Your payment in four weeks	Your payment in six weeks
Option 1	$4	$8	$2
Option 2	$7	$2	$5

Figure 2.2. Decision Interface—Discounting Dominance

This figure demonstrates an example of the decision interface, showing a choice participants would make only affected them personally. Note that option 2 is discounting dominant over option 1 but is not cumulatively dominant over option 1.

	Your payment in two weeks	Your payment in four weeks	Your payment in six weeks
Option 1	$3	$7	$3
Option 2	$5	$3	$5

Figure 2.3. Decision Interface—Discounting Dominance Again

This figure demonstrates an example of the decision interface, showing a choice participants would make which only affected them personally. Note that option 2 is discounting dominant but not cumulatively dominant over option 1.

	Player 1's payment in two weeks	Player 1's payment in four weeks	Player 1's payment in six weeks		Player 2's payment in two weeks	Player 2's payment in four weeks	Player 2's payment in six weeks
Option 1	$8	$3	$1		$6	$4	$2
Option 2	$12	$3	$1		$2	$4	$2

Figure 2.4. Decision Interface—Unperturbed Choice

This figure demonstrates an example of the decision interface, showing a choice before it is perturbed. Note that option 1 has (weakly) lower inequality according to all measures.

	Player 1's payment in two weeks	Player 1's payment in four weeks	Player 1's payment in six weeks		Player 2's payment in two weeks	Player 2's payment in four weeks	Player 2's payment in six weeks
Option 1	$8	$3	$1		$5.9	$4	$2
Option 2	$12	$3	$1		$2	$4	$2

Figure 2.5. Decision Interface—Perturbed Choice

This figure demonstrates an example of the decision interface, showing a choice after it is perturbed. Note that option 1 has (weakly) lower inequality according to all measures.

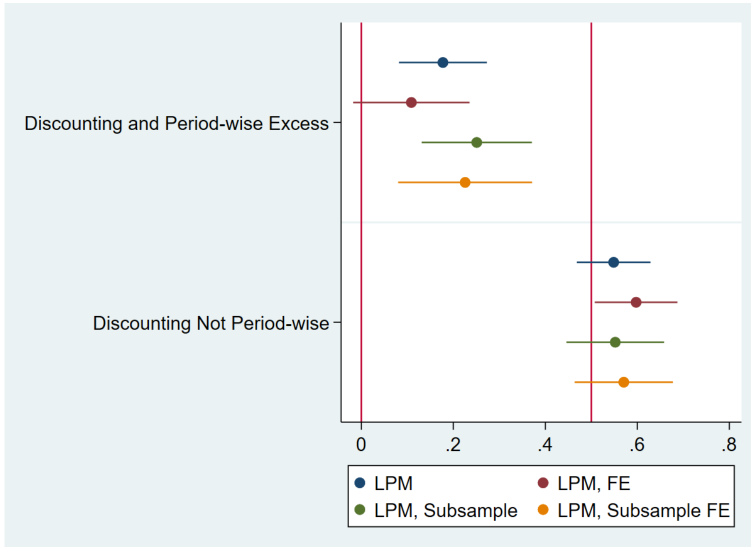


Figure 2.6. Coefficient Plot—Discounting Not Period

This figure displays regression results of the difference in likelihood of choosing the inequality averse option under discounting and period-wise dominance versus just discounting dominance. Regressions are computed with individual-level fixed-effects where indicated.

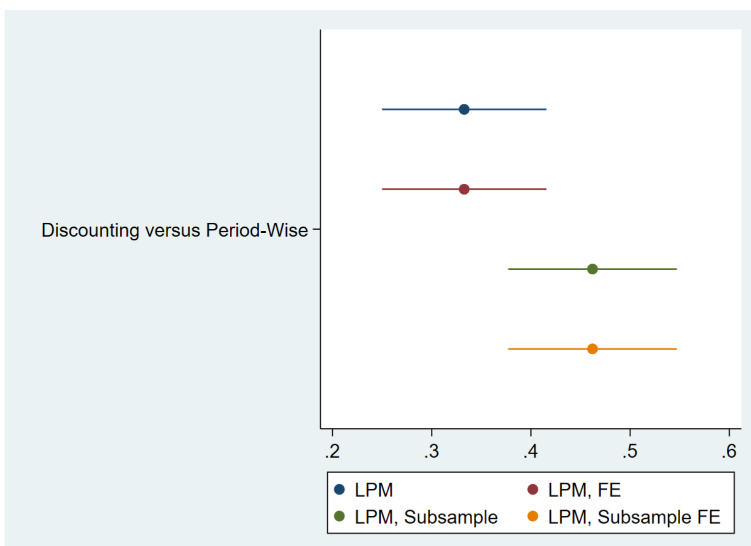


Figure 2.7. Coefficient Plot—Discounting Versus Period

This figure displays regression results of the difference in likelihood of choosing the lower inequality option under discounting dominance when the other option has lower inequality under period-wise dominance. Regressions are computed with individual-level fixed-effects where indicated.

Table 2.1. Choices for Oneself—Descriptive Statistics

	vector	cumulative	discounting
proportion of dominant choices	0.9870	0.8125	0.5410
proportion of participants making the dominant choice every time	0.9830	0.4661	0.2203
proportion of participants making the dominant choice at least half of the time	1	0.9915	0.7373

This table displays the proportion of dominant choices participants selected for themselves.

Table 2.2. Choices for Oneself—Regression Results

	LPM	LPM	LPM	LPM	Logit	Logit	Logit	Logit
cumulative, aggregate	-.1745 (.0158)***		-.1696 (.0157)***		-2.8643 (.6679)***		-2.8330 (.6687)***	
discounting, aggregate	-.4460 (.0275)***	-.4460 (.0275)***	-.4289 (.0282)***	-.4340 (.0282)***	-4.1664 (.7333)***	-4.1664 (.7333)***	-4.0575 (.7352)***	-4.0763 (.7382)***
cumulative, different total payment		-.0201 (.0167)**		-.0244 (.0111)**		-.9558 (.5815)*		-.9872 (.5809)*
cumulative, same total payment		-.2755 (.0224)***		-.2670 (.0226)***		-3.4279 (.6840)***		-3.3653 (.6884)***
constant (vector dominance)	.9870 (.0091)	.9870 (.0091)	.9782 (.0131)	.9808	4.3307 (.7075)	4.3307 (.7075)		-4.0763 (.7382)
Log Likelihood					-679.3026	-636.7355	-490.1338	-447.775
(pseudo) R^2	.1240	.1652	.1240	.1652	.1196	.1748	.1452	.1452
N	1,330	1,330	1,330	1,330	1,330	1,330	1,246	1,246
# clusters	118	118	118	118	118	118	106	106
Participant FE	No	No	Yes	Yes	No	No	Yes	Yes

This table displays regression results of the change in likelihood of choosing the dominant option when it is cumulatively dominant or discounting dominant (note that the baseline is vector dominant). Standard errors included in parenthesis and are clustered at the participant level. * is significance at 90%, ** is significance at 95%, and *** is significance at 99%.

Table 2.3. Choices for Others—Descriptive Statistics

dominance type	all sample	subsample
single-period	.7566 (.0198)	.8324 (.0199)***
vector dominance	.6985 (.0278)	.71 (.0321)
cumulative dominance	.6898 (.0190)	.7682 (.0201)***
discounting dominance	.7260 (.0237)	.8030 (.0245)***
only period-wise dominance	.6674 (.0217)	.7443 (.0233)***

This table displays the aggregate proportion of inequality averse choices across decision environments. Stars represent significant differences in proportions between the general sample and subsample, * at the 90% level, ** at the 95% level, and *** at the 99% level

Table 2.4. Choices for Others—Regression Results

	LPM	LPM	LPM	LPM	Logit	Logit	Logit	Logit
Vector	-.0578 (.0363)	-.0442 (.0362)	-.1224 (.0347)***	-.1126 (.0352)***	-.2925 (.1837)	-.2745 (.2194)	-.7073 (.1948)***	-.7390 (.2239)***
Cumulative	-.0665 (.0259)**	-.0665 (.0259) **	-.0642 (.0264)**	-.0642 (.0264)**	-.3335 (.1347)**	-.4043 (.1628)**	-.4046 (.1748)**	-.4561 (.1947)
Discounting	-.0303 (.0308)	-.0304 (.0308)	-.0294 (.0305)	-.0294 (.0305)	-.1584 (.1610)	-.1909 (.1944)	-.1973(.2042)	-.2208 (.2295)
Period-wise	-.0890 (.0318)***	-.0890 (.0318)***	-.0881 (.0316)***	-.0881 (.0316)***	-.4364 (.1592)***	-.5309 (.1933)***	-.5341 (.1954)***	-.6051 (.2197)
Constant (static)	.7564 (.0283)	.7546 (.0187)	.8324 (.0252)	.8311 (.0175)	1.1328 (.1536)		1.6026 (.1802)	
Log Likelihood					1,301.9919	-875.8778	-848.9060	-587.8545
(pseudo) R^2	.0049	.0048	.0092	.0091	.0041	.0074	.0087	.0128
N	2,160	2,160	1,608	1,608	2,160	2,160	1,608	1,410
# clusters	118	118	88	88	118	118	88	77
Subsample only	No	No	Yes	Yes	No	No	Yes	Yes
Participant FE	No	Yes	No	Yes	No	Yes	No	Yes

This table displays ultiple specifications comparing the proportion of inequality averse decisions made to proportion in single-period settings. Individual fixed effects (FE) are included where indicated, all standard are clustered at the individual level for logit specifications. * is significant at 90%, ** is significant at 95%, *** is significant at 99%.

2.8 Appendix—Participant Documentation

2.8.1 Consent Form

Please read the following consent form carefully.

Who is conducting the study, why you have been asked to participate, how you were selected, and what is the approximate number of participants in the study?

Adrian Wolanski and Evgenii Baranov are conducting a research study to find out more about collective intertemporal choice. You have been asked to participate in this study because you are a UCSD student in the Economics Laboratory subject pool. There will be approximately 20 participants in this session.

Why is this study being done?

The purpose of this study is to understand how individuals make decisions about income received by other people over time.

What will happen to you in this study and which procedures are standard of care and which are experimental?

If you agree to participate in this study, you will be randomly matched with two other participants in the room. You will then choose between pairs of payment schedules for those two participants to be paid. One of your choices will be randomly selected to become the decision-that-counts and paid to those participants. Each participant in the study will be choosing payment schedules for others, as well as be paid a payment schedule chosen this way. Finally, you will choose between a series of payment schedules for yourself. One of these choices will be randomly selected to become the decision-that-counts for yourself and paid accordingly. Overall, your payments will depend on random selection, the choices of others, and your own choices.

How much time will each study procedure take, what is your total time commitment, and how long will the study last?

The study will take approximately 1 hour.

What risks are associated with this study?

Participation in this study may involve some added risks or discomforts. These include the following:

Potential loss of confidentiality. Your choices will never be linked to your name. Each one of you has been given a participant number. This participant number will be used to register your choices and provide your payment. However, there does exist a record of your participation today. Research records will be kept confidential to the extent allowed by law. Research records may be reviewed by the UCSD Institutional Review Board. Because this is a research study, there may also be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings.

What are the alternatives to participating in this study?

The alternative to participation in this study is to exit the study.

What benefits can be reasonably expected?

There may or may not be any direct benefit to you from participating in this study. Direct benefits will be monetary and will depend on your choices and choices of others. Payments will typically range from $20 to $40. The investigators may learn more about collective dynamic choice, and society may benefit from this research and knowledge.

Can you choose to not participate or withdraw from the study without penalty or loss of benefits?

Participation in this study is entirely voluntary. You may refuse to participate or withdraw or refuse to answer specific questions in an interview or on a questionnaire at any time without penalty or loss of benefits to which you are entitled. If you decide that you no longer wish to continue in this study, you will be required to contact the research staff immediately to request to leave the study. You will be told if any important new information is found during this study that may affect your desire to continue.

Can you be withdrawn from the study without your consent?

The PI may remove you from the study without your consent if the PI feels that to do so is in your best interest or the best interest of the study. You may also be withdrawn from the study if you do not follow the instructions given to you by the study personnel.

Will you be compensated for participating in this study?

In compensation for your time, you will be paid based on the decisions in the decision-that-counts from others as well as your decisions in the decision-that-counts for yourself, as well as a show-up fee.

Are there any costs associated with participating in this study?

There will be no cost to you for participating in this study.

Who can you call if you have questions?

Adrian Wolanski and Evgenii Baranov have explained this study and answered possible questions. If you have other questions or research-related issues, you may reach Mr. Wolanski at 317-670-3278, or by email at awolansk@ucsd.edu. You may call the Human Research Protections Program Office at (858) 657-5100 to inquire about your rights as a research subject or to report research-related issues.

Your consent and acknowledgement of consent.

By clicking next, you acknowledge that you have read the consent form and agree to participate.

2.8.2 Instructions for Part One

This study takes place in two parts. The first part of the study examines peoples' decisions about monetary payments received by other people. For this, we have designed a series of choices of payment schedules that will be received by other people.

You will be presented with a pair of payment schedules and asked which of the schedules you would rather be given to two other participants, known to you as 'player 1' and 'player 2'. These two other participants have just been chosen at random from the participants in the session, before any decisions have been made. While these choices are random, no one will be making decisions for a participant who is also making decisions for them. You will make decisions for the same pair of participants in each choice, but their roles as player 1 and player 2 will be reselected at random between choices, so either participant could be player 1 or player 2 for a given choice.

After you have made all of your decisions for part 1, one of your choices will be selected at random and become your decision-that-counts for part 1–there is a 50 percent chance that your decision-that-counts for part 1 will be paid to other participants (more on this below). You will then flip over the card in front of you; this card contains three pieces of information. First, each card has a dedicated space for you to write the decision-that-counts from part 1. Second, each card lists which other participants (identified by ID number) you made decisions for and which was labelled as 'player 1' and which was 'player 2' in the decision-that-counts. This means you will only learn which participants you made decisions for after you have made your decisions, and at no point will any participant learn who made decisions for them. Third, each of these cards has a number in the top right-hand corner, either 0 or 1. These numbers have been randomly assigned to the cards, but have been done in such a way that there is exactly one card with each participant ID number and each corner number.

You will record your decision-that-counts in the dedicated space on the card. Then, either 0 or 1 will be selected at random. The experimenters will then collect all of the cards and sort them into the selected group and the unselected group. The experimenters will then distribute the cards from the selected group to the participants listed on them, and the decision-that-counts listed on that card will be that participant's payment for part 1–this is done to ensure that each person knows how much they are to be paid and when they are to be paid. The decisions listed on the unselected group will not be paid to anyone.

The payment schedule from the decision-that-counts listed on a participant's card will be their payment from part 1. This means that there is a 50 percent chance that your decision-that-counts from part 1 will be paid to two other participants in this session and a 50 percent chance that your decision-that-counts from part 1 will not be paid to anyone. As such, it is in your best interest to truthfully state which option you would rather be distributed to the other participants in each choice, since that decision could be paid to other participants. Each participant will receive exactly one card, and therefore each of you will receive exactly one payment schedule from the decision-that-counts in part 1.

Your total payment for the study consists of three components: your payment from part 1 (described above), your payment from your part 2 (described later), and your show-up payment.

If you have questions at any point during the experiment, please raise your hand and one of the experimenters will come by and assist you. Once you have read and understood these instructions, please click next to proceed to an example to improve your understanding of the experiment.

2.8.3 Instructions for Part Two

This study takes place in two parts. The second part of this study examines peoples' decisions concerning payment schedules for themselves. For this, we have designed a series of choices over payment schedules that you can receive. You will be presented with a pair of schedules and asked which pair you would rather receive. One of these choices will be selected at random, and the decision you made for that choice will become your decision-that-counts for part 2. The payment schedule from your decision-that-counts will be paid to you.

Chapter 3

Dynamic Inconsistency and Convex Commitment Devices

3.1 Introduction

Many researchers studying dynamically inconsistent preferences have treated demand of costly commitment devices as smoking gun evidence of present-biased dynamic inconsistency (O'Donoghue and Rabin (1999)). Commitment restricts future choice sets, which makes it easier for the decision-maker to avoid undesirable behavior. Commitment demand, therefore, indicates that the decision-maker believes she will take undesirable future actions. For the same reason, commitment devices are considered the gold standard for mitigating welfare loss from dynamic inconsistency. However, the welfare and policy value of commitment devices depends not just upon the degree of dynamic inconsistency, but also upon the degree of *perceived* dynamic inconsistency and upon the interaction between actual and perceived dynamic inconsistency. Recent studies have shown conflicting evidence regarding how sophisticated people are about their dynamic inconsistency, ranging from full naivete to partial sophistication to overly pessimistic perceptions.

In this chapter, we present a novel experimental design where we offer individuals convex commitment contracts during the performance of real-effort tasks. Similar to Augenblick and Rabin (2019), we ask participants to choose how much effort to supply for various wages. For a given wage and session, they make a labor supply decision in a previous session and at the current session. For each decision made in a previous session, we ask with what probability (between 10% and 90%) they would like to commit to the early decision rather than the late one, with higher commitment being more costly. We can then estimate structural time preference parameters from labor supply decisions and participants' implied perceptions of these parameters from commitment decisions.

We recruited 42 [1] undergraduate students at UC San Diego and ran two sets of experimental sessions, each one consisting of four decision-making periods. Despite commitment demand being costly, we observe many participants committing strongly and often. The labor supply decisions, in contrast, seem to be relatively time-consistent. This indicates that many of our participants were overly pessimistic about their future labor supply decisions. We quantify this by showing that both the ex-ante and ex-post welfare effects of commitment usage are negative and increasing in magnitude with commitment level, with much of this effect coming from the explicit cost of commitment.

In order to support these reduced-form results, we formulate a structural model of decision-making in our experiment consisting of the standard quasi-hyperbolic discounting model (O'Donoghue and Rabin (1999)) and a convex effort cost function (as in Augenblick and Rabin (2019)). We use the structural model to obtain parameter estimates of the present bias, exponential discount factor, disutility of labor, and the implied perception of their present bias. We find that our estimate of the perception of present bias is significantly lower than the estimate of the present bias itself, implying that the participants on average significantly overestimate the degree of their present bias.

[1]While our goal was to recruit between 100 and 150 participants, we were delayed by the costs incurred to conduct the experiment.

The results of our study have implications for future experimental design and welfare analysis. Our use of a convex commitment device appears to elicit commitment demand more precisely than binary devices, and allows our use of a structural model to infer perceptions of dynamic inconsistency and compare them against actual inconsistency displayed by the participants. The use of a convex, rather than binary, commitment device may also help alleviate a possible experimenter demand effect. Welfare implications of our analysis are straightforward: when people are overly pessimistic about their dynamic inconsistency, they demand too much commitment and lose out on potential earnings. Future studies can extend this analysis to other fields where dynamic inconsistency and commitment demand have been observed.

Empirical investigations of dynamic inconsistency have largely focused on measuring the degree of dynamic inconsistency, and have often found evidence relating dynamic inconsistency and demand[2] for costly commitment devices. There has been relatively little work, however, on measuring perceptions of dynamic inconsistency and the few projects attempting to do so yield conclusions contradicting each other and the previous body of literature.

Augenblick et al. (2015) present a test of dynamic inconsistency in real-effort tasks and investigate the demand of a binding commitment device in an experimental setting. The authors find evidence of dynamic inconsistency in the real-effort tasks, and find that dynamic inconsistency in effort tasks predicts demand for the binding commitment device—indicating that dynamically inconsistent participants are at least somewhat aware of their inconsistency. Augenblick and Rabin (2019) present another test of dynamic inconsistency in task performance, combining the real-effort tasks with incentivized belief elicitation to estimate perceived dynamic inconsistency. Somewhat surprisingly, the data indicate little to no aggregate awareness of dynamic inconsistency despite significant presence of dynamic inconsistency. In contrast to Augenblick and Rabin (2019), Carrera et al. (2019) find substantial evidence of partial (but not full) awareness of dynamic inconsistency. In a field experiment on gym attendance, the authors offer participants both

[2]See Laibson (2015) for a discussion of this relationship in more detail.

commitment contracts and anticommitment contracts, documenting demand for both types. The results suggest caution when interpreting commitment demand as reduced-form evidence of awareness of dynamic inconsistency, with the authors warning that experimenter demand effects and noisy valuations could also be significant drivers of commitment demand rather than a desire to change future behavior. Other recent studies involving commitment demand paint contrasting pictures of how dynamic inconsistency relates to commitment demand. Sadoff et al. (2020) present a field test of dynamic inconsistency in food choice, finding both a substantial degree of dynamic inconsistency and a substantial demand of commitment. Notably, however, the participants demanding commitment were less likely to exhibit actual dynamic inconsistency. Toussaert (2018) attempts to distinguish between commitment demand caused by awareness of dynamic inconsistency, such as in O'Donoghue and Rabin (2001), and commitment demand caused by the presence of temptation and self-control costs, such as in Gul and Pesendorfer (2003). Toussaert (2018) classifies a substantial number of experimental participants as demanding commitment because of temptation costs, while finding few subjects' behavior consistent with awareness of dynamic inconsistency.

Our project addresses the literature in several ways. First, our use of a structural model of commitment demand, rather than a reduced-form approach, allows us to interpret how much of commitment demand is coming from a desire to change future behavior and how much is coming from noise. Second, our use of a convex design also means we are robust to experimenter demand effects on commitment choice—participants may feel the experimenter wants them to select the commitment option when offered only binary choices. While we offer more commitment options than under binary choice, using multiple commitment options obscures possible inference about experimenter demands. We believe that this, in conjunction with our use of a structural model, addresses several concerns about commitment demand raised by Carrera et al. (2019).

Another contribution of this project is to address the large disconnect between the results in Augenblick and Rabin (2019) and previous literature on commitment demand. While a substantial body of previous literature finds that participants frequently demand commitment, the results in Augenblick and Rabin (2019) suggest that people have little understanding of their dynamic inconsistency, and therefore should not demand commitment. Our study attempts to connect these results through the use of the convex commitment device, which allows us to measure perceptions of dynamic inconsistency directly from commitment demand (in contrast to the belief elicitation technique used in Augenblick and Rabin (2019)). In addition, the results of our measured relationship between actual and perceived dynamic inconsistency could have an impact in model selection and welfare analysis. Both Toussaert (2018) and Sadoff et al. (2020) find a negative relationship between the degree of dynamic inconsistency and commitment demand, but differ on their assessment of the welfare effects of commitment devices by attributing commitment demand to different factors. We also find a negative relationship between the actual and perceived dynamic inconsistency parameters, but our welfare analysis is more in line with the Sadoff et al. (2020) interpretation that commitment offerings should be carefully tailored to the individuals involved to avoid welfare losses.

The rest of the chapter is structured as follows. Section 3.2 explains our experimental design. Section 3.3 provides a theoretical model of quasi-hyperbolic discounting which we use for structural estimation. Section 3.4 provides both reduced form results and structural estimates of parameters. Section 3.5 provides a welfare analysis, and section 3.6 concludes the chapter.

3.2 Experimental Design

We recruited 42 undergraduate students from the UCSD Economics Laboratory. Each session was scheduled on a Monday morning at 10:30, to avoid time or day effects. In addition to their earnings from task completion, participants were paid a $5 sign up fee plus a $15 completion bonus, both upon exit to avoid income effects during the experiment. We focus on a sub-sample of 27 participants who attended all sessions and made all decisions.[3]

The experiment involves participants making labor supply decisions for piece-rate wages across time. Labor consists of transcribing strings of alphanumeric characters, with each correct string counting as one unit of labor. All participants will receive their payments only during the final session of the experiment but will have to supply labor on multiple sessions during the experiment.

The experiment consists of 4 sessions, each one week apart. In sessions 1-3, the participants made decisions about how much labor to supply (i.e. how many strings to translate) in the following session. Each participant was randomly shown 8 wages ranging between $0.01 and $0.31 per task performed, and asked to report how many tasks they would like to perform during the following session at each of these wages.

In sessions 1-3, after reporting their desired labor supply, the participants were also asked to report how likely they want this decision to become the decision-that-counts in the following session. For each wage-labor decision they have just made, they were asked to make a decision about the probability (between 10% and 90%) this decision becomes the decision-that-counts in the following session; the complementary probability is the likelihood that the decision they make in the following period session becomes the decision-that-counts in that period. The probability choices are costly[4], with a higher probability of committing to the current session's decision coming at a higher cost.

In sessions 2-4, the participants made decisions about how much labor to supply in the current session. They were shown the same 8 wages they were shown in the previous sessions, and asked how many tasks they would like to perform in the current session at each of those wages.

[3]4 participants failed to attend all sessions, and 11 attended all sessions but were unable to make all decisions due to a computer error. Including the data available from these participants does not change any structural estimates or conclusions, but it does make the non-parametric analysis unnecessarily difficult to interpret.

[4]The marginal cost of commitment increases by $0.10 for every 10 percentage point increase in commitment, i.e. the cost of committing at 10% (the lowest possible level) was 0, the cost of committing at 20% was $0.10, the cost of committing at 30% was $0.30, etc.

In sessions 2-4, after reporting their desired labor supply, we selected one of the wages uniformly at random for each participant faced. We then randomly selected between their labor supply decision made in the current session and their decision made in the previous session, according to the probability the participant reported in the previous session. This decision is the decision-that-counts, and the participant was asked[5]. to perform that many tasks in exchange for payment.

3.3 Model

Consider a decisionmaker with preferences over outcome streams $x = \{x_t\}_{t=1}^{T}$ given by the $(\beta, \hat{\beta}, \delta)$ preferences in O'Donoghue and Rabin (2001). In each period τ, the decisionmaker has preferences given by the utility function $U_\tau(x) = u(x_\tau) + \beta \sum_{t=\tau+1}^{T} \delta^t u(x_t)$. The decisionmaker, however, believes in period τ that in period $\tau + k$ she will have preferences $\hat{U}_{\tau+k}(x) = u(x_{\tau+k}) + \hat{\beta} \sum_{t=\tau+k+1}^{T} \delta^t u(x_t)$. The parameter β is the decisionmaker's degree of present bias, the parameter $\hat{\beta}$ is the agent's belief about their degree of present bias, and the parameter δ is the agent's long run impatience.

The decisionmaker will receive monetary payments in exchange for providing labor, so $x_t = (M_t, L_t)$. [6] We assume that her Bernoulli function $u(x_t)$ is quasilinear [7], so

$$u(x_t) = M_t - C(L_t). \tag{3.1}$$

We will assume that $C(0) = 0$, and that $C' > 0$ and $C'' > 0$. Following the timing above, at time τ the decisionmaker solves for how much labor she plans to supply during period $t > \tau$ for payment

[5] All decisions were made using the oTree platform created by Chen et al. (2016)

[6] Since the decisionmaker recieves a linear wage payment for labor provision, note that $M_t = w \cdot L$ for some L.

[7] We must assume that the Bernoulli function is additively separable, since our experiment hinges on the costs and rewards of labor supply being in different periods and that the global utility function is additive separable across time. We use a quasilinear specification for simplicity and for structural estimation, but any additively separable specification will do.

received in period T. This is given by

$$L^*_{\tau,t,\neq} = \text{argmax } \beta \cdot \delta^{T-t} \cdot L \cdot w - \beta \cdot \delta^{\tau-t} \cdot C(L). \tag{3.2}$$

During period $t = \tau$, the decisionmaker must decide how much labor to supply during the current period. This is given by [8]

$$L^*_{\tau,t,=} = \text{argmax } \beta \cdot \delta^{T-t} \cdot L \cdot w - C(L). \tag{3.3}$$

At time τ the decisionmaker believes at time $t > \tau$ the actual amount of labor that she will supply will *not* by given by $L^*_{\tau,t,\neq}$. This is because the decisionmaker believes her preferences at time t are given by $\hat{U}_t$, rather than U_t. Therefore, the decisionmaker believes her labor supply at t will be chosen to solve

$$L^*_{\tau,t,p} = \text{argmax } \hat{\beta} \cdot \delta^{T-t} \cdot L \cdot w - \delta^{\tau-t} C(L). \tag{3.4}$$

Note that (3.4) is similar to (3.3), but rewards are weighted by $\hat{\beta}$ rather than β; this is because the agent believes that their degree of present bias is $\hat{\beta}$ rather than β. The decisionmaker then selects a probability $p \in [\underline{p}, \overline{p}]$ of having $L^*_{\tau,t,\neq}$ implemented at time t, for which the decisionmaker has to pay a cost $X(p)$ [9] at time T, where $X(0) = X'(0) = 0, X'(p) > 0$, and $X''(p) > 0$. Assuming that preferences over p are given by an expected utility function, the optimal choice of

[8]Note that in equation (3.2), both cost and payouts are weighted by β because both effort costs and monetary rewards are experienced on a future date. In equation (3.3), monetary rewards are weighted by β since those are experienced on future date while costs are not weighted by β since those are experienced immediately.

[9]The entire model can be redefined using $\tilde{p} = 1 - p$ and making $-X(\tilde{p}) = X(p)$ a payment for not committing rather than a cost for committing. This is a treatment we are interested in conducting during future sessions.

p is given by

$$p^*_{\tau,t,\neq} = \text{argmax } p(\beta \cdot \delta^{T-t} \cdot w \cdot L^*_{\tau,t,\neq} - \beta \cdot \delta^{\tau-t} \cdot C(L^*_{\tau,t,\neq})) \\ + (1-p)(\beta \cdot \delta^{T-t} \cdot w \cdot L^*_{\tau,t,p} - \beta \cdot \delta^{\tau-t} \cdot C(L^*_{\tau,t,p})) - \beta \cdot \delta^{T-t} \cdot X(p). \quad (3.5)$$

We will now characterize $L^*_{\tau,t,\neq}, L^*_{\tau,t,=}, L^*_{\tau,t,p}$ and $p^*_{\tau,t}$. Under the assumptions on C, C' has an inverse function which we denote D. The solutions to (3.2)-(3.4) are then $L^*_{\tau,t,\neq} = D(\delta^{T-\tau} \cdot w)$, $L^*_{\tau,t,=} = D(\beta \cdot \delta^{T-\tau} \cdot w)$, and $L^*_{\tau,t,p} = D(\hat{\beta} \cdot \delta^{T-\tau} \cdot w)$. Taking the derivative with respect to p in equation 3.5 and substituting in the solutions to 3.2 and 3.4 produces

$$\beta \cdot \big(\delta^{T-t} \cdot w \cdot D(w \cdot \delta^{T-\tau}) - \delta^{\tau-t} C(D(w \cdot \delta^{T-\tau})) \\ - w \cdot \delta^{T-t} \cdot D(w \cdot \hat{\beta} \cdot \delta^{T-\tau}) + \delta^{\tau-t} C(D(w \cdot \hat{\beta} \cdot \delta^{T-\tau})) - \delta^{T-t} X'(p)\big) \quad (3.6)$$

If $\hat{\beta} = 1$, this reduces to $-X^{'}(p)$ which implies $p^* = \underline{p}$. This is consistent with the idea that people who believe they have no present bias (time consistent and fully naive decisionmakers) would not choose costly commitment devices. When $\hat{\beta} < 1$, (6) is decreasing in $\hat{\beta}$ so the agent would choose $p^* > \underline{p}$. For choices of $p^* < \overline{p}$, equation 3.6 is set equal to 0. Thus, given w and C, $\hat{\beta}$ then uniquely determines p^*.

3.4 Results

3.4.1 Reduced-form Results

Table 3.1 summarizes our main observations of commitment demand. Commitment demand is widespread: many people commit frequently and substantially. This stands in stark contrast to a plethora of laboratory studies (Augenblick et al. (2015))where commitment demand is often limited. We are hesitant to claim we can fully explain the difference given our moderate sample size, but we conjecture that this difference comes from three features of our design. First, our convex device is mechanically more flexible than the binary commitment devices used in the literature. Decisionmakers who value both commitment and flexibility would be generically less interested in commitment demand than those without a preference for flexibility. The other factor we believe is (more) responsible for this difference is that the Augenblick et al. (2015) design puts the cost of commitment in different units (dollars) than the gain from commitment (units of effort), whereas our design has cost and benefits of commitment in the same unit (dollars). It is conceivable that participants find it easier to think about decisions in the same unit than in different units, which could explain why there was a large spike in commitment demand at the price of 0 in Augenblick et al. (2015). Third, our participants were allowed to make commitment decisions conditional upon the wage while in Augenblick et al. (2015) the participants made a single commitment decisions for all interest rates they faced, which also makes our commitment options more flexible. The wage and interest rate are relevant features of the economic environment, as evidenced by participants labor supply changing with wage in our experiment and with the interest rates in Augenblick et al. (2015), so it is conceivable that the value of commitment and flexibility is also affected by trade-off rates in the environment.

Figure 3.1 shows a positive correlation between wage and commitment choice, and thus between labor supply and commitment choice. When controlling for wage, however, labor supply does not differ significantly across time or across commitment levels—suggesting that the participants' preferences over labor and wage remain relatively stable over the course of the experiment. We also observe that commitment level drop as we move into further into the experiment, suggesting that participants may be slowly adjusting their perceptions of own present bias. These decreases, however, are moderate so these perceptions seem fairly stable. Figure 3.2 shows that labor supply decisions are increasing with the wage and are mostly consistent across time.

3.4.2 Structural Results

We make the structural assumption that $C(L) = \frac{1}{\varphi\gamma}L^{\gamma}$. In our experiment, $X'(p) = \frac{1}{10}(p - 0.1)$, $\overline{p} = 0.9$, and $\underline{p} = 0.1$. Therefore, $L^*_{\tau,t,\neq} = (w\delta^{T-\tau}\varphi)^{\frac{1}{\gamma-1}}$, $L^*_{\tau,t,=} = (\beta\delta^{T-\tau}w\varphi)^{\frac{1}{\gamma-1}}$, and $p^* = X'^{-1}\left(w(w\delta^{T-\tau}\varphi)^{\frac{1}{\gamma-1}} - \frac{1}{\varphi\gamma}(w\delta^{T-\tau}\varphi)^{\frac{\gamma}{\gamma-1}} - w(\hat{\beta}\delta^{T-\tau}w\varphi)^{\frac{1}{\gamma-1}} + \frac{1}{\varphi\gamma}(\hat{\beta}\delta^{T-\tau}w\varphi)^{\frac{\gamma}{\gamma-1}}\right)$.

Figure 3.2 shows our aggregate estimates. We see that $\beta \approx 1$ and $\delta \approx 1$, as we predicted from our graphs of the labor supply decisions. Parameter ϕ does not appear to be well-measured, likely due to heterogeneity of labor supply functions among participants.

The key measurement we want to focus on is $\hat{\beta}$ (which is called β_h to distinguish from the empirical estimate of β). The only empirical estimate of β_h that we are aware of is in Augenblick and Rabin (2019), who find $\beta_h \approx 1$. Our estimate of β_h is significantly lower than 1. We would like to attribute this difference in estimates to the difference in experimental designs, with Augenblick and Rabin (2019) asking participants for their predicted labor supply and our experiment using convex commitment choices. However, given our small sample, we cannot support this assertion without more data. Our estimate for β_h is also quite below the predicted lower bound of β in O'Donoghue and Rabin (2001).

The fact that β_h is estimated to be below 1 and even below β has a few implications. Our participants mostly seem to believe that they are dynamically inconsistent. However, they on average overestimate the degree of their present bias. This is also mostly true when you consider individual estimates of β and β_h, however imprecise they may be due to small number of observations. Overall, the discrepancy between β and β_h have led participants to over-demand commitment, with negative consequences for their welfare. Section 7 considers these consequences in more detail.

Figure 3.3 shows the individual structural estimates of β and β_h for our participants. The red line shows $\beta = \beta_h$, and as we can see, many participants lie below that line Removing the outlier above of $\beta \approx 3$ does not affect the aggregate estimates of β or $\hat{\beta}$.. Indeed, many of our participants have estimates for β_h that are below the assumed lower bound of β in O'Donoghue and Rabin (2001). This is consistent with our aggregate structural estimate that $\hat{\beta}$ is significantly lower than β.

3.5 Welfare Analysis

Figure 3.5 demonstrates the welfare effects of commitment demand. The green line shows the cost of commitment. The red line considers the difference between the expected earnings under the given commitment choice and the expected earnings under the lowest, cheapest commitment choice of $p = 0.1$. The blue line simply takes the difference between the earnings from the participant's labor choice the period before labor and the earnings from the labor choice the period labor is performed, minus the cost of commitment they chose.

Both ex-ante and ex-post welfare calculations demonstrate losses from commitment, which are increasing with the commitment probability and closely follow the shape and levels the explicit commitment cost. This contrasts sharply with John (2020), who documented losses at low levels of commitment but gains at high levels of commitment. The results of John (2020) are well-explained by partial sophistication among inconsistent individuals—decisionmakers who have limited sophistication make limited commitment choices, which are costly but insufficiently so to generate behavioral change. On the other hand, decisionmakers with higher sophistication make stronger commitments which, while costlier, result in actual behavioral change. Our results stand in sharp contrast; our participants on average affect no behavioral change due to commitment, so losses closely mirror the explicit cost of commitment.

3.6 Conclusion

Commitment devices have received substantial academic and policy attention as a way to mitigate the effects of dynamic inconsistency, but this position rests on the assumption that individuals correctly perceive their own inconsistency. While there has been a growing literature focused on understanding when inconsistent individuals do not demand commitment, this chapter presents evidence of the reverse problem: dynamically consistent individuals who demand commitment. We document many individuals who are incorrectly pessimistic about their future selves, and demand costly commitment resulting in ex-post earnings losses that closely follow the cost of commitment.

Our results present a significant hurdle for policymakers and academics; until now, the focus has been on finding ways to encourage inconsistent decisionmakers to utilize commitment devices. The problem now becomes ensuring *correct* utilization of commitment devices, regardless of the relationship between actual and percieved degress of dynamic inconsistency.

3.7 Acknowledgements

Chapter 3 is currently being prepared for submission for publication of the material under the title "Dynamic Inconsistency and Convex Commitment Devices". Wolanski, Adrian; Dmitriev, Danil. The dissertation author, Adrian Wolanski, and Danil Dmitriev are the principal coauthors of this chapter.

3.8 Figures and Tables

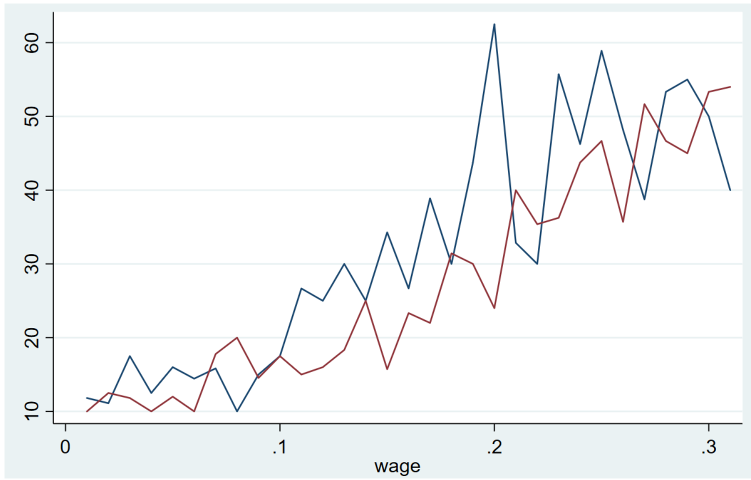


Figure 3.1. Demand for commitment as a function of the wage.

This figure describes the commitment demand function, aggregated across all individuals. The blue line is average commitment demanded during period 1, while he red line is the average commitment demanded during period 3 (the last period in which commitment is offered).

Figure 3.2. Supply of labor as a function of the wage.

This figure describes the labor supply function, aggregated across all individuals and periods. The blue line is average labor supply chosen before the period in which labor is performed, while red line is the average labor supply chosen during the period in which labor is performed.

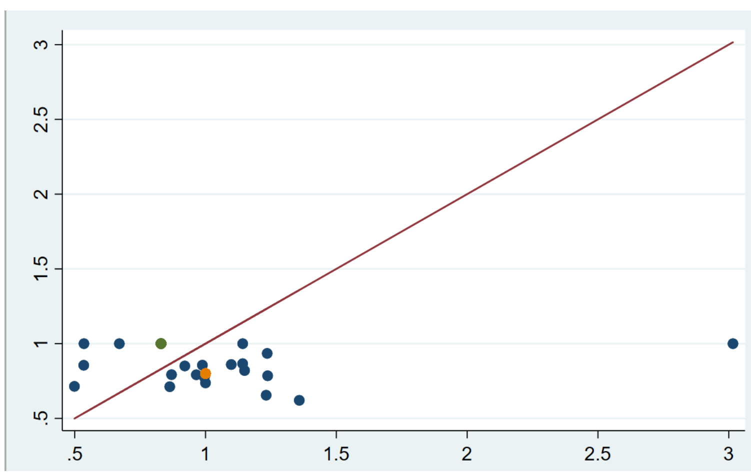


Figure 3.3. Individual Analysis

Blue dots are individual measures of β (horizontal axis) and $\hat{\beta}$ (vertical axis). The orange dot is our aggregate estimate, and green dot is the aggregate estimate from Augenblick and Rabin (2019), and the red line is a reference line for $\beta = \hat{\beta}$. Nearly 80% of our participants have estimated $\beta_h < \beta$, which is the theoretical lower bound.

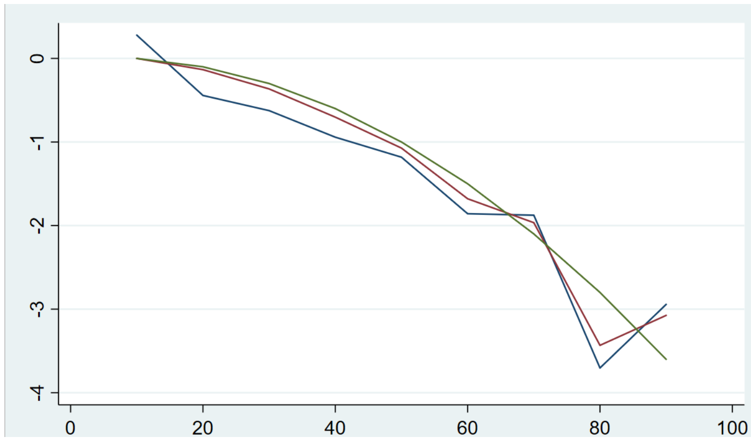
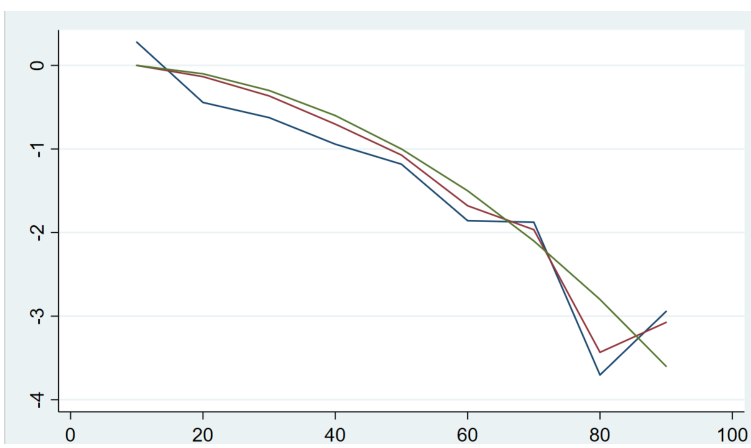


Figure 3.4. Welfare Effects of Commitment

This graph demonstrates both ex-ante (red line) and ex-post (blue line) average earnings difference as a function of commitment probability. The green line is a reference line displaying the costs of commitment.



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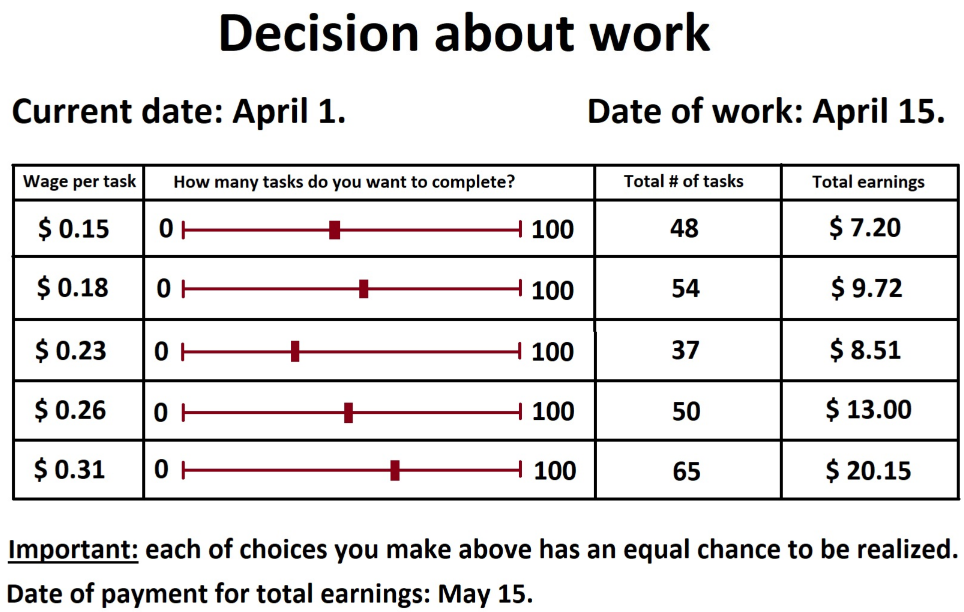

Figure 3.6. Example Interface–Labor Supply Decisions

This figure presents the interface that participants saw when making their labor supply decisions. For each wage presented, participants had a slider that moved between 0 and 100 tasks. The amount of tasks and earnings were also displayed.

Table 3.1. Reduced-form Commitment Demand Statistics

Category	Number of people	Share of sample
Total in sample	28	100%
Pay for commitment at least once	26	93%
Pay at least $1 for commitment at least once	22	79%
Pay the maximal amount for commitment at least once	13	46%
Pay for commitment on at least 50% of decisions	19	68%
Pay for commitment on at least 75% of decisions	13	46%
Pay for commitment on every decision	3	11%

This table displays the proportion of participants who purchased varying levels of commitment. Nearly half of the sample paid the maximum possible amount for commitment at least once, and nearly $\frac{2}{3}$ purchase commitment at least half of the time.

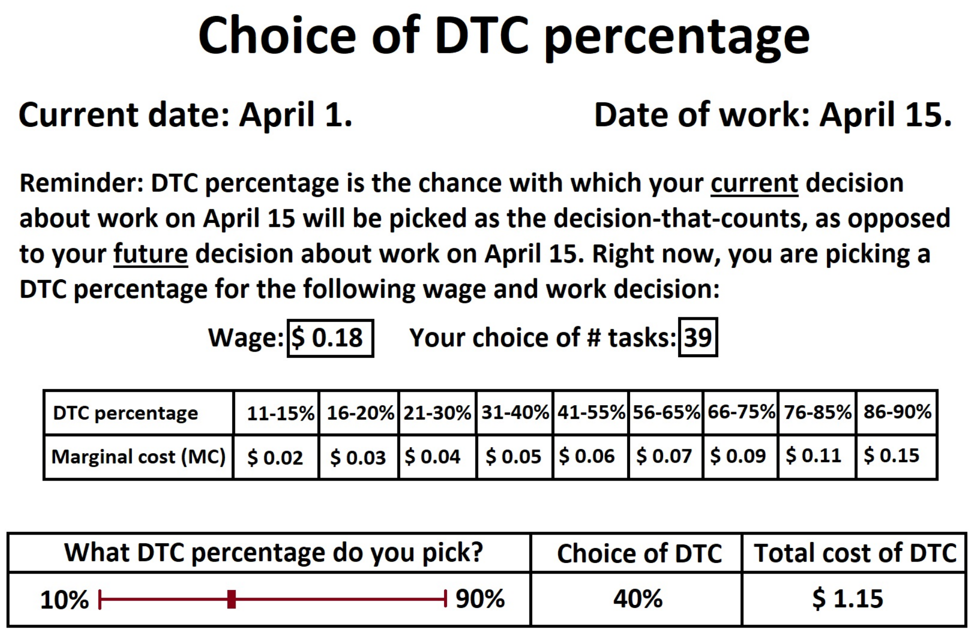

Figure 3.7. Example Interface–Commitment Decisions

This figure presents the interface that participants saw when making their commitment decisions. For each wage presented, participants had a slider that moved between 10% and 90% tasks. The number of tasks selected for each wage, as well as the cost of commitment, were also displayed.

Table 3.2. Structural Estimates of Parameters

Parameter Name	Parameter Estimate (Standard Deviation)	95% Confidence Interval
β	1.0069 (0.0486)	[0.9116,1.1021]
β_h	.8016 (0.0250)	[0.7525,0.8506]
δ	1.0006 (0.0452)	[0.9119,1.0892]
ϕ	2011.81 (2166.194)	[-2223.853,657.472]
γ	2.4018 (0.2420)	[1.9275,2.8760]
σ	39.6146 (4.8127)	[30.1817,49.0474]
N	2088	
#Clusters	29	
Log likelihood	-7289.0083	

This table displays the results of maximum likelihood estimation with Tobit correction. All standard errors are clustered at the individual level. The estimate of β_h is significantly different from null hypothesis of naivete ($\beta_h = 1$) and lower than the theoretical lower bound of $\beta = \beta_h$.

3.9 Appendix–Participant Documentation

3.9.1 Consent Form

Please read the following consent form carefully.

Who is conducting the study, why you have been asked to participate, how you were selected, and what is the approximate number of participants in the study?

Adrian Wolanski and Danil Dmitriev are conducting a research study to find out more about working over time. You have been asked to participate in this study because you are a UCSD student in the Economics Laboratory subject pool. There will be approximately 20 participants in this study.

Why is this study being done?

The purpose of this study is to understand how individuals make decisions about working over time, and how much they prefer decisions made at different times.

What will happen to you in this study and which procedures are standard of care and which are experimental?

If you agree to be in this study, the following will happen to you: you will be asked to attend 4 sessions. At each session, you will be asked to make decisions about how many tasks you would like to perform at a variety of possible wages. These decisions will involve task performance at various dates; some of these decisions will be for tasks performed during that session, and some of these decisions will be for future sessions. In each session, one of your task decisions will be randomly selected as the decision-that-counts, and you will be asked to perform those tasks in exchange for the associated wage payment. You will also be asked for how much you would like some of your decisions to become the decision-that-counts. Thus, your payments will involve some chance and will depend on your choices and on the decisions-that-counts.

How much time will each study procedure take, what is your total time commitment, and how long will the study last?

The study will take place in 4 sessions, each one week apart. The first session will take approximately 60 minutes, the following sessions will take less time (15-60 minutes).

What risks are associated with this study?

Participation in this study may involve some added risks or discomforts. These include the following:

Potential loss of confidentiality. Your choices will never be linked to your name. Each of you has been given a participant number. This participant number will be used to register your choices and for providing you payment. However, there does exist a record of your participation today. Research records will be kept confidential to the extent allowed by law. Research records may be reviewed by the UCSD Institutional Review Board.

Because this is a research study, there may also be some unknown risks that are currently unforeseeable. You will be informed of any significant new findings.

What are the alternatives to participating in this study?

The alternatives to participation in this study are to receive a show-up payment and exit the study.

What benefits can be reasonably expected?

There may or may not be any direct benefit to you from participating this study. If there is a direct benefit to you it will be monetary and will depend on your choices. Your minimum benefit will be your $5 participation payment. The choices you make will involve payments ranging from $80 to $0. The investigator may learn more about working over time, and society may benefit from this knowledge.

Can you choose to not participate or withdraw from the study without penalty or loss of benefits?

Participation in research is entirely voluntary. You may refuse to participate or withdraw or refuse to answer specific questions in an interview or on a questionnaire at any time without penalty or loss of benefits to which you are entitled. If you decide that you no longer wish to continue in this study, you will be required to contact the research staff immediately and request to leave the study.

You will be told if any important new information is found during the course of this study that may affect your wanting to continue.

Can you be withdrawn from the study without your consent?

The PI may remove you from the study without your consent if the PI feels it is in your best interest or the best interest of the study. You may also be withdrawn from the study if you do not follow the instructions given you by the study personnel.

Will you be compensated for participating in this study?

In compensation for your time and travel, you will receive $5 for participating in this research. Additionally you will be paid based on your decisions in the decisions-that-counts, as well as a $15 completion bonus if you attend and participate during all sessions.

Are there any costs associated with participating in this study?

There will be no cost to you for participating in this study.

Who can you call if you have questions?

Adrian Wolanski and Danil Dmitriev have explained this study to you and answered your questions. If you have other questions or research-related problems, you may reach Mr. Wolanski at 317-670-3278, or by email at awolansk@ucsd.edu. You may call the Human Research Protections Program Office at (858) 657-5100 to inquire about your rights as a research subject or to report research-related problems.

Your consent and acknowledgement of consent:

By clicking next, you acknowledge that you have read the consent form and agree to participate.

3.9.2 Instructions

This experiment requires your participation in 4 different experimental sessions, one per week for 4 weeks. Each session will occur at 10:30 on Monday mornings. If you know that you will not be able to participate in all sessions, please notify the experimenter to be removed from the experiment. You will still receive your show-up payment.

This study examines peoples' decisions about doing work for monetary payments. For this study, we have designed a series of transcription tasks that you may choose to do for payment. Each task requires you to transcribe a different string of 30 alphanumeric characters (example: ' 4O5an6jtHCiE8VtmQKwThMtXYrkEOC'). These tasks are of no value to us beyond understanding these decisions.

Today, you will be offered 8 different wages and asked how many tasks you want to perform at each wage during next week's session. During next week's session, you will again be offered the same wages and asked how many tasks you want to perform at each wage during that session. One of these 16 decisions you make will be randomly determined as the decision-that-counts; you will perform only the tasks in this decision and receive the associated wage payment. In other words, you will NOT have to do all the tasks from every decision; you will only do the tasks specified in the decision-that-counts. In addition to making decisions about task performance, today you will also be asked about how we should determine the decision-that-counts. You will be asked to choose what we call an implementation probability for each task performance decision. The higher the implementation probability for a decision, the more likely that today's decision becomes the decision-that-counts next session, and the less likely that next week's decision becomes the decision-that-counts next session. The implementation probabilities are costly; choosing a higher probability requires you to pay an additional monetary cost, which will be subtracted from your earnings at the end of the experiment.

The decision-that-counts will be chosen at random using the following process. First, the computer will select a number between 1 and 8 with equal probability. If this number is one, the decision-that-counts will come from the first wage we offered you. If the number is two, the decision-that-counts will come from the second wage we offered you, etc. Then, the computer will choose a number between 1 and 100 with equal probability and compare it with the implementation probability you chose for the wage selected above. If the random number is less than or equal to the implementation probability you chose, you will perform the number of tasks decided during today's session. If the random number is greater than the implementation probability you chose, you will perform the number of tasks decided during the next session. It is important you know that there is a positive probability that each of your decisions becomes the decision-that-counts, and that it is in your best interest to answer all questions honestly.

Your payment will be issued at the end of the fourth (final) experimental session and will consist of three components. First, you will receive a $5 show-up payment. Then, you will receive all earnings from tasks performed from all experimental sessions. Finally, you will receive a $15 completion payment if you participate in all experimental sessions. If you miss any sessions, you will be removed from the study and will not receive the completion bonus. You will, however, still receive your show-up payment and all earnings for any tasks you have completed.

If you have questions, please ask them now. We will also walk through an example session on the next page.

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