Economic Experiments on

Impulsive Urges, Control, and Irrationality

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Vorgelegt von:

Katharine Bendrick

an der Universität Konstanz

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1. Referent: Prof. Dr. Urs Fischbacher

2. Referent und Vorsitzender: Prof. Dr. Gerald Eisenkopf

3. Referentin: Prof. Dr. Lisa Bruttel

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Summary

One of the standard assumptions underlying microeconomic theory is that more opportunities from which to choose cannot be a bad thing. Indeed, it is logical from a welfare perspective and a common sense perspective that things like more employment opportunities, including the social mobility that goes along with it, allows a person to realize a higher level of life satisfaction in multiple realms. Logically this freedom to choose should extend to the decision situation, in that a person should not seek to restrict the type of environment she makes her decisions in. This thesis contains studies in which decision environments have potential negative effects on the decision-maker's welfare. We believe that if the decision maker were to be made aware of the type of effect the environment had on her decision, she would seek to restrict her environment, in other words the way information is transmitted to her.

More generally, the thesis looks at different types of control. All studies have to do with control, whether it be internal control, such as dealing with one's own temptations, or external control, such as an irrational aversion to ceding another person control over your own payoff (for example, allowsing someone else to be in the driver seat despite equal abilities, or an unwillingness to let a computer determine one's own lotto ticket numbers). It looks at human decision making through the lens of dual processing models, specifically the interplay between an impulsive, fast, and biased decision-making system; and a slow, deliberative, rational one (2004). Much behavioral research is pointing to the importance of understanding these two motivations in everything from exercise patterns to reciprocity in social interactions (DellaVigna & Malmendier (2006), Knoch, et al., (2006)).

This thesis is divided into four chapters. The first three chapters concern behavioral biases and inconsistencies brought to light by psychologists and behavioral scientists. The fourth chapter concerns the processing of conflicting motivation in social decision making. The first two chapters concern intertemporal decision making. Chapters 1, 2, and 4 are joint work with Urs Fischbacher.

Chapter 1 (Projection Bias: The Price for Food Craving) is about biases in how people predict their future preferences. Most decisions people make involve consequences for the future, and as a result the decision maker must make an estimate about what her

preferences will be at that time. Of course these predictions are just that, an estimate, and are subject to error. They are also subject to biases. Projection bias (Loewenstein, O'Donoghue, & Rabin, 2003) is the tendency for predictions about preferences in a different "state" from the one at the moment of deciding are biased toward the one a person is in at the moment of deciding. For instance, Read & van Leeuwen (1998) show that people who are hungry when making a decision are more likely to choose an unhealthy snack **to be received at a future date** than people who are not hungry when making the decision. Indeed, if a person expects to be hungry in the future it makes sense to pick the more calorie rich food item, and people do. However, current levels of hunger should have no effect, but there is a large impact. Of those who are hungry when making their decision, 78% (56%) choose the unhealthy snack when they predict they will be hungry (satiated) at the time of receipt, whereas for those who are satiated when decided, only 42% (26%) choose the unhealthy, calorie-rich snack.

Our study also deals with projection bias under hungry and sated states. We use an exogenous and randomly assigned treatment variable to manipulate hunger levels of our subjects, whereas past studies have not. In our experiment, subjects participate in a Vickrey auction for high quality chocolates to be received on a later day, in which it is optimal for a subject to bid her true willingness to pay for a good. We are therefore not only able to show the existence of projection bias, we also show its effect on willingness to pay for products. Projection bias in the marketplace affects consumption decisions, and therefore lifetime patterns of consumption, saving, and overall well-being. It is also a potential source of gain for firms, and for these reasons understanding how projection bias translates into willingness to pay is important. We show that hungry subjects are willing to pay 58% higher prices for a small box of chocolates than sated subjects.

Chapter 2 (Battling Impulses: Intertemporal Choice in the Short Term) contains three experiments about self-control. Many studies look at impatience by giving subjects a series of decisions between an early, small payoff and a later, larger payoff (delay discounting tasks). These types of studies generally go further to estimate the implied discount rates of subjects' decisions. Most studies (with a few exceptions) use monetary rewards as a payoff medium, and time spans of the tradeoffs are quite large, on the order of months or years. Using monetary rewards to study time preferences involves problems, mainly because money is not a primary reward; rather it is an opportunity set from which people can obtain a real reward. We are interested in this study in very short term impatience. Indeed, we think that short term impatience is very important to understand, as most temptation occurs when the prospect of a reward is imminent. This makes the use of monetary rewards even less appropriate for our study. We therefore introduce the paradigm of computer games as a medium of reward. Past studies have used food; we viewed this as problematic as we expect it to violate the "more is not worse" concept. We chose our computer game to be tempting and enjoyable, and contrasted it with an annoying task. We did this, so that even if subjects did not want expressly to play the game (which they were not forced to play), they would still desire to avoid the annoying task. Our treatment variable is intended to manipulate the degree to which impulsive motivations are given priority. In the less impulse-friendly environment, subjects make decisions regarding the game before they start the experiment, so before they are actually involved in the game or the task. In the more impulsive environment, they make their decisions while doing the game or task.

We link behavior in our "temptation tasks" to scores on the Barratt Impulsiveness Scale (BIS), and in experiment 3 of the series we show that behavior is additionally linked to the delay discounting task, using Amazon.de gift certificates as a reward medium. We find that scores on the BIS have more predictive power in the impulsive condition. Furthermore, we find with the delay discounting task that subjects are "present-oriented"; that is have declining discount rates.

Chapter 3 (Handing Over the Reins: On the Social Nature of the Illusion of Control) concerns the "Illusion of Control" brought to light by Ellen Langer (1975). The illusion of control appears in decisions involving risk. Risky decisions in real life not only involve risk preferences, but also skill. For example, skiing is a risky activity, but the more skill a person has, the lower the probability of an accident (holding choice of slope difficulty constant). In this situation, the amount of risk a person takes depends in part on her assessment of her own skill level. Ellen Langer argues that people conflate the skill with the risk elements, and even when they are given control over elements of a task that have no influence on the risk involved, act as if they have been given (partial) control over the lottery. This leads people to potentially take more risk in situations in which they have a feeling of control.

Recent work by economists has challenged this notion, and found conflicting results over whether the bias exists. Past studies have generally compared giving illusory control to the decision maker versus giving it to another person. In this paper, I put forth the proposition that this comparison may lead to many different conclusions about whether there is an illusion of control if there is heterogeneity in subjects' perceptions of their own skill level versus that of another person. I therefore conduct an experiment which makes it possible to observe whether there is heterogeneity in the type of illusion subjects have, and to then categorize them according to this characteristic. I also link their observed illusion of control to scores on the Magical Ideation Scale, which has been shown to be positively related to illusion of control biases (Brugger & Graves, 1997). I find in my study what initially looks like an illusion of control. However, I argue that it is actually the result of randomization. I also observe a link between scores on the Magical Ideation Scale and the amount of variation in individual answers, which I also will argue is the result of randomization, coupled with the unusual distribution of Magical Ideation scores.

Chapter 4 (Social Decision-Making Processes) looks at the processing of different types of conflict in social decision making through observing reaction times. We look at ego conflict (conflict between selfish and social motives) and social conflict (conflict between different social motivating factors). We use traditional (1^{st} party) dictator games, where a decision maker decides over distributions of money for herself and another person, as well as 3^{rd} party dictator games in which a decision maker decides over distributions for two other people with no consequences for her own payoff. We record reaction times as a way of measuring the conflict in a decision and as a way to assess the automaticity or controlled nature of selfish and social motivations. Rubinstein (2007) showed that reaction times could be used as a measurement of conflict when making a decision, and that lower conflict results in quick responses and higher conflict in faster responses. Additionally, automatic processes are thought to be fast and cheap, whereas controlled ones are slow and expensive; therefore we will be able to use reaction times to examine this idea. In our context, the 1^{st} party decisions contain more conflict than the 3^{rd} party conditions, simply because they contain a selfish motivation. With the 3^{rd} party condition, we can assess the individual's personal norm; that is her attitude about what is a fair way to allocate between two people independent of any selfish motivation. Past studies have shown that selfish decisions are made more quickly than decisions in favor of another person. We introduce the 3rd party condition to assess whether it is the selfish aspect of the decision that results in faster reaction times for selfish decisions, or another, previously undetected property of the decision.

We find that an increase in social conflict (that is, conflict between social motivating factors) results in increased reaction times. We further found that though selfish decisions are made faster, this is not the result of selfish motivation, but of other aspects of the decision. We also find that the personal norm is not well characterized according to our three identified types of social motivation (efficiency, maximin preferences, and absolute inequality aversion). The personal norm predicts increases in reaction times better than ego conflict with any particular social property, showing that the personal norm captures individual heterogeneity in values.

Zusammenfassung

Eine der Standard-Annahmen der mikroökonomischen Theorie ist, dass mehr Auswahlmöglichkeiten keine schlechte Sache sein können. In der Tat erscheint es von einem wohlfahrtsökonomischen Standpunkt logisch, dass mehr Beschäftigungsmöglichkeiten und die damit einhergehende soziale Mobilität die Zufriedenheit von Personen in verschiedenen Bereichen erhöhen können. Eine Vielfalt an Möglichkeiten sollte sich also auch positiv auf die Entscheidungssituation selbst auswirken, insofern dass Personen sich nicht auf bestimmte Umgebungen beschränken möchten, in denen sie ihre Entscheidungen treffen. Die vorliegende Arbeit beinhaltet Studien, in denen sich bestimmte Entscheidungsumgebungen negativ auf das Entscheidungsverhalten des Entscheiders auswirken können. Es ist davon auszugehen, dass Personen, die sich des negativen Einflusses bestimmter Entscheidungsumgebungen bewusst sind, ihre Entscheidungsumgebung (das heißt, die Art und Weise, in der Informationen als Entscheidungsgrundlage bereitgestellt werden) systematisch beschränken.

Generell befasst sich diese vorliegende Dissertation mit unterschiedlichen Arten von Kontrolle: Einerseits wird die interne Kontrolle betrachtet, bei der es darum geht, den eigenen Versuchungen zu widerstehen. Andererseits wird die externe Kontrolle untersucht und die irrationale Aversion vor selbiger, bei der man versucht Situationen auszuweichen, in denen andere Personen die Kontrolle über die eigene Wohlfahrt haben (z.B. lässt man trotz gleicher Fähigkeiten ungern andere auf den Fahrersitz oder man lässt ungern den Computer über die eigenen Lottozahlen entscheiden). Die Dissertation betrachtet menschliches Entscheidungsverhalten aus der Perspektive von Dualen-Prozess-Modellen, d.h. insbesondere wird auf das Zusammenspiel von impulsiven schnellen und verzerrten Entscheidungen und langsamen wohlüberlegten und rationalen Entscheidungen eingegangen (Strack & Deutsch, 2004). Eine Vielzahl an Studien aus der Verhaltensforschung verdeutlicht die Wichtigkeit des Verständnisses dieser beiden Motivationen bei individuellen und sozialen Entscheidungen, so z.B. beim individuellen Trainingsverhalten im Sport oder bei der Reziprozität in sozialen Interaktionen (DellaVigna & Malmendier (2006), Knoch, et al., (2006)).

Die vorliegende Arbeit ist in vier Kapitel unterteilt. Die ersten drei Kapitel befassen sich mit Verhaltensmustern und Inkonsistenzen, die durch Psychologen und Verhaltensforscher aufgedeckt wurden. Das vierte Kapitel behandelt die Verarbeitung

von widersprüchlichen Motivation in sozialen Entscheidungsprozessen. Die ersten beiden Kapitel befassen sich mit intertemporalen Entscheidungen. Die Kapitel 1, 2 und 4 sind aus gemeinsamen Arbeiten mit Urs Fischbacher entstanden.

Kapitel 1 (Projection Bias: The Price for Food Craving) beschäftigt sich mit der Fähigkeit von Menschen, ihre zukünftigen Präferenzen vorhersagen zu können. In der Regel treffen Menschen Entscheidungen, die Konsequenzen für die Zukunft haben. Das bedeutet, der Entscheidungsträger muss zum Zeitpunkt der Entscheidung eine Einschätzung über seine zukünftigen Präferenzen treffen. Diese Schätzung ist in der Regel mit Fehlern behaftet, oder gar systematisch verzerrt. Der sogenannte Projektions-Bias (Loewenstein, O'Donoghue, und Rabin, 2003) ist die Tendenz, die gegenwärtigen Präferenzen, d.h. die Präferenzen zum Zeitpunkt der Entscheidung, auf zukünftige Vorlieben zu projektzieren, so dass die zukünftigen Erwartungen systematisch in Richtung der gegenwärtigen Präferenzen verzerrt sind. Zum Beispiel zeigen Read und van Leeuwen (1998), dass die Entscheidung darüber, einen ungesunden oder einen gesunden Snack **in der Zukunft** zu erhalten, davon abhängt, ob der Entscheidungsträger in der Gegenwart hungrig ist oder nicht. Wenn eine Person erwartet, in der Zukunft hungrig zu sein, ist es sinnvoll, das kalorienreichere Nahrungsmittel zu bevorzugen. Dieses Verhalten ist auch bei den Teilnehmern zu beobachten. Allerdings sollte der derzeitige Hunger keinen Einfluss auf die Entscheidung über das in der Zukunft konsumierte gut haben. Wenn Teilnehmer hungrig sind, 78% (56%) entscheiden sie sich für das ungesundes Essen wenn sie denken, dass sie hungrig (nicht hungrig) im Zukunft sein werden; wenn sie nicht hungrig sind, nur 42% (26%) entscheiden sich für das ungesundes Essen.

Unsere Studie befasst sich ebenfalls mit dem Projektions-Bias in Abhängigkeit des Hungerzustandes. Im Unterschied Vorgängerstudien nutzen wir eine exogene und randomisierte Treatmentvariable, um den Hunger der Experimentteilnehmer zu manipulieren. Im Experiment nehmen die Teilnehmer an einer Vickrey-Auktion teil, in der sie auf eine Schokolade höchster Qualität bieten. Die Schokolade erhalten die Teilnehmer an einem Folgetag. Diese Vickrey-Auktionsform ist Anreiz kompatibel. Das heißt, es ist für jeden Teilnehmer optimal, seine wahre Zahlungsbereitschaft anzugeben. Daher ist es uns nicht nur möglich, die Existenz des Projektionsbias zu bestätigen, sondern auch zu quantifizieren, inwiefern er ökonomische Größen, nämlich die

Zahlungsbereitschaft für zukünftige Güter beeinflusst. Der Projektions-Bias hat also Konsequenzen für Konsumentscheidungen in realen Märkten und somit auch für Konsumentscheidungen über die Lebenszeit, Sparentscheidungen und das allgemeine Wohlbefinden. Zudem bietet das gewonnene Verständnis für den Projektions-Bias hinsichtlich der Zahlungsbereitschaft von Konsumenten Implikationen zur Gewinnsteigerung für Unternehmen. Unsere Studie zeigt, dass hungrige Konsumenten eine Zahlungsbereitschaft für eine kleine Schachtel Schokolade haben, die im Schnitt 58 Prozent über der Zahlungsbereitschaft der gesättigten Teilnehmer liegt.

Kapitel 2 (Battling Impulses: Intertemporal Choice in the Short Term) enthält drei Experimente zur Selbstkontrolle. Viele Studien untersuchen Ungeduld von Teilnehmern, in dem sie die Teilnehmer zwischen kleinen Geldbeträgen heute und unterschiedlichen größeren Geldbeträgen in der Zukunft entscheiden lassen (sogenannte „delay discounting tasks“). Diese Studien gehen in der Regel noch einen Schritt weiter und schätzen den impliziten Diskontfaktor der Teilnehmer. Dabei wird (mit wenigen Ausnahmen) Geld als Entlohnungsmedium genutzt und die Auszahlungszeitpunkte liegen sehr weit auseinander (Monate oder gar Jahre). Geld als Medium zu nutzen, wenn man Zeitpräferenzen misst, führt zu Problemen, da Geld weniger als direkte Belohnung sondern eher als Möglichkeit, sich selbst zu belohnen, wahrgenommen wird. Im Gegensatz zu diesen Studien untersuchen wir in unseren Experimenten eine kurzfristigere Form der Ungeduld. Das Verständnis von kurzfristigen Formen der Ungeduld ist wichtig, da Versuchungen häufig auftreten, wenn die Belohnung sehr zeitnah stattfindet. Für diese Untersuchung erscheint Geld als Medium der Entlohnung weniger geeignet. Aus diesem Grund haben wir ein „Paradigma der Computerspiele“ als Belohnungsmedium entwickelt. Andere Studien in diesem Bereich haben Essen als Belohnungsmedium genutzt, allerdings verletzt dieses die Annahme der Nichtsättigung. Wir wählten ein Computerspiel, das Spaß bereitete und eine Versuchung für die Teilnehmer darstellte und stellten diesem eine eintönige und langweilige Arbeitsaufgabe gegenüber. Auf diese Weise konnten wir sicherstellen, dass Teilnehmer, die das Computerspiel nicht unbedingt spielen wollten, zumindest der langweiligen Arbeitsaufgabe ausweichen konnten. Die Treatmentvariable bestand in der Entscheidungsumgebung. Sie wurde so gewählt, dass impulsive Entscheidungen in einer Umgebung stärker als in der anderen geschehen konnten. In der Umgebung, die weniger impulsivitätsfreundlich war, trafen die Teilnehmer bereits vor dem Start des Experiments, also bevor sie mit dem Spiel oder der Aufgabe beschäftigt waren, Entscheidungen bezüglich des Spiels. In der impulsivitätsfreundlichen Umgebung entschieden die Teilnehmer, während sie mit dem Spiel oder der Aufgabe beschäftigt waren.

Für die Analyse verknüpften wir das Verhalten im Experiment mit dem Score der Teilnehmer auf dem Barratt Impulsiveness Scale (BIS). In Experiment 3 wurde das Verhalten zusätzlich in Relation zu einer sogenannten „delay discounting task" gebracht, in dem eine Amazon Gutschein als Belohnungsmedium genutzt wurde. Unsere Ergebnisse zeigen, dass die BIS Scores eine bessere Vorhersagekraft in der impulsivitätsfreundlichen Umgebung als in der impulsivitätsfeindlichen Umgebung haben. Zudem zeigte sich im delay discounting task, dass die Teilnehmer „present-biased" sind, d.h. dass ihr Verhalten mit abnehmenden Diskontraten übereinstimmt.

Kapitel 3 (Handing Over the Reins: On the Social Nature of the Illusion of Control) befasst sich mit der „Kontrollillusion", die erstmals durch Ellen Langer (1975) hervorgehoben wurde. Kontrollillusion tritt in Situation auf, die Risiken beinhalten. In der Realität spielen in riskanten Situationen sowohl die Risikopräferenzen als auch die Fähigkeiten von einzelnen Personen eine Rolle. Zum Beispiel stellt Skifahren eine riskante Beschäftigung dar, jedoch ist Skifahren umso weniger riskant, umso bessere Fähigkeiten eine Person hinsichtlich des Skifahrens besitzt (gegeben die Wahl der befahrene Skipiste wird als fix angenommen). In einer solchen Situation ist also das Risiko, das eine Person eingeht, davon abhängig wie fähig diese Person ist. Ellen Langer argumentiert, dass Personen häufig ihre eigenen Fähigkeiten und die Risikoelemente der Entscheidungen verwechseln, selbst bei Aufgaben, bei denen sie lediglich Kontrolle über Dinge haben, die das Risiko nicht beeinflussen.

Jüngere ökonomische Studien liefern allerdings Ergebnisse, die dieser Herangehensweise teilweise widersprechen. In den meisten Studien haben die Teilnehmer dabei fiktive Kontrolle, oder eine dritte Person erhält die Kontrolle. In diesem Kapitel verdeutliche ich, dass Vergleiche auf dieser Basis problematisch sein können, wenn sich die Teilnehmer in der subjektiven Wahrnehmung bezüglich der eigenen Fähigkeiten und der Fähigkeiten der anderen Teilnehmer unterscheiden. Um herauszufinden, ob diese Wahrnehmungen tatsächlich heterogen sind, habe ich ein Experiment entwickelt, in dem es möglich ist, die subjektive Kontrollillusionen einzelner Teilnehmer zu untersuchen und die Teilnehmer dann gemäß dieser zu kategorisieren. Zusätzlich verknüpfe ich die beobachtete Kontrollillusion der Teilnehmer mit ihren Scores auf dem „Magical Ideation Scale“, für den bereits gezeigt wurde, dass er in positivem Zusammenhang mit der Kontrollillusion steht (Brugger und Graves, 1997). Auf den ersten Blick scheinen die Ergebnisse meiner Studie zu zeigen, dass die Teilnehmer eine Kontrollillusion haben. Ich argumentiere jedoch, dass diese das Resultat einer Randomisierung gepaart mit einer ungewöhnlichen Verteilung der Magical Ideation Scores ist.

Kapitel 4 (Social Decision-Making Processes) befasst sich mit einer Studie, in der wir die kognitive Verarbeitung von unterschiedlichen sozialen Konflikten durch die Messung von Reaktionszeiten schätzen. Die betrachteten unterschiedlichen Konflikte sind dabei einerseits ein Ego-Konflikt (also ein Konflikt zwischen eigener Auszahlung und anderen sozialen Motiven) und andererseits ein sozialer Konflikt (also ein Konflikt zwischen unterschiedlichen sozialen Faktoren). Um das Verhalten der Teilnehmer zu untersuchen, partizipieren diese einerseits in (1st party) Diktator Spielen, in denen der Entscheidungsträger über Auszahlungsverteilungen, die ihn selbst und andere betreffen entscheidet, und andererseits in 3rd party Diktator Spielen, in denen der Entscheidungsträger über Auszahlungsverteilungen für zwei andere Teilnehmer entscheidet. Wir messen die Reaktionszeiten als einen Näherungswert für den Konflikt in einer Entscheidung und als Maß für die automatisierte oder kontrollierte Natur egoistischer und sozialer Motive. Rubinstein (2007) hat gezeigt, dass Reaktionszeiten als Konfliktmaß verwendet werden können, und dass die Reaktionszeiten umso kürzer ausfallen, umso geringer der Konflikt ist. Zudem wird davon ausgegangen, dass automatisierte Prozesse schnell und wenig Ressourcen benötigen während kontrollierte Prozesse als langsam und Ressourcen intensiv gelten. In unserer Studie beinhalteten die Entscheidungen in den 1st party Diktator Spielen mehr Konflikte als in den 3rd party Diktator Spielen, da egoistische Motive und die daraus resultierenden Konflikte nur in den ersteren Spielen vorhanden waren. In den 3rd party Diktator Spielen konnten dagegen individuelle Normen, gemessen werden, d.h. die Einstellungen der Teilnehmer hinsichtlich einer fairen Aufteilung eines Geldbetrages unabhängig der eigenen egoistischen Motive. Studien haben gezeigt, dass egoistische Handlungen schneller getroffen werden als Entscheidungen zum Wohle anderer. Die 3rd party Diktator Spiele dienen dazu herauszufinden, ob es tatsächlich die egoistischen Motive sind, die zu schnelleren Entscheidungen führen oder ob diesem Phänomen andere Mechanismen zugrunde liegen.

Wir finden dass ein Anstieg im sozialen Konflikt (das bedeutet ein Konflikt zwischen unterschiedlichen sozialen Motiven) zu längeren Reaktionszeiten führt. Zudem finden wir, dass obwohl egoistische Entscheidungen schneller getroffen werden, die damit verbundenen kürzeren Reaktionszeiten nicht das Resultat egoistischer Motive sind, sondern von anderen Aspekten der Entscheidungen abhängen. Um die individuellen Normen der Teilnehmer zu identifizieren, wurden drei Typen sozialer Motivation (Effizienz, Maximin-Präferenzen und absolute Ungleichheitsaversion) unterstellt. Jedoch stellte sich heraus, dass eine Charakterisierung der sogenannten persönlichen Norm durch Kombination der drei Typen dennoch schwierig ist. Dennoch erklärt die persönlichen Norm Anstiege in den Reaktionszeiten besser als der Ego-Konflikt mit den von uns bestimmten sozialen Motivationen. Dies deutet darauf hin, dass die persönliche Norm die Heterogenität der persönlichen Werte besser erfasst.

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1. Projection Bias: The Price for Food Craving

Abstract

We use an incentivized laboratory experiment to confirm the presence of projection bias in hungry participants. We use a Vickrey (single bid, second price) auction to elicit participants' willingness to pay for a box of high quality chocolates. Hungry subjects had an average willingness to pay 58% higher than that of not hungry subjects. Hunger was manipulated by randomly assigning subjects to one of two conditions: either they received a pen (hunger) or a pretzel (no hunger).

1.1 **Introduction**

The colloquial advice "don't go to the grocery store hungry" contains quietly packaged within it the troublesome relationship that, Americans especially, have with obesity. It also reveals the presence of a phenomenon dubbed "projection bias." Projection bias is the tendency for one's current state of being, such as hunger, to affect decisions about preferences the decision maker will have in a different state of being, like being sated (Loewenstein, et al., 2003). Buying an extra candy bar at the grocery store to sate current hunger is not projection bias; buying a bag of pretzels, a box of muffins, and a tub of ice cream as the result of a false assessment of what you will want to eat tomorrow, is.

Read & van Leeuwen (1998) showed that, rationally, if people expected they would be hungry at a later specified time then they would pick a more calorie rich snack to receive at that time. They also showed that, not so rationally, the people who were asked before going to lunch were more likely to choose the calorie rich snack than those asked after lunch. This is evidence that the level of hunger that was being experienced at the moment of decision making affected decisions about preferences in a state of being that was the same for both groups. Gilbert, et al. (2002) found that, for subjects under cognitive load, those who reported higher levels of hunger rated the prospect of a spaghetti breakfast higher than those who reported lower levels of hunger. However, those not under cognitive load did not report different levels of predicted satisfaction from a spaghetti breakfast, lending credence to a process in which people's initial estimate of future preferences are current ones, and this initial estimate is then corrected with a controlled process. In Atance & Meltzoff (2006), the authors study projection bias in preschoolers. They performed a controlled experiment with the random allocation of children to a group in which they were allowed to eat pretzels, and one in which they were not. They then asked the children whether they would prefer water or pretzels to be received on the following day. Children in the pretzel group were more likely to choose water than those in the no-pretzel group. Badger, et al. (2007) elicit the willingness to pay for an extra maintenance dose of a heroin substitute among heroin addicts, to be received 5 days after the choice is made. They find a large difference in willingness to pay depending on whether subjects made the decision before or after having received a maintenance dose.

The above literature certainly makes a good case for the existence of projection bias. However, save the study on drug addicts, the effect on actual demand in a market has never been looked at. Indeed, it is important for projection bias not only affects the items purchased by a consumer, it effects their willingness to pay, and in consequence their wealth level. The effect on demand is also naturally very interesting for firms and could result, on aggregate, in substantial extra revenue. Although willingness to pay was elicited in one study, it was in a group of people who potentially have become drug addicts simply because of their higher than normal projection bias, meaning we still do not know the role it plays among more common (and numerous) consumption decisions. Additionally, previous studies have individually lacked in at least one dimension each, making our study the first to provide both the required experimental control, as well as using a group of people who make consumption decisions, and studying a type of consumption which all people engage in. In both Read & van Leeuwen (1998) as well as Gilbert, et al. (2002) the state of being is uncontrolled. It is reasonable to seek that hunger should be implemented as a controlled variable, since it is entirely plausible that those who are more hungry now might also be more hungry later, perhaps because they are on a diet, or even just because of heterogeneity. In Atance & Meltzoff (2006) the state of being is a controlled treatment variable, but the study is on very small children, whom we would expect to most likely have a greater projection bias than adults. And as mentioned, in Badger et al. (2007), drug addicts are potentially people with the very highest projection bias. Therefore, previous studies may overstate the extent of the bias.

In our study, we observed adult subjects (university students) and manipulate hunger randomly in two conditions. We establish the effect that projection bias has on willingness to pay for a tempting good with the use of a Vickrey auction. In other words, we quantify the effect of projection bias in a market environment with real incentives. Our participants are adults who we can expect to be making similar consumption decisions in their everyday lives. Students in Germany do not live in a dormitory setting as is often the case in the U.S., rather they live in apartments and deal with all the matters that arise in sustaining a household. This means that our results can be considered to give information about consumption decisions outside of the laboratory, and that the information we gain here about the effects of projection bias on willingness to pay for tempting goods is indeed valuable.

1.2 Procedure & Predictions

Participants were invited to the lab through the recruiting system ORSEE (Greiner, 2004). The experiments began at 4:15 pm and participants were requested via email not to eat after 1 pm on the day of the experiment. They were given the option to opt-out of the experiment if this was not manageable for them; no one did. The treatment variable was varied within a session and known to subjects. Half of the computer cubicles contained a pretzel, and half a pen. Subjects were randomly assigned to a cubicle and told they were allowed to eat the pretzel if they had one. They read instructions and answered control questions to ensure that they understood the incentives. The experiment was appended to the end of a series of several other experiments. The preceding experiments elicited risk and social preferences using both monetary and chocolate payoffs to be received after all experiments were completed. In the projection bias experiment, the prize to be auctioned was a small box of chocolates from the company Läderach. The receipt of the chocolates was not on the day of the experiment, but rather a future date of their choice (the winner arranged a day with the experimenter at the end of the experiment) at 4 pm. This ensured that current hunger would not realistically have an influence on the decision maker's state at the time of receipt. Participants were shown a lifesize picture of the box on their computer screen before participating in the auction. They then participated in a Vickrey auction: each participant made one bid for the product, and whoever bid the most received the prize at the second highest bid price. With this procedure, the optimal bid is one's true willingness to pay, so bids reflect individuals' willingness to pay for the chocolates. Bids in increments of 1 cent were allowed, and the maximum possible bid was 5 €, which is lower than the price of the good outside the lab. 104 people participated in experiment, which took place at the Lakelab (University of Constance, Germany) in July 2011. The experiment was programmed with z-Tree (Fischbacher, 2007) and all participants were paid 5€ for participation. Earnings from the preceding experiments were only revealed after the Vickrey auction to preclude income effects.

We also gathered participants' age, field of study, and sex. Additionally we had participants fill out the Autism Quotient questionnaire (Freitag, et al., 2007). We were interested in finding out whether theory of mind (the ability to understand that others have a different experience, and therefore different understanding and knowledge, from oneself) plays a role in projection bias. Autistic people are known to have poor theory of mind (Baron-cohen, Leslie, & Frith, 1985), so we included the Autism Quotient (AQ) questionnaire for an exploratative first look into this idea. We expect projection bias to be higher for higher AQ scores in the pen condition as a result of the link between theory of mind and autism; that is we expect a positive interaction effect between AQ scores and the pen treatment.

1.3 Results & Discussion

As a check whether our treatment variable manipulated hunger in the desired direction, we asked participants both whether they ate the pretzel and how hungry they were at the end of the experiment. 32 of the 52 people who received the pretzel reported eating the whole pretzel, 10 ate part, and only 10 did not eat any at all. Figure 1.1 shows the self-reported levels of hunger, and we do see that those in the pretzel condition report lower levels of hunger than those in the pen condition.

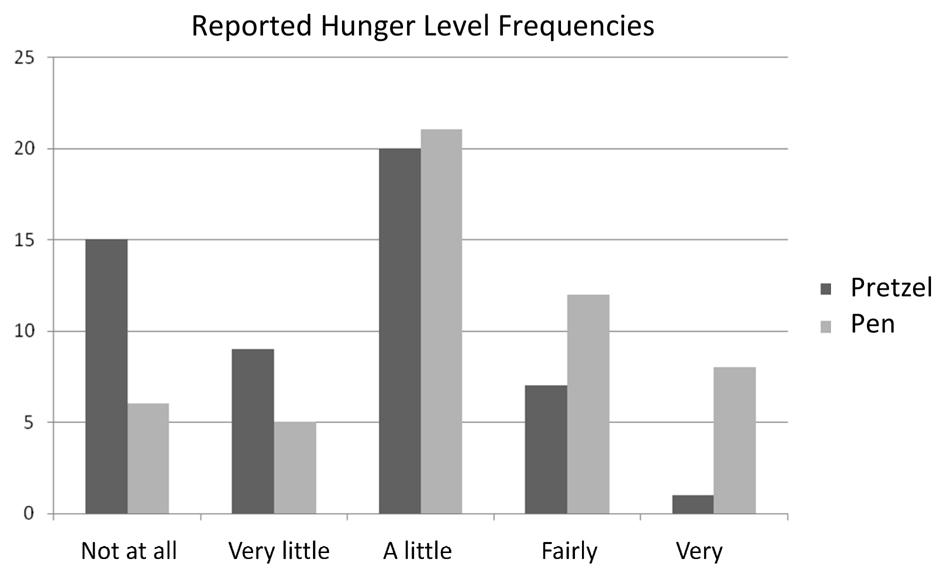


Figure 1.1

The distribution of bids is shown in Figure 1.2. On average bids were 58% higher in the pen condition, with the average in the pretzel condition at 0.87 € and 1.38 € in the pen condition. This difference is significant (one-sided t-test, p=0.014).

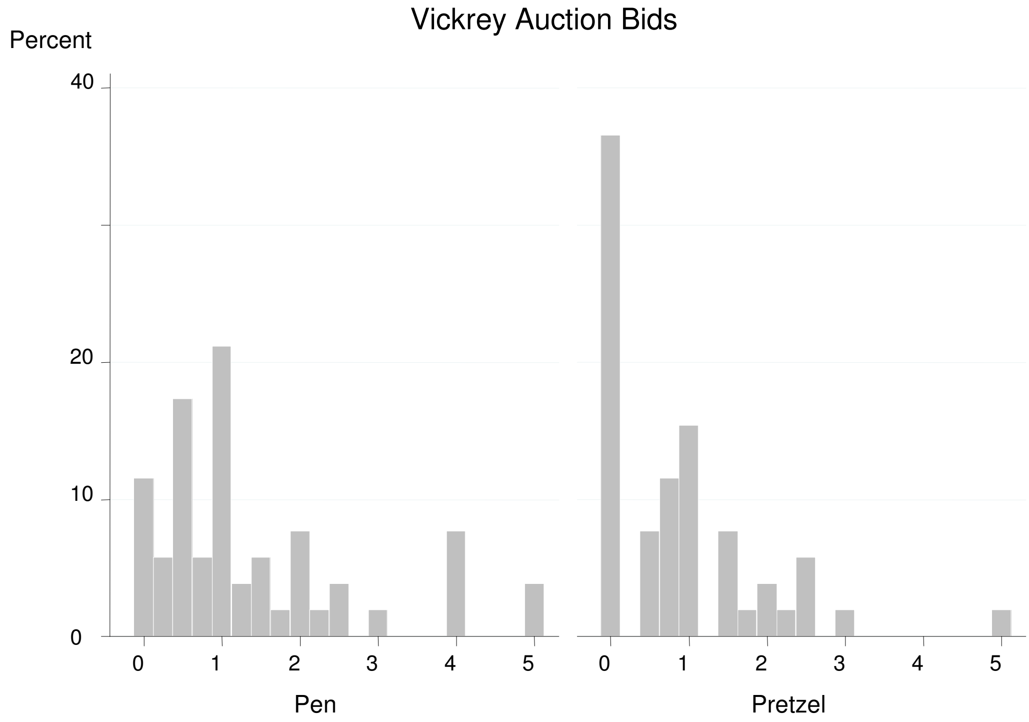


Figure 1.2

Distribution of bids for a box of chocolates across treatments (52 observations per treatment).

This provides controlled evidence for the existence and robustness of projection bias, not to mention the validity of the advice about when (not) to go grocery shopping. The number of satiated participants who bid zero (15) is triple that of the hungry participants. This raises the question of the quality of the bias. According to the functional form introduced by Loewenstein et al. (2003), projection bias is simply a convex combination of an accurate prediction and utility that would be gained from consumption at the time of decision. With this form, one would expect the distributions to be similar in shape but with a shift in the hunger treatment to the right. What we see is rather a sharp increase in the number of people bidding zero, with little change to the rest of the distribution, as seen in Figure 1.3. At these price levels, hunger functions like an on-off switch, actually affecting not only what price a person is willing to pay, but whether they are willing to purchase the item at all. Another possibility, though pure speculation at this point, is that decisions are two-stage, in that first a person decided whether or not they wish to buy a product, and then decide how much they wish to pay for it.

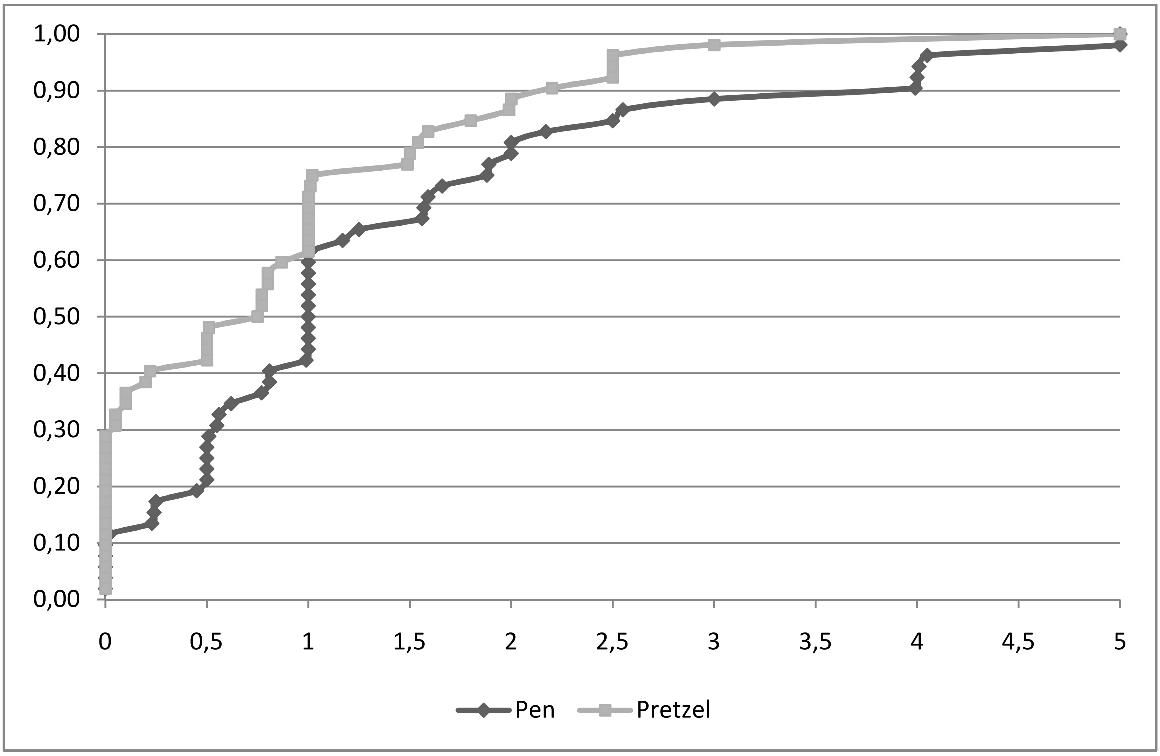


Figure 1.3

Projection bias is qualitatively the same for men and women, with a WTP difference between the treatments of about 55 (46) cents for women (men). 60 women and 44 men participated in our experiment, and similar numbers of men and women were in each treatment condition as shown in Table 1.1.

	Pen	Pretzel
Men	23	21
Women	29	31

Table 1.1
Number of participants falling into each category

The difference in differences, that is the difference in the size of the treatment effect between men and women, is insignificant ($p = .853$) as revealed by the OLS interaction term. The overall WTP for the chocolates was similar between men and women as well, with men willing to pay on average a mere 2 cents more than women (t-test $p = .931$).

Additionally we looked at behavior in our experiment and answers to post-experiment questionnaires. The first questionnaire we investigate is the Autism Quotient (AQ). As said in the predictions section, we expected a positive interaction effect of the pen treatment and the AQ. We expected this because we expected higher projection bias for those with high AQs as a result of the link between theory of mind and autism.

VARIABLES	bid
pen	1.777**
	(0.0363)
AQ score (summed)	0.0496
	(0.214)
Pen * AQ score (summed)	-0.0762
	(0.119)
Constant	0.0378
	(0.956)
Observations	104
R-squared	0.070

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.2

Answers on the AQ questionnaire go from "strongly disagree" to "strongly agree" in four steps. As is specified for the AQ (Dammann, 2002), we categorized answers further assigning a dummy variable of 1 for subjects who agreed with a statement (whether strongly or only somewhat), and 0 if not. We obtained the AQ by summing (with certain questions reverse coded) this variable. As the regression in Table 1.2 shows, we can reject this connection as the interaction effect is negative, to the extent that the overall affect of a higher AQ in the pen treatment is actually negative (although far from significant.

Next we analyze subjects' willingness to pay based on how much of the pretzel they actually ate. In the questionnaire section we asked them whether they ate the pretzel before or during the experiment. Possible answers were "yes", "partly", or "no". The spearman rank correlation shows that the more subjects ate of the pretzel (given they were in the pretzel condition), the higher the willingness to pay for chocolate (ρ = .309, p = .026). This indicates that those who ate the pretzel were more interested in food, whether it be because they remained hungrier than those who did not eat the pretzel, or because they simply put higher value on food than those who did not eat the pretzel.

1.4 Conclusion

This paper provides two contributions to the literature on projection bias. First, we establish the effect of projection bias on willingness to pay and therefore real demand for a product. Second, we confirm the presence of projection bias in a controlled laboratory experiment with adults, whereas previous studies have been either less controlled or studied the bias in particularly susceptible populations. Our study fills an important gap, in that in both cases above the general likelihood is for the bias to be overstated. We find that current hunger exerts a strong bias on the predictions of future preferences. In our case, the average bid price is 58% higher for hungry subjects. Furthermore, sated subjects most commonly resolved not to purchase the product at all. Hunger caused this resolve to crumble to a mere 1/3 of that of sated subjects, with hungry subjects most commonly choosing a willingness to pay of 1 €. We see a complete absence of demand translated into positive willingness to pay for calorie-rich, tempting foods. If this carries over to the grocery store where unhealthy, heavily processed foods are often the cheapest, the problem is indeed not one to be discarded as trivial.

Appendix

Instructions for the Experiment Procedure

We cordially welcome you to this economics experiment.

Your decisions and possibly others' decisions influence your payment in this experiment. It is therefore very important that you read these instructions very carefully. **For the entire period of the experiment, communication with other participants is not allowed.** We therefore request you not to speak with one another. Should you not understand something, please look through the experiment instructions once more. If you then still have questions, please raise your hand. We will then come to you and answer your question personally.

You will receive either a pretzel or a pen before the experiment. Please do not exchange them with other participants. You may eat the pretzel right away if you wish to.

During the experiment you can earn **points**, which will subsequently be converted into euros or **chocolate squares**.

The following exchange rate holds: **1 point = 1 euro**

1 point = 1 chocolate

The experiment is composed of five different parts. You will do the experiment once for each payment medium (money and chocolate). Parts 1 through 5 will therefore be completed first for the one payment medium and then for the other. Whether you will play for money or chocolate will be shown to you on-screen. Next will follow part 5.

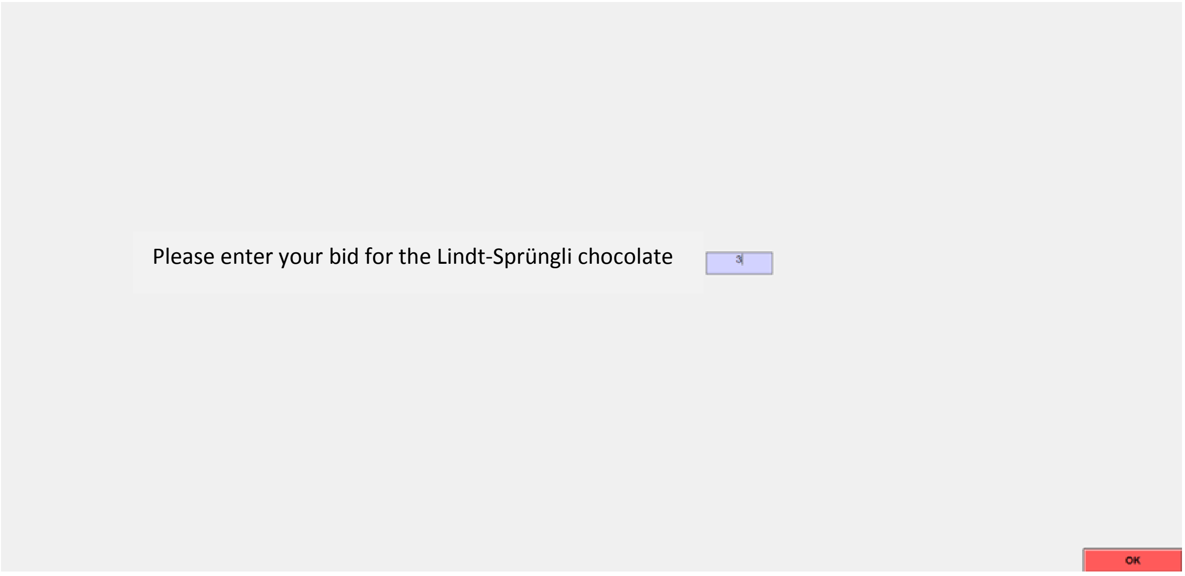
At the end, which decision will be relevant for your payment will be drawn. First we will carefully explain the 5 parts. Afterwards we will explain how your payment will be determined.

Part 5

In the fifth part of the experiment you can use the participation payment of 5 euros.

In it you will bid on high quality Swiss chocolates in an auction. For this purpose the 5 euros that you received are available. You can choose every number between zero and 5 euros in steps of one cent. The winner of the auction is the person who makes the highest bid. The winner however pays only the second highest bid from the auction. The bids will be made simultaneously, secretly, and decisively. In this auction it is optimal for you when your bid reflects your true value for the chocolate. The winner of the auction can pick up the chocolate on one of the next days in office F313 at 4pm. You can arrange the day on which you would like to pick up the chocolates with the experimenter after the experiment.

You will see the following screen:



Example:

Four participants participate in the auction, participants A, B, C, and D.

Participant A bids 2 euros.

Participant B bids 1 euro.

Participant C bids 3 euros.

Participant D bids 2 euros.

Participant C wins the auction for the chocolates and pays the second highest bid of 2 euros.

Example:

Four participants participate in the auction, participants A, B, C, and D.

Participant A bids 50 cents.

Participant B bids 50 cents.

Participant C bids 25 cents.

Participant D bids 1 cent.

Participant A or participant B will be randomly drawn and wins the auction for the chocolates and pays the second highest bid of 25 cents.

Comprehension questions [only those for part 5 shown]

Your answers to the control questions have no influence on your payment at the end of the experiment.
True or false?

□T□F In part 5 the highest bidder pays the second highest bid

In **part 5**the four highest bids are: 5 euros, 2 euros, and two times 1 euro each.

1a) How muchdoes the highest bidder pay for the chocolate? ___________________________

1b) When does the highest bidder receive the chocolate from part 5? - _______________________

In **part 5** the four highest bids are: 4 euros, 1.50, and two times 1 euro each.

2a) How much does the highest bidder pay for the chocolate? ___________________________

2b) When does the highest bidder receive the chocolate from part 5? _____________________

In **part 5**the four highest bids are: 1 cent, 1 cent, and two times zero each.

3a) How much does the highest bidder pay for the chocolate? ___________________________

3b) When does the highest bidder receive the chocolate from part 5? _____________________

In **part 5** the four highest bids are: 1.45 euros, 1.45 euros, 55 cents and 35 cents.

4a) How much does the highest bidder pay for the chocolate? ________________________

4b) When does the highest bidder receive the chocolate from part 5? ___________________

References to chapter 1

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2. Battling Impulses: Intertemporal Choice in the Short Term

Abstract

We study self-control problems in a laboratory experiment under the idea that many important impulsive decisions take place in the very short run. We provide a new experimental paradigm for the provision of immediate, controlled rewards in adult humans. We link the Barratt Impulsiveness Scale (BIS) version 10 with behavioral data, and show that impulsive behavior is predicted by the questionnaire. Moreover, we give preliminary evidence for the nature of the impulsiveness that the BIS measures, and conclude that it indeed measures what economists call impulsiveness, in the sense of an inconsistent preference for immediate gratification. We observe a gender effect which is robust to controlling for risk and impulsiveness. This may be the result of risk preferences that are not measured by Holt & Laury risk elicitation.

2.1 **Introduction**

Impatience and impulsiveness lead to behavior that places a much higher value on current concerns than future ones. Given this tendency to produce similar behavior, it is easy to think of the two as similar concepts. We argue however, that there is an important difference, and that is a person's satisfaction with his or her own tendency to discount the future. We will make the following important discrimination between impatience and impulsiveness in this paper: we will consider impatience to be a time-consistent preference property, and impulsiveness not to be time-consistent. This means that a purely impatient person is "content" with her preferences. On the other hand, an impulsive person need not be happy with her disregard for the future. It is the difference between a self-proclaimed hedonist, and a self-tortured addict. The impulsive person may have a preference to restrict her future choice set to prevent her impulsiveness from reigning over her decisions. In the language of quasihyperbolic discounting (Laibson, 1997) impatience captures the δ component of the discount function, and impulsiveness the β component.

Much work has been devoted to the study of time preferences in economics, including work investigating time-(in)consistency, self-control, and present-bias (for a rather comprehensive review of the literature see Frederick, Loewenstein, & O'Donoghue (2002)). Empirical work abounds, including the experimental variety. For the most part, experimental studies use some form of delay discounting task, which gives decision makers the option between two amounts of money at two different delays. Cubitt & Read (2007) raise the issue of whether these types of tasks are appropriate to measure time preference, because money is not a reward itself, it is merely an opportunity set from which people can draw future rewards. To truly measure time preferences, the time of the reward must be controlled. This is the goal of our paper. There are a few studies which do the same, but as of yet the number is very small. Two notable contributions are McClure, et al. (2007) and Houser, et al. (2010). McClure and coauthors use water and juice as reward mediums, while Houser and coauthors combine the controlled and tempting reward of surfing the internet with money. McClure et al. find evidence of a present-bias using this medium, chiefly when the time between the early and late rewards is 5 minutes. Houser, et al. find evidence of people willing to pay a cost in order to prevent the possibility of succumbing to temptation. They also find that there is not a self-control problem. That is to say, even though people are willing to use a commitment device, an increase in the price of this commitment device does not lead to a larger likelihood to succumb to temptation. This implies that there is a psychic source of self-control, which in the absence of a reasonably priced external alternative, can be used (at a cost) to avoid tempting activities.

The second goal of our paper is to examine the notion of self-control as it relates to the psychological construct impulsiveness as measured by the Barratt Impulsiveness Scale (BIS) (Patton, Stanford, & Barratt, 1995). As the above section makes clear, behavioral studies of impulsiveness are riddled with issues which make results difficult to interpret clearly. Therefore, we use the BIS to have an established and reliable nonbehavioral measure of impulsiveness. This scale includes a subscale which, as it is described in the publication (Patton, et al., 1995), is intended to capture exactly what economists call discounting the future, or as they put it "a 'present orientation' or a lack of 'futuring'" (p. 769). A disregard for the future, however, encompasses both impulsiveness and impatience. Since the distinction between the two has important implications for a person's welfare over time, assessing what indeed the BIS measures is a worthwhile task. For this reason, we analyze whether the BIS measures the tendency to be happily present-oriented (the δ discount factor) or a more frustration-riddled present-orientation (the β discount factor). The link between the BIS and behavior has been shown in cigarette smokers, where those with higher impulsiveness scores on the Barratt Impulsiveness Scale version 11 relapsed earlier (Doran, Spring, McChargue, Pergadia, & Richmond, 2004). However, other studies have shown the link between discounting and the BIS to be somewhat unclear. McLeish & Oxoby (2007) concluded that there was no relation between delay discounting and the BIS, except in the particular case when subjects had received negative feedback beforehand (52 participants out of 259). Yoon et al. (2007) however show both a relationship between cigarette relapse and the BIS as well as a relationship between delay discounting and cigarette relapse. This implies a potential relation between the BIS and delay discounting. We show a relation between behavior and the BIS in all three of our experiments. We additionally show a relation between delay discounting and the BIS, as well as a relation between delay discounting and behavior in our experimental tasks.

We investigate intertemporal decisions in a short-term environment and the self-control problems therein. To do this, we use a novel reward medium whose utility is realized within the laboratory upon consumption: the open-source computer game "Stacker Blocks 3D", similar to Tetris. In experiment 1, the game is the only reward available, as we wanted to isolate decisions to incentives which were relevant only during the time of the experiment. Subjects played a lottery in which the longer they waited, the higher the probability was that they would get to play the game, and the shorter the amount of time they would be allowed to play. Experiment 2 was also a lottery decision, in which subjects began the experiment playing Stacker Blocks 3D and decided when to stop playing. Their stop time was linked to a monetary lottery, in which the longer they played the higher the sum they could win, but the lower the probability they would receive it. We reintroduced monetary incentives which opposed the game incentives to test the robustness of the phenomena from experiment 1, that is, to see whether the monetary incentives drown out any observed impulsiveness due to their strength. Additionally, we reversed the risk structure relative to experiment 1, to isolate behavior attributable to impulsiveness from that due to risk attitudes. Risk aversion in experiment 1 leads a participant to prefer shorter playing times, whereas in experiment 2 it leads to longer playing times. In both of these experiments we additionally elicited risk preferences with the Holt & Laury (2002) risk task to control for variance that occurs as a result of risk preferences. In experiment 3 we eliminated risk elements altogether, and allowed subjects to pay (money) to play Stacker Blocks 3D, with costs increasing in the amount of time played. As a control here, we additionally elicited willingness to pay for a fixed amount of the game to control for how much a person likes the game overall.

All three experiments have two treatment conditions, one in which subjects make their decisions before the start of the experiment (which is precisely timed), and one in which subjects make their decisions during the experiment time. We expect that the latter condition fosters more impulsive decisions, since rewards are imminent. Although we do include monetary incentives in experiments 2 & 3, there is always the game reward medium from which we draw conclusions about time preference. By using the decisions about the game, and doing it in the very, very short-run, we go beyond previous work to study the very small and potentially procrastination-ridden decisions that occur in large multitudes everyday, and we do it in a controlled manner.

The paper is organized as follows: each of the next three sections presents the design specific to each experiment and then the results of each. In section 2.5 there is a discussion and conclusion.

2.2 Experiment 1

2.2.1 Experimental Design & Procedures

In experiment 1, participants had the opportunity to try to start the game Stacker Blocks 3D. If a subject was successful in starting the game, she was allowed to play the game until the end of the experimental period. If not, she was required to wait (or do a so-called click task) until the end of the experimental period. The experiment lasted 16 minutes and 40 seconds (1000 seconds). The click task required subjects to click on a button that appeared on either the left or right side of the screen every 10 to 20 seconds. This task was incentivized; if the button was not clicked, the subject lost points equal to the number of seconds for which the button was visible. This task was designed for subjects to be able to complete perfectly, as long as they paid attention, and served only to make sure that the alternative to playing the game was not preferable. Subjects were endowed with 1000 points, or 3 euros (it was framed that they "earned" one point per second), so they did not make losses. Each subject had a pencil and paper (from the practice questions) and no other materials were allowed. The later an attempt to start the game was made, the higher the probability it was successful. The specific probability that a game successfully started was *p = elapsed time/total time*. For risk neutral subjects the optimal time to make an attempt was at 500 seconds. We use a lottery in order to have heterogeneous observations. An alternative design, which we use later in experiment 3, is to allow subjects to simply pay to play the game. This has the advantage of not being tied in any way with risk attitudes, but the downside of very low heterogeneity of decisions in the form of many decisions to not play the game at all. Thus in this experiment (and experiment 2) we use a lottery to have an optimal game time that is not at the boundary of possibilities. We expect such an environment to pick up even subtle or weak impulsiveness among subjects.

There were two experimental conditions, *decide before* and *decide during.* In *decide before*, subjects decided when to attempt to play before the beginning of the 1000 seconds. These subjects entered the time in elapsed seconds at which they would like to attempt the game. In *decide during*, subjects decided during the 1000 seconds. These subjects saw a screen with a button they could click on at any time to attempt the game.

Using a quasi-hyperbolic discounting model, the expected discounted gain for a subject who makes an attempt after 1000-t seconds (and potentially plays for t seconds) is:

$$\frac{\beta\delta^{1000-t-t_0}(1000-t)U(\delta,t)}{1000} \qquad (1)$$

where β equals 1 if $t=t_0$ and is otherwise a constant value between 0 and 1, δ is the constant discount factor, and the term $\frac{(1000-t)}{1000}$ is equal to the probability a subject will get to play. U(δ,t) is the discounted utility stream of playing Stacker Blocks 3D for t seconds, that is:

$$\sum_{i=0}^{t-1}\delta^{i}u(t_i) \qquad (2)$$

where $u(t_i)$ is the instantaneous utility of playing Stacker Blocks 3D.

Our first hypothesis is that:

Hypothesis 1: Subjects in condition *during* will play longer than those in *before*.

This is seen by the fact that the decision between making an attempt to play Tetris at time t_1 or t_2 where $t_1<t_2$,simplifies in the *before* condition to a comparison between $\frac{U(\delta,t_1)(1000-t_1)}{1000}$ and $\frac{\delta^{t_2-t_1}U(\delta,t_2)(1000-t_2)}{1000}$, whereas in the *during* condition the comparison reduces to $\frac{U(t_1)(1000-t_1)}{1000}$ and $\frac{\beta\delta^{t_2-t_1}U(t_2)(1000-t_2)}{1000}$. The additional parameter β ensures that t_2 will be, compared to t_1, valued (weakly) less in the *during* condition than in the *before* condition, implying t_1 to be chosen more often in the *during* condition than in the *before* condition.

377 students participated in this experiment. Subjects in this experiment also participated in an unrelated experiment in each session, sometimes before and sometimes after. Participants in all three experiments were from the University of Konstanz, and all three experiments were programmed using z-Tree (Fischbacher, 2007), and subjects were recruited using ORSEE (Greiner, 2004). In some sessions we additionally elicited risk preferences with a risky choice task (Holt & Laury, 2002) as a control for the risky nature of our experimental task.

2.2.2 Results

We did not observe the expected treatment effect in experiment 1. On average, in *decide before* people waited 512 seconds, in *decide during* 505 seconds (insignificant difference, t-test, p = .698). Figure 2.1 shows the cumulative distribution of attempt times, separated by whether or not subjects had the click task or not. It reveals very similar patterns between the two treatments in each case.

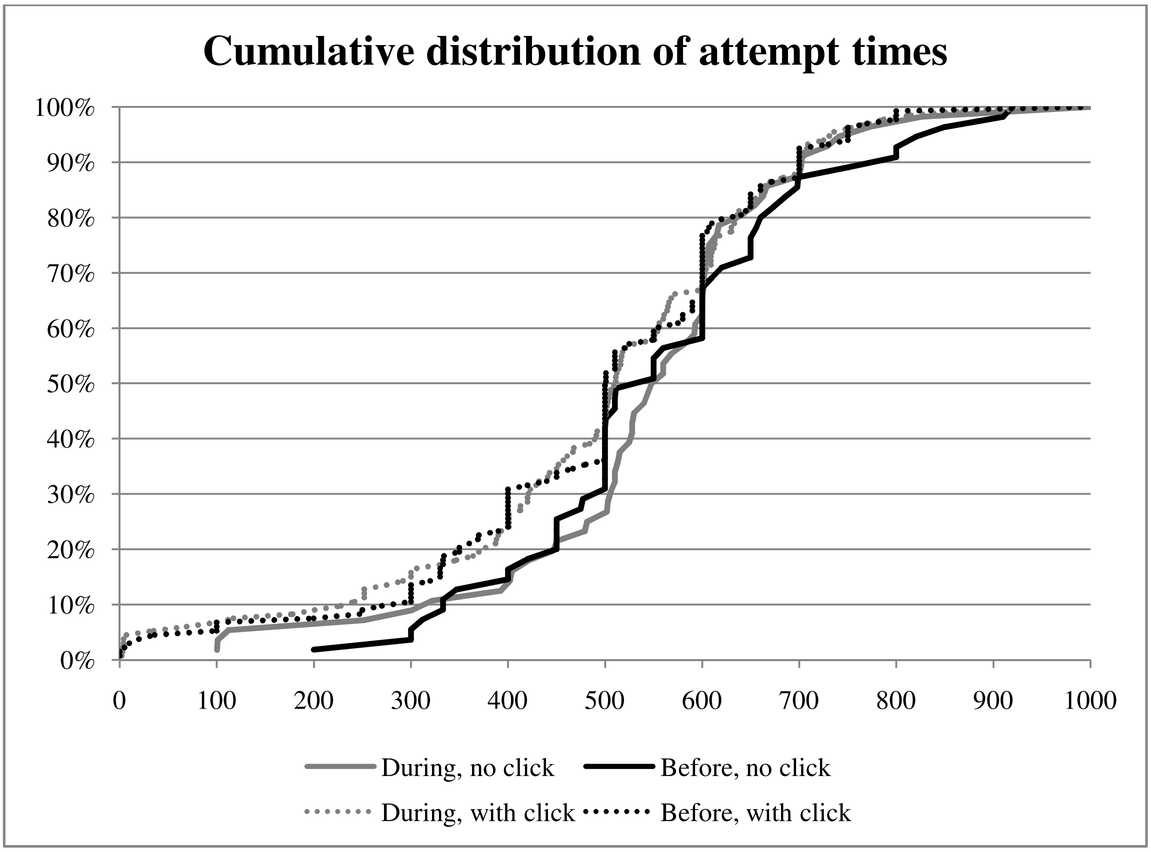


Figure 2.1

The click task lowered switch times by about 40 seconds, though overall patterns of attempt times between the two treatments were similar regardless of whether they performed the click task or not. For this reason in the following analysis we pool the data. In addition to the BIS questionnaire, we gathered basic socioeconomic information (sex, age, employment dummy, employment hours, desired wage after studies, disposable income, number of roommates, political orientiation, religiousness, charitibility, and number of younger/older brothers/sisters). There is a strong gender effect in attempt times, and a weakly significant age effect.

We next look at the BIS and its relation to the time at which people made their attempts to play the game. We look at the overall BIS score as well as subscores for nonplanning, motor impulsiveness, and cognitive impulsiveness. The subscale "nonplanning" is the best measure of decisions. As formulated by Patton & Barratt (1995), this is the subscale most in line with the concept of future discounting, and therefore indicates to us that we are indeed measuring the type of impulsiveness we seek to, rather than a more distant concept such as an inability to censor physical impulses. This is especially interesting considering the possibility in our experiment that motor impulsivity drives decisions, since the decision is binding once made. This is in contrast to some situations in which an impulsive action can be revised, for instance the case of a giant box of chocolates placed in the grocery cart, which can be returned to the grocery store shelf. In Table 2.2, column 2 shows that the nonplanning measure has the largest magnitude, as compared to the overall score (column 1) and the other two subscales (columns 3 and 4). Column 5 shows all subscales as regressors in one model, and the nonplanning score remains largest in magnitude, furthering the conclusion that it is the most relevant construct in our decision framework.

VARIABLES	(1) Attempt time	(2) Attempt time	(3) Attempt time	(4) Attempt time	(5) Attempt time
standardized BIS score	-20.54*				
	(0.0730)				
standardized nonplanning score		-23.54**			-21.91
		(0.0424)			(0.114)
standardized motor impulsiveness score			-11.74		2.812
			(0.318)		(0.852)
standardized cognitive impulsiveness score				-14.52	-6.040
				(0.193)	(0.670)
Constant	507.8***	507.6***	508.1***	507.6***	507.5***
	(0)	(0)	(0)	(0)	(0)
Observations	257	257	257	257	257
R-squared	0.013	0.016	0.004	0.007	0.017

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.1

Looking at the relation between game attempt times between treatments and the BIS (Table 2.3), we observe an effect of the BIS in the direction of what we expect from the perspective of impulsive time preferences. An individual's BIS score is predictive of behavior in the *decide during* condition (column 2), but not in the *decide before* condition (column 1). Although the coefficient in the *decide during* condition is quite a bit larger than in *decide before*, it is not significantly so (OLS regression interaction term p-value= .363). Although we do not observe a treatment effect, the result shown in Table 2.3 is an indication that (1) our reward medium is effective, and (2) that our treatment variable does vary the temptation present in the decision.

	Attempt time	
	(1)	(2)
VARIABLES	*decide before*	*decide during*
Standardized nonplanning score	-15.36	-36.62**
	(0.330)	(0.0358)
Constant	496.6***	520.8***
	(0)	(0)
Observations	128	129
R-squared	0.008	0.034

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.2

In the regression in Table 2.4 we include risk preferences as assessed by the Holt & Laury risk task in our analysis.[1] We include risk elicitation for several reasons. One reason is simply that our behavioral measure is a lottery, and therefore the Holt & Laury, also being a lottery, may be able to capture some of the explanation for how people decide in our environment. More importantly, we included them as a check to the claim that hyperbolic discounting arises solely out of risk preferences, i.e. that impulsivity is simply a result of risk attitudes (Epper, Fehr-Duda, & Bruhin, 2010). We find that the relation between the BIS and behavior is robust to controlling for risk. Additionally in Table 2.4 we include gender. There is a strong gender effect, which is not the result of risk or BIS scores. We include the treatment variable in the presence of controls as well to check whether it is heterogeneity in other factors which masked an effect of the treatment variable. In columns 3 and 4 we see this is not the case, in fact the sign of the treatment variable coefficient is opposite what we expected, though not significant.

[1] We test with regression whether behavior in the Holt & Laury task is predictive of behavior in the BIS and find no relation, and therefore concluded they are indeed measuring distinct concepts.

VARIABLES	(1) Attempt time	(2) Attempt time	(3) Attempt time	(4) Attempt time
Standardized nonplanning score	-23.54**	-25.86*	-28.02**	-18.33
	(0.0424)	(0.0608)	(0.0426)	(0.190)
Number of non-risky choices in the Holt & Laury		23.68***	22.63**	19.42**
		(0.00848)	(0.0117)	(0.0285)
Before condition			-41.40	-31.38
			(0.128)	(0.243)
Male				-75.46***
				(0.00793)
Constant	507.6***	390.7***	417.4***	464.4***
	(0)	(0)	(0)	(0)
Observations	257	161	161	161
R-squared	0.016	0.074	0.087	0.128

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.3

2.3 **Experiment 2**

2.3.1 Experimental Design & Procedures

In experiment 2 subjects again made decisions over game-playing and a lottery. This time however the lottery was monetary, and all subjects were able to play Stacker Blocks 3D for sure. Instead of deciding when to attempt to start playing, Stacker Blocks 3D started automatically, and participants chose when to stop playing, with a monetary lottery depending on this decision. The later a subject ended the game, the higher the amount they could win. However, the later they ended the game, the smaller the probability that they would receive the money. Specifically, the possible reward was two times the elapsed time (i.e. two times the number of seconds they played the game), and the probability of receiving the money was $1 - \frac{1}{500}$*elapsed time. As in experiment 1 we used the lottery format in order to have an interior solution, which we expect is more responsive to even weak impulses. The experiment lasted 8 minutes 20 seconds, or 500 seconds. As in experiment 1, there was a *decide before* condition and a *decide during* condition. Just as in experiment 1, the hypothesis was that subjects in *decide during* would play longer than those in *decide before*. Since the monetary lottery outcome was realized at the end of the experiment (when payoffs were distributed), as well as the fact that the money could only be used to purchase primary rewards at some later point, the β parameter discounts all monetary rewards. This means that no dynamic inconsistency can occur solely as a result of rewards from the lottery decision. However, the value of playing the game for a little longer (or having a bit more free time) in the *during* treatment is not discounted by β, meaning that a subject is more likely to value the next unit of playing the game (plus the analogous future lottery) more highly than the lottery available at the very moment. Hence the higher likelihood that subjects choose to play longer in the *during* condition.

In experiment 1, impulsivity resulted in earlier attempt times. Since earlier attempt times could be the result of error, we designed experiment 2 in a way that higher impulsivity should lead to actions that occur later, ruling out the likelihood that observed impulsive behavior is actually the result of motor or understanding errors. 68 participants participated in the experiment during June, 2010.

2.3.2 Results

Repeating our analysis of socioeconomic variables from experiment 1, we do observe a gender effect in the same direction, though not significant (men play the game 16.98 seconds longer, t-test $p = .227$)

Looking at the BIS questionnaire, we find that nonplanning is again the most predictive subscale. However, in this case the overall BIS score is the best overall predictor of behavior. Nonetheless, it is clear that the concept of discounting is still more relevant than that of motor or cognitive impulsiveness. Comparing column 2 to columns 3 and 4, we see the nonplanning subscale is again superior to the other subscales, and column 5 reveals this is true when all subscales are included in the model together.

VARIABLES	(1) Game end time	(2) Game end time	(3) Game end time	(4) Game end time	(5) Game end time
standardized BIS score	16.96**				
	(0.0151)				
standardized nonplanning score		16.64**			11.39
		(0.0260)			(0.209)
standardized motor impulsiveness score			12.76*		3.405
			(0.0568)		(0.711)
standardized cognitive impulsiveness score				14.23*	6.500
				(0.0539)	(0.500)
Constant	223.2***	223.2***	224.2***	223.2***	223.0***
	(0)	(0)	(0)	(0)	(0)
Observations	68	68	68	68	68
R-squared	0.086	0.073	0.054	0.055	0.089

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4

We next look at differences between the two treatment conditions in playing times and BIS scores. On average subjects in *decide during* played 228 seconds and those in *decide before* played 221 seconds. There were 34 subjects per condition, and again we observe no treatment effect. We do observe again an effect of the BIS in the direction of what we expect from the perspective of impulsive time preferences: There is a relation between the BIS and the time at which people ended the game and played the lottery. An individual's BIS score was, as in experiment 1, predictive of behavior in the *decide during* condition, but not in the *decide before* condition (with an insignificant difference between the two, interaction term from OLS regression p-value = .598). Table 2.7 shows this result: looking at column 2 the coefficient for the nonplanning score is almost double that in column 1, and is significant in column 2 but not in column 1.

VARIABLES	Game end time *decide before*	 *decide during*
Standardized nonplanning score	10.99	19.29**
	(0.398)	(0.0405)
Constant	220.5***	225.7***
	(0)	(0)
Observations	34	34
R-squared	0.022	0.125

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.5

As seen in Table 2.8, we extend the analysis as in experiment 1 to include risk preferences and a gender dummy variable. As expected, behavior in the Holt & Laury is related to that in our behavioral task. This is somewhat more predictable in experiment 2, since not only did the decision involve a lottery, it involved a monetary one. We find the same qualitative effect of gender, but it is not significant. We did not include the treatment interaction with the BIS in Table 2.8, as the coefficient is small and insignificant, and the results shown hold even if we do so. This information taken together with the regression in Table 2.8 shows that controlling for heterogeneity of risk preferences does not reveal a treatment effect. The BIS score, again in experiment 2, is a predictor robust to controlling for risk preferences.

VARIABLES	(1) Game end time	(2) Game end time	(3) Game end time	(4) Game end time
Standardized nonplanning score	16.64**	17.73**	17.65**	15.82**
	(0.0260)	(0.0151)	(0.0165)	(0.0355)
Number of non-risky choices in the Holt & Laury		-8.582**	-8.505**	-8.899**
		(0.0261)	(0.0297)	(0.0236)
Before condition			-2.284	-4.502
			(0.862)	(0.735)
Male				14.90
				(0.286)
Constant	223.2***	270.4***	271.2***	268.6***
	(0)	(0)	(0)	(0)
Observations	68	68	68	68
R-squared	0.073	0.141	0.142	0.157

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.6
Here we show the nonplanning subscale; using the overall scale yields qualitatively identical results.

2.4 Experiment 3

2.4.1 Experimental Design & Procedures

In experiment 3, subjects decided how long to play Stacker Blocks 3D, though this time there was no risk element involved. Experiment 3 contained 3 parts. The first part is similar in purpose to experiments 1 and 2 already explained. Subjects could pay to play Stacker Blocks 3D, otherwise they had to perform the click task. Costs equaled $\text{playtime}^2/300$, and there were two treatment variations: whether they decided *before* or *during* the task how long to play, and whether they decided on when to *start* or *stop* playing. As in experiments 1 and 2, we hypothesized that subjects in the *decide during* condition would play for longer, and therefore pay more for the opportunity to play the game. The cost that subjects paid was less than the show up fee that they received for participating. In essence they were simply choosing the optimal combination of how much money they wanted and how much time they would like for the game at the given prices. As in experiment 2, the money reward is discounted in the same way in both conditions; only time for the game is discounted differently between the two treatments. Specifically, time spent playing the game in the *decide during* condition is valued relatively more highly than the money reward than it is in the *decide before* condition. We also expected subjects to play longer in the *stop* than in the *start* condition, under the idea that resisting utility already being experienced is more difficult than simply avoiding it in the first place. Experiment 3 controls for the risk elements present in experiments 1 and 2. Several studies have explored the notion that impulsivity could arise out of risk preferences (Andersen, Harrison, Lau, & Rutström, 2008), (Epper, et al., 2010). Therefore in experiment 3 we remove the lottery element and concentrate on certain outcomes.

Next, we elicited subjects' willingness to pay for 5 minutes of Stacker Blocks 3D, against the alternative of the same click task as in previous experiments. We did this to control for heterogeneity of preferences for playing the game. In experiments 1 & 2 decisions do not depend on the preferences for playing the game, but they do depend on risk preferences. Here, decisions depend on preferences for playing the game but risk plays no role. For this reason we controlled with the Holt & Laury in experiments 1 & 2, and with the WTP for the game here in experiment 3. We used the incentive compatible method by Becker, Degroot, & Marschak (1964) so that subjects' optimal bids were their true willingnesses to pay.

Last, we elicited subjects' discount rates using a delay-discounting task. Subjects made decisions about whether to receive an Amazon gift certificate, valid for all purchases on Amazon.de, in a smaller amount at an early point in time or a larger amount at a later point in time. The time between the early and late receipt dates was fixed at 4 weeks. In half of the decisions the early receipt date was the experiment day (immediate reward, called *immediate* condition) and in the other half it was 4 weeks from the experiment day (*front-end delay* condition). Each subject made decisions in both the *immediate* and *front-end delay* conditions in order to have a within-subject comparison. There were forty different pairs of small-early and late-large monetary rewards. The early amounts ranged from 30.08 euros to 39.57 euros, and the late amounts from 30.13 euros to 65.64 euros. The monthly discount rates implied by the choices ranged from .17% to 97.5%. Which of the forty decisions involved had the early reward that was immediate and which had one of 4 weeks was randomly determined for each participant. An alternative procedure, which is simpler and makes it easier to estimate β and δ discount factors, could have also been implemented by having each monetary pair appear twice:

once with an immediate early reward and once with a front-end delay of 4 weeks (so the early option is available after 4 weeks). We did not use this procedure because such a procedure may lead people to see the decisions as more similar, leading them to exhibit more time-consistency. Since our objective was to detect even a very small present-bias, rather than test the robustness of it, we refrained from using this more conservative method. Two subjects were randomly chosen to actually be paid (which, as was all payoff information, was known to subjects). The gift certificates were sent via email to the email addresses provided by the winning participants. All gift certificates were sent at 5pm. This procedure allowed us to keep transaction costs constant while still having the immediate reward as imminent as possible.

We additionally measure reaction times in the delay discounting task. We designed the response procedure in a way that allowed us to cleanly observe reaction times: each decision was made by clicking on a button on either the left or right side of the screen, and after each decision there was an "in-between" screen in which subjects had to click a centered button to proceed. This ensured that reaction times did not depend on a subject's past decision. We use reaction times in order to assess a subject's impulse-control in the brain, through inclusion of the concept of dual system impulse control in the brain (Knoch & Fehr (2007), Knoch, et al. (2006), McClure, et al. (2007)) to extend our analysis of impulsive decision making. Decisions involving a present consumption option have been shown to activate an additional area in the brain which is associated with primary rewards (McClure, et al., 2007), whereas when there is no present consumption only deliberation centers such as the prefrontal cortex are active. Interpreting this in a dual context scenario means that our *immediate* condition will involve more conflict than our *front-end delay* condition, because there is a temptation factor from an additional neural system. Based on the notion that choices for the early option are impulsive, we therefore hypothesize that reaction times in the *front-end delay* condition will be similar for early option choices and late option choices, but that in the *immediate* condition reaction times will be faster for early option choices and slower for late option choices. We expect no difference in the *front-end delay* condition based on the results of McClure, et al. (2007) that decisions when there is no present option are made exclusively with the deliberative prefrontal cortex. Our hypothesis concerning the *immediate* condition derives from the idea that the controlled process, i.e. patience, is slower than the automatic process which is associated with instant gratification.

81 participants took part in this experiment in May and December of 2011.

2.4.2 Results

Most people did not play Stacker Blocks 3D, but for the few who did, it looks like there is a potential self-control problem in the *during* condition. We see this in Figure 2.2. In the *start* condition most notably, we see a cluster of decisions in the top left corner, which are subjects who played essentially the whole time in *decide during* and not at all in *decide before*. However, considering the fact that we only see this phenomenon in the *start* condition, and not in the *stop* condition where we would actually expect the effect to be stronger (because resisting an enjoyable activity already begun is harder than not starting at all), we conclude that this effect is actually the result of mistakes, that is people who mistakenly clicked on the "start game" button. As in any decision, mistakes can be expected, and this particular mistake is more likely in the *start* condition. In the *stop* condition the game appears automatically, and it is quite clear to a subject that any action taken will end the game; whereas when the game screen is not immediately present, it is possible to erroneously think that clicking a button may provide the option to either start the game or the click task. Moreover, the cluster of decisions involves people who played the *whole* time in the *decide during* condition. If we observed decisions to play for rather less time, we would be more likely to interpret it as a true preference, since by then it would have become clear to the participant which of the two tasks (click-task or game) was currently relevant.

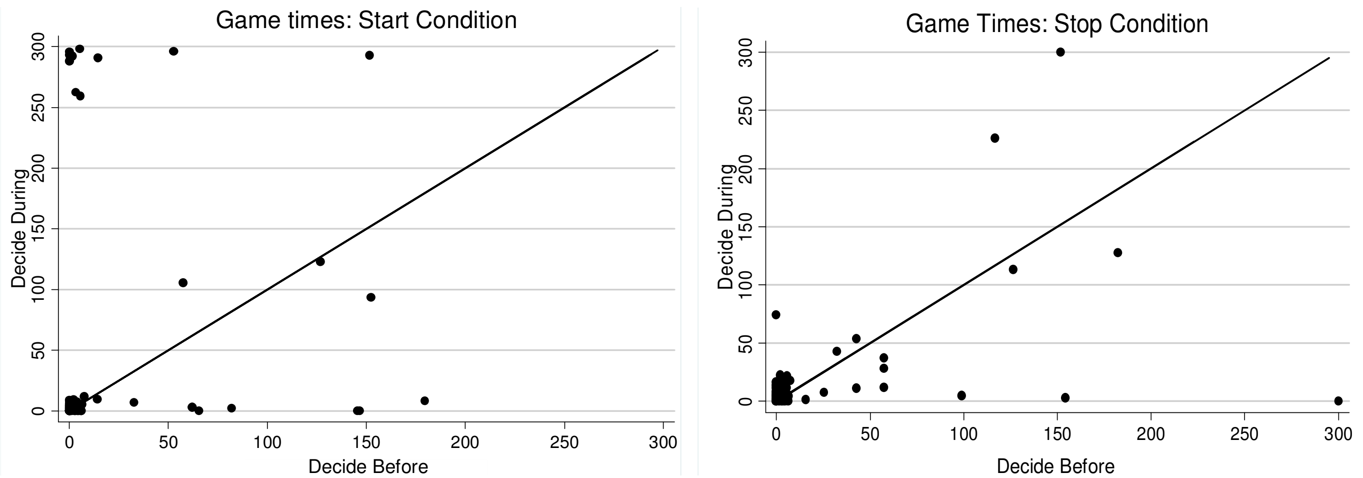


Figure 2.2

We find a significant treatment effect of the *decide before/decide during* manipulation. We also find a significant effect of the *start/stop* manipulation, however it is in the opposite direction of what we expected. The significance of the treatment effect is again not reasonably interpreted as an effect of impulsiveness, but rather a result of errors in the *start* condition. We nonetheless include the regression in Table 2.9 for the sake of completeness. Subjects saw the treatments in randomized order, and there was no trend in game times based on the sequence in which they saw treatments. Including the willingness to pay for the game as a control does not reveal any change in the treatment effects.

	(1)	(2)	(3)	(4)
VARIABLES	Game time	Game time	Game time	Game time
before condition	-12.63*		-12.63*	-12.63*
	(0.0510)		(0.0514)	(0.0517)
start condition		12.48*	12.48*	12.48*
		(0.0555)	(0.0559)	(0.0562)
WTP for 5 min. of the game				7.871
				(0.306)
Constant	29.64***	17.08***	23.40***	20.15***
	(1.85e-05)	(0.000381)	(3.08e-05)	(8.91e-05)
Observations	324	324	324	324
R-squared	0.010	0.010	0.019	0.026

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.7

We next turn back to the BIS and its subscales with Table 2.10. We aggregated decisions within a subject, using a person's average game time as the dependent variable. Nonplanning is again the best subscale, as in both experiments 1 and 2. It is also the best overall predictor, as in experiment 1 (but not 2). Column 2 shows that the nonplanning coefficient is largest (and significant), compared to both other subscale scores (columns 3 and 4). Column 5 includes all subscales in one model, and consistent with both previous experiments, the nonplanning score provides more explanatory power than motor or cognitive impulsiveness.

VARIABLES	(1) Average game time	(2) Average game time	(3) Average game time	(4) Average game time	(5) Average game time
standardized BIS score	5.249				
	(0.242)				
standardized nonplanning score		7.649*			9.467
		(0.0869)			(0.109)
standardized motor impulsiveness score			3.881		0.378
			(0.388)		(0.952)
standardized cognitive impulsiveness score				1.347	-3.882
				(0.765)	(0.502)
Constant	23.32***	23.32***	23.32***	23.32***	23.32***
	(1.18e-06)	(9.51e-07)	(1.28e-06)	(1.40e-06)	(1.22e-06)
Observations	81	81	81	81	81
R-squared	0.017	0.037	0.009	0.001	0.043

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8

The pattern of BIS scores in the *decide during* condition as compared to the *decide before* shows a similar pattern as in experiments 1 and 2, however it is insignificant in experiment 3. The BIS score is insignificant in predicting behavior in the *before* condition, as can be seen in Table 2.11; it is also not predictive in the *during* condition (effect size 10.87 p-value .156). The qualitative result is nonetheless remarkably similar to that in experiments 1 and 2.

VARIABLES	Game time *decide before*	*decide during*
Standardized nonplanning score	4.424	10.87
	(0.293)	(0.155)
Constant	17.01***	29.64***
	(0.000105)	(1.40e-05)
Observations	162	162
R-squared	0.009	0.019

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.9

We next look at the delay discounting task and its relation to BIS overall and sub-scores. Given the unusual nature of our delay-discounting task, we use the following measure to assess present biases. For both sets of decisions (those in *immediate* and *front-end delay*) we calculated the discount rate implied by indifference between the two monetary amounts, which we will call V. For each subject we recorded the maximum value of V for which the subject chose the early reward. Regression shows the nonplanning subscale to indeed be the best subscale of all types of behavior in our experiment (see Tables 2.12 and 2.13 for the maximum discount rate at which the early option was chosen in conditions *immediate* and *front-end delay*), so we will restrict regressions to using the nonplanning subscale. In the case of the *front-end delay* condition, the overall score performs slightly better than the nonplanning score, but the difference is quite small. Looking at Table 2.12 we see in column 2 that the nonplanning score has the most predictive power as compared to the two other subscales as well as the overall score. Table 2.13 shows that the nonplanning score (column 2) is larger than the other subscales (columns 3 and 4), but that, as in experiment 2, the overall BIS score does slightly better than the nonplanning subscale.

VARIABLES	(1) max DR, *immediate*	(2) max DR, *immediate*	(3) max DR, *immediate*	(4) max DR, *immediate*
standardized BIS score	0.0344			
	(0.100)			
standardized nonplanning score		0.0463**		
		(0.0258)		
standardized motor impulsiveness score			0.0132	
			(0.533)	
standardized cognitive impulsiveness score				0.0269
				(0.201)
Constant	0.145***	0.145***	0.145***	0.145***
	(5.47e-10)	(3.47e-10)	(8.62e-10)	(6.75e-10)
Observations	81	81	81	81
R-squared	0.034	0.061	0.005	0.021

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.10

Independent variable: maximum DR at which the early option is chosen in *immediate* condition

VARIABLES	(1) max DR, *front-end delay*	(2) max DR, *front-end delay*	(3) max DR, *front-end delay*	(4) max DR, *front-end delay*
standardized BIS score	0.0354***			
	(0.00280)			
standardized nonplanning score		0.0339***		
		(0.00438)		
standardized motor impulsiveness score			0.0281**	
			(0.0189)	
standardized cognitive impulsiveness score				0.0279**
standardized BIS score				(0.0200)
Constant	0.0954***	0.0954***	0.0954***	0.0954***
	(0)	(0)	(0)	(0)
Observations	81	81	81	81
R-squared	0.108	0.098	0.068	0.067

pval in parentheses
*** p<0.01, ** p<0.05, *

Table 2.11

Independent variable: maximum DR at which the early option is chosen in *front-end delay* condition

Does the BIS measure impatience or impulsivity? Table 2.14 shows the maximum discount rates at which the early option is chosen, in the *immediate* and *front-end delay* conditions, as regressors for the standardized nonplanning score. By setting up the regression in this fashion we look at whether the nonplanning score is more related to impulsiveness (*immediate* condition) or impatience (*front-end delay* condition). Interestingly it seems that the decision made with a front-end delay of 4 weeks has more predictive power for the BIS, implying that it may be measuring something more like impatience than impulsiveness. This is at odds with what we find when looking at the BIS and it's relation to behavior in the *before* and *during* conditions. We see in column 3 of Table 2.14 that even including maximum discount rates from both conditions, the *front-end delay* coefficient remains larger; moreover both coefficients are stable from columns 1 and 2 to column 3.

VARIABLES	(1) Standardized nonplanning score	(2) Standardized nonplanning score	(3) Standardized nonplanning score
Max DR at which chose "early" in *immediate*	1.323**		1.072*
	(0.0258)		(0.0640)
Max DR at which chose "early" in *front-end delay*		2.901***	2.589**
		(0.00438)	(0.0105)
Constant	-0.192	-0.277*	-0.403**
	(0.166)	(0.0549)	(0.0112)
Observations	81	81	81
R-squared	0.061	0.098	0.137

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.12

We find evidence of a general tendency toward present-biasedness. Present-biasedness predicts a tendency for the points to lie above the 45° line in Figure 2.3. Points above the 45° line are people who choose the early option for higher discount rates in the *immediate* condition than in the *front-end delay* condition. We do find a tendency in this direction: the points tend to lie above the line if not clustered near it. A 2-sided sign-rank test reveals that the maximum discount rates at which the early option was chosen in the *front-end delay* condition (on average 9.5%) are significantly different than those in the *immediate* condition (on average 14.5%), with a p-value of 0.017.

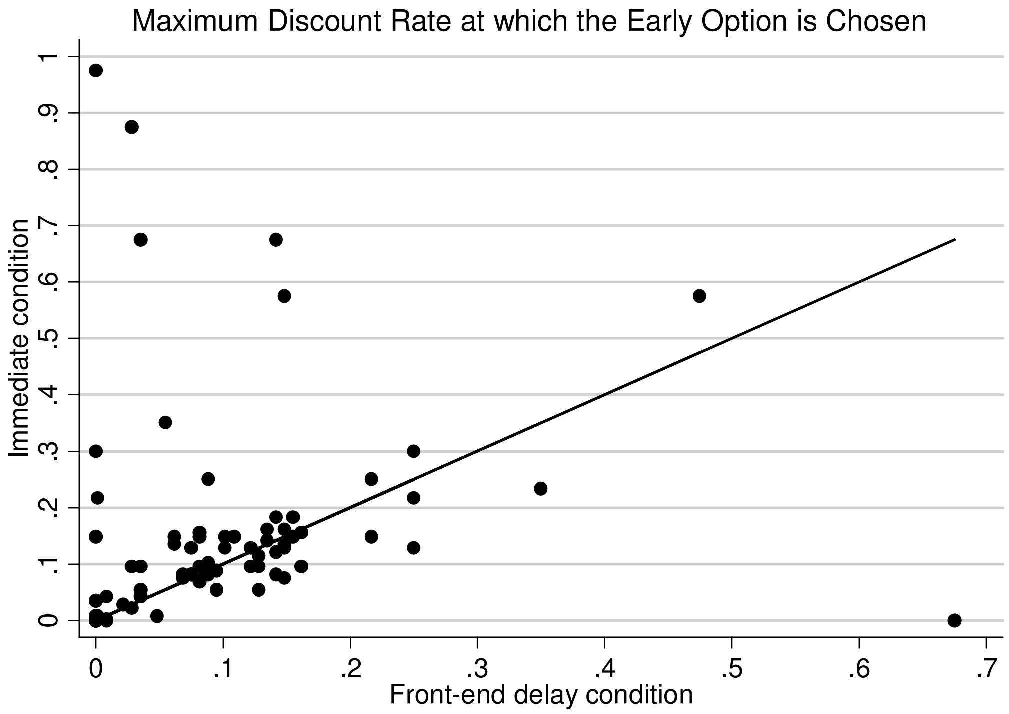


Figure 2.3

In Table 2.13 we present the results concerning reaction times in the behavioral task. We designed the response procedure in a way that allowed us to cleanly observe reaction times: each decision was made by clicking on a button on either the left or right side of the screen, and after each decision there was an "in-between" screen in which subjects had to click a centered button to proceed. This ensured that reaction times did not depend on a subject's past decision. With a fixed effects regression, we show that it is the within-person variance that drives our results. In contrast with our hypothesis, choices for the early option are made more slowly. Also contrary to our hypothesis, this effect is more pronounced in the *front-end delay* condition than in the *immediate* condition. The effect of the variable "chose early" remains positive and significant, though less pronounced, in the *immediate* condition. This implies to us the possibility that the impulsive response is simply to take the higher monetary amount rather than the earliest possible reward. However considering our method of analysis (reaction times) coupled with previous results by McClure et al., (2004) who find evidence using fMRI analysis that when there is an immediate payoff option impulsive centers are active in the brain, we should consider the alternative possibility that conflict level was confounded with choices. That is, we did not include any questions in which the early option was clearly preferable and there was very low conflict, for example where the early reward is larger than the later reward. For this reason we treat this result with skepticism.

VARIABLES	(1) natural log of reaction time	(2) natural log of reaction time	(3) natural log of reaction time
decision number	-0.00968***	-0.00967***	-0.00968***
	(0)	(0)	(0)
immediate condition		0.00424	0.00885
		(0.742)	(0.574)
chose early	0.130***		0.164***
	(0)		(0)
immediate condition* chose early			-0.0587**
			(0.0423)
Constant	0.902***	0.942***	0.898***
	(0)	(0)	(0)
Observations	3,240	3,240	3,240
R-squared	0.107	0.087	0.109
Number of participants	81	81	81

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.13
Fixed effects regression

Why is our task important for economists? Because it affects decisions about money, and is an attribute of impulsiveness although it is not captured by delay discounting. Studies about time preference need to have a consumption decision which is actually consumed in the present; solely money-based decisions are not adequate. Similar to the absence of significant, but nonetheless consistent with experiments 1 & 2, link between behavior in our task and the BIS, we see a slight tendency for decisions in the delay discounting task to be related to behavior in our task (Table 2.14).[2]

[2] We additionally looked at the relation between the delay discounting task and gametimes in the *before* and *during* conditions, but found no differences.

VARIABLES	(1) Average game time	(2) Average game time	(3) Average game time
Max DR at which chose “early” in *immediate*	39.65*		37.71
	(0.0971)		(0.121)
Max DR at which chose “early” in *front-end delay*		30.95	19.96
		(0.458)	(0.634)
Constant	17.56***	20.37***	15.94**
	(0.00231)	(0.000992)	(0.0172)
Observations	81	81	81
R-squared	0.034	0.007	0.037

pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.14

2.5 Conclusion

We investigate impulsive decision making with a novel paradigm as a reward medium which cannot be temporally reallocated: entertainment in the form of a computer game. The experiments were designed to study time preferences within the time-frame of the experiment, i.e. between one and two hours, and therefore involved a reward mechanism that could not be reallocated to a time outside the experiment period. We relate behavior in this paradigm to the Barratt Impulsiveness Scale (BIS). In all experiments there were two treatment conditions, one in which subjects made decisions before the experiment task began and one in which they made decisions during the experiment task. We expected subjects to experience more temptation in the latter condition. In experiment 1 subjects played a lottery in which the reward was playing the game Stacker Blocks 3D. Since the experiment involved risky elements, we also had subjects perform the Holt & Laury risk task. In experiment 2 we reintroduced monetary incentives to test the robustness of impulsive motivation in the face of strong opposing incentives (that is, cold hard cash). We additionally reversed how time preferences and risk preferences would motivate a subject to behave in time: in experiment 1 impulsiveness (risk aversion) would motivate a subject to take action earlier (later), and in experiment 2 impulsiveness would motivate a subject to take action later and risk aversion to take action earlier. In experiment 3 we discarded all risk elements to uncouple risk from the decision altogether, and allowed subjects to pay to play the game. Costs were an increasing and convex function of the amount of time subjects played the game. We additionally elicited subjects' willingness to pay for a fixed amount of the game to allow us to control for heterogeneity of preferences for the game. Last as part of experiment 3, subjects performed a delay discounting task, choosing between a smaller, earlier reward or a later, larger reward (both in the form of an Amazon.de gift certificate). We did this to check for signs of a bias toward earlier rewards in a dimension without a truly immediate reward.

In experiment 1 we show that a link exists between impulsiveness in the experimental task and BIS scores in the direction that we expected, indicating that impulsiveness does play a role in our experimental paradigm. In the *decide during* condition there is a significant link between scores on the BIS and decisions about when to make their attempt to play Stacker Blocks 3D, whereas in the *decide before* condition we do not observe this relation. In experiment 2 we show that the same result holds even when monetary incentives would motivate a person not to act impulsively. In experiment 3 we find a relation between (1) the delay discounting task and the BIS, (2) between our behavioral task and the BIS, as well as (3) the delay discounting task and our behavioral task. We observe a treatment effect of the *before/during* treatment variation. We again observe what looks like a stronger relation between BIS scores and the length of time subjects played the game in the *decide during* condition than in the *decide before* condition, though the significance of the effect is borderline. We take this as an indication that our behavioral treatment, although somewhat weak, does induce impulsiveness. Furthermore, we see definite signs of present-bias in the delay discounting task. Moreover it seems that the construct of impulsiveness as pyschologists (or at least the BIS) conceive of it is in line with that of economists, making the prospect for interdisciplinary exchange promising.

In all three experiments we look at the subscales of the BIS and how they relate to impulsive decisions concerning the computer game. In experiment 2 the overall BIS score predicts behavior better than any one subscale, and in experiments 1 & 3 it is the nonplanning subscale that predicts behavior the best. Looking exclusively at the three subscales, "nonplanning", "cognitive impulsiveness", and "motor impulsiveness", nonplanning is always a superior predictor to the other two subscales. We consider the nonplanning measure to be a facet of impulsiveness most similar to what economists conceptualize with a discount rate. The fact that the nonplanning scale predicts impulsiveness better than cognitive or motor impulsiveness, even in an experimental paradigm where the latter two could very well be expected to motivate behavior, speaks positively for the concept of a discount rate as a useful description of decision makers' motivation and as a psychological construct. It also provides an indication that both behavioral measures and questionnaire answers can be depended on, independently or together, as different ways of measuring the same underlying preferences and traits of a decision maker.

Despite the present bias in the delay discounting task, we do not find clear evidence of a self-control problem in the behavioral task. This may be due to the weakness of our temptation good. However, it may as well be because controlling one's impulses is a good which can, if need be, be produced by the self. Producing this control may however inflict a psychological cost, leading it to be better to purchase it externally if the cost is low enough. We cannot observe whether this is the case in our experiment. Houser et al. (2010) show demand for impulse control, but this does not necessarily imply a self-control problem. Given their evidence it is reasonable to consider the possibility that subjects in our experiment could have benefited from purchasing an impulse-control product, although they did not exhibit any self-control problem.

Last, we look at reaction times in the delay discounting task from experiment 3 to asssess the interplay between automatic and controlled processes in two situations with different levels of temptation. We find that the pattern of reaction times are similar in the two conditions, and that contrary to our hypothesis decisions in favor of early smaller rewards are made more slowly than decisions in favor of larger later rewards. However, we did not include any questions in which the early option was clearly preferable and there was very low conflict, for example where the early reward is larger than the later reward, meaning we must treat this result with skepticism.

We find the link between the different types of assessments of impulsiveness we have provided in the 3 experiments promising, and it speaks for the importance of continuing the search for novel methods of reward in an experimental setting, as well as rewards which are powerful enough to be tempting.

Appendix

Translated Instructions for Experiment 1 (with click task)

The following experiment will last 16 minutes and 40 seconds, which is 1000 seconds. During this time you have a task to work on. The task consists of clicking a button every 10 to 20 seconds. You also have the possibility to start a game. The game is like Tetris, and is called Stacker Blocks 3D. You can try to start the game at a time of your choice. Whether it works depends on chance. If the attempt is successful you can start the game, and you may play the game until the end of the 1000 seconds. If it is not successful, you must work on the task for the rest of the 1000 seconds. There is no second attempt possible. The later you attempt to start the game, the higher the chance that you will actually be able to start the game. However, the later you attempt to start the game, the less time you have to play the game from the 1000 seconds. With every second that passes, the probability that it works increases by 0.1%. That means, if you start the game right at the beginning it won't be successful and if you try at the very end it will be successful for sure. If you make your attempt after, for example, 3 minutes, it has an 18% chance of being successful, since 180 out of 1000 seconds have already gone by.

In this experiment you will earn one point per second. You earn these points during the game automatically. During the task you do not receive these points automatically. Red buttons will appear which you must click on to receive these points. Every 10 to 20 seconds, a new button will appear. You must click every button. Once you have clicked it, it will disappear. If you fail to click a button before the next one appears, you will receive no points for the time in which the button was visible. So when you miss a button once, you receive 10 to 20 points fewer than the 1000 points you could theoretically receive. These loss points will be shown to you. As already mentioned above, you receive the points during the game-playing time without having to do anything to get them.

The points you earn during the experiment will be converted to euros as follows:

100 points = € 0.30

[Decide Before]: You will see the following screen:

Enter how many seconds you would like to wait before the attempt. If you want to see the probability that the attempt is successful, press the grey button. You can display the probability as often as you like. The longer the time is that you enter, the higher the probability that the game actually starts.

Once you have decided, click the red “OK” button.

This is the task screen. You can see your wait time, time, the possible game time and loss points. Every 10 to 20 seconds a red button will appear with the word “click” written on it. You have between 10 and 20 seconds time to click the button. Once you have pressed it, it will disappear.

You will then see the following screen:

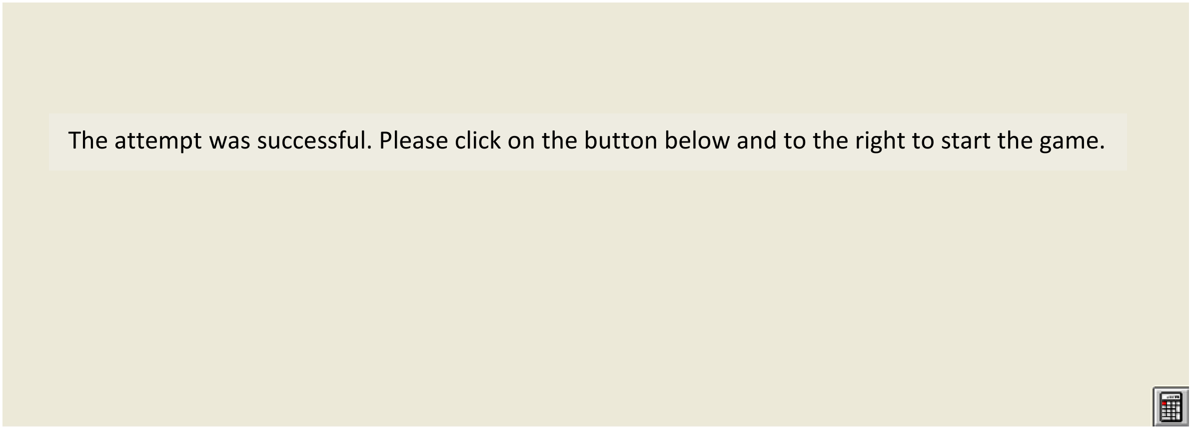
The next red button appears in a different spot. You have time until the next button appears to press the button. If you don’t click the red button in this time, you receive no points for the time in which the button was visible. That means you get 10 to 20 points fewer. The sum of lost points will be shown to you onscreen.

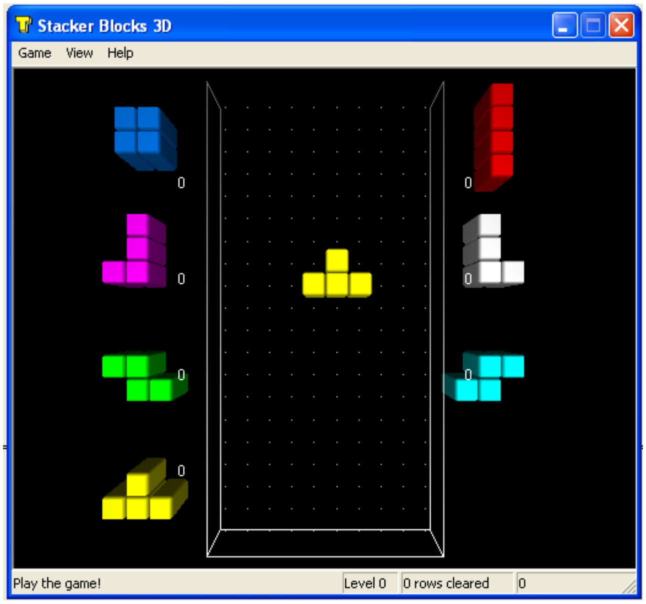
[Decide During]: When the experiment begins you will see the following screen:

This is the task screen. You can see the time, the possible playing time and the loss points. Every 10 to 20 points a red button will appear which has "click" written on it. You have between 10 and 20 seconds time to click on the button. Once you have pressed it, it will disappear. The next red button will then appear in a different place. You have time until the next button appears to click on the button. If you do not press the red button within this time you will not receive any points for the time during which the button was visible. That means you will receive 10 to 20 points fewer. The sum of your lost points will be shown to you on screen.

On the screen you can also see the probability with which your attempt will be successful. Underneath you can see the grey "attempt" button. When you would like to attempt to start the game, click the "attempt" button. You can make your attempt to start the game at any time. You have however only one attempt. The longer you wait, the higher the probability that the game will actually start. If the attempt is successful, you can start the game, and you may play the game until the end of the 1000 seconds. If it is not successful, you must work on the task until the end of the 1000 seconds.

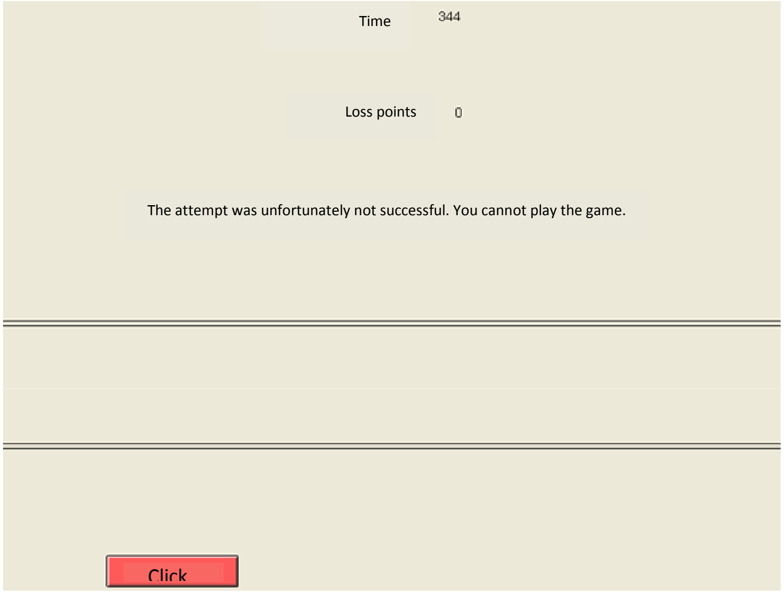
[Both]: If your attempt is successful, you will see the following screen:





The goal is to make complete rows of blocks. When you have made a row, the corresponding blocks disappear. Press the appropriate arrow key on the keyboard to move the blocks left, right, or down. To turn the blocks, press the up arrow key. You may play the game until the game time is over. During this time the points will be automatically credited to you.

If your attempt is not successful, you will see the following screen. If this is the case you must work on the task until the end of the 1000 seconds.



Exercises

In the following is a new example with a few control questions.

Your answers to the control questions have no influence on your payment at the end of the experiment.

1) Imagine you clicked every red button during the task time and played the game.
   How many points will you get?

2) Imagine you missed two red click buttons, where the first was visible for 12 seconds and the second 18 seconds. You did not play the game.
   How many points will you get?

3) Please complete the table.

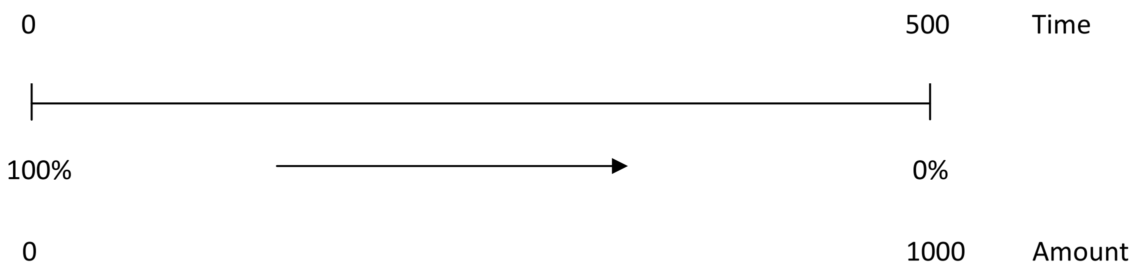
Wait time in seconds	Probability that the attempt is successful (You may play the game)	Length of time you may play the game if the attempt is successful
320	32%	1000 - 320 = 680
610		
230		
100		
820		

Experiment 2

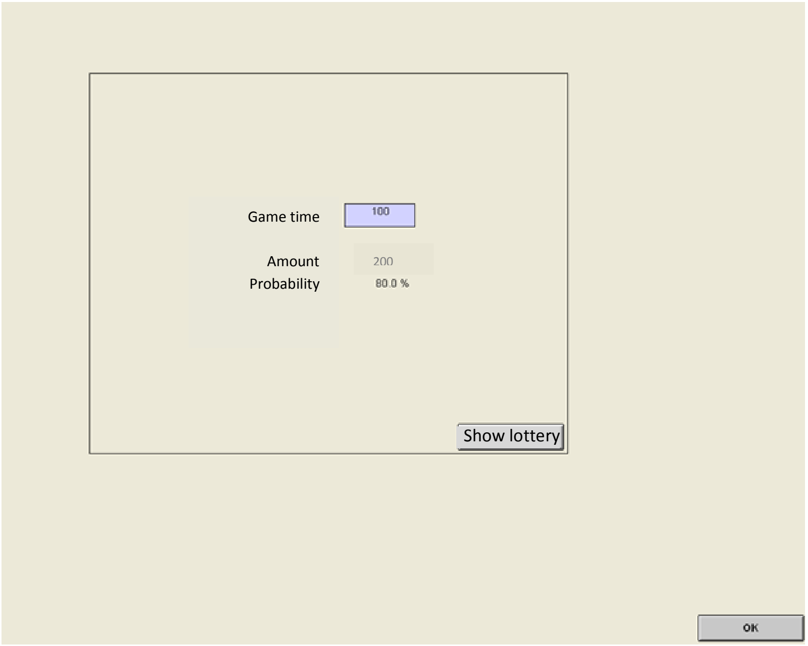
The following experiment lasts 8 minutes and 20 seconds, which is 500 seconds. In this experiment you receive a base income of one point per second. You receive these points automatically. In addition you can play a game in this experiment. The game is similar to Tetris, and is called Stacker Blocks 3D. The game starts automatically and you can end the game at a time you wish. You can win a sum of money, depending on the point in time you end the game.

The later you end the game, the higher the sum you can win. But the later you end the game, the smaller the chance that you actually receive the sum. That means when you end the game at the very beginning or end, you will not receive any money. With every second that passes the probability of success decreases by 0.2% and the amount you could win goes up by two points. If you for example end the game after 100 seconds, you can receive a sum of 200 points with a probability of 80%. If you end it after 400 seconds, you can receive a sum of 800 points with a probability of 20%. The current probability and current amounts you could win will always be shown on the screen.

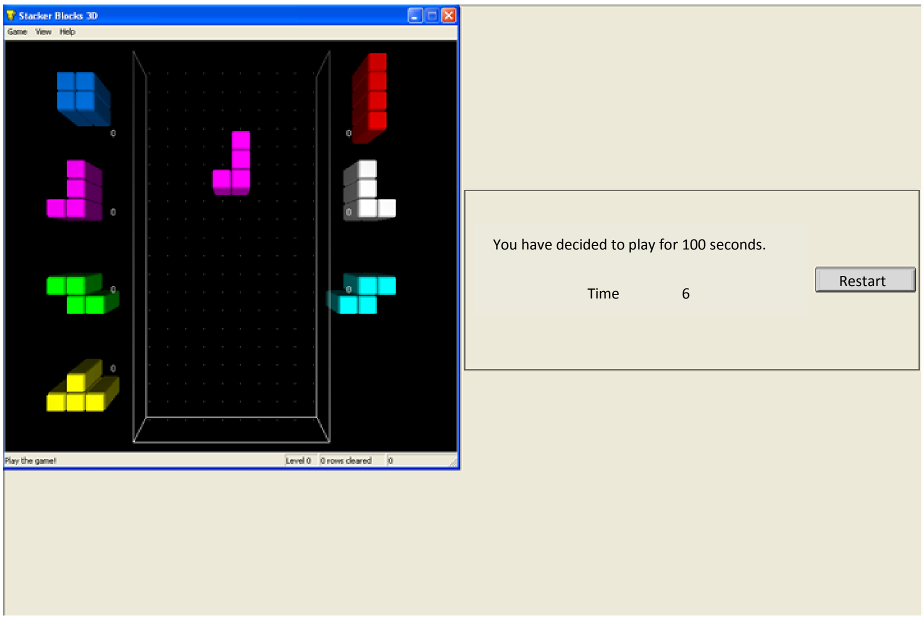
Below is an illustration of how the probability and the amount change over time.



You will see the following screen:

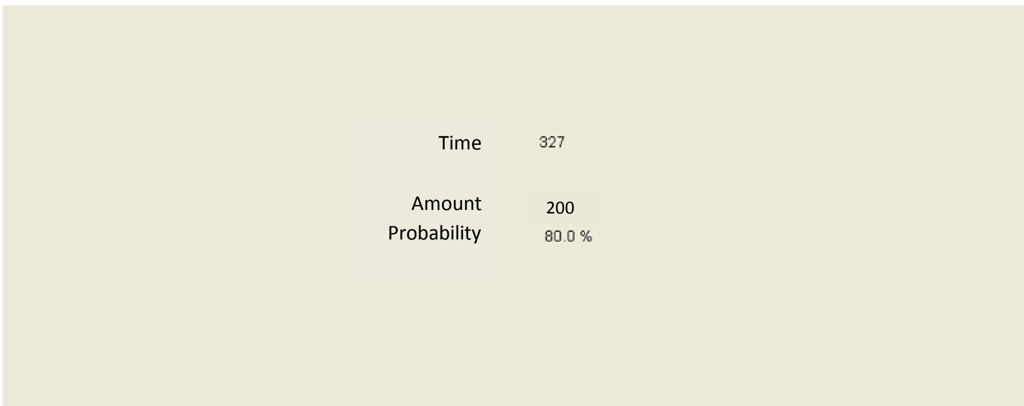


Enter the number of seconds after which you would like to end the game. You can click on the "show lottery" button to see what amount goes with the entered game time, and with what probability you actually receive the amount. To confirm your decision, click the "OK" button.You will then see the following screen:



The goal is to make complete rows of blocks. Once you've made a row, the blocks in it will disappear. Press the appropriate arrow key on the keyboard to move the blocks to the left, right, or down. To turn the blocks, press the up arrow key. You can play the game until the game time is over. If the game window happens to close, you can restart the game with the "restart" button.

When the game time is up, you will see the following screen.



You must wait until the end of the 500 seconds. It is not possible to start the game a second time.

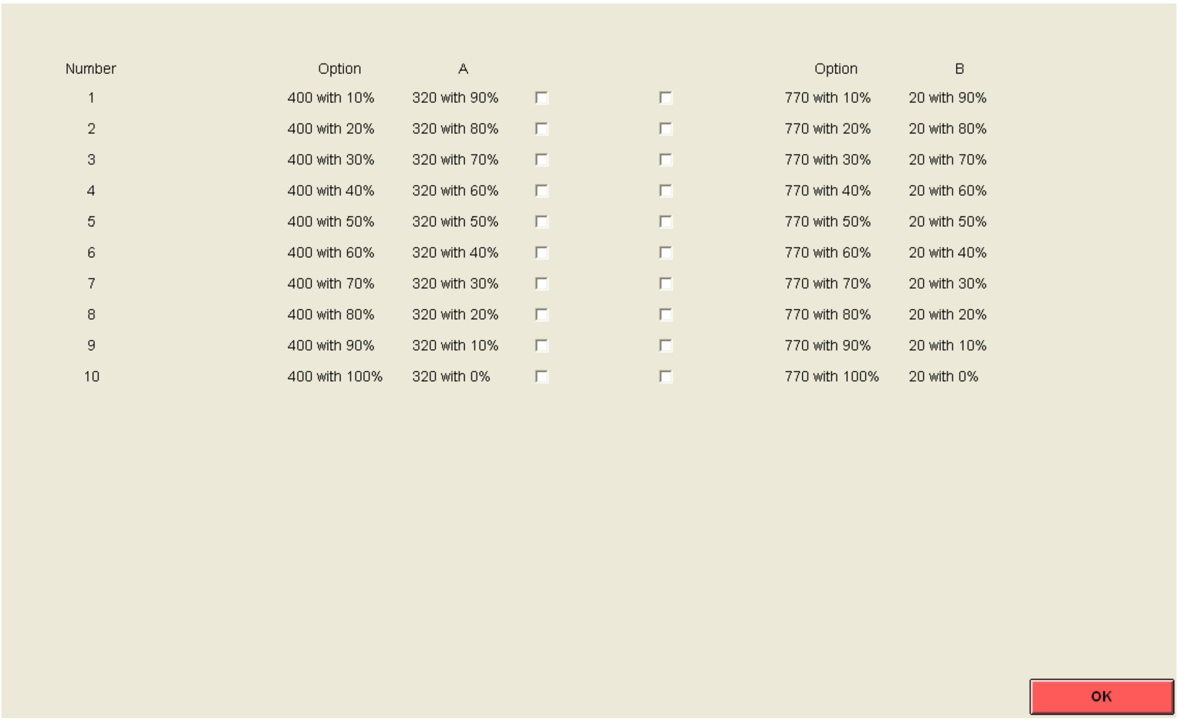
Holt & Laury Instructions

(included in experiment 2 and some sessions of experiment 1)

100 points = 0.8 euros

In this experiment you will make 10 decisions. For each decision you have the possibility to choose either option A or option B. The options differ in the amount of profit you can achieve. With option A you can obtain 400 points at the highest and 320 points at the lowest, with option B the profits are 770 points at the highest and 20 points at the lowest. In each decision the probabilities of the high and low profits are different.

You will see the following screen:



Number	Option	A			Option	B
1	400 with 10%	320 with 90%	☐	☐	770 with 10%	20 with 90%
2	400 with 20%	320 with 80%	☐	☐	770 with 20%	20 with 80%
3	400 with 30%	320 with 70%	☐	☐	770 with 30%	20 with 70%
4	400 with 40%	320 with 60%	☐	☐	770 with 40%	20 with 60%
5	400 with 50%	320 with 50%	☐	☐	770 with 50%	20 with 50%
6	400 with 60%	320 with 40%	☐	☐	770 with 60%	20 with 40%
7	400 with 70%	320 with 30%	☐	☐	770 with 70%	20 with 30%
8	400 with 80%	320 with 20%	☐	☐	770 with 80%	20 with 20%
9	400 with 90%	320 with 10%	☐	☐	770 with 90%	20 with 10%
10	400 with 100%	320 with 0%	☐	☐	770 with 100%	20 with 0%

OK

In each row you make the decision whether to choose option A or option B. To do so check the box by either A or B. The probability to achieve the higher amount increase with every row for both option A and option B. Regardless of which option you choose, you will always make a profit.

As soon as you have made all 10 decision, please click the OK button to continue.

Afterwards it will be determined which of your decisions will be relevant for payment. The person conducting the experiment will roll a 10-sided die. One participant will type the number that was rolled into the program. The number rolled determines which row is relevant for payment. Please note that the number rolled corresponds to the code number (see the following picture) and not the row number.

After the experiment you will see the following image. It shows you which decisions you made and additionally contains a series which assigns a code number to each decision. If the number 7 is rolled for example, then below the second to last decision would be relevant for payment later on.

Number	Code	Option	A			Option	B
1	1	400 with 10%	320 with 90%	☐	☐	770 with 10%	20 with 90%
2	6	400 with 20%	320 with 80%	☐	☐	770 with 20%	20 with 80%
3	2	400 with 30%	320 with 70%	☐	☐	770 with 30%	20 with 70%
4	10	400 with 40%	320 with 60%	☐	☐	770 with 40%	20 with 60%
5	8	400 with 50%	320 with 50%	☐	☐	770 with 50%	20 with 50%
6	9	400 with 60%	320 with 40%	☐	☐	770 with 60%	20 with 40%
7	3	400 with 70%	320 with 30%	☐	☐	770 with 70%	20 with 30%
8	5	400 with 80%	320 with 20%	☐	☐	770 with 80%	20 with 20%
9	7	400 with 90%	320 with 10%	☐	☐	770 with 90%	20 with 10%
10	4	400 with 100%	320 with 0%	☐	☐	770 with 100%	20 with 0%

If you in this example chose option A, you could obtain 400 points with a probability of 90% and 320 points with a probability of 10%. If you chose option B, you could obtain 770 points with a probability of 90% and 20 points with a probability of 10%. Afterwards your income will be shown to you.

Exercises

In the following you will find a new example with some control questions.

Your answers to the control questions have no influence on your payment at the end of the experiment.

1) What is the minimum number of points you can obtain?

Option A _________

Option B _________

2) What is the maximum number of points you can obtain?

Option A _________

Option B _________

3) Use the following picture to answer the questions.

Number	Code	Option	A			Option	B
1	1	400 with 10%	320 with 90%	☐	☐	770 with 10%	20 with 90%
2	6	400 with 20%	320 with 80%	☐	☐	770 with 20%	20 with 80%
3	2	400 with 30%	320 with 70%	☐	☐	770 with 30%	20 with 70%
4	10	400 with 40%	320 with 60%	☐	☐	770 with 40%	20 with 60%
5	8	400 with 50%	320 with 50%	☐	☐	770 with 50%	20 with 50%
6	9	400 with 60%	320 with 40%	☐	☐	770 with 60%	20 with 40%
7	3	400 with 70%	320 with 30%	☐	☐	770 with 70%	20 with 30%
8	5	400 with 80%	320 with 20%	☐	☐	770 with 80%	20 with 20%
9	7	400 with 90%	320 with 10%	☐	☐	770 with 90%	20 with 10%
10	4	400 with 100%	320 with 0%	☐	☐	770 with 100%	20 with 0%

If the person conducting the experiment rolls a 9, which decision will be relevant for payment?

With what probability could you obtain the high amount?

Option A ________

Option B ________

Experiment 3a

The following experiment will last 5 minutes, which is 300 seconds. In this time you have work to perform. The work consists of clicking a button every 10 to 20 seconds. You also have the possibility to play a game. This game is like Tetris, and is called Stacker Blocks 3D. You must pay for this game. The longer you play, the higher the price. The costs will be explained later on.

In this experiment you earn one point per second. During the work red buttons will appear which you have to click in order to get points. Every 10 to 20 seconds a new button will appear. You must click on every button. When you click it, it will disappear. If you do not click on a button before the next one appears, you will not receive any points for the time in which the button was visible. If you miss one button you will then receive 10 to 20 points less than the 300 points you theoretically could have received. These loss points will be shown to you on the screen. During the game you do not have to click any buttons: you will earn one point per second automatically. You receive points during the game time without having to do anything.

There are four variants of this experiment, so four times 5 minutes.Each variant will be explained at the beginning on screen.

The number of points you earn during the experiment will be converted to euros at the end as follows:

100 points = 0.30 euros

This is the work screen.

Every 10 to 20 seconds a red button will appear which has "click" written on it. You have between 10 and 20 seconds time to click the button. Once you have clicked it, it will disappear. The next red button will then appear in a different place.

You have time to click on the button up until the next red button appears. If you do not click on the red button within this time, you will not receive any points for the time during which the button was visible. That means you will get 10 to 20 points less. The sum of the loss points will be shown to you on screen.

You can, as mentioned above, also play the game. During the game time you do not have to perform the work task, and **you receive one point per second anyway.** An important note is that you cannot interrupt the game time. Once the game time has begun, you may naturally play the game several times, but the game time cannot be divided up. Also, you must pay for the game. The costs are calculated as follows:

$$\frac{Gametime^2}{300}$$

The costs will always be calculated automatically and shown on-screen.

Below are the costs for some possible game times:

Game time (in seconds)	**Costs (in points)**	**Game time (in seconds)**	**Costs (in points)**
10	0.33	160	85.33
20	1.33	170	96.33
30	3.00	180	108.00
40	5.33	190	120.33
50	8.33	200	133.33
60	12.00	210	147.00
70	16.33	220	161.33
80	21.33	230	176.33
90	27.00	240	192.00
100	33.33	250	208.33
110	40.33	260	225.33
120	48.00	270	243.00
130	56.33	280	261.33
140	65.33	290	280.33
150	75.00	300	300.00

In the last row (right) you can see that the maximum cost is 300 points. 300 points is exactly the maximum amount you can earn. That means that during the game time there is no danger that the game costs more than you earn.

The game works as follows: [Explanation of Stacker Blocks 3D as in previous experiments]

Exercises

In the following you will find a new example with some control questions.

Your answers to the control questions have no influence on your payment at the end of the experiment.

1) Imagine you clicked on every red "click" button during the work time and played the game for 60 seconds.
   How many points do you get?

   ____________ – ____________ – ____________ =

   Points earned Loss points Game costs

2) Imagine you missed two red "click" buttons during the work time; the first time the button was visible for 12 seconds, the second time for 18 seconds. You played the game for 30 seconds.
   How many points do you get?

3) Imagine you clicked every red "click" button during the work time and played the game for 250 seconds.
   How many points do you get?

4) Imagine you missed two red "click" buttons during the work time; the first time the button was visible for 12 seconds, the second time for 18 seconds. You played the game for 150 seconds.
   How many points do you get?

Experiment 3b

In this experiment you will be requested to indicate the amount of money it is worth for you to obtain the possibility to play the game for 5 minutes (300 seconds). The game is called Stacker Blocks 3D and is identical to Tetris. It is not certain in advance that you will actually get to play the game. If you are not allowed to play the game, you will receive money.

What is always true is that you will reach the best result for you personally if you enter the true amount that it is worth to you to have a game time of 5 minutes.

The procedure is the following:

You enter on the screen the value of being able to play the game. Then the computer will pick an amount of money between 0 and 3.00 €. This amount is randomly chosen and every amount in steps of 0.10 € (0.00€; 0.10€; 0.20€; 0.30€; usw. bis 3.00 €) will be drawn with the same probability. You can imagine it as if there were a jar with a ball for every amount of money between 0 and 3 € and one of these balls is randomly drawn. If the amount of money drawn by the computer is smaller or equal to your personal value of the game, then you will play the game. If the amount of money drawn by the computer is higher than your personal value of the game, then you will not play the game, rather you receive the amount of money randomly drawn by the computer.

Here are a few examples:

Imagine your value for getting the possibility to play Tetris for 300 seconds is 2 € and thecomputer draws an amount of 1.50 €. Since your value for playing Tetris is higher than the drawn amount, you may play Tetris.

Imagine your value for getting the possibility to play Tetris for 300 seconds is 1 € and the computer draws again an amount of 1.50 €. Since your value for playing Tetris is lower than the drawn amount, you receive the 1.50 €.

As you can see, you always receive, regardless of what happens, whatever has the higher value for you, either the opportunity to play Tetris or the money. For this reason it is best for you to give the true value for you of having the opportunity to play Tetris, no higher and no lower. To clarify why this is so, consider again the example from before.

Imagine the value that gives you the opportunity to play Tetris for 300 seconds is 2 € for you, but you enter a value of only 1 €. The computer draws an amount of 1.50 €. Since your entered value for playing Tetris is less than the drawn amount, you cannot play Tetris, although it would have had a higher value for you than the money that you receive.

As you can see, it is therefore best for you to enter the true value, so that you always receive whatever has the higher value for you, be it the money or the opportunity to play the game for 5 minutes.

As in the previous experiment you receive aside from the above-mentioned income a further income, this time of one **cent** per second. If you do not play Tetris, you must perform the same "click task" as in the previous experiment.

Here too it is true: if you do not click on the button when it appears, you will not receive any money for that time period.

Experiment 3b Exercises

1) You enter the value that it is worth to you to have the possibility to play Tetris for _____ minutes.

2) Imagine you enter an amount of 20 cents to be able to play the game. The computer draws a random value of 90 cents.

    a. Will you play the game or receive the money?

    b. Do you receive 1 cent per second?

    c. Do you have to perform the "click task"?

3) Imagine you enter an amount of 1.10 € to be able to play the game. The computer draws a random value of 90 cents.

[Questions a, b, and c as in question 2]

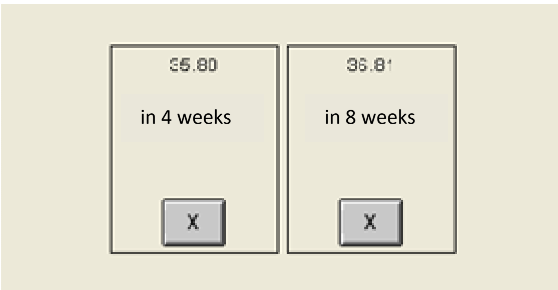
4) Imagine you enter an amount of 1.10 € to be able to play the game. The computer draws a random value of 1.10 €.

[Questions a, b, and c as in question 2]

Experiment 3c

In this experiment you must decide whether you would rather have an Amazon gift certificate of a lower value at an earlier point in time or an Amazon gift certificate of a higher value at a later point in time. en Wert zu einem späteren Zeitpunkt haben wollen. Different situations will be shown on screen, for example "'Would you rather have 35.80 in 4 weeks' or'36.81 in 8 weeks'?". You must then decide in each case what you would rather have.

In total there are 40 situations in which you will make a decision. That is, you will make in total 40 decisions. For every situation, the screen looks as follows:



If you prefer the lower Amazon gift certificate at an earlier point in time, then click on the left "X" button. If you prefer the higher Amazon gift certificate at a later point in time, then click on the right "X" button. Your decision is binding, this means if you have decided for an amount you cannot change it anymore. Once you have decided for an amount at a certain time and have clicked on the "X" button, the following screen will appear:

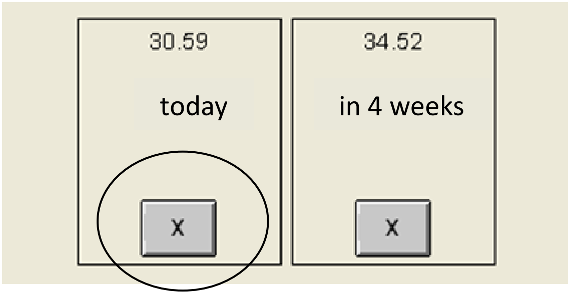
On this screen you can see the number of your next situation. When you click on the “Continue” button, the displayed situation will appear (on the example on-screen, situation 2).

At the end two participants will be drawn. For each of these two participants, one situation will be drawn. The drawn participants receive the amount of money (as an Amazon gift certificate) at the point in time they chose for the drawn situation. Let us assume the above shown decision was drawn. Then you receive, if you are drawn and you chose “35.80 in 4 weeks” in the above situation, an Amazon gift certificate at a value of 35.80€ in 4 weeks by email. If you chose “36.81 in 8 weeks”, then you would receive an Amazon gift certificate at a value of 36.81€ in 8 weeks by email.

We will send you the Amazon gift certificate by email at 5pm on the day you chose. The people who hav been drawn will be requested to provide their email addresses so that they can receive their gift certificates. The sending of the gift certificates will be carried out anonymously and independently of all the experimental data. The Amazon gift certificate can be used for all purchases on Amazon.de without entering any personal information.

Experiment 3 exercises

1) How many participants will be paid?
2) Imagine that you are drawn to be paid and the decision below was chosen as relevant for payment. You clicked on the circled “X” button.

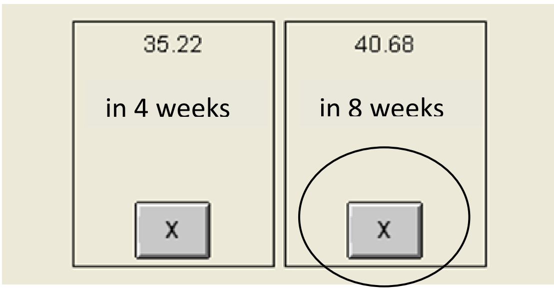


How much will your Amazon gift certificate be worth?

When will you receive the Amazon gift certificate?

In _______ weeks at ________ o'clock

3) Imagine that you are drawn to be paid and the decision below was chosen as relevant for payment. You clicked on the circled "X" button.



How much will your Amazon gift certificate be worth?

When will you receive the Amazon gift certificate?

In _______ weeks at ________ o'clock

References to chapter 2

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3. Handing Over the Reins: On the Social Nature of the Illusion of Control

Abstract

I study the “illusion of control” bias (Langer, 1975) in a laboratory experiment, pursuing the idea that an illusion of control may result in both increased risk-taking (as past work has assumed) or decreased risk-taking. I perform a within-subjects analysis, varying illusory control, in which subjects were not informed about the exact treatment variation beforehand, allowing a between-subjects analysis as well. I further include a treatment in which people can choose whether to keep or give away control, making it possible to sort subjects according to their potentially heterogeneous control biases. I observe conflicting evidence as to whether there is an illusion of control bias: Empirically there is one within subjects, but not between subjects. To make sense of the conflicting evidence I examine the heterogeneity of behavior and conclude that there is no illusion of control bias.

3.1 Introduction

Imagine you are sitting at the helm of a horse-drawn carriage, and next to you is your twin sibling. Better yet, next to you is a perfect clone of yourself, with the same preferences, skill, and ability as you. Now, being in control of the reins involves risk of accident, meaning the most capable person should hold on to them. In our example though, it does not matter to whom control goes, because both parties are exactly the same in every way. Aside from any utility or disutility from the act of driving the carriage (let's assume the passenger has an equally entertaining or tedious task to perform), the question we are asking in is: would you rather keep hold of the reins or hand them over to your companion? What this paper explores is the so-called illusion of control, in which people conflate skill with chance. If you have the security of holding on to the straps of leather, do your risk preferences change compared to your clone companion? Moreover, do you have an aversion to handing over the reins, and if so, is it merely the fact of giving them away that causes distress, or is it the fact that they are going into the hands of another person?

Since the seminal paper of Ellen Langer, the illusion of control has received much attention in psychology and recently in economics. The illusion of control occurs in situations of chance, in which people act as if skill plays a roll. In daily life most often risky situations do involve an element of skill, and Langer hypothesized that people perceive them to co-occur even when they in reality do not. Many experimental studies have confirmed the truth of this notion, and shown that people take more risk when they are given control over actions in an experiment, despite the fact that control over the actions results in no increased control over the outcomes (Langer, 1975). The hypothesis of Langer's paper was that people conflate chance situations with skill situations. By showing increased risk-taking in situations where the participant had more control, these studies showed additionally that participants on average inherently valued their own skill over another's.

Presson & Benassi (1996) performed a meta-analysis on 53 experiments from 29 studies on the illusion of control. The effect from every single experiment was positive, indicating that there is an illusion of control bias, and that people value control on average positively. Since studies were between-subject, however, the illusion of control bias is potentially vastly larger than previously reported, because of responsibility attribution which works in the opposite direction as illusion of control. In this paper I wish to draw attention to a motivation which would work to counteract the effect of overconfidence. Overconfidence is the tendency to overestimate one's relative capabilities, that is compared to others. Svenson (1981) showed that most people estimated themselves to be above average on many positive traits, although that cannot be possible for a symmetrically distributed attribute. I examine the notion that in addition to an illusion of control bias, there is a phenomenon which would, in our experimental setting, cause the exact opposite of an illusion of control bias. I propose that in addition to ideas about overconfidence linked to the illusion of control, people may also be motivated by the desire to avoid regret, which would occur when one feels responsible for a bad lottery outcome. A person possessing illusory control over a situation not only has control, but also the accompanying responsibility; indeed the adage "with great power comes great responsibility" makes our point quite succinctly. Another way to put it is that for some, the illusion of control will cause them to wish to cede control. Those with a traditional illusion of control bias correspond to overconfident decision makers, and those with an illusion of control, coupled with a distaste for regret, wish to avoid responsibility involved in a decision.

Past studies have not taken into account the role responsibility plays in shifting control, which may in part determine control biases. For example, in one of Langer's studies, there were two conditions manipulating illusion of control, one in which the subject received illusory control and one in which the experimenter did. The latter was intended to be a situation in which the subject simply did not receive illusory control, but in fact it was a more complex condition, in which illusory control was given to another individual. I think the difference between giving up control, to for instance a random mechanism or machine, is much different than giving it up to another human being, and it is this notion that this paper investigates. Gamblers are notably superstitious, and I suspect that the idea of giving over ones' dice to another person involves more anxiety than, say, letting them be rolled by putting them in a bingo cage (though most likely both would result in some anxiety). Indeed the evidence from recent literature in economics on the nature of the illusion of control is mixed: Fong & McCabe (1999) found a lower value placed on a lottery when the participant had illusory control (when they got to roll the dice instead of the experimenter), and

Charness & Gneezy (2010) found no illusion of control bias at all. This implies that there may indeed be substantial variation in the whether control or responsibility concerns are more important to an individual.

Charness & Gneezy (2010) suggest that the illusion of control is a manifestation of overconfidence. Regret, as defined in Zeelenberg (1999), "is a negative, cognitively based emotion that we experience when realizing or imagining that our present situation would have been better, had we decided differently." In his work, he presents the idea that decision makers are aware of this extra negative emotion, and decide in a way to reduce the potential regret associated with the outcome of their choices.

This paper uses the idea of self-signaling (Bénabou & Tirole, 2011; Koszegi, 2010) as the source from which both overconfidence and regret minimization spring in our experiment. On the one hand, an overconfident subject maintains an inaccurate belief about her skills, meaning she ignores information about outcomes which would put beliefs in line with reality. On the other hand, a regret minimizer who cares about avoiding regret related to responsibility is seeking to maintain a positive self-image about being a good decision maker.

To disentangle overconfident behavior from regret-minimizing behavior, I conduct a within-subject study. All subjects face a lottery four times, and must decide on the level of risk they are willing to tolerate. The four lottery decisions are presented as card games, which vary only in the level of illusory control. We manipulate illusory control by manipulating who (or what) gets to select the cards to be turned over in a lottery. In one treatment a subject selects her own cards to be turned over, in another the selection is done by another person, for whom this decision is irrelevant. In another treatment condition, the cards are selected by a random computer mechanism. These three conditions cover more completely the dimensions in which illusory control is relevant: in the first the self possesses illusory control, in the second another person, and the third, as close as we can get to a situation of perceived pure chance. In the fourth condition subjects may choose whether to be allowed to select their own cards or give this task to another person, allowing classification of subjects according to their control preferences. The fact that it is within-subject means that subjects are presented both with a situation in which the illusory control bias is triggered, and one in which it is not, allowing the subject to potentially correct their error. Indeed, the method is risky. It is quite reasonable to expect that a person, upon seeing that the only change in situation is the possesser of illusory control, will recognize that the situations are identical and therefore exhibit consistent behavior across treatments, resulting in the disappearance of the bias (within a subject). Moreover, consistent and well-defined risk preferences as well as anchoring would also contribute to a decreased or absent illusion of control bias.

This experiment recognizes the possibility that attitudes toward control involve concerns about responsibility, and makes explicit that there is more to control than simply having it or not having it: you may have it, not have it, or allow another *person* to have it. I show that the vast majority of participants are not consistent, and within this inconsistency an illusion of control bias persists even when the equivalence of the environments is quite salient. However, I observe the illusion of control only within-subjects, not across subjects. Indeed, the experiment was designed so that the first treatment was different for all subjects, and subjects were not informed about the specifics of each condition beforehand, so there is one between-subject observation. The fact that the first treatment to which subjects were exposed did not yield a similar pattern to that which arose over time led me to question whether the pattern in the data is indeed a true illusion of control. Upon analyzing the different patterns of responses across the four treatments as well as questionnaire data, I concluded that, although the end result of responses looks like an illusion of control, it is merely the result of randomization behavior across the four treatments.

The rest of the paper is structured as follows. Section 3.2 presents the design and predictions, section 3.3 the results, and section 3.4 concludes.

3.2 Experimental Design & Procedures

As a risk task, I use a sequence of lottery choices which is equivalent to an unconditional version of the devil's task (Slovic, 1966). Our measure of the illusion of control bias is the change in risk-taking across the different treatment conditions (shown in Table 3.1). The lotteries were presented on a computer screen as a card game, and participants could decide how many cards they wanted to have turned over. For each card turned over (save one), participants earned one point. One card among the 25, called the destruction card, canceled all earnings if turned over. The location of the destruction card was randomly determined, and participants were informed of this. Each participant saw on the computer screen 25 cards laid out face down, and the experiment proceeded in three steps. In the first step, the participant typed in the number of cards she wished to have turned over. In the second step, the cards to be turned over were selected. In the third step, the selected cards were revealed.

The treatment variation occurred at step two, with four different conditions. So that participants did not receive any feedback about their payoffs until the very end, participants made their decisions about how many cards to have turned over (step one) for all four treatments before proceeding to step two. Likewise, they selected cards for all four treatments in step two before proceeding to step three, at which point they learned their payoffs. The four treatment variations are shown in Table 3.1. The four treatment conditions are designed to disentangle the social and nonsocial aspects of control biases, and sort participants according to whether they have a *self* control bias or an *other* control bias. The condition which we treat as a baseline is the "computer" treatment, in which the cards to be turned over are selected randomly by the computer. The "self" treatment is the one in which we induce illusory control, because subjects control the card selection relevant for their own payoff. In the "other" treatment we take illusory control away from the person for whom the task is payoff relevant; i.e. a random other participant selects which cards should be turned over for the payoff recipient. In "choose" we allow subjects to choose between the "self" or the "other" condition, and then they go through whichever condition they chose.

Treatment condition	**Description**
Self	The recipient and the card selecter are the same person
Computer	Cards are randomly selected via the computer
Other	A random subject who is not the recipient selects the cards
Choose	Subjects decide between "self" and "other", then the applicable treatment is run

Table 3.1

At the start of the experiment, each participant sat at a randomly assigned and separated PC terminal and was given a copy of instructions.[3] The order in which treatments were presented was randomly determined for each participant, and participants were informed of the treatment type on-screen when it occurred, not

[3] A copy of translated instructions can be found in the appendix.

beforehand. A set of control questions was provided to ensure that participants understood the procedure. If any participant repeatedly failed to answer correctly, the experimenter provided an oral explanation. Immediately after finishing the entire decision-making task subjects filled out questionnaires including the Magical Ideation Scale (Eckblad & Chapman, 1983), Locus of Control (Rotter, 1966), and the Duke Religiosity Index (Koenig, Parkerson Jr, & Meador, 1997). No form of communication was allowed during the experiment. All sessions took place at the LakeLab (University of Constance, Germany) during December 2010 and February 2011. The data was collected over 8 sessions, with 210 participants in total. Participants received a show-up fee of 3 euros ($4.00 at that time). The experiment took about 45 minutes and average income (including the show-up fee) was about 10.39 euros ($13.95). The experiment was programmed and conducted using z-Tree (Fischbacher, 2007), and participants were recruited using the online recruiting system ORSEE (Greiner, 2004). Participants were part of the LakeLab subject pool, including undergraduate and graduate students of all fields of study.

3.3 Predictions

Our measure of illusion of control bias is the variation in the number of cards participants wish to have turned over in the different treatments. The more skill the person (or mechanism) turning over the cards is perceived to have, the more risk the participant will be willing to take. The treatment conditions *self*, *computer*, and *other* are structured to isolate the social from the nonsocial features of control biases. To return to our example of the reins, what we mean by this is whether the illusion of control is a phenomenon that results merely from possession of control, or whether it is social in nature and people care about whether that control goes to another *person.* The former we can measure by comparing the *self* with the *computer* condition; the latter we will explore by additionally looking at the *other* condition. The *choose* condition is designed to sort participants according to whether they have a *self* control bias or an *other* control bias. A rational decision maker will not respond systematically to the treatment variation, since the outcome is one of pure chance. The rational decision maker faces an expected payoff of $x\frac{(25-x)}{25}$ and chooses $x \in [1,2, \dots 25]$, where x is the number of cards to turn over, to maximize her utility. Risk neutral decision makers are indifferent between turning over either 12 or 13 cards, risk averse decision makers prefer to turn over at most 12. If decision makers know their risk preferences and do not have an illusory control bias, the number of cards turned over across treatments should not differ systematically.

I now present the hypotheses in the context of the two main theoretical foundations, overconfidence and responsibility attribution. Variations in both illusory control and illusory responsibility occur across the treatments, since treatments vary in who is given control over irrelevant actions in the experiment. In the *self* treatment it is the own skill that comes into question (in choosing which cards to turn over wisely), in the *other* treatment it is the other person's skill to which control and responsibility is attributed, and the *computer* treatment is again the baseline for which skill is irrelevant. Here I consider responsibility for an outcome (win or lose) to be relevant to subjects because of the desire to have a positive self-image. Work such as Bénabou & Tirole (2011) is based on the idea that self-image is of value to people and that they therefore have an incentive to perceive themselves in a positive light. In the *self* condition a decision-maker is fully responsible for any outcome, whereas in *other* responsibility is shifted to the partner. The same applies for control. I consider *computer* to lie between the two aforementioned treatments in terms of control and responsibility attribution, since bad outcomes can only be justified to the extent of chance, whereas in the *other* treatment it is possible to attribute a very bad outcome indeed to extraordinarily poor skills.

Although the notion of overconfidence generally (and with reason) focuses solely on the overestimation of one's own capabilities, I will also consider the corollary: the underestimation of another's capabilities. Indeed, this is a requirement in order to be able to rationalize one's own superiority. As a baseline for comparison I take the *computer* treatment. In this treatment illusory control is not given to any person. I assume that overconfidence is a form of self-signaling, and that people try to rationalize the conditions surrounding it to make it believable to themselves. This means that subjects believe that on aggregate the success rate of the population is equal to that of chance. A subject may consider herself to be better than chance, but in order for this belief to be believable, she must also believe that others in the population are equally worse than chance. Compared to this baseline, the first column of Table 3.2 shows the predictions resulting from this logic.

	Overconfidence	Regret minimization
Self	increase	decrease
Other	decrease	increase

Table 3.2

Concerning regret, the motive for a decision maker is always to see herself in the most positive light, i.e. with a high skill level. A subject who wishes to see herself as a good decision maker takes the outcome of her decision (win or lose) as an indication of her overall ability to make good choices. Relative to the *computer* treatment, in the *self* treatment she wants to lower the possibility that she will be responsible for a bad outcome, since it would signal a low skill level, so she reduces the number of cards she turns over relative to the baseline. In *other* she is free from the negative self-signal of a bad outcome, so she will turn over more cards.

I expect to replicate the findings of Langer and others, i.e. that there is indeed an illusion of control, with hypothesis I.

Hypothesis I: On average, participants turn over more cards in self *than in* computer*.*

Beyond hypothesis I, I expect heterogeneity of types concerning overconfidence and regret minimization. This implies different patterns across treatments for different subjects. Subjects who are relatively more overconfident behave as stated in column 1 of Table 3.2 and subjects who seek to minimize regret will behave as in the 2nd column.

Our next hypothesis concerns the preference for control. In the condition *choose*, subjects may choose whether they prefer their payoff to be dependent on cards they themselves turned over, or on cards that another participant turned over. I expect that subjects will sort into their preferred condition based on who they perceive will better control the uncontrollable. In contrast to the case in the last hypothesis, I assume that responsibility will not play a role here. This is because in *choose* responsibility is maintained even if subjects select into *other*, quite simply because of the fact that they chose it.

Hypothesis II: Participants for whom the number of cards turned over in self *is larger (smaller) than in* other *will choose to turn cards over themselves (to have someone else turn them over) in the* choose *condition.*

I have two hypotheses relating to our questionnaires.[4] I expect to replicate the finding of Brugger & Graves (1997) who have shown that people who score higher on the Magical Ideation Scale have a higher illusion of control. Since I consider the possibility of control biases in both directions, rather than having a directional hypothesis, I expect higher differences between conditions for magical thinkers.

Hypothesis III: For each individual, the variance of the number of cards to be turned over across treatments is higher for magical thinkers.

Last, is a hypothesis concerning the interaction of locus of control and magical thinking. I expect that:

Hypothesis IV: Subjects with high magical thinking and internal (external) locus of control will prefer a larger (smaller) number of cards to be turned over in self *than in* other *or* computer*.*

This hypothesis derives from the expectation that 1) illusion of control is higher for magical thinkers and 2) a higher (lower) sense of internal control is likely to result in a self- (other-) oriented control bias.

3.4 **Results & Discussion**

Table 1 shows the number of cards turned over in each treatment and according to sequence. 31% of participants were perfectly consistent in that they never altered the number of cards they wished to turn over. A nonparametric trend test as well as regression revealed no time trend in the data (p=.595 and .539 respectively).

[4] We include a religiosity questionnaire because we expect it to be related to beliefs on causal attribution, but we do not have a specific hypothesis concerning it.

	1st	2nd	3rd	4th	Grand Total
self	11.146 (41)	11.415 (53)	12.000 (43)	12.041 (73)	11.700
computer	11.414 (58)	11.396 (53)	11.327 (55)	11.136 (44)	11.329
other	11.396 (53)	11.018 (57)	11.060 (50)	10.880 (50)	11.090
choose	11.638 (58)	11.000 (47)	11.548 (62)	11.488 (43)	11.438
Grand Total	11.419	11.210	11.467	11.461	11.389

Table 3.3
Number of cards turned over across treatments and sequence (n in parentheses)

I cannot confirm hypothesis I. The number of cards turned over in *self* is insignificantly higher than the number in *computer* (2-sided sign-rank test, p=.6). However, the number of cards people turn over in *other* is smaller than the number in *computer* (2-sided sign-rank test, p=.086). Moreover, the number of cards turned over in *self* is significantly higher than the number in *other* at the 1% level (2-sided sign-rank test, p=.002). Taking into account the fact that the number of cards turned over in *computer* is very close to the midpoint between that in *self* and *other*, this outcome implies that the general tendency is toward overconfidence. Most studies (Camerer & Lovallo (1999), Chambers & Windschitl (2004), Kruger (1999)) tend to focus on the overconfidence in the self rather than underconfidence in others, and brush off perception of others deeming it “reference group neglect” or “egocentrism”. One exception is Moore & Cain (2007). They proposes the idea that perception of others’ performance is regressive, and that underestimation of others’ ability is simply the result of Bayesian updating. Our data, like theirs, show that people underestimate others’ performance when the task is simple. However, unlike theirs, our data indicates that this is not the result of Bayesian updating. Their argument applied to this situation goes as follows: there is a prior belief (which here I will take to be chance level), and the own performance is taken as information about the difficulty of the task which in turn reveals information about the population’s performance when facing this particular task. So, if people do not gain new information while performing our task, they should still expect others to perform at chance level. If we consider that they may actually be gaining information about the difficulty (or simplicity) of the task simply by experiencing the task, even though they are not given any success/failure feedback, the estimate of the other’s performance on this task should, if anything, move toward the own performance rather than away from it. This would mean a number of cards turned over in *other* somewhere between that in *computer* and *self*. This is exactly not the case in our findings.

Our evidence does not support hypothesis II. Those who opened more cards in *self* did not have a higher propensity to select into the condition. As expected, the majority of subjects for whom risk-taking in *self* was greater than that in *other* selected into the *self* condition (81%). Just the contrary is to be expected for the lower row in Table 3.4; however they too preferred for their own cards to be payoff relevant (86% selected into *self*)[5]. There is the possibility that people made errors in selection. To check whether this is the case, I looked at the number of cards people turned over in the selected condition. I consider an error to be a mismatch between the selected condition and the favored condition based on risk-taking when no selection was possible. I expect risk-taking in the selected condition to be lower in mismatches, so in Table 3.4 for the numbers in the top left and bottom right cells to be greater. We expect this because an overconfident person (or person "underconfident" in others) who erringly ends up in the *other* condition distrusts the ability of the person turning over the cards, and therefore lowers the number of cards she wishes to have turned over. In the case of a person who wants to shirk responsibility by choosing *other* but mistakenly chooses *self*, any responsibility for a bad outcome will be borne by herself and she therefore reduces the number of cards she wishes to have turned over to lower the possibility of regret. As the table shows, this is not systematically the case, further confirming that subjects did not choose in a way that allows us to sort them by their preference for control.

	Decision	
	selected *self*	selected *other*
Risk-taking in *self* > Risk-taking in *other*	11.38	12.21
Risk-taking in *other* > Risk-taking in *self*	11.32	13.17

Table 3.4
Average number of cards turned over in the selected condition

Regarding hypothesis III, I confirm that subjects high in magical ideation exhibit low consistency, that is they have significantly higher individual variance in the number of

[5] To control for activity levels in the experiment, in the second four sessions subjects were required to turn over two sets of cards, one for themselves and one for a potential other. The selection decision was then merely whose card tables should be payoff relevant. The results from both sets of sessions are qualitatively the same.

cards they turn over across treatments. Subjects with a magical thinking score of less than or equal to the median (n=61) have an average variance of 1.97, whereas for subjects with a score higher than the median (n=47) it is 5.37 (2-sided ranksum p=.003).

In addition to finding evidence in favor of hypothesis III, I find that those who self-select into the *other* condition have lower magical ideation scores and lower individual variance. On average, it is only the relatively more consistent who allow someone else to possess illusory control over their payoffs. For those that selected into *self* the average individual variance in number of cards turned over is 4.18, for those selecting into *other* it is 3.46 (2-sided rank-sum p=.022). The average magical ideation scores are 8.71 and 6.79 respectively (2-sided rank-sum p=.088).

There is an alternative possible source of the results regarding magical ideation and illusion of control, which is the result of special distribution of magical ideation scores in this sample. The distribution is right-skewed, and I suspect that high magical ideation scores, which are less likely, are the result of randomizing answers (that is, careless answering of the questionnaire), and that the link between magical ideation scores and high variance in decisions in the experiment is all simply a result of a lower level of preference for exerting costly cognitive concentration in the experiment. In the following I provide analysis regarding the alternative possibility from which our findings can arise. The conclusion that I draw from this analysis is that the correspondence between variance and magical thinking in the data is actually a result of randomizing behavior in the experiment in general. This calls into question the results of Brugger & Graves (1997). Because of the relatively low mean (median) answer on the questionnaire, I suspect that subjects who do not want to make the effort to carefully fill out the questionnaire randomize among the possible answers (in this case “true” or “false”). The range of possible scores is from 0 to 30, the observed range from 0 to 27, and the mean (median) choice was 8.21 (8). Figure 3.1 shows the histogram of magical ideation scores.

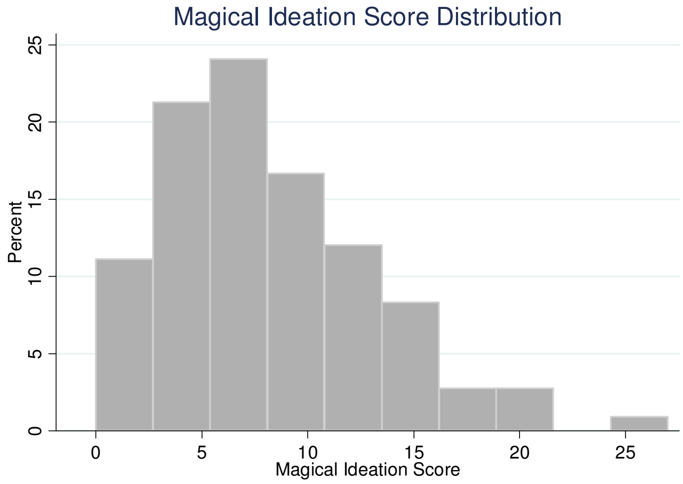


Figure 3.1

Assuming that the distribution of answers by randomizers is symmetric, then the nonrandomizers have a relatively low magical ideation score, and the magical ideation score of the randomizers is more likely to be above the observed median than below it. This implies that the correlation between a high variance in the number of cards subjects turn over and the magical ideation score may actually simply be the result of randomizing behavior in general.

To further investigate whether subjects are not exhibiting preferences through their different choices across treatments, but rather exploring in a random manner, I analyzed the randomness of observed patterns of behavior. I bootstrapped the data to find the likelihood that certain patterns occur out of randomization, given the observed number of cards that subjects turned over in each of the treatments. I restricted analysis to the treatments *self, computer,* and *other.* The most commonly occurring pattern observed was perfect consistency (the number of cards turned over was the same in all three treatments). Of our 210 observations, I took a subsample of 100 randomly matched observations from the three treatments and repeated this 1000 times. I then calculated the p-value by looking at how often the proportion of observations in which the number of cards turned over was equal in all three treatments was greater or equal to what is observed in the data. The p-value was .000, implying that consistency was a preference rather than a side effect of subjects' randomization.

In the next step I repeated the bootstrap but eliminated the 70 consistent observations from the analysis. I looked at the proportion of subjects exhibiting overconfidence and the proportion exhibiting regret minimization. I classified subjects with the number of cards turned over in *self* > the number of cards turned over in *computer* & the number of cards turned over in *self* > the number of cards turned over in *other* as overconfident, and subjects with the number of cards turned over in *other* > the number of cards turned over in *comp* & the number of cards turned over in *other* > the number of cards turned over in *self* as regret minimizers. The resulting p=.754/.611 means that there is not an observed tendency for overconfidence or regret minimization to occur more than randomization would predict.[6] This indicates to us that those subjects who do exhibit overconfidence or regret minimization seem to be randomizing over the treatments. The main conclusion from this line of analysis is that I suspect that the link between more erratic choices of how many cards to turn over across treatments is not linked to magical ideation, and is merely a side effect of the response structure of the questionnaire. Moreover, it reveals that subjects do not fall into the types I have proposed. So although I observe an empirical treatment effect, I must conclude that it is not the result of an illusion of control as I and other researchers have conceived of it. Rather it is the result of exploration or errors by subjects. This idea is reinforced by the fact that I do not observe a between-subjects effect. The pattern of responses across treatments in the first treatment, before subjects received information about any further treatments, shows no significant differences and does not even show the same ordering of number of cards turned over across the treatments as that which is observed in the within-subject analysis.

Our data contradicts hypothesis IV. For subjects with high magical ideation, the difference in risk-taking between *self* and *other* or *computer* is positive or near zero for both those with an internal and those with an external locus of control. All are insignificantly different from zero, except for the bottom right cell, which is significant and with opposite sign than expected (2-sided t-test p=.07). This points against our idea of a two-sided illusion of control.

	self vs. *computer*	*self* vs. *other*
Internal locus of control (n=19)	.158	-.053
External locus of control (n=28)	.321	.964

Table 3.5
Number of cards turned over in *self* minus the number of cards turned over in *computer* and *other*.

[6] We repeated the analysis with the stricter definition self>comp>other/other>comp>self with qualitatively the same results.

3.5 Conclusion

I show, in contrast to recent literature in economics on the illusion of control, what at first glance looks like an illusion of control bias. I confirm that it is on average in the direction of the vast majority of previous literature. I also find that it persists even in a within-subject design. By learning the treatment variable, people are given in a very plain way information which could potentially allow them to correct their error. Yet the bias remains. I take from this that a degree of uncertainty in one's own preferences enhances the opportunity for the illusion of control bias to occur. Theories such as Bénabou & Tirole (2002) have dealt with the topic of decision-makers' incomplete knowledge over their own preferences. If a person could be 100% sure about her own risk preferences, she would behave completely consistently, and thus the bias would not occur. But because of the explorative nature of behavior across the treatments (i.e. inconsistent behavior), it remains, even in a situation in which error correction is more feasible than is common in most situations.

Although I observe an empirical treatment effect in the data which points toward an illusion of control, our bootstrap analysis revealed that it is not the result of preferences in line with overconfidence or regret minimization. Rather, our analysis revealed two key points: first, that the link between magical ideation and illusion of control may be spurious. Magical ideation scores could range between 0 and 30, and were on average quite a bit lower than the midpoint of 15. As a result of this, we suspect that those with relatively high magical ideation scores are actually people answering the questionnaire in a careless way, rather than "magical ideators". That behavior in the experiment is more erratic is to be expected for subjects who behave carelessly in the questionnaire. Second, I conclude that except for perfectly consistent decision makers, behavior across treatments appears to be the result of subjects randomly varying the number of cards they wish to turn over. In light of this I conclude that it is not a true illusion of control which is observed here, rather the result of imperfect knowledge of one's own preferences. I emphasize nonetheless that further studies in both economics and psychology should take into account the possibility of heterogeneity of subjects' perception of both their own and others' skill, illusory or not.

Appendix

General Instructions

Today you are participating in an economics experiment. If you read the following instructions carefully, then you can, depending on your decisions, earn money in addition to the **3 euros** that you receive as starting money for your participation. It is therefore very important that you read these instructions carefully.

For the entire length of the experiment communication with the other participants is not allowed. We therefore request you not to speak with one another. If there is something you do not understand, please look again at the experiment instructions. If you then still have questions please raise your hand. We will then come to you and answer your question personally.

During the experiment we will not use euros but points. The points you attain during the experiment will be converted to euros according to the following exchange:

1 point = 0.50 €

At the end of today's experiment you will receive from us the **3**euros for your attendance as well as all attained points exchanged into euros in **cash.**

On the following pages we explain the exact procedure of the experiment. First we want to make you familiar with the basic decision-making situations. At the end of this explanation you will find control questions that are intended to help you understand the situations. The experiment will begin once all participants are completely familiar with the procedure of the experiment.

The Experiment

In this experiment you will play a simple card game. There are 4 versions of this card game, and you will play each version. The explanation below pertains to all 4 versions; you will receive the details for each particular game on screen.

You will see 25 cards on a screen like the one shown below. When you first see the cards they will all be face down. You may determine the number of cards you want to be turned over, and each card turned over is worth one point. However, there is a "deactivation card" that, if turned over, causes all points to be lost. The location of this card is random, just as it would be with a shuffled card deck. The procedure is as follows:

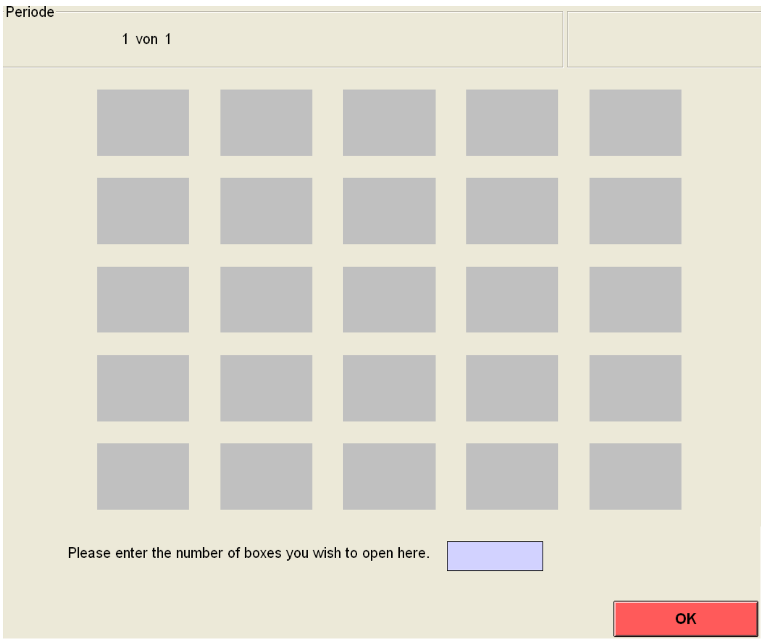
Step 1) For each of the four variants, you decide how many cards should be turned over.

Step 2) The cards are selected. It is in this stage where the four versions of the game differ. Again, you will learn the relevant details on screen.

Step 3) You may turn over the selected cards.

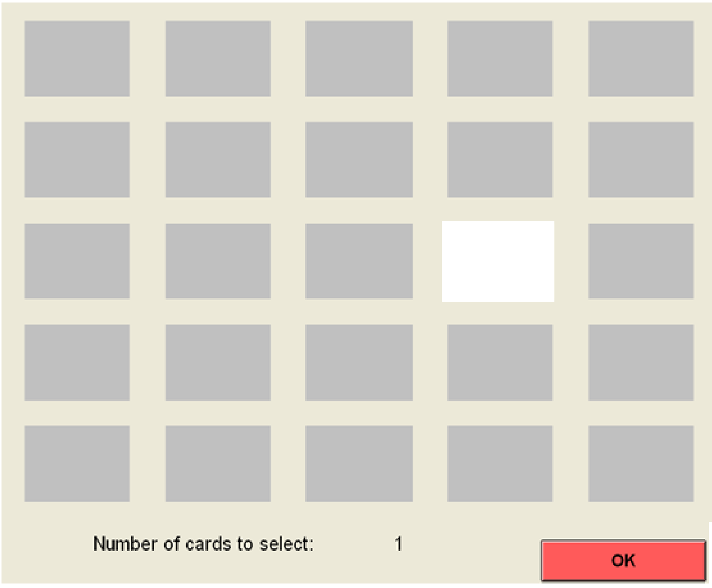
So, you will do step 1 four times in a row (once for each variant), then step 2 four times, then step 3 four times.

In step 1 you will see the following screen:



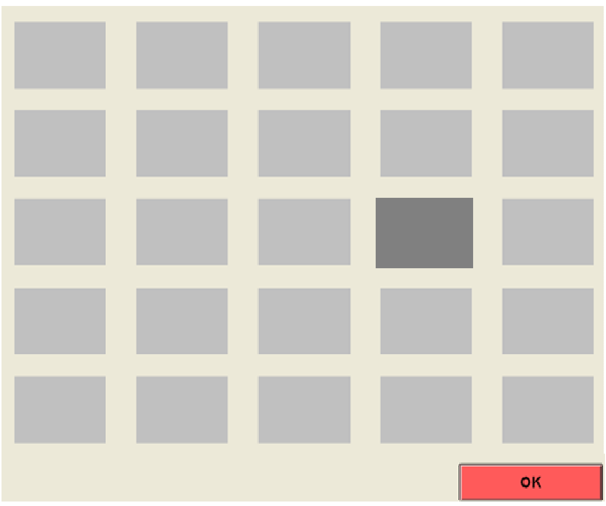
Once you have entered the number of cards, you may click the OK button to proceed.

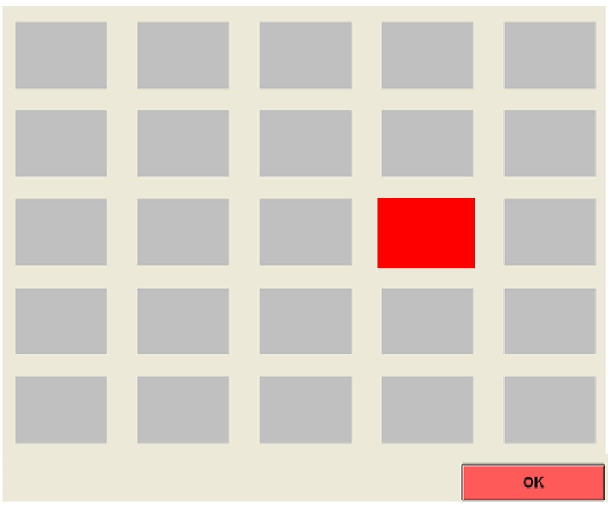
In step 2 the selection process takes place. A selected card appears in white (see below).



The OK button will only appear once you have selected the right number of cards.

In step 3, you will see the selected cards in white. To turn them over, simply click on them. If it is a normal card, it will appear in dark grey (see below left). If it is a deactivation card, it will appear in red (see below right). You will also see the number of points you earned. If there is no deactivation card turned over, you receive points equal to the number of cards turned over. If there is a deactivation card turned over, you will receive zero for that variant.





Once you have finished, you will see a summary of your payoff from each game. The total amount from all 4 variants will be converted to euros and paid out according to the exchange rate 1 point = 0.50 cents.

Control questions

In the following you will find a few control questions. Please answer them in written form. **Your answers to the control questions have no influence on your payment at the end of the experiment.**

1) Suppose you chose the number of cards to be turned over to be 10 in each of the four variants of the game. The deactivation card did not appear in any of the four variants. How many points will you earn?

2) Suppose you chose the number of cards to be turned over to be 12 in each of the four variants of the game. In one variant, the deactivation card appeared, in the other three it did not. How many points will you earn?

3) Suppose you chose the number of cards to be turned over to be 14, 17, 22, and 8 in each of the four variants of the game, respectively. In all four variants, the deactivation card appeared. How many points will you earn?

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4. Social Decision-Making Processes

Abstract

We analyze reaction times in dictator games to study the processing of conflicting motives in social decisions. We analyze reaction times for decision makers deciding how to allocate money (1) between themselves and another participant, and (2) between two other participants. The latter allows us to study social preferences in the absence of selfish motives and relate reaction times to those when the decision maker has a stake in the outcome. We study distributional preferences such as efficiency and minimax preferences and how they interact with selfishness. We find that reaction times increase with our objective measure of conflict. We find also that subjects experience more conflict when their selfish motives and social motives contradict each other. We classify subjects according to their personal norm type, and find that subjects' personal norms are not adequately represented by commonly considered social preference concepts.

"How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortune of others, and render their happiness necessary to him, though he derives nothing from it except the pleasure of seeing it."

-Adam Smith, *The Theory of Moral Sentiments*

4.1 Introduction

The pursuit of one's own well-being in the form of selfishness can be considered virtue, vice, or a confusing mix of the two; and is no doubt a natural part of being a human. Adding to the confusion is the idea that humans are at least in part altruistic, and indeed do incur satisfaction via others' satisfaction. In an environment of limited resources like the world we live in, the self-interested motive and altruistic motive are in conflict more often than not. How people process these conflicts is the topic of this paper. Economic models of decision making typically consider the end product of the processing of different motivations, in the form of "preference". Recent work however has shown the importance of incorporating psychological and neurological processes into decision-making models, as preference depends on factors such as whether a motive is the result of an "impulsive" process or a "controlled" one (Strack & Deutsch, 2004). Alternative models of decision making from psychology such as decision field theory (see Busemeyer & Diederich (2002) for a review) emphasize the process by which people make decisions, especially when the environment prevents full and unlimited concentration on the decision at hand. In such models, decision makers focus their attention on attributes of the situation, and the relative favoring of one option over another can shift over time. This implies naturally that choices depend on the time allowed for a decision. More generally, it means that the conflict leading to a decision is an important factor in understanding decisions, making it valuable to study reaction times.

Rubinstein (2007) was the first to systematically analyze behavior in games using reaction times. Reaction time analysis is widely used in psychology, but because of the large variance in response times to typical decisions in economics experiments, as well as the typically small sample sizes, reaction times were not considered to be a useful tool. Rubinstein conducted an internet experiment, collecting several thousand data points, as a solution to the problem. He proposed that choices based on emotional reactions are faster, and those based on cognitive deliberation are slower. Rather than collecting data from several thousand individuals, Piovesan & Wengström (2009) collect within subject data to deal with large variance. We do the same. Moreover, whereas Rubinstein recorded reaction times including the time it took to read instructions, we record only decisions, thereby greatly improving the power of our measure. We make this possible by having several decisions with the same structure, so that one set of instructions is sufficient for all decisions. Piovesan & Wengström use dictator decisions to study distributional preferences. As we said above, this is necessary to understand altruistic behavior as opposed to retaliatory behavior. However, they use only 1st party decisions, whereas our study introduces 3rd party decisions to control for the elements of the decisions not related to selfish motivation. More concretely, in Piovesan & Wengström subjects who are making decisions always have a stake in the decision. In our 3rd party condition, decision makers determine the payoffs of two other subjects, and therefore are not motivated by any selfish concerns in these decisions. By this means we are able to directly study subjects' social preferences, without any conflict or confound of selfish motivation. The goal of our study is to to assess the automaticity of selfish decisions. Since social motives are heterogeneous, we include the 3rd party condition to identify and focus on exactly the social contexts in which subjects actually experience conflict between selfish and other-regarding motives.

There are many different ways of conceptualizing how exactly the welfare of others contributes to one's own welfare. In our study we consider the social motivation concepts of efficiency, maximin preferences, and preferences for equality. With our design we are able to exactly observe peoples' preference, in the absence of any selfish concerns, regarding these concepts. We use the term personal norm to refer to a person's purely social preference absent any selfish motives. We define the different motivations as follows: efficiency is maximizing the total or social payoff; maximin preferences maximize the minimum payoff to a subject, thereby guaranteeing that no one is left in a very bad position, at least relative to others. A preference for equality additionally encompasses an element of envy.

Engelmann & Strobel (2004) showed the importance that efficiency and maximin preferences play in social decision making. They compared the performance of two fairness models (Fehr & Schmidt (1999) and Bolten & Ockenfels (2000)) as well as concerns for efficiency and maximin preferences and concluded that the latter two are more useful in explaining behavior than the former two mentioned models (see Fehr, et al. (2006) for a rebuttal). Funaki et al. (2010) used eye tracking technology to assess whether efficiency, maximin, and envy are the true motivators of behavior by checking whether people actually looked at these properties, or whether behavior just happened to be in accordance with them. Both of these studies used 3-person dictator games. Andreoni & Miller (2002) used two person dictator games as we do. They classify people in terms of their social preferences, and find that 43% percent of subjects conform 100% to efficiency, selfish, or maximin preferences. Funaki et al. (2011), with 18 games, find that 86% of subjects conform to one and only one decision rule (efficiency, maximin, or equity). We have 50 third party questions, and we do not find any participant conforming to one rule, let alone a majority. Despite heterogeneity, we find a strong tendency for people to choose most often in line with efficiency or maximin preferences.

This paper focuses on how social and selfish motives interact to end up as the revealed preference we observe. The processing of social preferences is interesting regarding self-control issues. We focus on the interplay between impulsive responses and the subsequent controlling of those responses in the brain. Controlling selfish impulses is a necessary social skill important for the maintenance of all relationships among humans, from the personal to the professional. Deficiencies in the ability to control selfish impulses can lead to criminal behavior that, at the extreme, can lead to an inability to function in society as well as incarceration, not to mention the damage that it can cause other people. Sanfey et al. (2003) show that areas of the brain associated with emotions (particularly with disgust) are activated when respondents in the ultimatum game receive unfair offers. According to Miller & Cohen (2001), the dorsolateral prefrontal cortex (dlPFC) is thought to be largely an area of cognitive control. Sanfey et al. (2003) find that "[u]nfair offers that are subsequently rejected have greater anterior insula than dlPFC activation, whereas accepted offers exhibit greater dlPFC than anterior insula. The contrast in activation between these two areas is significantly different for accepted and rejected offers ($P = 0.033$, one-tailed), consistent with the hypothesis that competition between these two regions influences behavior." This leads to the conclusion that the automatic process is to reject low offers, and the controlled response is to accept them. However, this initial study was correlational, so van 't Wout et al. (2005) used the technique of repetitive transcranial magnetic stimulation, which externally delivers short magnetic pulses which go through the skull and temporarily modify neural processing. The study however lacked an appropriate control treatment and failed to show a behavioral treatment effect. Knoch et al. (2006) repeated the study using appropriate controls and a larger subject pool. Using rTMS, they disrupted (lowered the activity in) the right dlPFC of participants responding in an ultimatum game. Judging from the results of Sanfey et al., one would expect this to lower the acceptance rate. As intimated above, it did just the opposite. Acceptance rates for subjects with TMS to the right dlPFC were triple the rate for those with TMS to the left dlPFC, and quadruple the level of those in the sham condition.

So far, the dual-process literature differs from the studies mentioned studying purely distributional preferences. The studies looking at automatic and controlled processes look at responses in the ultimatum game, whereas the distributional preference studies look at dictator games. One can certainly draw conclusions about fairness and altruism with both games, however the end measure is fundamentally very different. The motivation to reject an unfair offer in the ultimatum game is one that results from, to speculate, the insult or pain that arises from being exploited. This is indeed hardly the same as a decision to be altruistic in a dictator game, which is the corresponding "non-selfish" decision. The ultimatum game is a reaction, whereas the dictator decision is an initiating one. More importantly, a selfish reaction in the ultimatum game is a sign of socially beneficial behavior (for the two players of the game), whereas in the dictator game it causes harm to the other party. So, in one case (the ultimatum game) a so-called selfish reaction is actually for the greater good, whereas in the dictator game the greater good, at least in terms of total wealth, remains unaffected. To fully understand how selfish motives interact with social ones in terms of processing, dual-process studies of social decisions must move beyond the ultimatum game, which is what we do in this study. We use reaction times to identify the conflict subjects actually experience, rather than assuming subjects experience conflict among social motivating factors in the same way. The next section elaborates on our design in full.

4.2 Experimental Design & Procedure

Each subject faced decisions between two distributions of money. There were 50 different sets of decisions, and subjects faced each set twice, once in the 1st party condition and once in the 3rd party condition. In the 1st party case they decided which option to choose for themselves and a partner, and in the 3rd party case they decided which option to choose for two other people, with no effect on the decision maker.

Decision between divisions for you and participant B			
For you	100	For you	200
For participant B	100	For participant B	400

Decision between divisions for participant C and participant D			
For participant C	100	For participant C	200
For participant D	100	For participant D	400

Table 4.1
1st party decision layout (above) and 3rd party decision layout (below).

Table 4.1 shows what subjects saw on screen. They show one of the 50 allocations, shown for both the 1st party condition (top) and the 3rd party condition (bottom). The order of decisions was randomized to avoid sequence effects. To make the difference between the 1st and 3rd party decisions salient, their position on-screen was different. Some subjects saw the 1st party decisions on the top of the screen and the 3rd party decisions on the bottom, and some subjects saw the opposite. Which type of decision subjects saw on top was randomly determined. To decide for the option on the left side of the screen, subjects pressed the "f" key on the keyboard, and the "j" key for the option on the right. After each decision they saw a waiting screen and were required to press the space bar to continue to the next decision. The experiment was programmed and run using z-Tree (Fischbacher, 2007).

Possible payoff amounts were 100, 200, 400, or 800 points (see the appendix for the complete set of decisions). We found all permutations of payoffs possible with those numbers, then characterized each decision set in terms of which option selfishness, altruism, efficiency, maximin preferences, and equality favored. We then eliminated cases with redundant properties to keep the number of decisions reasonable. We proceeded in this way in order to have decisions with varying levels of complexity and as many different conflicts (or absence of conflicts) between the different motivations we are interested in. For the sake of clarity, we will refer to conflicting motives between only social motives as social conflict, and reserve the term ego conflict for a situation in which selfish motives oppose social ones (regardless of which ones). In the rest of the paper, we will refer to the option which selfishness favors (as well as its corollary in the 3rd party condition) as option X, and the other option as option Y. In cases in which selfish motives do not favor one option over the other, then option X is the one in which the partner's payoff is higher (again, with the matching decision in the 3rd party condition coded in the same way). The percentage of the time the option is favored by each individual motivation is shown in percentages here:

	Selfish	Altruism	Efficiency	Maximin	Inequality aversion
X	80	48	74	54	32
indifferent	20	12	4	30	6
Y	0	40	22	16	64

Table 4.2

100 participants took part in this experiment at the Lakelab, University of Constance, Germany, in June 2011. The experiment took about 45 minutes and subjects earned on average 12.16€ including a 3€ flat payment for their participation.

4.3 Predictions

We expect the conflict of a decision to influence reaction times in the decisions in our experiment. As in Rubinstein (2007) we expect social and ego conflict to be positively related to reaction times. In the 3rd party condition there is less conflict overall because all decisions have the same social properties, but lack a selfish dimension.

Our first hypothesis concerns overall social conflict. We varied the level of conflict in our decisions to the extent that for some decisions, all motivating factors point to one option. The less polarized the motivating factors are, the more conflict there is in making the decision between the two options. We construct our measure of conflict as follows. For each social property (efficiency, maximin, and inequity aversion) we assign a code of 1 if the property favors option Y, a code of -1 if it favors option X, and a code of 0 if it favors neither. We then calculate the variance of this value as a measure of how polarized motivation is for one option. In line with Rubinstein (2007), we predict:

Hypothesis 1: Greater social conflict leads to longer reaction times.

Depending further on the idea that greater conflict leads to longer reaction times, we next look at reaction times in the 1st party condition when there is an ego conflict between the own norm and selfish motives. We have the following hypothesis:

Hypothesis 2: Reaction times in the 1st party condition are higher when there is a conflict between the selfish motive and the own norm (i.e., what a subject chose in the same situation in the 3rd party condition).

Additionally concerning hypothesis 2 we will look at heterogeneity of preferences. A subject's choice in the 3rd party condition reflects her personal norm, and, as mentioned, we expect reaction times to increase when there is a conflict with selfishness and the personal norm. If this phenomenon remains after controlling for ego conflict between selfishness and the choice attributes (efficiency, maximin, inequality), then we can conclude that experienced conflict does not simply rise as a result of conflict between selfish motives and *any* social property, rather as a result of conflict with the *personal* norm.

In our next piece of analysis we classify subjects into types. As mentioned earlier, in Andreoni & Miller (2002) 43% percent of subjects conform 100% to efficiency, selfish, or maximin preferences, where the elicitation measure involved 8 or 11 dictator decisions. Funaki et al. (2011), with 18 games, find that 86% of subjects conform to one and only one decision rule (efficiency, maximin, or equity). Our third party condition allows us to illicit our subjects' personal norms, and therefore to then classify the subjects by type.

Next, we ask whether the norm is stable, that is whether those falling into a given classification in the 3rd party condition tend to act in accordance with the same norms in the 1st party condition. In other words, given they do not choose selfishly, we look at whether people choose their own norm as identified by the 3rd party condition, or something entirely different.

Following type classification, we will check the robustness of our type classification. The "true" personal norm is characterized simply by the decisions made in the 3rd party condition. We will test how well our type classification reflects the true personal norm. Given that different properties have different levels of importance for different people, we expect subjects to give consideration preferentially to those properties they care about. For example, given a situation with a conflict between maximin and efficiency, someone who only cares about efficiency will not perceive the situation as containing conflict. This leads us to:

Hypothesis 3: Subjects will have higher reaction times when there is conflict concerning the own norm as classified by our type classification procedure, and lower reaction times when the conflict concerns a less important norm.

Turning to the subject of automatic vs. controlled processes, Piovesan & Wengström (2009) find that selfish decisions are made faster, and Knoch, et al. (2006) find that disrupting control processes results in more selfish decisions. These two studies, with different methodology and a different domain, indicate selfishness is the impulsive process. We expect to replicate these results; that is we expect that social preferences are a controlled process. This leads us to our next hypothesis:

Hypothesis 4: Selfishness is the automatic process, that is selfish decisions in the 1st party condition are faster than nonselfish decisions.

4.4 **Results & Discussion**

Before addressing the specific hypotheses, we will take a general look at the data. Then we will address hypotheses 1 and 2, which look at reaction times as a result of increases in social and ego conflict respectively. After hypotheses 1 and 2 have been addressed, we will characterize subjects' norm types by looking at exactly which social properties people appear to favor. We will classify people based on how often they choose in line with efficiency, how often with maximin preferences, and with inequity aversion. Then, based on this classification we will address hypothesis 3. Last we will turn to hypothesis 4 by looking at individual differences in reaction times between the 1st and 3rd party conditions, in the situation where they chose option Y in the 3rd party condition. This allows us to look at changes in reaction times based on whether they suppressed their selfish desire or not.

First we look at the proportion of choices for option X between the two conditions. Even in the 3rd party condition, option X was more often preferred to option Y: in the 3rd party condition it was chosen 74.2% of the time, in the 1st party condition 87.5% of the time. The high proportion of choices in favor of option X is a result of the instances in which option X was favorable (or not unfavorable) to both recipients. If cases in which option

X is strictly better than option Y for both recipients are excluded, 36 cases remain for which the percentages are 83 and 67 for the 1st and 3rd party respectively. If we further exclude cases in which option X was weakly favorable (i.e. not unfavorable) to both participants (20 out of 50 cases), the percentages are 76 and 47 for the 1st and 3rd party respectively. As one would expect, choices for option X occurred about half the time when the situation did not lead to a "no-brainer" choice, that is where one option was better in all respects than the other.

Next we look at reaction times in the 3rd party vs. 1st party condition, and observe no overall difference. The fixed effects regression in Table 4.3 shows this, where the 1st party dummy variable is small and statistically insignificant. We use (in further regressions as well) a fixed effects model to account for individual heterogeneity in reaction times, and robust standard errors to account for reaction times which are correlated within a participant.

VARIABLES	(1) natural log of reaction time
1st party	0.00281
	(0.849)
Constant	0.879***
	(0)
Observations	10,000
R-squared	0.000

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.3
Fixed effects regression, clusters on participant

We can confirm hypothesis 1 that an increase in social conflict within a condition results in an increase in reaction times. The regressions in Table 4.4 show in the left hand column the effect of social conflict in 1st party decisions, and in the right hand column the effect of social conflict in 3rd party decisions. We tentatively suggest that, as one would expect, social conflict is less important in the 1st party condition. We say tentatively, because putting all data in one regression with a dummy variable for 1st party decisions and an interaction between 1st party and social conflict, the interaction is insignificant (p = .532). Nonetheless, we see below that the effect of social conflict in

1st party decisions is insignificant, and in 3rd party decisions is larger (though not significantly) and statistically significant.

VARIABLES	(1) natural log of reaction times, 1st party decisions	(2) natural log of reaction times, 3rd party decisions
decision number	-0.00577***	-0.00527***
	(0)	(0)
social conflict	0.0232	0.0377**
	(0.162)	(0.0398)
Constant	1.158***	1.123***
	(0)	(0)
Observations	5,000	5,000
R-squared	0.123	0.113
Number of participant	100	100

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.4
Fixed effects regression, clusters on participant

In Table 4.5 we show the effect of an individual's personal norm as revealed by decisions. The variable "chose Y in 3rd party" is a way to characterize the personal norm without any external assumptions made about the types of social properties people are interested in, that is it is a revealed-preference measure of the personal norm. Moreover, in the corresponding decision in the 1st party condition there is ego conflict, because selfishness always motivates a person to choose X. So, following from hypothesis 2 we expect this variable to increase reaction times. Our evidence favors hypothesis 2 that an ego conflict specifically with the personal norm increases reaction times. When there is a conflict between the own norm and the selfish option, reaction times in the 1st party condition are longer. If a person chose Y in the 3rd party, her reaction times for the matching decisions in the 1st party condition were longer than reaction times in the 1st party condition when she chose X in 3rd party corollary. To check whether the personal norm reflects interindividual differences, we include ego conflict with each of the social motivators we expected to be relevant. If including the ego conflict with each individual property were to make the "chose Y in 3rd party" variable lose its predictive power, it would be an indication of homogeneous social values. We see however that the effect of the personal norm as characterized by "chose Y in 3rd party" is robust to controlling for the different types of conflict, meaning it captures individual heterogeneity, i.e. the *personal* norm. In other words, subjects have higher reaction times when there is conflict concerning the own norm, but do not when the conflict concerns a less important norm. There is one exception: ego conflict with efficiency, even when people had chosen X in the 3rd party condition, is predictive. Moreover, including it in the regression reduces the size of the personal norm coefficient. Ego conflict with either of the other two social properties does not have substantial or significant predictive power.

VARIABLES	(1) 1st party reaction time (ln)	(2) 1st party reaction time (ln)	(3) 1st party reaction time (ln)	(4) 1st party reaction time (ln)	(5) 1st party reaction time (ln)	(6) 1st party reaction time (ln)
decision number		-0.00581***	-0.00581***	-0.00581***	-0.00581***	-0.00581***
		(0)	(0)	(0)	(0)	(0)
chose Y in 3rd party	0.0988***	0.108***	0.0626***	0.103***	0.106***	0.0604***
	(1.90e-05)	(3.67e-06)	(0.00310)	(3.60e-05)	(4.87e-06)	(0.00929)
efficiency conflict			0.0873***			0.0924***
			(0.000157)			(0.000223)
maximin conflict				0.0150		0.00284
				(0.505)		(0.903)
inequality aversion conflict					-0.0164	0.00990
					(0.190)	(0.502)
Constant	0.856***	1.146***	1.139***	1.145***	1.157***	1.131***
	(0)	(0)	(0)	(0)	(0)	(0)
Observations	5,000	5,000	5,000	5,000	5,000	5,000
R-squared	0.008	0.132	0.137	0.132	0.133	0.137
Number of participant	100	100	100	100	100	100

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.5
Fixed effects regression, clusters on participant

Next we look at personal norm values through decisions in the 3rd party condition. We find that efficiency and maximin concerns are overall the most important for subjects. We required subjects to make many more decisions than past studies, and as such we found a much lower tendency for people to choose in line with only one preference dimension. It seems the idea of a norm "rule" is unrealistic, and other-regarding preferences actually occur in many constellations.

The regression in Table 4.6 indicates that efficiency and maximin are potential norms. In the 1st party condition non-norm attributes such as selfishness and altruism (defined simply as positive caring for the other party) are important, whereas in the 3rd party condition norm attributes are more important. We see this by comparing column 1 to column 2 and column 3 to column 4. With only social motives included in the model (columns 1 and 2) we see that social motives are more predictive of behavior in the 3rd party condition than in the 1st party condition, that is the coefficients in column 2 are greater than in column 1. The same is true of column 4 as compared to column 3, and additionally selfishness and altruism are larger in column 3 than in column 4 [7]. Although the variables selfishness and altruism may seem strange to use in the 3rd party regression, they are not entirely meaningless, although their meaning does not reflect selfish or nonselfish motives. Rather, they reflect the benefit toward one participant or the other. Interpreting them in this fashion, it makes sense to include them in the regression, especially because it is entirely possible that the two variables together explain decisions better than the more constructed social motives like efficiency. We see in column 4 this is not the case; rather people do seem to be motivated by the more complex constructs of efficiency and maximin concerns.

VARIABLES	(1) chose Y in 1st party	(2) chose Y in 3rd party	(3) chose Y in 1st party	(4) chose Y in 3rd party
selfish			0.673***	-0.153**
			(0)	(0.0225)
altruism			0.185**	0.00903
			(0.0245)	(0.840)
efficiency	0.361***	0.735***	0.397***	0.702***
	(0)	(0)	(0)	(0)
maximin	0.507***	0.683***	0.448***	0.675***
	(0)	(0)	(0)	(0)
ineqavers	-0.0796**	-0.0199	-0.0264	-0.0321
	(0.0173)	(0.741)	(0.458)	(0.589)
Constant	-0.927***	-0.187***	-0.447***	-0.327***
	(0)	(0)	(9.94e-08)	(1.59e-08)
Observations	5,000	5,000	5,000	5,000

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.6

[7] Regression combining columns 1 and 2 of Table 4.6 into one model with interactions reveals the following differences between coefficient sizes in the 1st and 3rd party conditions: efficiency difference = .374, p < .001; maximin difference = .176, p = .022; inequality aversion not significantly different). Regression combining columns 3 and 4 into one model with interactions reveals the following differences between coefficient sizes in the 1st and 3rd party conditions: selfish difference = .826, p < .001; altruism difference = .176, p = .051; efficiency difference = .305, p < .001; maximin difference = .227, p = .002; inequality aversion not significantly different.

Probit regression clustering on participant

To obtain the personal norm, we regressed the variable "chose Y", a dummy variable which takes the value 1 when a subject chose option Y, on efficiency, maximin, and inequality aversion in the 3rd party condition for each individual. The independent variables are the same as already mentioned above, coded with a value of 1 if they favor option Y, -1 if they favor option X, and 0 if they favor neither. Our first measure of the personal norm is the coefficient of each of the three properties.

Figures 4.1, 4.2, and 4.3 show histograms of the personal norm coefficients arrived at by our procedure. Efficiency and maximin coefficients are generally well above zero, whereas inequality aversion coefficients are distributed quite narrowly around zero.

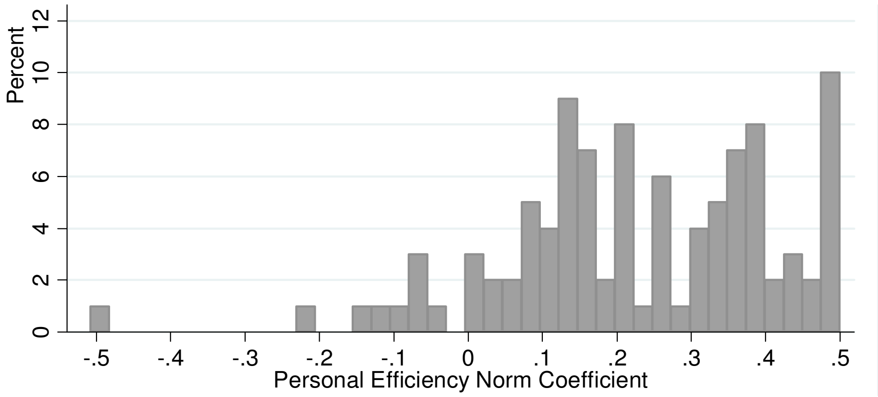


Figure 4.1

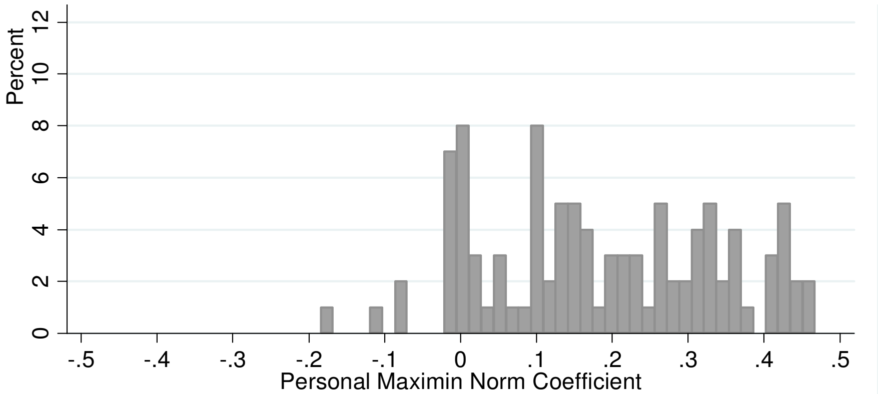


Figure 4.2

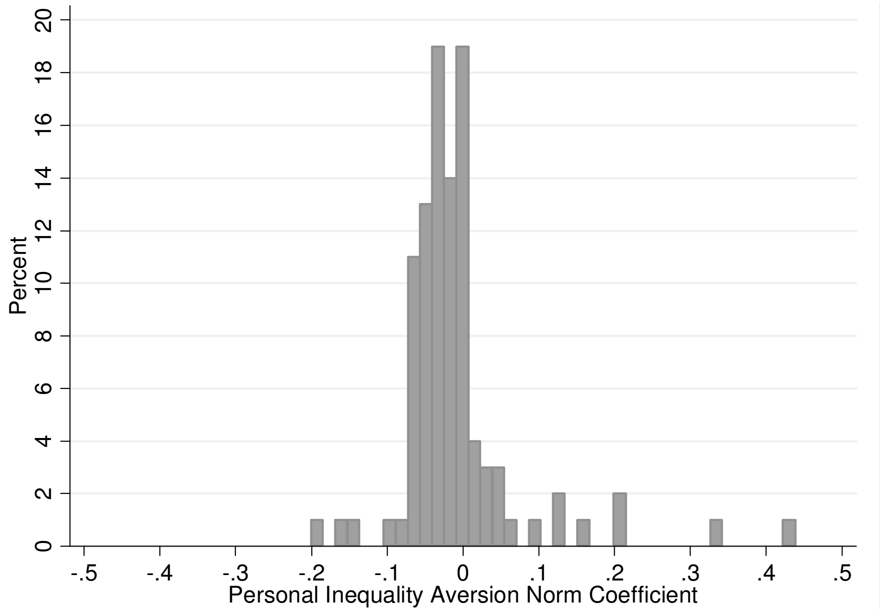


Figure 4.3

We next classified subjects by which norm is dominant, in other words which coefficient is the largest. Once they were classified, we looked at the number of times people followed a certain norm. Clearly there is variation; histograms also show that there is large variation in the degree to which people follow a given property. We have 50 third party questions, and we do not find any participant conforming to one rule, let alone a majority.

		Type classification (regression method)		
		maximin	efficiency	inequity averse
Percent choosing in line with property	maximin	74.5	64.8	40.5
	efficiency	64.3	75.6	21.7
	inequity av.	24.6	16.6	47.8
	n	45	50	5

Table 4.7

As a second method to classify norms, and also to test the validity of the first, we used Ward's clustering method (Ward, 1963). With three groups, the results of the cluster analysis correspond quite closely to those resulting from regression. The Ward method classifies 88% of people in the same way as the regression method. We can therefore depend on the classification method as reliable. The Ward method however finds that the majority of subjects are maximin types.

		Type classification (Ward's method)		
		maximin	efficiency	inequity averse
Percent choosing in line with property	maximin	75.1	63.6	43.9
	efficiency	67.2	76.4	27.1
	inequity av.	23.2	15.9	42.8
	n	51	41	8

Table 4.8

We have isolated several different norm types, and in the following we analyze behavior of the different types between the two conditions. First we look at whether the norm is stable from the 3rd to the 1st party, or in other words, whether we can really consider the norm to be a stable personal preference. In testable language, this means: do people in the 1st party condition choose more in line with the own norm than people with a different norm?

As Table 4.9 shows, the answer is yes. For the majority, subjects of a given type choose the majority of decisions in line with that type (as can be seen by looking at the rows of the table). The exception applies only to the minority classified as inequality averse. In that case, they nonetheless choose in line with inequality aversion more than those subjects classified into the other type categories.

		Average decisions in line with property below in 1st party condition		
		Maximin	Efficiency	Inequality
Type as classified by Ward's method from 3rd party decisions	Maximin	.6889	.6316	.2260
	Efficiency	.6225	.6732	.1865
	Inequality av.	.5901	.5742	.2453

Table 4.9

To further make sure that our type classification is depicting what we expect it to, in the appendix we include scatter plots of subjects' decisions sorted by type. For each type as classified by Ward's method, we plot the percentage of the time that subjects choose in line with a particular norm. All scatter plots show what we would expect of a proper classification, which is that people choose more in line with the property representative of their own type than with other properties.

For a further look into how people process norm information, we turn our attention now to hypothesis 3: that subjects will have higher reaction times in decisions when there is ego conflict with the personal norm than when there is ego conflict with an irrelevant norm. We look at what information people with different personal norms take into account. We expect that uninformative information, i.e. information about a property that is unimportant to a person as shown by choice data, is ignored. To gain a first view, we form a measure of how extreme or balanced one's norm orientation is. Those with a more balanced value profile, rather than those with a single rule governing decisions, should take longer in their decisions, since they have to take more considerations into account. We developed a measure of polarization, which is the Euclidian distance in the 3 social dimensions to the individual, standardized 'norm point'. In other words, if the personal norm were represented as a point in 3 dimensions by the individual coefficients ($\beta_{efficiency}$, $\beta_{maximin}$, $\beta_{inequality\ aversion}$), scaled so that each coefficient is a measure of the relative valuation among only these three properties, the polarization measure is the length of the line from the origin to the norm point. To put it precisely:

$$polarization = \frac{\sqrt{\beta_{efficiency}^{\ 2} + \beta_{maximin}^{\ 2} + \beta_{inequality\ aversion}^{\ 2}}}{\beta_{efficiency} + \beta_{maximin} + \beta_{inequality\ aversion}}$$

In this analysis and in all following analysis using the personal norm coefficients, we reset negative values of the personal norm coefficients to zero, since a negative value is an indication that the property is not valued. Naturally, the more a subject tends toward one particular social consideration, the higher the measure of polarization. We regressed reaction times on the degree of polarization. We would expect a negative coefficient (higher polarization means faster decisions). We do find that this is the case, however not significantly so.

VARIABLES	(1) lawnrt
polarization	-0.296
	(0.268)
Constant	1.122***
	(6.87e-07)
Observations	10,000
R-squared	0.003

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.10
OLS regression clustering on participant

To look further at this question, we look at the ego conflict with the individual social motivators. We include the individuals' norm importance by using the regression coefficient they obtained in our norm classification procedure (again, negative coefficients were set to zero). In the first column of Table 4.11, the first 3 variables are dummy variables which equal 1 when there is ego conflict with the 3 different social motivators. The second 3 variables are the individual norm importance coefficients (obtained from the norm classification) times the dummy variable for the social motivator. We expect that ego conflict with a social property which is important to a subject will increase reaction times, whereas ego conflict with an unimportant social property will not. That is, we expect for the dummy variables alone to be unpredictive of reaction times, whereas we expect the dummy multipled by the norm importance coefficient to be positive.

VARIABLES	(1) natural log of reaction time	(2) natural log of reaction time	(3) natural log of reaction time
decision number	-0.00581***	-0.00581***	-0.00581***
	(0)	(0)	(0)
efficiency conflict	0.146***	0.0924***	
	(0.000807)	(0.000223)	
maximin conflict	0.0273	0.00284	
	(0.403)	(0.903)	
inequality aversion conflict	-0.00182	0.00990	
	(0.903)	(0.502)	
efficiency norm coefficient * efficiency conflict	-0.110		0.164*
	(0.408)		(0.0607)
maximin norm coefficient * maximin conflict	-0.0139		0.0182
	(0.922)		(0.861)
inequality aversion norm coefficient * inequality aversion conflict	0.480***		0.135
	(0.00433)		(0.392)
choosey_3rd		0.0604***	0.0766**
		(0.00929)	(0.0106)
Constant	1.138***	1.131***	1.143***
	(0)	(0)	(0)
Observations	5,000	5,000	5,000
R-squared	0.136	0.137	0.133
Number of participant	100	100	100

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.11
Fixed effects regression clustering on participant

Looking at the first column, we see that ego conflict with efficiency matters for people regardless of their personal norm classification, i.e. the dummy variable is predictive and the dummy*norm importance coefficient is not. Ego conflict with maximin preferences has no effect regardless of the norm classification. The only social property for which the pattern is as we expected is inequality aversion: the dummy variable is unpredictive and the dummy*norm classification coefficient is positive and significant. In this category we have only 5 people, so the result should be taken cautiously. In columns 2 and 3, we set the two sets of variables (pure dummy for ego conflict with a social motivator vs. that same dummy weighted by personal norm coefficients) against each other and look at which performs better. In column 2 are the pure dummies, in column 3 the weighted ones. In each model we additionally include the dummy variable for a subject's actual choice of Y in the 3rd party condition; i.e. the subject's true personal norm, which results in ego conflict in the 1st party condition. We would expect two main results of this comparison. The first is that the first 3 variables in model 3 take away more predictive power from the "chose Y in 3rd party" dummy than the first 3 variables in model 2. Second, we expect model 3 to have a higher R^2 than model 2. We expect these results because it should be only the personally valued social properties that result in experienced conflict, not irrelevant social properties. As the two columns show, neither of our expectations are fulfilled. It is indeed model 2, which includes absolute ego conflict with the different social properties, that provides more predictive power. We conclude that despite observed preference in favor of the social properties we have enumerated, our evidence indicates that the structure of subjects' underlying preferences are fundamentally different from these aspects, and subjects' personal norms cannot be usefully classified according to these 3 concepts alone.

We obtain mixed evidence concerning hypothesis 3 that the automatic process is selfishness after controlling for 3rd party behavior. Piovesan & Wengstrom (2009) found that "egoistic subjects make faster decisions than subjects with social preferences." They additionally rule out the idea that it is the result of interindividual differences, concluding selfish decisions are faster. Naturally since it is a reaction time study, causality cannot be attributed to the selfish property of the choice. It could have been the result of another property of the decision, perhaps even ease of visual processing. We confirm their result that selfish choices are made faster, but challenge the notion that it is actually the result of selfish preferences. Indeed, decisions in favor of option X (the selfish option in the 1st party condition) are made faster in the 1st party condition, but they are also made faster in the 3rd party condition. The responses in favor of selfishness were no faster than those made in the third party decision, as seen by the interaction term in the first column of Table 4.12.

VARIABLES	(1) natural log of reaction time, all data	(2) natural log of reaction time, X > Y	(3) natural log of reaction time X ≥ Y
chose X	-0.174***	-0.113***	-0.0264
	(0)	(2.17e-07)	(0.256)
1st party	0.0334	0.00695	-0.0246
	(0.463)	(0.870)	(0.610)
chose X * 1st party	-0.00835	0.000631	-0.0426
	(0.855)	(0.989)	(0.441)
Constant	1.008***	1.012***	1.021***
	(0)	(0)	(0)
Observations	10,000	7,200	4,000
R-squared	0.017	0.008	0.005

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.12
OLS regression, clusters on participant. Model 1 uses all data, model 2 excludes cases where option X was strictly better for both recipients than option Y, model 3 excludes cases where option X was weakly better for both recipients than option Y.

The first column in Table 4.12 makes clear the importance of using proper control when using reaction time studies to draw conclusions about automatic and controlled processes in social decision making. Indeed on average reaction times in the two conditions are the same. The fact that reaction times are faster for option X even in the 3rd party condition implies that it is the properties of the decision other than selfish motivation which result in faster reaction times. Subjects answered control questions correctly, and the difference between the two conditions was made salient by locating the two types of decisions in different areas of the screen, so we are confident that this result is not one based on confusion of the subjects. Since order of decisions was randomized and there is no possibility for selfishness to have any influence on decisions in the third party condition, we concluded that the faster responses are the result of something other than the automaticity of selfish motivation, namely the cases in which the payoffs in option X were at least as high as those in option Y for both people. We repeat the regression from the first column of Table 4.12 but excluding the cases for which both payoffs in option X are greater or equal to those in option Y. As column 3 shows, when the cases where X is at least as good as option Y for both subjects are excluded, the option X is no longer chosen more quickly than Y. Importantly, decisions in favor of option X in the 1st party condition are still not meaningfully faster than those in the 3rd party. In the Piovesan & Wengström paper, it is also possible that the choice of the selfish option being faster is driven by the "no-brainer" decisions, as it is in our framework, as their options included both no-brainer options as well as situations with conflict between options.

With Tables 4.14 and 4.15, we further investigate reaction times given the decision made in the 3rd party, given a subject's choice in the 3rd party.

	natural log of 1st party reaction times		
	chose X in 1st	chose Y in 1st	Overall
chose X in 3rd	.8317 (n=3529)	1.1709 (n=180)	.8481 (n=3709)
chose Y in 3rd	.9706 (n=848)	.9883 (n=443)	.9766 (n= 1291)

Table 4.13

We will make brief mention of the first row of Table 4.14 (and column of the regression in Table 4.15) before continuing to the main point (the second row and column). The first row and column show the case where there is no ego conflict with the personal norm, that is when people chose X in the 3rd party condition. As is to be expected, the majority of the time people go on to also choose option X in the 1st party condition. We do however see some cases (180 of them) where people choose Y in the 1st party condition. Judging from the infrequency of this outcome (0.18% of total decisions) we believe it to be the result of error.

VARIABLES	(1) natural log of reaction time (chose X in 3rd party)	(2) natural log of reaction time (chose Y in 3rd party)
chose X in 1st party	-0.342***	-0.0173
	(2.54e-08)	(0.713)
Constant	1.174***	0.988***
	(0)	(0)
Observations	3,709	1,291
R-squared	0.023	0.000
Number of participant	100	100

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.14
Fixed effects regression, clusters on participant. Column one consists of decisions for X in the 3rd party condition, column two decisions for Y in the 3rd party

Looking at the second row of Table 4.14 and the second column of Table 4.15, we see that given there is an ego conflict, reaction times in the 1st party decision are no different based on whether they then chose in favor of selfishness or not. This is another way to look at hypothesis 4, and we again reject the hypothesis.

Although we reject the statement that selfish decisions in the 1st party are faster than nonselfish decisions, we do obtain evidence that selfish decisions are indeed the automatic process. We look at an individual's average change in reaction time between the 1st and 3rd parties, that is the average of the reaction time in the 1st party minus that in the 3rd party. We then compare this to the share of selfish decisions made in the 1st party condition, given that option Y was chosen in the 3rd party condition (which we will call "switch share" for short). If the selfish motivation is the automatic process as we expect, then for subjects with a higher proportion of switches (i.e., selfish choices in the 1st party condition), we would expect decisions to be faster in the 1st party decisions than in 3rd party decisions, since the automatic process drove the decision. For subjects with a low proportion of switches (i.e., nonselfish choices in the 1st party condition) we would expect decisions to be slower in the 1st party than in the 3rd party condition, since the controlled process overcame the automatic impulse. Therefore, we would expect overall a negative relationship between the switch share and the difference between 1st and 3rd party reaction times, and more specifically that for high switch shares the difference is below zero, and for low switch shares the difference is above zero. Indeed, what we find is that the greater (smaller) the switch share, the faster (slower) subjects became relative to the 3rd party condition. Moreover we find as expected that for high switch shares the difference is below zero, and for low switch shares the difference is above zero (see Figure 4.4). This indicates that subjects choosing Y in the 1st party condition were exerting more control, which takes time, than those choosing X. We therefore provide evidence in favor of hypothesis 4 that selfishness is the automatic process.

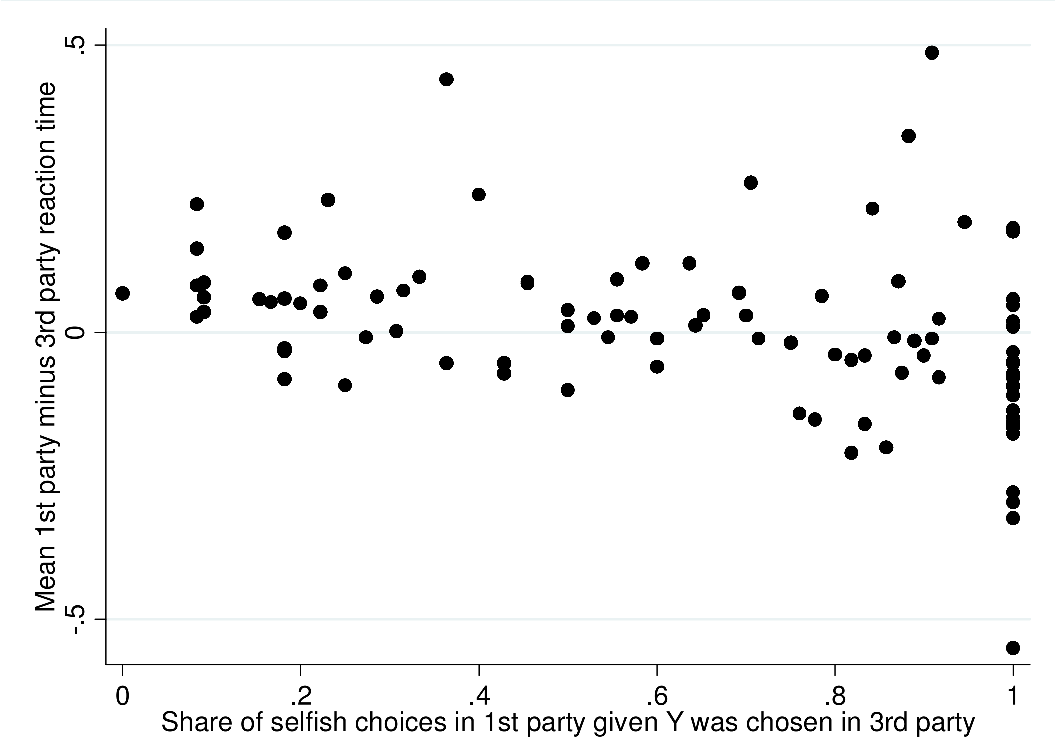


Figure 4.4
The natural log of reaction times are shown

The regression in Table 4.16 shows that indeed this effect is significant. In the regression we used the standardized switch share, so that the constant reflects the difference at the average switch share. This is simply to emphasize that 1st party reaction times are actually slower than 3rd party reaction times when the switch share is particulary low, and 1st party reaction times are faster than those in the 3rd party when the switch share is particularly high. Again, we still cannot conclude causality regarding this hypothesis, and further studies exogenously manipulating the dominance of automatic or controlled processes are necessary.

VARIABLES	(1) average ln(reaction time) in 1st party - average ln(reaction time) in 3rd party
share of selfish choices made given Y was chosen in the 3rd party (standardized)	-0.0531***
	(0.000134)
Constant	0.000264
	(0.985)
Observations	100
R-squared	0.139

pval in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.15
OLS regression

In addition to our findings above, we also observe an interesting phenomenon concerning cases of indifference. There are two decisions in which option X and option Y are identical in terms of efficiency, maximin, and inequality, but the two options directly benefit a different person. For example, the decision between 800 for participant C and 100 for participant D, or 100 for participant C and 800 for participant D. In the 1st party condition, there is a clear "scale-tipping" selfish motivation. However, in the 3rd party condition the decision is one of complete indifference, so long as one perceives the two participants C and D as equal. In this case we find that reaction times are much higher than average. In the first party condition this can be attributed to the conflict between selfish and altruistic motives. In the 3rd party however, each option is identically advantageous to one person and (relatively) disadvantageous to another. In the regression below we see that in the 3rd party condition, reaction times are even higher than they are in the 1st party condition, although the conflict is lower.

VARIABLES	(1) natural log of reaction time	(2) natural log of reaction time
indifference	0.226***	0.129***
	(0)	(0.00137)
3rd party		-0.0105
		(0.473)
indifference * 3rd party		0.193***
		(5.27e-05)
Constant	0.871***	0.876***
	(0)	(0)
Observations	10,000	10,000
R-squared	0.007	0.008

Robust pval in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4.16
OLS clustering on participant

We liken this result to the variant of Buridan's donkey (which is an entirely different decision paradox than the original): coming upon two identical haystacks, a donkey unable to differentiate and therefore choose between the two, starves to death. This result reinforces the idea that people make decisions based on differentiating between options, and consequently relative valuation, not on absolute value of those options. Moreover, the absence of a differentiating criterion makes the decision difficult, although indifference is in theory quite easy to solve through arbitrary choice. However our evidence indicates that the absence of differentiation criteria in this decision makes decisions take longer. As Dan Ariely (2008) puts it:

> We don't have an internal value meter that tells us how much things are worth. Rather, we focus on the relative advantage of one thing over another, and estimate value accordingly.

4.5 Conclusion

We used a combination of 3rd party and 1st party dictator games to study social norms and how they interact with selfish motivation. We recorded reaction times to assess the social conflict produced by the occurrence of different combinations of social motivating factors, as well as to study ego conflict with social motivators. Additionally we used them to have a first look at the automaticity of selfish motivation, based on the notion that automatic decisions are faster and controlled decisions slower.

We confirm that an increase in social conflict as measured by the three properties we identified results in increased reaction times. We also confirm that ego conflict with the personal norm increases reaction times, and that this effect is robust to controlling for absolute ego conflict with any one of the three social properties. This confirms that the personal norm captures individual preference heterogeneity. That said, absolute ego conflict with efficiency results in higher reaction times, aside from the effect of the personal norm.

We found that although it seems selfish decisions are made faster, the notion that this is the result of selfish motivation is unfounded. In a 3rd party decision-making condition, in which the point distributions were identical to those in the 1st party condition but relevant for two other participants and not the decision maker, decisions in favor of option X (the selfish option in the 1st party condition) were made faster as well. We found that this was the result of "no-brainer" decisions, i.e. those for which option X was (weakly) better for both recipients. In fact, we found that once no-brainer decisions are excluded, selfish decisions are not made faster. We argue that further studies must take this result into account when studying social preferences using reaction time analysis.

We found that most subjects care primarily about efficiency and maximin properties, with a minority caring primarily about absolute equality. In contrast to past studies, we found that the degree to which a person cares for a particular property is quite mixed and many people value several properties. We characterized these preferences for each individual, and found that our measure of the personal norm does not adequately capture the complexity of the true personal norm.

This study, as mentioned, provided a first look at automatic and controlled processes in social decision making. Future studies considering social decision making should incorporate exogenous manipulation of the influence of automatic and controlled processes, as well as use simple measures of altruism (like the dictator game) rather than more complex, reactionary measures (the ultimatum game). The importance of psychological considerations in decision making models is slowly becoming known to economists, and future studies should take their findings into account in order to gain an accurate understanding of human decision making.

Appendix

id	Xself	Xother	Yself	Yother
1	400	400	100	200
2	800	800	200	100
3	800	800	400	100
4	400	400	200	100
5	800	400	100	200
6	800	400	100	100
7	800	400	200	100
8	800	200	400	100
9	800	400	400	100
10	200	800	100	400
11	200	800	100	200
12	200	800	100	100
13	400	800	200	100
14	200	400	100	100
15	800	200	100	200
16	800	400	200	400
17	800	100	200	100
18	400	100	100	100
19	400	100	200	100
20	400	200	200	200
21	800	200	100	400
22	800	100	100	400
23	800	100	100	200
24	800	100	200	400
25	800	100	400	200
26	800	100	400	400
27	800	200	400	400
28	800	100	100	800
29	200	100	100	200
30	400	200	100	800
31	200	200	100	800
32	200	400	100	800
33	400	400	200	800
34	400	100	100	800
35	200	100	100	800
36	400	200	200	800
37	400	100	200	800
38	800	100	200	800
39	800	100	400	800
40	800	200	400	800
41	800	200	800	100
42	800	400	800	100
43	800	400	800	200
44	800	800	800	400
45	200	400	200	100
46	200	800	200	100
47	100	800	100	400
48	100	800	100	200
49	200	800	200	400
50	100	800	100	100

Table A.1: Decision amounts. Option X is the selfish option

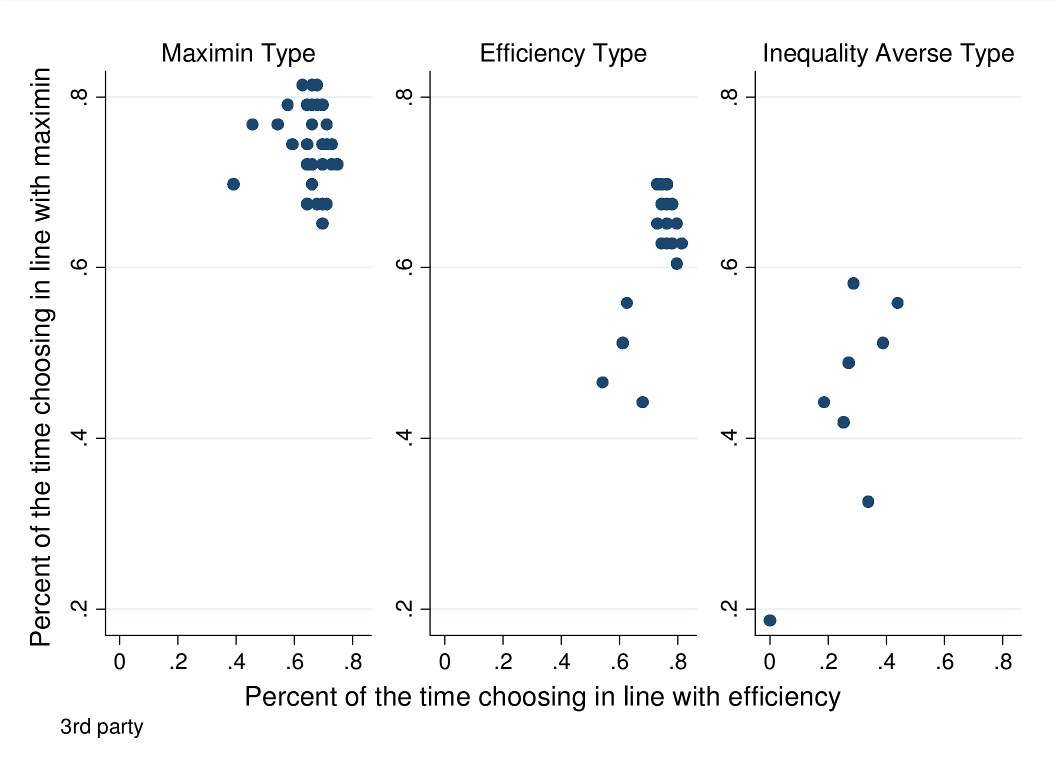


Figure A.1

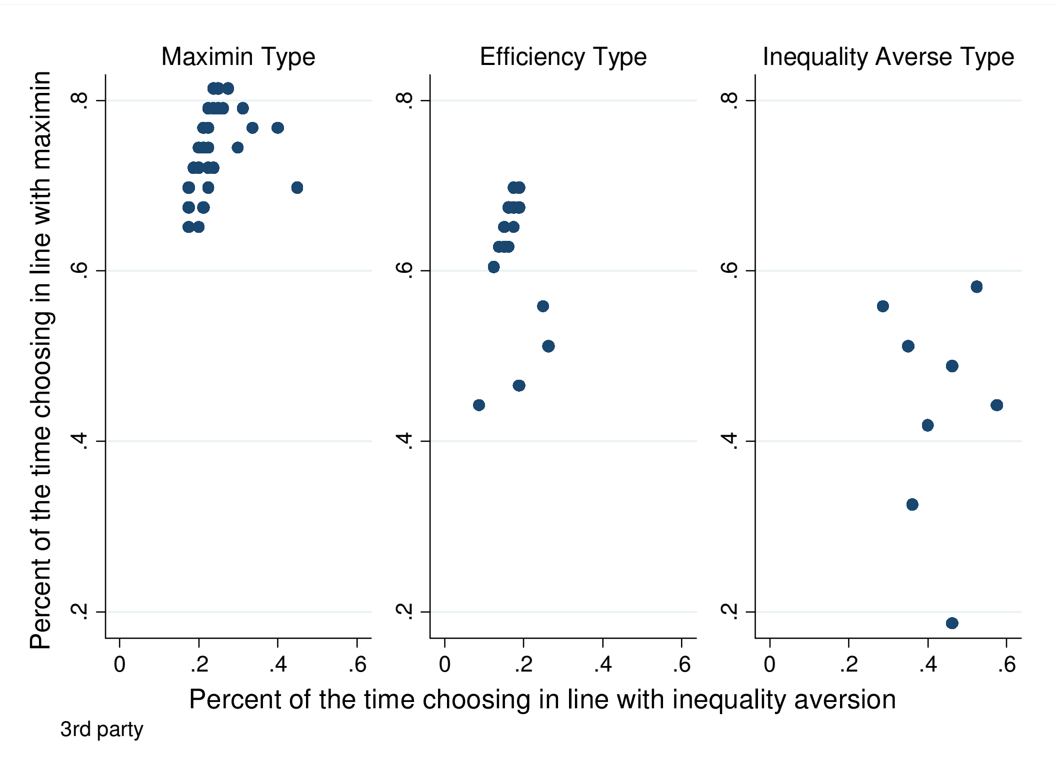


Figure A.2

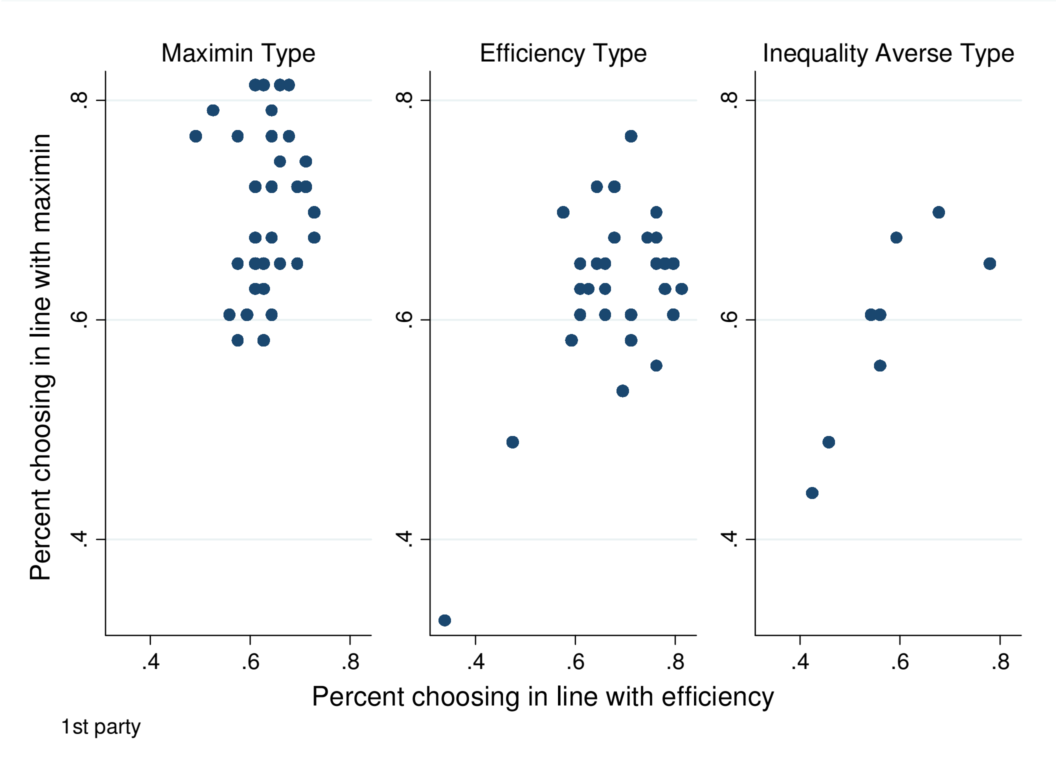


Figure A.3

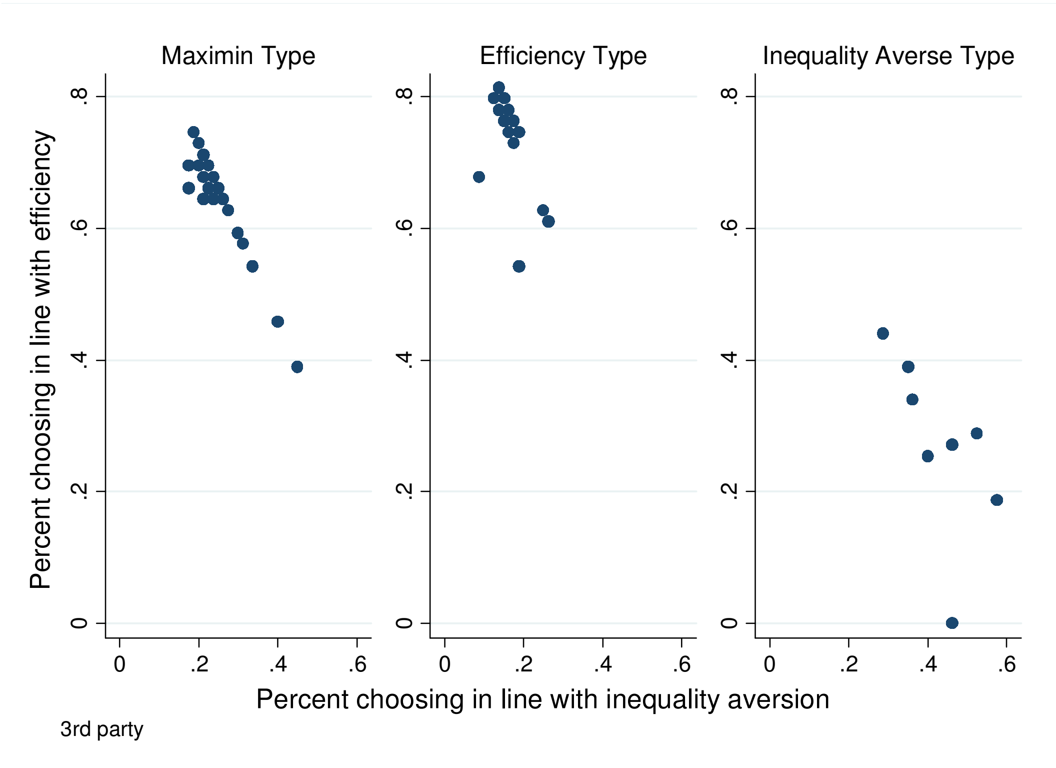


Figure A.4

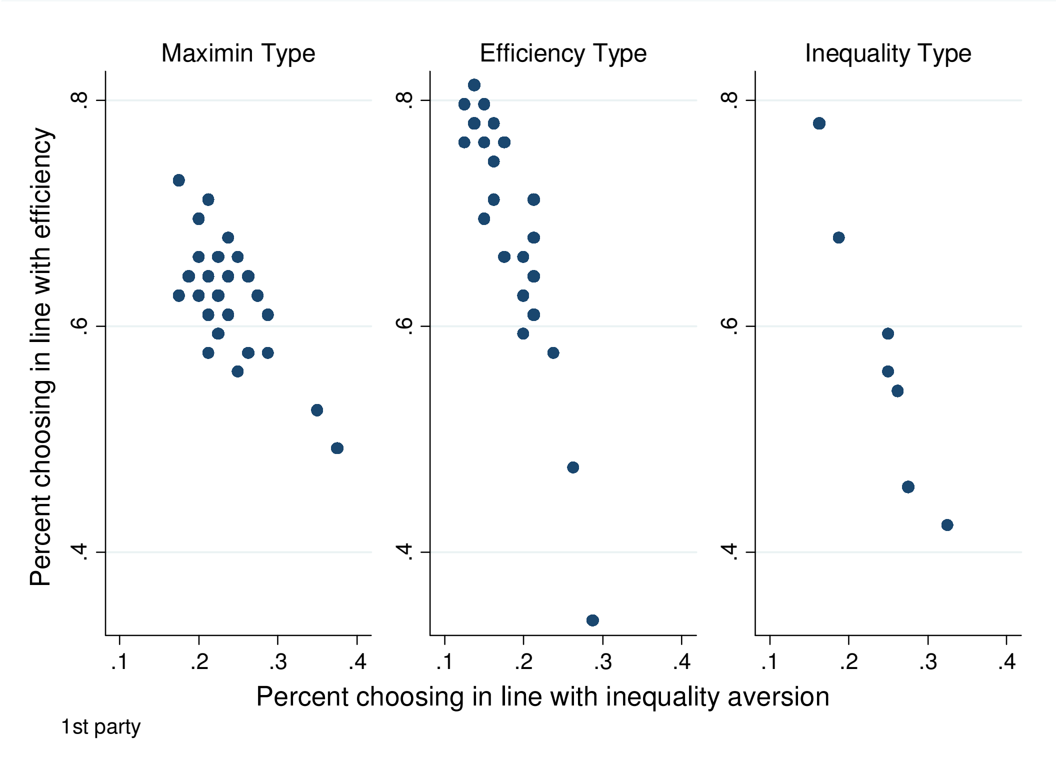


Figure A.5

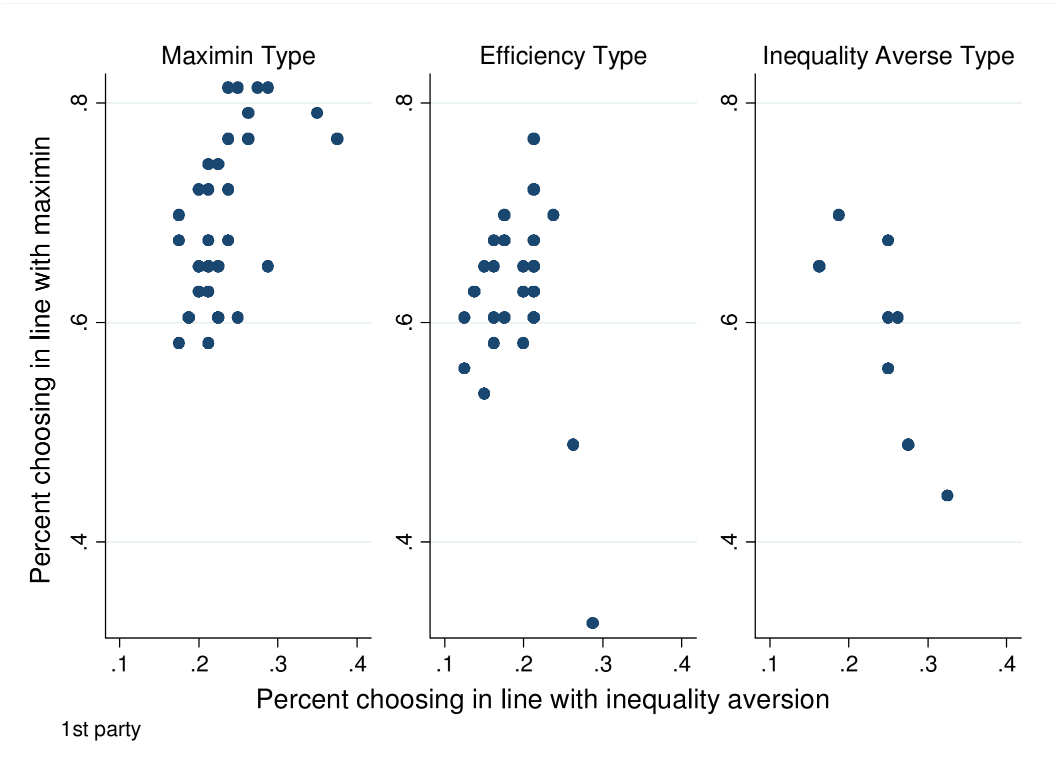


Figure A.6

Instructions

You are now taking part in an economics experiment that is financed by several research funding agencies. The instructions that you have received from us are exclusively your private information. **During the experiment communication is strictly forbidden.** If you have questions, please direct them to us. Failure to comply with this rule will lead to exclusion from the experiment and all payments.

In this experiment there are two types of decisions, and two people for whom the decisions are relevant in each case. In the first type there is you ("participant A") and "participant B", and in the other " participant C" and "participant D".

You will make 50 decisions in each of the two types of decisions. In total that is 100 decisions. In each decision two participants are in a group, either you and a participant B, or a participant C and a participant D. If it concerns you and participant B, you decide about amounts for you yourself and another participant. If it concerns participant C and participant D, you decide about amounts for **two other participants**, meaning that your payoff does not depend on your decision. At the end one of the 100 decisions will be drawn using a 10-sided die, and this decision will then be relevant for payment.

During the experiment we will not use euros, but points. Your total income will first be calculated in points. The total points you earn in the experiment will be converted to euros at the end with the following exchange rate

100 points = 2 euros.

At the end of today's experiment you will receive from us the number of points you earned in the experiment, plus your starting money of 3 euros, in cash.

On the following pages we will explain the exact procedure of the experiment.

Decisions over payments for you and participant B

You will choose one of two point distributions. These distributions determine how many points you and participant B receive.

Example: With one distribution you get 100 points and participant B gets 500 points; with the other distribution you get 200 points and participant B gets 300 points. As you can see with this example, the sum of points is not necessarily the same in both distributions.

Decisions over payments for participant C and participant D

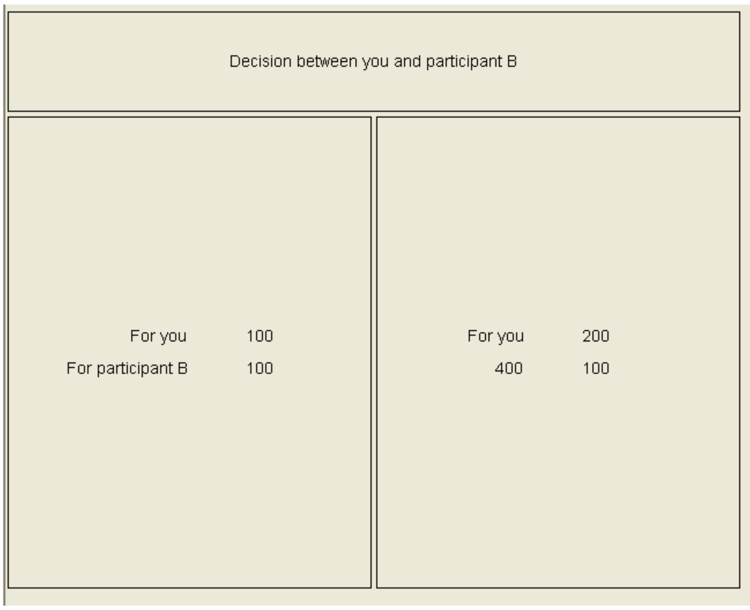
You will choose one of two point distributions. These distributions determine how many points participant C and participant D get.

Example: With one distribution participant C gets 100 points and participant D 500 points; with the other distribution participant C gets 200 points and participant D 300 points. As you can see in this example, the sum of points is not necessarily the same in both distributions.

Procedure in detail

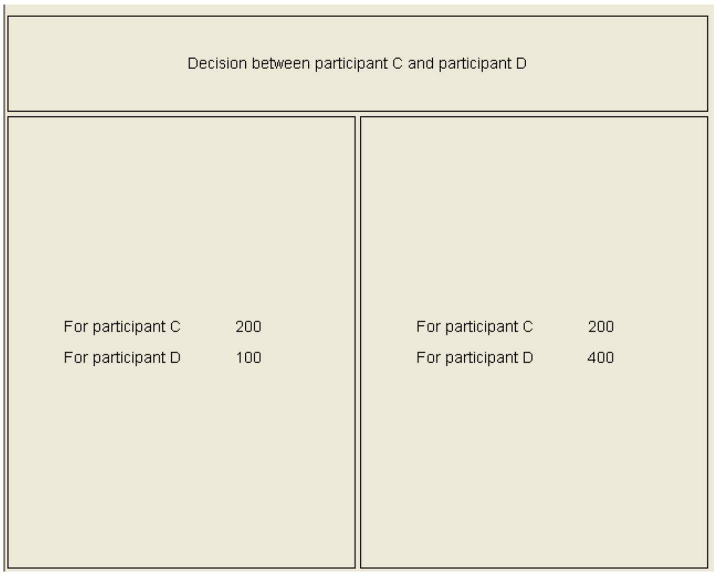
Decisions over payments for you and participant B

In all decisions you must choose one of the two point distributions. The two distributions will be shown on the screen as in the hypothetical example below. In the example shown you have to choose between the following distributions: in the distribution on the left side you get 100 points and participant B gets 100 points; in the distribution on the right side you get 200 points and participant B gets 400 points. You make your decision with the keyboard. If you want to choose the distribution on the left side for example (in the example 100 points for both), press the “f” key; if you want to choose the distribution on the right side (in the example 200 points for you and 400 points for participant B), press the “j” key. After every input an intermediate screen will appear. To proceed to the next input you must click the space bar.



Decisions over payments for participant C and participant D

When you must decide over payments between C and D, both distributions will be shown on screen as in the example shown below. In the example shown you have to choose between the following distributions: in the distribution on the left side participant C gets 200 points and participant D 100 points; in the distribution on the right side participant C gets 200 points and participant D 400 points. You will make this decision with the keyboard too. If you want to choose the distribution on the left side (in the example 200 points for participant C and 100 points for participant D), press the "f" key; if you want to choose the distribution on the right side (in the example 200 points for participant C and 400 points for participant D), press the "j" key. After every input an intermediate screen will appear. To Nach jeder Eingabe erscheint ein Zwischenbildschirm. To proceed to the next input you must click the space bar.



The two types of decisions appear in a random order. You will be informed on screen whether you are making a decision that affects you and participant B, or if you are making a decision that affects participant C participant D. To make it as clear as possible which type of decision is happening, the decisions between distributions for you and participant B will be shown one part of the screen and the decisions between distributions for participant C and participant D on another part of the screen.

The decisions appear one after another. After every input an intermediate screen will appear. To go on to the next input, you must press the space bar and dwait until the decision screen appears.

At the end the decision that is relevant for payment will be drawn out of the 100 decisions made with the help of a 10-sided die. You will see a screen that is structured as follows.

Code	Decision Number	Code	Decision Number	Code	Decision Number	Code	Decision Number
1	19	26	55	51	83	76	9
2	63	27	36	52	100	77	66
3	74	28	56	53	42	78	47
4	60	29	26	54	50	79	11
5	92	30	85	55	24	80	53
6	27	31	29	56	54	81	37
7	23	32	15	57	65	82	1
8	91	33	17	58	18	83	98
9	80	34	81	59	38	84	62
10	86	35	93	60	13	85	4
11	3	36	48	61	90	86	75
12	61	37	72	62	71	87	44
13	34	38	7	63	30	88	25
14	73	39	35	64	20	89	52
15	94	40	39	65	6	90	46
16	21	41	57	66	10	91	89
17	51	42	84	67	97	92	69
18	77	43	5	68	76	93	87
19	32	44	12	69	79	94	67
20	45	45	95	70	99	95	8
21	78	46	88	71	31	96	96
22	28	47	64	72	16	97	49
23	41	48	40	73	33	98	22
24	58	49	59	74	70	99	68
25	43	50	2	75	82	100	14

OK

Each decision corresponds to a code number which will determine the drawn decision relevant for your payment. The 10-sided die will therefore be rolled twice, once for the first digit, and once for the second digit of the code number. If for example a 9 and then a 7 is rolled, then decision number 49 will be relevant. (If two zeros are rolled then code number 100 is relevant). The same decision will be paid for every participant, even though the order of the decisions can be different for every participant.

Next, a coin will be used to determine which roll you will be paid for. A decision between A and B or a decision between C and D could have been selected. Therefore there are different possibilities which could be relevant for your payment:

If a decision between A and B is drawn:

Possibility 1: You will be paid as participant A

Possibility 2: You will be paid as participant B

If a decision between C and D is drawn:

Possibility 3: You will be paid as participant C

Possibility 4: You will be paid as participant D

With possibility 1) and 2) you are in a group with exactly one other participant. If possibility 1 is drawn with the coin flip, you will be paid based on your own decision. If possibility 2 is drawn, you will be paid as participant B based on the decision of your fellow group member.

In possibility 3) and 4) you will be paid as participant C or D for the decision of another participant. You are still in a group with one other participant, but the decision that is relevant for your payment is made by a participant who is **not** in your group. If possibility 3 is drawn, you will be paid as participant C based on the decision of another participant, and your fellow group member from your own group as participant D. If possibility 4 is drawn, you will be paid as participant D and your fellow group member from your own group as participant C. Likewise, your decision can be relevant for the members of another group.

You will not learn the identity of your fellow group member. You will learn neither whom you are in a group with (possibility 1 or 2) nor for what two other participants your decision could be relevant (possibility 3 or 4). Likewise the other people will not learn that they are in a group with you or that their decision is relevant for you.

Control questions

1. Imagine you can choose between “100 points for yourself and 400 points for B” and “200 points for yourself and 100 points for B”. You decide for the first variant. Your fellow group member decides for the second variant. This decision is drawn at the end as relevant for payment, and you are paid as participant A.

   How high is your income in points?

   How high is participant B’s income in points?

2. Imagine you can choose between “100 points for yourself and 400 points for B” and “200 points for yourself and 100 points for B”. You decide for the first variant. Your fellow group member decides for the second variant. This decision is drawn at the end as relevant for payment and you are paid as participant B.

   How high is your income in points?

How high is participant B's income in points?

3. Imagine you can choose between "200 points for C and 800 points for D" and "400 points for C and 100 points for D". You decide for the first variant. This decision is drawn at the end as relevant payment.

   How high is participant C's income in points?

   How high is participant D's income in points?

   Does this decision have any influence on your personal payment?

4. Imagine the decision options are between "200 points for C and 800 points for D" and "400 points for C and 100 points for D". This decision is chosen at the end as relevant for payment, and the other participant that made the relevant decision decided for the second variant. You are paid as participant C.

   How high is your income in points?

   How high is your fellow group member's income in points?

   Does this decision haven any influence on the payment of the participant who made the decision?

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Erklärung

Ich erkläre hiermit, dass ich die vorliegende Arbeit mit dem Titel

Economic Experiments on Impulsive Urges, Control, and Irrationality

ohne unzulässige Hilfe Dritter und ohne Benutzung anderer als der angegebenen Hilfsmittel angefertigt habe. Die aus anderen Quellen direkt oder indirekt übernommenen Daten und Konzepte sind unter Angabe der Quelle gekennzeichnet. Weitere Personen, insbesondere Promotionsberater, waren an der inhaltlich materiellen Erstellung dieser Arbeit nicht beteiligt.[8] Die Arbeit wurde bisher weder im In- noch im Ausland in gleicher oder ähnlicher Form einer anderen Prüfungsbehörde vorgelegt.

Konstanz, den 9. November 2012 ______________________________

(Katharine Bendrick)

[8] Siehe hierzu die Abgrenzung auf der folgenden Seite.

Abgrenzung

Ich versichere hiermit, dass ich die Kapitel 1 bis 4 der vorliegenden Arbeit ohne unzulässige Hilfe Dritter und ohne Benutzung anderer als der angegebenen Hilfsmittel angefertigt habe.

Kapitel 1: *Projection Bias: The Price for Food Craving* entstammt einer gemeinsamen Arbeit mit Herrn Prof. Dr. Urs Fischbacher (Universität Konstanz) und basiert auf meiner Idee. Das experimentelle Design wurde gemeinsam erarbeitet, die Programmierung, Durchführung des Experiments und die Analyse habe ich selbstständig durchgeführt. Die erste Version des Artikels entstammt meiner Hand und wurde gemeinsam überarbeitet.

Kapitel 2: *Battling Impulses: Intertemporal Choice in the Short Term* entstammt ebenfalls einer gemeinsamen Arbeit mit Herrn Prof. Dr. Urs Fischbacher. Es basiert auf meiner Idee. Das experimentelle Design wurde gemeinsam erarbeitet. Programmiert, durchgeführt und analysiert wurde das Experiment durch mich. Die erste Version des Artikels entstammt meiner Hand und wurde gemeinsam überarbeitet.

Kapitel 3: *Handing Over the Reins: On the Social Nature of the Illusion of Control* habe ich selbstständig ohne Hilfe Dritter erarbeitet.

Kapitel 4: *Social Decision-Making Processes* entstammt einer gemeinsamen Arbeit mit Herrn Prof. Dr. Urs Fischbacher. Es basiert sich auf einer Idee von Herr Fischbacher und das experimentelle Design wurde gemeinsam erarbeitet. die Programmierung, Durchführung des Experiments und die Analyse habe ich selbstständig durchgeführt. Die erste Version des Artikels entstammt meiner Hand und wurde gemeinsam überarbeitet.

Konstanz, den 9. November 2012 ______________________________

(Katharine Bendrick)